

ABORTION LEGALIZATION AND CHILD LIVING CIRCUMSTANCES: WHO IS THE "MARGINAL CHILD"?

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We examine the impact of increased abortion availability on the average living standards of children through a selection effect. Would the marginal child who was not born have grown up in different circumstances than the average child? We use variation in the timing of abortion legalization across states to answer this question. Cohorts born after legalized abortion experienced a significant reduction in a number of adverse outcomes. We find that the marginal child would have been 40–60 percent more likely to live in a single-parent family, to live in poverty, to receive welfare, and to die as an infant.

Access to abortion is one of the most contentious public policy issues facing the United States today. The period since the legalization of abortion under the *Roe v. Wade* decision of 1973 has been marked by incessant debate over the appropriate government financing and legal status of abortions. Meanwhile, pregnancy resolution through abortion is a very common outcome in the United States; roughly 25 percent of all pregnancies are aborted [Ventura et al. 1995]. As a result, major changes in abortion access could have substantial effects on the birthrate. Indeed, Levine, Staiger, Kane, and Zimmerman find that the legalization of abortion in the early 1970s led to an 8 percent reduction in the birthrate.¹

To the extent that abortion access reduces the size of a birth cohort, one question of particular interest from a policy perspective is its effect on the living circumstances of the children who are born. Inherently, this is a question about selection: would those children who were not born because of abortion access have lived in different circumstances than the average child in their cohort? For example, if the women who terminate their births would have borne children into families that were single female-headed or poor or both, then improved abortion access could lead to a

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1. Although Kane and Staiger [1996] and Levine, Trainor, and Zimmerman [1996] do not find increases in the birthrate from more modest changes in abortion access.

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reduction in the rate of child poverty and welfare utilization.² Yet, there is little direct evidence on the effect of abortion access on child living circumstances.

The key to answering this question is understanding how abortion influences the *selection* of which women carry pregnancies to term. A priori, the direction and size of selection is unclear. On the one hand, if women use abortion to avoid bearing children into adverse circumstances, positive selection would result: the living circumstances of the marginal child are not so good as those of the average child, so that increased abortion access will raise average living standards of the children who are born. On the other hand, negative selection would result if, for instance, the most disadvantaged women are constrained in their abortion access, either geographically or financially.³ In this case, the living circumstances of the marginal child would be more advantageous than those of the average child, and abortion legalization may reduce the living standards of those children who are born. Ultimately, the direction of selection, and the effects on subsequent child living circumstances, is an empirical question.

In this paper we examine the effect of the largest change in abortion availability in the United States, increased access in the early 1970s through *Roe v. Wade* and comparable state laws, on the living circumstances of the cohorts of children born in these years. More specifically, following Levine et al. [1996], we note that *Roe v. Wade* followed on the heels of abortion legalization in five states around 1970. This generates two "natural experiments" for analyzing the effect of abortion access: the change in these five states, versus the remainder of the country, beginning in 1971 (incorporating a gestation lag after the 1970 legal changes), and the change for the remainder of the country, versus these five states, beginning in 1974 following the 1973 *Roe* decision. The large reduction in the number of births associated with legalization, as documented by Levine et al., provides the impetus for focusing on the resultant living standards of the remaining cohort of children.

2. This point has been recognized, in reverse, in the recent debates over welfare reform, as opponents of abortion expressed concern that reducing welfare generosity could lead single (potential) mothers to terminate their pregnancies.

3. In fact, there appears to be a strong geographic correlation between access and income: in 1980, 27.2 percent of counties with poverty rates of 15 percent or below had abortion providers, while only 10.6 percent of counties with poverty rates above 15 percent had providers (author's tabulation of 1980 census data, using data on abortion provider location from Kane and Staiger [1996]).

We carry out our analysis using the 5 percent Public Use Micro Sample (PUMS) of the 1980 Census. The PUMS data allow us to observe the living circumstances in 1980 for cohorts of children by state of birth and year of birth. In addition, the PUMS provides sufficiently large samples to identify the relatively small expected effects on average living standards. We also use data on birthrates and birth outcomes available by state and year of birth from the U. S. Vital Statistics.

We find evidence of sizable positive selection: the average living circumstances of cohorts of children born immediately after abortion became legalized improved substantially relative to preceding cohorts, and relative to places where the legal status of abortion was not changing. Our results suggest that the marginal children who were not born as a result of abortion legalization would have systematically been born into less favorable circumstances if the pregnancies had not been terminated: they 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.

I. REVIEW OF RELATED LITERATURE

Several types of studies have either directly or indirectly addressed the issue of selection in response to changes in abortion access. The first is state-level analyses of abortion, which regress abortion rates on state characteristics. Such analyses consistently find a strong positive correlation between abortion rates and state income per capita (i.e., Blank, George, and London [1994]). Even as states get richer and increase abortion access, however, there is no obvious implication for which women within the states are obtaining abortions, so that these studies offer little insight into the process of selection.

The second type of study is micro-data analyses of the abortion decision, focusing on the characteristics of women that are correlated with their decision to terminate their pregnancies. These studies yield mixed results: abortion among teens is more likely if they are unmarried [Joyce 1988], but among unmarried teens it is positively correlated with their mother's education and with living with both parents [Cooksley 1990; Lundberg and Plotnick 1995]. But these studies do not necessarily have implications for average living standards, since there may be selection

along unobservable dimensions which counteracts, or augments, the selection that is observed. Moreover, most of these studies suffer from the notoriously poor quality of data on abortion that are available in micro-data surveys, particularly the National Longitudinal Survey of Youth (NLSY), the data used by both Cooksley and Lundberg and Plotnick. Based on national comparisons with administrative data on abortions, Jones and Forrest [1992] find that only 40 percent of abortions are reported in the NLSY, and that underreporting of abortions is largest for disadvantaged groups such as nonwhites and unmarried women. If the quality of the abortion data varies systematically with living circumstances, estimates of selection using micro data will be biased.

Third, and more closely related to our approach, is studies of the effect of abortion availability on infant outcomes. A large number of studies demonstrate that there is positive selection on fetal health: abortion access, as measured by number of providers or abortion rates, is correlated with a sizable improvement in infant outcomes such as low birth weight or neonatal mortality [Grossman and Jacobowitz 1981; Corman and Grossman 1985; Joyce 1987; Grossman and Joyce 1990; Currie, Nixon, and Cole 1996]. But this evidence may not be pertinent for assessing the relative living circumstances of cohorts who do and do not have access to abortion; the pregnancies that are terminated may have been those of well-off women who had unhealthy fetuses, so that selection on living circumstances is negative even as selection on fetal health is positive. Moreover, the effects of marginal variation in provider access may be different than the large changes inherent in legalization.⁴

Perhaps the most relevant evidence comes from Levine et al. [1996], who employ the same quasi-experimental framework used here to identify the effect of abortion legalization on birthrates. They find that births to teens, women over age 35, nonwhite women, and unmarried women fell the most in response to abortion legalization. These findings do not tell a completely consistent story regarding the anticipated effect on children's

4. This follows from the fact, noted above, that Levine et al. [1996] find much larger effects of legalization on birthrates than do other studies of variation in Medicaid funding restrictions [Levine, Trainor, and Zimmerman 1996] or provider access [Kane and Staiger 1996]. Grossman and Jacobowitz [1981] do include a variable for abortion reform in 1970 in their cross-sectional neonatal mortality regression; but they rely on a broader set of repeal states (see the discussion below) and do not exploit the changes in legalization over time.

future living circumstances. For instance, the smaller share of births born to teens or single mothers may improve average living standards, but the smaller share born to older mothers may reduce them. Moreover, within each group, those mothers choosing to abort may have been positively or negatively selected. The results in Cooksley [1990] and Lundberg and Plotnick [1995] actually suggest this: the set of teen mothers who choose to abort in response to legalization come from higher income families. We therefore turn to a more direct analysis using the 1980 census data.

II. EMPIRICAL STRATEGY

We take a direct approach to measuring the effect of abortion access on the living circumstances of subsequent cohorts of children, relying on the major changes in the legal status of abortion across the United States in the early 1970s for identification. 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, there was a repeal of antiabortion laws: New York, Washington, Alaska, and Hawaii. 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.⁵ Following the 1973 Supreme Court decision in *Roe v. Wade*, abortion became legal in all states.

This legislative history enables us to employ a "differences-in-differences" strategy to estimate the effect of abortion legalization on average living circumstances, following Levine et al. [1996]. The basic idea is to compare differences over time in the living circumstances of cohorts born in the "repeal" states (the five states listed above), relative to cohorts born in other states. Levine et al. show that there was little effect of the more modest reforms in

5. Furthermore, evidence indicates that legal abortion was widely available in California beginning in 1970, with legal abortion rates among women living in California being comparable to rates for women living in New York. See Potts, Diggory, and Peel [1977] pp. 75-77, 149, and see Garrow [1994] pp. 377-380, 410-411, and 457, and references cited in footnotes 25 and 76 from Chapter 7.

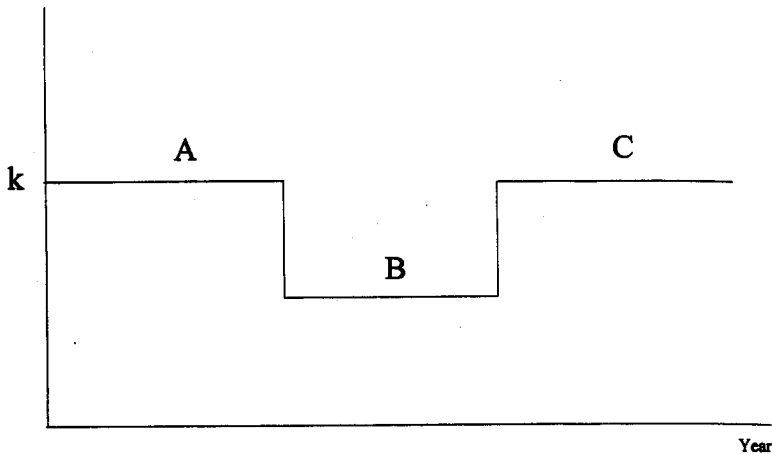
Difference in
Poverty Rates

FIGURE I

Hypothetical Difference in Poverty Rates between Cohorts Born in Repeal and Nonrepeal States

other states on birthrates, so we include these states in our control group.⁶ We depict this strategy hypothetically in Figure I, for a typical measure of living circumstances, the percentage of the cohort living in poverty. We consider the case of positive selection, whereby abortion legalization improves living standards. The line in this figure is the *difference* in poverty rates between the cohorts born in repeal and nonrepeal states over time. Time in this figure refers to the year of birth of the cohort.

In region A, which is the 1970 and earlier cohorts, there is some constant difference in poverty rates between these two sets of states (k), due to underlying differences in the population of residents. Then, under our assumption of positive selection, the poverty rate falls in the repeal states relative to other states for cohorts born after 1970, as abortion becomes widely available in the repeal states. That is, since the children who are aborted were those who would have lived in the most adverse circumstances, the average living standards of the remaining children improve. Thus, in region B the poverty rate is (relatively) low among cohorts born in repeal states from 1971 to 1973. In 1973 abortion

6. Our results are very similar if we exclude this group of states from our controls, or if we incorporate them into our analysis as a second treatment group.

becomes legal nationwide. At that point, poverty rates are once again equalized across these two sets of states in region *C*, as nonrepeal states experience a similar improvement in average living circumstances following legalization of abortion. Once abortion is legal nationwide, the difference between the poverty rates in these two states returns to its steady-state (prelegalization) value.

This stylized depiction suggests two simple tests of the effect of legalization on average living standards. The first is to compare region *B* with region *A*: if there is in fact positive selection, as is depicted here, living circumstances should improve for cohorts born after 1970 in the repeal states, relative to the nonrepeal states. The second test is to compare region *C* with region *B*: once again, under the hypothesis of positive selection, relative living circumstances should improve for cohorts born after 1973 in the nonrepeal states (or relatively deteriorate in the repeal states). These are the basic tests that we carry out below.

Reality, of course, deviates in at least three significant aspects from this stylized depiction. First, in the absence of abortion reform, there may have been underlying trends in living standards across these sets of states. For example, there may have been a falling poverty rate in the repeal states, relative to other states, for reasons other than abortion legalization. This would potentially confound our first test, since there would be a relative decline in adverse living circumstances over time regardless of abortion policy. As we discuss below, this problem is exacerbated in the census data by our use of a single 1980 cross section, which confounds aging and time effects.

We deal with this problem in three ways in our empirical work. First, we introduce distinct quadratic time trends for each state in our analysis, allowing us to distinguish our effects from a (parametric) trend. Second, we control for other state-specific time-varying factors in the year of birth which could have affected selection into the cohort. Finally, by finding consistent results from *both* of the tests described above, we can rule out spurious trends unless they reverse for some reason after period *B*.

A second shortcoming of this framework is that the "bounceback" from segment *B* to segment *C* may occur less rapidly than does the reduction in adverse circumstances from segment *A* to segment *B*. By being first movers in increasing abortion access, the five repeal states revealed their willingness to make abortion available. The states that were forced into legalization by *Roe v.*

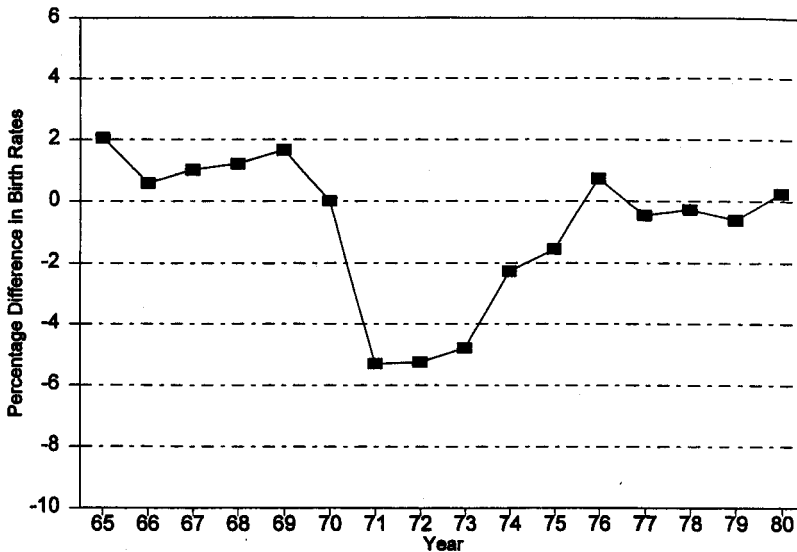


FIGURE II

Percentage Difference in Birthrates between Repeal States and Nonrepeal States

Source: Levine et al. [1996], Figure 3. (Percentage Differences Are Normalized to Equal Zero in 1970.)

Wade may have been less positively disposed toward abortion availability, so even *de jure* legalization may not have implied a large increase in *de facto* access.⁷ Moreover, the women who wanted abortions most in the nonrepeal states may have traveled to the repeal states to obtain them, so that the shift in use of abortion after *Roe v. Wade* was muted.

In fact, this view is supported by the evidence on abortion legalization and birthrates in Levine et al. [1996]. Their results for the effect of legalization on birthrates are depicted in Figure II. This figure graphs the raw difference in birthrates for all women of childbearing age between repeal and nonrepeal states over time (normalized so that the difference in 1970 is set equal to zero). Following legalization of abortion in the repeal states in 1970, birthrates in these states fell precipitously relative to birthrates in other states. There is then a corresponding fall in birthrates in the nonrepeal states after 1973, so that by 1976 relative birth-

7. By 1976 the nonrepeal states continued to have lower abortion rates and a lower percentage of women living near an abortion provider as compared with repeal states. See Levine et al. [1996], Table 4.

rates were once again equalized. However, the bounceback is slow, only reducing the gap somewhat by 1974–1975. Levine et al. present regression results that support the narrative above: relative birthrates fell precipitously in the repeal states during 1971–1973, recovered to some extent by 1974–1975, and fully recovered by 1976–1980. Overall, abortion legalization appears to be correlated with roughly a 6 percent decline in relative birthrates, which occurred immediately in the repeal states and more gradually in the nonrepeal states.⁸

A third shortcoming of this framework is that it assumes that changes in birth patterns brought about by abortion legalization do not have spillover effects onto those who would have been born anyway. However, all children born around the time of abortion legalization may directly benefit from this policy change. In particular, abortion legalization may reduce family size, and this may lead to improvements in living circumstances for all children in the family through either a mechanical reduction in poverty status or a quantity/quality trade-off as in Becker [1981].⁹ These family effects may affect all cohorts, even those born before legalization. To the extent that smaller families from abortion legalization improve the living situation of children born prior to legalization, we will tend to understate the overall effect of abortion legalization on child outcomes.

On the other hand, our empirical framework will tend to isolate the effect of selection on child outcomes from family size effects. Selection effects should appear discontinuously at the time of legalization, while family size effects will appear more gradually and tend to be differenced out in our framework. Of course, to the extent that abortion legalization has larger effects on family size for cohorts born after legalization as compared with cohorts

8. However, note that the effect of abortion legalization on *relative* birthrates understates the total effect of abortion on birthrates, since birthrates may have fallen in nonrepeal states as many women traveled to repeal states to obtain abortions. Levine et al. [1996] estimate that the total effect on birthrates was 8 percent, based on a comparison of repeal states and states that were more than 750 miles from repeal states.

9. Alternatively, falling cohort sizes may reduce congestion costs in some public resources like education: if teachers unions or other rigidities make it difficult to adjust the number of teachers as cohort size falls, then there will be fewer students per teacher and potentially improved student outcomes as a result. Indeed, changes in cohort size have been used in two recent papers [Hoxby 1997; Angrist and Lavy 1997] to identify the effect of class size on student outcomes. However, it is difficult to construct congestion-type stories for the outcomes that we examine, household characteristics or birth outcomes; for welfare receipt, such stories would presumably bias *against* our findings if states maintain their welfare budgets in the face of a declining cohort.

born before legalization, we will tend to overstate the effects of selection alone on child outcomes. In other words, some of the improvements in child outcomes following abortion legalization may be due to legalization's impact on family size rather than due to differences between the marginal and average child (selection).

III. DATA AND REGRESSION FRAMEWORK

Data

Our primary data for this exercise are the 5 percent Public Use sample of the 1980 Census.¹⁰ These data have two important advantages for our purposes. First, they include information on state of birth for each child. State of birth, rather than current state of residence, is necessary for correctly determining how abortion laws at the time of birth affected selection into the cohort. Using state of residence in 1980 would potentially bias our findings if there is selective migration; for example, if those children born into poor living circumstances because abortion is not available are more likely to move, using state of residence would understate the impact of making abortion available. Therefore, all of our analyses define cohort according to the child's state and year of birth.

Second, the census samples are the largest available for analysis of living circumstances. This is important because, even with the fairly large change in birthrates documented by Levine et al. [1996], changes in *average* living circumstances of a birth cohort will only change quite modestly.¹¹ For example, consider the effects of abortion legalization if there is positive selection. Suppose that the baseline poverty rate in a birth cohort is 20 percent, and that the marginal births that do not occur because abortion is available would have been 50 percent more likely to live in poverty than the average birth in a cohort. This implies a

10. Ideally, we could use the 1990 Census to consider the effects of abortion access for later outcomes (i.e., schooling completion, fertility decisions, labor force behavior) of these cohorts. Unfortunately, our attempt to use these data was not fruitful. The confounding of aging and time effects, described below, in those data were much more serious than in 1980, perhaps because the ages of the relevant cohorts in 1990 (roughly ages 13–20) are ages of extensive transition in living circumstances, schooling, and work. As a result, parametric trends were insufficient to capture trends in the relative patterns of behavior across states.

11. In fact, the selection effect we are interested in is considerably smaller than the dramatic shifts in child's living circumstances, such as the share of children living in single-parent households, taking place over this period. The methodology we employ, described below, is designed to abstract from the general social changes taking place and to focus on the effect of abortion legalization itself.

fall in the share of children living in poverty of about a percentage point from even a 10 percent drop in the birthrate, a quite small change on average despite this sizable positive selection effect on the margin. Thus, very large sample sizes are required to identify even sizable selection effects.

The major cost to using the census for our analysis, as opposed to some source of annual data, is that we cannot separately identify *aging* effects and *time* effects within a given cohort; children born later will also be younger in the 1980 census. The importance of this problem is illustrated by considering a simple comparison of the poverty rates of children born in nonrepeal states in 1972 and 1976, using the 1980 census data. If there is a positive selection into abortion, then the average poverty rates of those born in the nonrepeal states should decline between these two years. But at the same time, those born in 1972 and 1976 are of different ages in 1980 (eight and four, respectively). Suppose further that mothers of children who are less than six do not work, but once children enter school they go to work. Then we would automatically see a countervailing increase in poverty rates over time in the nonrepeal states, simply because the children born later are less likely to have a mother in the labor force. Indeed, on average, poverty rates fall with child's age in the 1980 census sample, so that they rise with birth year in this cross section. This increase could mask true positive selection effects. Thus, by confounding aging and time effects, we potentially introduce a spurious trend into our analysis.

Of course, this is not necessarily a problem for our analysis, since we are comparing the *relative* change in poverty rates (or other outcomes) in the repeal and nonrepeal states. But it may introduce problems if there are reasons to believe that child age effects have different impacts in different states. Indeed, visual inspection of the data suggests that there are underlying trends in the state differentials, with a steady relative rise in adverse living circumstances in the repeal states for cohorts born between 1965 and 1980. Thus, we include state-specific quadratic trends in our regression framework that is described in more detail below.¹² These controls should capture differences across states, for example, in the work patterns of mothers as their children age. Our estimates of interest are identified by the deviation of average

12. The sensitivity of our findings to alternative specifications of trends is reported below.

living circumstances around these state-specific trends when abortion is legalized.

Our sample consists of all noninstitutionalized children in the 1980 census born between 1965 and 1979 (all children up to and including age 15 in 1980), which is roughly 2.4 million observations.¹³ We focus on three measures of living circumstances: living in poverty; living in a single-parent household; and living in a household receiving welfare. To construct the first measure, we simply compare total household income with the poverty line for that size household. The second measure is a dummy variable that is equal to one if the child has an unmarried mother or is the child of a male head of household/subfamily with no spouse/partner present, and is zero if there is a second parent.¹⁴ The final measure is a dummy variable for whether the child's household reports receiving any public assistance income.¹⁵

While we have 2.4 million observations in the Census, our independent variables of interest vary only at the state and year level. Therefore, we aggregate these census data into state of birth/year of birth cells, and perform all of the analysis at the cell level, weighting the regressions by the cell counts. This aggregation leaves us with 750 observations from the 50 states and 15 years of birth cohorts (1965–1979).¹⁶

In addition to the census data, we use data on birthrates and birth outcomes, available by state of birth and year of birth from *Vital Statistics of the United States*. Average birthrates are calculated for all women between the ages of 15 and 44. For each state-of-birth/year-of-birth cohort we focus on two additional measures of adverse living circumstances using these data: the

13. Children born in 1980 are excluded because the income-based measures used as dependent variables in some specifications refer to 1979. We do include children living in group quarters, such as halfway houses.

14. A key issue in constructing this measure is assigning children who are not clearly identified as a child of the household head or of a subfamily head. If other relatives of the head were present, we were able to assign some children to those persons; i.e. nieces of the head could be assigned if the sister of the head was coresiding and reported having had children. If there was a coresiding partner of the head, children of the head were assigned to that partner as a second parent (if she reports having had children). Finally, if there was a roommate, boarder, or employee in the household who reported having had children, then unrelated children in the household were assigned to that person. Of the children living in single-parent households, 10.4 percent were living with the father, and the remainder with the mother.

15. This may consist of either AFDC income or other forms of welfare income (i.e., state general assistance programs). For the purposes of our policy simulations below, we assume that this is only AFDC income.

16. We do not include the District of Columbia because some observations contain missing values for the control variables included in the analysis.

infant mortality rate, or the proportion of children born who die in the first year of life; and the proportion of births that were low birth weight (under 2500 grams). Both measures are standard measures of adverse birth outcomes that have been shown by others (using methods quite different from ours) to be negatively related to abortion access. Furthermore, being born low birth weight has been shown to be correlated with subsequent adverse child outcomes such as cerebral palsy of significant degree, major seizure disorders, blindness, deafness, and learning disorders [McCormick et al. 1992; U. S. OTA 1987; Chaikind and Corman 1990]. While our primary focus is on how abortion affects living circumstances, it is obviously also of interest to assess the health at birth of the marginal child.

Finally, we control for economic and demographic conditions in the state and year of birth which may effect selection into the birth cohort through the economic and social 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. The insured unemployment rate is obtained from the United States Department of Labor, Employment Training Administration [1983].

Regression Framework

Our discussion thus far suggests the following regression framework for estimating the effect of abortion legalization on average living circumstances:

$$(1) \text{ OUTCOME}_{st} = \beta_1 \text{ REPEAL}_s^* D7173 + \beta_2 \text{ REPEAL}_s^* D7475 \\ + \beta_3 \text{ REPEAL}_s^* D7679 + \beta_4 \delta_s + \beta_5 \tau_t \\ + \beta_6 \delta_s^* \text{ TREND} + \beta_7 \delta_s^* \text{ TRENDSQ} + \beta_8 X_{st} + \epsilon_{st},$$

where OUTCOME_{st} is one of the measures of living circumstances described above for the cohort 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 $D7680$ 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 and TRENDSQ are linear and squared time trends; and X_{st} are state-specific time-varying control variables.¹⁷ This fixed-effects specification provides ge-

17. Abortion legalization in these states may have been endogenous to changes in birthrates within these states, but we believe it is unlikely to bias our results. Our models are identified from deviations from within-state (nonlinear)

neric controls for the multitude of otherwise unobservable differences that exist across regions or take place over time.

Within this regression framework, the impact of abortion legalization in the repeal states (the change from segment *A* to segment *B* in Figure I) is captured by coefficient β_1 ; for example, if there is positive selection, we will see a coefficient $\beta_1 < 0$ (since these are adverse measures). The impact of abortion legalization in the nonrepeal states (segment *B* versus *C* in Figure I) is captured by $\beta_3 - \beta_1$; again, if there is positive selection, we will see $\beta_3 > \beta_1$. In addition, it is independently of interest to assess whether there is a full bounceback ($\beta_3 = 0$).

In addition, we therefore pursue two additional tests, which place some more structure on the model but allow us more power in testing for the effects of legalization. First, we impose the restriction that the repeal*76-79 interaction (β_3) is zero (which is never rejected in our results below); that is, that there is full bounceback. Imposing this restriction increases the precision with which the repeal*71-73 interaction (β_1) is estimated. An even weaker test is simply to ask whether there is any evidence of a shift in the relative time/age pattern of these variables, conditional on including trends; that is, to test for the presence of any change and bounceback, rather than testing individually for each. We do so by testing whether the coefficients on the repeal interactions are jointly zero, as would be true if there were no significant breaks in the relative time/age trend.

The Marginal Child

Estimates from the reduced-form model expressed in equation (1) indicate the change in children's *average* living circumstances brought about by abortion legalization. A structural model can be used to identify what the living circumstances would have been for the *marginal* child that was aborted following legalization. The derivation of our approach is identical to that used to express the relationship between average cost curves and mar-

trends, so the precise timing of abortion legalization is critically important. Anecdotal evidence provided by Garrow [1994] indicates that legalization in New York, for instance, came as a complete surprise and even came within one vote of being reversed before *Roe v. Wade*. The Supreme Court's 1973 ruling in *Roe* was a 5-4 decision. Moreover, this 1973 ruling led to a reversal of treatment and control groups in our quasi-experimental framework and makes the form of endogeneity required to bias our results very complicated. Both the initial effect of legalization mainly in New York and California and the later effect in most of the remainder of the country had to coincide precisely with the timing of changes in preferences toward abortion.

ginal cost curves (cf. Berndt [1991]). Let O_{st}/B_{st} represent the average outcome, O , of the B births born in state, s and year t . Taking the partial derivative of O_{st}/B_{st} with respect to the natural log of the size of the birth cohort ($\ln B_{st}$), implies that (suppressing subscripts for convenience) $\partial(O/B)/\partial(\ln B) = \partial O/\partial B - O/B$. In other words, the change in the average outcome of a birth cohort is equal to the difference in outcomes between the marginal birth and the average birth.

This relationship can be estimated in the following regression framework:

$$(2) \quad \text{OUTCOME}_{st} \equiv O_{st}/B_{st} = \alpha_1 \ln(\text{BIRTHRATE}_{st}) \\ + \alpha_2 \delta_s + \alpha_3 \tau_t + \alpha_4 \delta_s^* \text{TREND} \\ + \alpha_5 \delta_s^* \text{TRENDSQ} + \beta_8 X_{st} + \epsilon_{st},$$

where BIRTHRATE_{st} is the birthrate to women of childbearing age (i.e., $\text{BIRTHRATE}_{st} = B_{st}/(\text{number of women of childbearing age in state } s \text{ and year } t)$). Taking the derivative of equation (2) with respect to the log size of the birth cohort ($\ln B_{st}$) implies that α_1 is an estimate of the gap between the marginal and the average outcome in the cohort.

Alternatively, if we specify the relationship between average outcomes and the birthrate as

$$(2') \quad \ln(\text{OUTCOME}_{st}) \equiv \ln(O_{st}/B_{st}) = \alpha'_1 \ln(\text{BIRTHRATE}_{st}) \\ + \alpha'_2 \delta_s + \alpha'_3 \tau_t + \alpha'_4 \delta_s^* \text{TREND} \\ + \alpha'_5 \delta_s^* \text{TRENDSQ} + \beta_8 X_{st} + \epsilon_{st},$$

then similar logic implies that $\alpha'_1 = (\partial O/\partial B - O/B)/(O/B)$. In other words, if the dependent variable is in logs, the coefficient α_1 becomes an estimate of the gap between the marginal outcome and the average outcome stated in percentage terms.

Estimates of equation (2) or (2') by ordinary least squares (OLS) will misstate the amount of selection associated with abortion legalization, because much of the variation in birthrates is not due to changes in abortion access. For example, if transitory economic conditions influence the timing of births (but not eventual family size), then much of the year-to-year variation in birthrates may be unrelated to child outcomes. Similarly, if permanent improvements in the expected living circumstances of families result in higher birthrates, then there would tend to be a positive correlation between year-to-year variation in birthrates

and child well-being. Therefore, OLS estimates will misstate the differences between the average child and the marginal child not born due to abortion access, and may be biased toward finding no difference or toward finding that the marginal child would have been better off than the average child.

In order to isolate the selection effects of abortion legalization, we therefore estimate these equations by two-stage least squares (TSLS), using the variation in abortion legalization across states and years to instrument for the birthrate. The first-stage equation is

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

Thus, equations (1) and (3) are reduced-form equations relating abortion legalization to average child outcomes and to birthrates. Equation (2) or (2'), estimated by TSLS, is the structural equation from which we estimate the difference between the average child outcome and the outcome of the marginal child whose birth is affected by abortion legalization.

IV. RESULTS

Impact of Abortion Legalization on Average Child Outcomes

Regression results for the reduced-form equations (1) and (3) are in Table I. Each column of the table has test results and coefficients of interest from a separate regression. For each regression we report the coefficients for the repeal interactions (β_1 through β_3 in equation (1)), the p -value from a test of $\beta_1 = \beta_3$ (i.e., comparing segment *B* versus segment *C* in Figure I), and the p -value from a test of joint significance of the repeal interactions (the weak test described above).

The first two columns report estimates of the first-stage equation (3), in which the dependent variable is the log birthrate. These columns replicate the results of Levine et al. [1996], and the coefficients suggest that abortion legalization reduced the birthrate by about 6 percent. In particular, birthrates fell by about 6 percent in repeal states relative to nonrepeal states in 1971–1973 ($\beta_1 = -0.056$), but then birthrates in nonrepeal states fell relative

TABLE I
OLS ESTIMATES OF REDUCED-FORM EQUATIONS FOR BIRTHRATE AND BIRTH OUTCOMES
(STANDARD ERRORS IN PARENTHESES)

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	ln (birthrate)	Percent living with single parent in 1980 (mean = 18.6%)	Percent living in poverty in 1980 (mean = 18.7%)	Percent with welfare receipt in 1980 (mean = 10.6%)	Percent who die in first year (mean = 1.9%)	Percent with low birth weight (mean = 7.7%)						
Independent variable:												
repeal * 1971-1973	-0.056 (0.006)	-0.059 (0.005)	-0.729 (0.303)	-0.869 (0.225)	-0.302 (0.295)	-0.541 (0.219)	-0.412 (0.233)	-0.364 (0.173)	-0.042 (0.027)	-0.051 (0.020)	-0.058 (0.070)	-0.098 (0.052)
repeal * 1974-1975	-0.015 (0.009)	-0.021 (0.005)	-0.514 (0.457)	-0.776 (0.255)	0.225 (0.445)	-0.221 (0.248)	-0.576 (0.351)	-0.487 (0.196)	-0.030 (0.041)	-0.048 (0.023)	-0.112 (0.106)	-0.187 (0.059)
repeal * 1976-1979	0.010 (0.014)	0.468 (0.679)	0.799 (0.660)				-0.160 (0.521)		0.033 (0.062)		0.135 (0.160)	
P-value for test of equality of coefficients on repeal * 1971-1973 and 1974-1975	0.000	0.023	0.032	0.533	0.121	0.120						
P-value for test of joint significance of reported coefficients	0.000	0.000	0.001	0.000	0.053	0.045	0.064	0.028	0.054	0.025	0.014	0.007

Coefficients are those on repeal interactions from regression specifications such as equation (1), including the following other regressors: 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. Odd-numbered columns in each panel show results from equation (1); even-numbered columns show results from regression models restricting the coefficient on repeal * 1976-1979 (β_3) to be zero. In all specifications, 750 observations are available representing the 50 states and 15 years of birth cohorts (1965-1979).

to repeal states following legalization in all states in 1973 ($\beta_1 - \beta_3 = -0.066$). Thus, we estimate significant effects of abortion on birthrates of similar magnitude in both the repeal and nonrepeal states. As a result, we cannot reject the hypothesis that there was a full bounceback in relative birthrates ($\beta_3 = 0$). The second column reports the results when β_3 is constrained to be zero, and the results are quite similar. In both specifications the repeal interactions are highly jointly significant.

The remaining columns of Table I estimate equation (1) for alternative outcome measures (in each case, we multiply the coefficients of interest by 100, so that they can be interpreted as percentage point effects). For the percentage of a cohort living in single-parent households, the pattern of coefficients indicates positive selection; that is, the legalization of abortion lowers the share of children living in single-parent households. Legalization in the repeal states over the 1971–1973 period is associated with a reduction in the percentage of children living in single-parent families of 0.73 percentage points (supporting positive selection for segment B versus segment A). This effect was reduced to some extent over the 1974–1975 period, and had actually reversed by 1976–1979, although this estimate is not significantly different from zero.¹⁸ More importantly, the coefficients on the 1971–1973 and 1976–1979 interactions are significantly different from each other (confirming positive selection in the nonrepeal states, segment C versus segment B). Whether or not we impose the full bounceback ($\beta_3 = 0$), the repeal interactions are very jointly significant.

For the percentage of the cohort in poverty, the directions of the coefficients support positive selection, but the estimates are imprecise. There is a negative coefficient on the 1971–1973 interaction but it is insignificant, and there is an insignificant positive coefficient on the 1976–1979 interaction, although it is fairly large. Despite the imprecision of these estimates, however, we can reject that the 1971–1973 and 1976–1979 interactions are equal at the .032 level, which supports positive selection. If we

18. In this and other specifications reported in this table, the repeal effect in 1974–1975 is roughly of a magnitude similar to that in 1971–1973 even though we would expect it to be smaller. In fact, the repeal effect is never significantly different in these transitional years from 1971–1973. However, the repeal effect in 1974–1975 is also not significantly different from that in 1976–1979 in any of these models. These findings suggest that tests for the partial convergence in outcomes between repeal and nonrepeal states through the 1974–1975 period are relatively weak.

impose $\beta_3 = 0$, then the 1971–1973 interaction becomes negative and significant, suggesting that abortion legalization reduced the percentage of the cohort living in poverty by 0.54 percentage points. And, once again, the repeal interactions are jointly significant.

For welfare receipt, the coefficients are also supportive of positive selection, with a negative and significant (at the 10 percent level) coefficient on the 1971–1973 interaction, and a zero coefficient on the 1976–1979 interaction. The results indicate that abortion legalization lowered welfare receipt rates by 0.41 percentage points on average. However, the relative imprecision here means that we are unable to reject the equality of the 1971–1973 and 1976–1979 interactions. Unexpectedly, the 1974–1975 interaction is actually larger than the 1971–1973 interaction although these coefficients are not very precisely estimated. And we can again reject the joint insignificance of the repeal interactions.

The final columns look at the impact of abortion legalization on infant mortality and on the percentage of the birth cohort that was low birth weight. As with the measures of living circumstances in 1980 from the census data, these birth outcome measures suggest positive selection: a negative coefficient on the 1971–1973 interaction that reverses by 1976–1979, although none are statistically significant. When we restrict the 1976–1979 interaction coefficient to equal zero, however, we observe a negative and significant reduction in both infant mortality and in low birth weight in repeal states in the 1971–1973 period of roughly 0.05 percent and 0.1 percent, respectively.

Overall, the pattern of estimates is very suggestive of positive selection. The 1971–1973 interaction (β_1) is negative in every case. When we impose the restriction that the repeal*76–79 interaction is zero (which is not rejected in any of our models), it becomes significant in every case (although only at the 10 percent level for low birth weight). Similarly, in each regression we find that this difference is reversed after legalization of abortion in the nonrepeal states in 1973 (i.e., $\beta_3 > \beta_1$), although this difference is only significant for living in a single-parent or poverty household. And, for all outcomes, the repeal interactions are found to be jointly significant at least at the 10 percent level. Thus, abortion legalization appears to be associated with an improvement in the average living circumstances and birth outcomes among a birth cohort.

Our results thus far indicate that abortion legalization leads

to significant reductions in both the share of children living in single-parent families and the share living in poverty. Single-parent families are much poorer than other families, however. As a result, our finding that fewer children are living in poor households may arise mechanically from the reduction in the odds of residing in a single-parent household, rather than from increases in the average income of families, conditional on family structure. To examine this question, we compute state/year specific poverty rates separately for the single-parent and two-parent family samples, and use both as dependent variables. Once stratified by family structure, we find no effect of legalization on either the share of single-parent families in poverty or the share of other families in poverty; we fail every single test described above (although standard errors are considerably larger as well, particularly for the single-parent sample). Thus, it appears that our findings for changes in poverty when abortion becomes available is driven solely by changes in the distribution of family structure.¹⁹

Outcomes for the Marginal Child

Although our findings for these average measures of disadvantage in a cohort are fairly consistent in their signs and significance, it is of interest to interpret these magnitudes in terms of the size of the selection effect. That is, we can ask the following question: what are the characteristics of the marginal children who were not born due to abortion legalization? Estimates of the difference between the marginal and the average birth outcome are presented in Table II.

Each column in Table II reports OLS and TSLS estimates of the coefficient on the log birthrate from a regression of our average outcome measures on the log birthrate (equation (2) or (2')). The TSLS estimates instrument for the birthrate with the repeal interactions. Thus, column (1) of Table I reports the first-stage equation for the TSLS estimates.²⁰ As discussed earlier, these coefficients estimate the gap in child outcomes between the marginal and the average child. For specifications in which the

19. Previous literature suggests that we should find a *reduction* in the average income of children living in single-parent families. Cooksley [1990] and Lundberg and Plotnick [1995] find evidence of negative selection on abortion decisions among unmarried teens in data from the NLSY. Our contradictory finding of no selection conditional on family structure suggests that these earlier findings may be due to the reporting bias in the NLSY data on abortions.

20. The first-stage *F* statistics for the excluded instruments in this table are very high; in every case the *F* is over 60.

TABLE II
ESTIMATES OF RELATIONSHIP BETWEEN BIRTHRATES AND CHILD OUTCOMES COEFFICIENTS ARE MULTIPLIED BY 100
(STANDARD ERRORS IN PARENTHESES)

Dependent variable:	Percent living with single parent in 1980 (mean = 18.6%)		Percent living in poverty in 1980 (mean = 18.8%)		Percent with welfare receipt in 1980 (mean = 10.6%)		Percent who die in first year (mean = 1.9%)		Percent with low birth weight (mean = 7.6%)	
	no	yes	no	yes	no	yes	no	yes	no	yes
Is dependent variable logged?										
Coefficient estimates for $\ln(\text{birthrate})$:										
OLS	4.83 (1.72)	0.271 (0.104)	2.38 (1.66)	0.097 (0.095)	0.769 (1.31)	0.096 (0.145)	0.192 (0.154)	0.105 (0.086)	0.544 (0.400)	0.074 (0.053)
TSLs using instruments:										
repeal * 1971-1973	13.16 (3.85)	0.603 (0.231)	9.28 (3.71)	0.479 (0.212)	4.83 (2.91)	0.439 (0.320)	0.770 (0.340)	0.396 (0.189)	1.15 (0.874)	0.136 (0.115)
repeal * 1974-1975										
repeal * 1976-1979										
P-value for Hausman test of equivalence of OLS and TSLs estimates	0.013	0.104	0.034	0.040	0.114	0.227	0.053	0.080	0.434	0.539
P-value for Basman test of overidentifying restrictions	0.105	0.165	0.557	0.699	0.112	0.072	0.313	0.229	0.012	0.010
OLS estimates, excluding 1971-1975	4.25 (2.59)	0.276 (0.162)	1.05 (2.37)	0.058 (0.138)	-0.228 (2.02)	0.069 (0.232)	-0.105 (0.228)	-0.001 (0.013)	-0.197 (0.594)	-0.019 (0.079)

Coefficients are those from estimation of equations (2) and (2'), by OLS (in first row), TSLs (in second row), and OLS excluding the years 1971-1975 (in final row). Odd-numbered columns in each panel show results from equation (2), and even-numbered columns show results from equation (2'). 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 (1966-1979).

dependent variable is logged, the coefficient gives this gap as a percentage of the average outcome. In addition to the OLS and TSLS estimates, we report the p -value of a test that the OLS and TSLS estimates are equivalent, and the p -value from a test of the overidentifying restrictions in the TSLS models.

The first two columns considers the likelihood that the marginal child would have lived in a single-parent household in 1980. The OLS estimate in the first column indicates that the marginal child would have been 5 percentage points more likely to live in such a household, as compared with the average child in their cohort. Alternatively, column (2) estimates that the marginal birth was approximately 27 percent more likely to live in a single-parent household. As discussed earlier, however, one would expect OLS estimates to understate any positive selection effects of legalization because much of the variation in birthrates is not the result of changes in abortion access.

Indeed, when we instrument in the second row, our estimates rise appreciably. The TSLS estimates indicate that the marginal birth being affected by abortion legalization would have been 13.2 percentage points more likely to have been living in a single-parent household in 1980, as compared with the average child in their cohort. Column (2) shows that the marginal birth was 60 percent more likely to live in a single-parent household. Both estimates are fairly precisely estimated. Finally, the tests of the overidentifying restrictions have p -values of 0.105 and 0.165, providing no evidence of misspecification in the TSLS models.

The remaining columns provide similar estimates for other outcome measures. The TSLS estimates imply that, compared with the average outcome in their birth cohort, the marginal birth would have had higher poverty rates in 1980 (by 9.3 percentage points or 48 percent), and been more likely to have lived in a family receiving welfare in 1980 (by 4.8 percentage points or 44 percent). Similarly, the marginal birth would have had higher infant mortality rates (by 0.77 percentage points or 40 percent) and higher incidence of low birth weight (by 1.15 percentage points or 14 percent). With the exception of low birth weight, these estimates are statistically significant at or near the 5 percent level, and pass the test of overidentifying restrictions.

All of the TSLS estimates are larger than the OLS estimates. As discussed earlier, this would be expected if there were stronger selection arising from changes in abortion access, than arising from other factors that influence the birthrate. In fact, the gap

between OLS and TSLS estimates may understate the difference in selection due to abortion versus other factors, since a large share of the year-to-year variation in birthrates that identifies our OLS estimate comes from changes in abortion access. We address this in the final row of the table, by reestimating our OLS regression excluding the years 1971–1975. By considering only those years during which there was no difference across states in the legality of abortion, we provide an estimate of the effect of year-to-year variation in birthrates for reasons other than abortion legality. Indeed, our estimates here are generally smaller, and some point estimates even become negative.

Alternative Specifications

In all of the reported results, we have allowed quadratic state-specific trends to capture potentially nonlinear changes in children's living circumstances as they age that differ across states. Although the discrete nature of the legal status of abortion should still enable us to estimate its impact in a model with these trends, including them does eliminate a substantial share of the variation in the data. To examine the sensitivity of our findings, we reestimated these models allowing for other specifications of trends, including no trends, linear trends, and a linear spline kinked in 1974 (having a school-age child makes it easier for mothers to work and six-year-old children in 1980 were born in that year).

Results obtained for all models including the linear spline were quite similar to those using a quadratic trend reported here. We even obtained similar estimates in models of living in a single-parent family, infant mortality, and low birth weight that included no trend and a linear trend, but models of welfare receipt and living in poverty were very sensitive to these two alternatives. For instance, results from Table II indicate that the marginal child is 5 percent more likely to live in a household receiving welfare in 1980, (column (5)), the analogous models omitting any trend and with a linear trend suggests that these children are actually 7 percent and 4 percent *less* likely to live in a welfare household, respectively. This sensitivity may not be surprising, however, because these are precisely the two outcomes for which having a child in school rather than at home should have the greatest impact. Based on our a priori beliefs regarding the nonlinearity in children's living circumstances by age, we still

favor those models that include the quadratic trends (or linear splines).

Another way in which our results may be sensitive to alternative specifications is that the distance between a woman's state of residence and a repeal state may be important. Women who live close to a repeal state may still have some access to abortion through travel and differences in birth outcomes and children's living circumstances may emerge between these women and those residing farther from a repeal state. In fact, Levine et al. [1996] find that the decline in births brought about by abortion legalization follows this pattern. The decline in New York, California, and other early legalization states was roughly twice as great when compared with the decline in far away states rather than in neighboring states. When we estimate reduced-form models of child outcomes (as in Table I) that allow the estimated effect to differ by travel distance to a repeal state, we find no such significant differences—although the standard errors on the estimates are sufficient that it is impossible to rule out fairly large differences. Regardless, the estimates of outcomes for the marginal child (as in Table II) are very similar when we use a larger list of instruments that include interactions between the affected years and distance between each state and the nearest repeal state (measured in the categories <250, 250–750, and >750 miles).

Differences by Race

The fact that living circumstances for black children are far different from those for white children has been well documented, and these differences are apparent in our data as well. For instance, in our data nonwhite children are almost twice as likely to die in the first year of life and are three times more likely to live in poverty compared with white children.

These differences in average living circumstances indicate that abortion legalization could have had considerably different effects for blacks compared with whites. In fact, if black and white women make similar abortion decisions under similar circumstances, then these differences on average would lead us to expect black women to abort more frequently. For any threshold level of children's outcomes, more black children are likely to fall below that level. Moreover, if those white and black children who are aborted would have had similar living circumstances, then the difference between the marginal and average child will be greater

for whites than for blacks because the average living circumstances of whites are higher.

Some empirical support for both of these propositions is reported in Table III. The first two columns of the table report estimates of the effect of abortion legalization on birthrates separately for whites and nonwhites. Consistent with the results in Levine et al. [1996], birthrates in those states legalizing abortion in advance of *Roe v. Wade* fell by about 5 percent for whites and 12 percent for blacks. Following *Roe v. Wade*, the differences across states were eliminated for both whites and blacks.

The remainder of the table compares the estimated effect on child living circumstances separately by race: estimates in the top panel are analogous to those reported in Table I, and estimates in the bottom panel are analogous to the TSLS estimates reported in Table II. In all cases, results for whites are similar to those for the population as a whole because they represent the vast majority of the population. In comparing results across racial groups, finding statistically significant differences is hampered by the relative imprecision with which parameters are estimated. Nevertheless, the point estimates reported here are generally supportive of the second proposition. The TSLS estimates of the difference between the marginal and average child are larger among whites than nonwhites for all of the outcomes except welfare receipt.²¹

Implications

The implication from these findings is that the marginal children who were not born due to abortion legalization would have lived in more disadvantaged circumstances than the average child in their cohort. This indicates sizable positive selection among those pregnancies that were carried to term following legalization of abortion. In other words, this evidence strongly suggests that abortion is used by women to avoid bearing children who would grow up in adverse circumstances.

21. Our findings by race contradict those of Grossman and Jacobowitz [1981], who estimate a 0.059 percentage point decline in neonatal mortality for whites in abortion reform states after 1970, and a 0.177 percentage point decline for blacks. Our estimates for whites are very similar but for blacks we find no effect. Their estimates are identified from between-state variation in abortion availability in the early 1970s. However, the states that had legalized abortion in 1970 had lower infant mortality rates for blacks (but not for whites) even prior to 1970. This fact most likely explains the difference between our within-state estimates and their between-state estimates and suggests that the between-state estimates are biased, particularly for blacks.

TABLE III
ESTIMATES OF BIRTHRATE AND CHILD OUTCOME EQUATIONS BY RACE
(STANDARD ERRORS IN PARENTHESES)

Dependent variable:	ln (birthrate)		Percent living with single parent in 1980		Percent living in poverty in 1980		Percent with welfare receipt in 1980		Percent who die in first year		Percent with low birth weight	
	White (1)	Nonwhite (2)	White (3)	Nonwhite (4)	White (5)	Nonwhite (6)	White (7)	Nonwhite (8)	White (9)	Nonwhite (10)	White (11)	Nonwhite (12)
Mean	7.5%	9.4%	13.6%	39.0%	13.6%	39.5%	6.9%	25.6%	1.6%	2.9%	6.5%	13.2%
	Reduced-form equations											
Independent variable:												
repeal * 1971-1973	-0.047 (0.007)	-0.116 (0.016)	-0.522 (0.315)	-0.655 (0.712)	-0.280 (0.297)	0.294 (0.763)	-0.204 (0.221)	-0.813 (0.707)	-0.048 (0.027)	0.052 (0.079)	-0.084 (0.137)	0.356 (0.300)
repeal * 1974-1975	-0.012 (0.011)	-0.038 (0.022)	-0.344 (0.480)	-0.654 (1.035)	0.084 (0.453)	0.707 (1.110)	-0.268 (0.336)	-1.961 (1.028)	-0.054 (0.042)	0.105 (0.115)	-0.164 (0.209)	0.116 (0.436)
repeal * 1976-1979	0.008 (0.016)	0.016 (0.030)	0.456 (0.716)	0.226 (1.516)	0.551 (0.676)	0.968 (1.626)	-0.361 (0.502)	-0.515 (1.506)	-0.004 (0.063)	0.189 (0.169)	0.029 (0.316)	0.519 (0.640)
	Two-stage least squares											
Coefficient estimates for ln (birthrate):		11.95 (4.95)	5.35 (4.30)	9.00 (4.59)	0.420 (4.58)	1.75 (3.33)	3.43 (4.32)	0.778 (0.411)	0.077 (2.02)	1.19 (1.83)	-1.83 (1.83)	

Coefficients in the top and bottom panels can be compared with the odd-numbered columns in Tables I and II, respectively. Notes at the end of those tables are applicable here. The repeated mean in the column labeled ln (birthrate) is in levels, not logs. In all specifications, 750 observations are available representing the 50 states and 16 years of birth cohorts (1965-1979).

As noted above, this is a purely positive exercise, and we do not have much evidence on the long-run implications of this change in average living standards. One clear implication of our findings, however, is that there was an effect of abortion legalization on the budgets of federal and state governments, through reduced welfare receipt. We can compute the budgetary savings in 1980 (the year of our data) to the government through the reduced welfare receipt of the average child after abortion legalization. In doing so, we abstract from welfare savings due to the overall drop in cohort size; we are only estimating the differential savings arising from the change in cohort mix through positive selection.

Our calculation proceeds as follows. First, we compute the number of children in each age cohort in 1980 who were born after abortion legalization, counting all children born in 1971 or later in repeal states and all children born in 1974 or later in nonrepeal states. We then compute the drop in the number of children born into welfare households in 1980 as the product of the number of children born after legalization and the estimated decline in welfare receipt after legalization (0.364 percent from column (8) of Table I). We estimate that in 1980, the lower average welfare receipt rate, due to positive selection only, reduced the welfare caseload by 73,500 families. In 1995 dollars the average welfare payment per family of two (mother and child) in 1980 was \$6500.²² This implies that the government saved \$480 million in 1980 because of abortion legalization. If all children in 1980 had been born at a time when abortion was legal, our estimates imply that positive selection would have reduced the welfare caseload in 1980 by 173,500 families for a total savings of \$1.1 billion in 1980.

V. CONCLUSIONS

The most important change in government fertility policy over the past 30 years was the legalization of abortion under the *Roe v. Wade* decision. As has been shown elsewhere, this change had a dramatic effect on the size of birth cohorts. As we demonstrate in this paper, the change also had a significant effect on the living circumstances of the cohorts who were born after legalization. Subsequent cohorts were less likely to be in single-parent households, and as a result less likely to live in poverty, and less

22. We use benefits for a family of two because the mechanism through which abortion legalization reduced welfare receipt appears to be through reducing the initial formation of single-female-headed families.

likely to receive welfare. In addition, these cohorts experienced lower infant mortality. In particular, we find that for the marginal child not born due to increased abortion access, the odds of living in a single-parent family would have been roughly 60 percent higher, the odds of living in poverty nearly 50 percent higher, the odds of welfare receipt 45 percent higher, and the odds of dying as an infant 40 percent higher.

Perhaps more importantly, these findings may also have implications for the lifelong prospects of the average child born after legalization. The children not born due to abortion availability would have grown up in adverse living circumstances that other studies have shown may be detrimental to later prospects.²³ Of course, this conclusion is complicated by the fact that we cannot necessarily apply the effects on the average child of living in poverty (for example) to the effects on the marginal child who would live in poverty if their pregnancy were not terminated. However, as these cohorts age, researchers will be able to directly observe outcomes such as educational attainment, income, and family structure. This is an important question that should be the focus of future analysis.

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23. See Haveman and Wolfe [1995]. On the other hand, Mayer [1997] and others have argued that the relationship between growing up in poverty and subsequent outcomes is not causal.

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