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ABORTION AND SELECTION

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ABSTRACT

The introduction of legalized abortion in the early 1970s led to dramatic changes in fertility behavior. Some research has suggested as well that there were important impacts on cohort outcomes, but this literature has been limited and controversial. In this paper, we provide a framework for understanding the mechanisms through which abortion access affects cohort outcomes, and use that framework to both address inconsistent past methodological approaches, and provide evidence on the long-run impact on cohort characteristics. Our results provide convincing evidence that abortion legalization altered young adult outcomes through selection. In particular, we find evidence that lower costs of abortion led to improved outcomes in the birth cohort in the form of an increased likelihood of college graduation, lower rates of welfare use, and lower odds of being a single parent. We also find that our empirical innovations do not substantially alter earlier results regarding the relationship between abortion and crime, although most of that relationship appears to reflect cohort size effects rather than selection.

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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. 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 altered the characteristics of birth cohorts. In particular, children's outcomes may have improved on average, because they were more likely to be born into a household in which they were wanted.

Two earlier papers investigated the implications of such positive selection through abortion for the quality of cohorts born after abortion legalization. Gruber, Levine and Staiger (GLS, 1999) found that the legalization of abortion led to significant improvements in the circumstances of children born into cohorts where abortion was legal. Such cohorts of children lived in households with lower rates of single motherhood, welfare receipt and poverty, and experienced lower infant mortality than nearby cohorts of children. Donohue and Levitt (DL, 2001) focused on a relevant outcome for children at older ages and young adults, crime.[1] They found that increased use of abortion in the 1970s resulted in lower crime rates among the cohorts born in that era when those cohorts were in their late teens and early 20s.

This paper will consider the impact of abortion legalization on a broader array of cohort characteristics at older ages. Since the relevant cohorts were born in the early 1970s, children born at that time are now in their 30s, so we can examine adult outcomes like completed educational attainment, employment, and poverty status. We accomplish this using data from the 2000 Census.

[1] Other papers addressing the crime issue followed and will be discussed subsequently.

Beyond focusing on adult outcomes, this paper also makes important contributions by addressing the inconsistent methodological approaches and the implicit model of selection used in past research. In particular, GLS assume that all of the selection due to abortion was manifested through lower cohort sizes. DL pursued a different approach, linking selection to the rate at which abortions were performed. Since additional abortions do not map perfectly into reduced births due to the possibility that pregnancy behavior may change, these two approaches are not equivalent. Moreover, GLS base their identification on the natural experiment established by the differential timing of abortion legalization across states. In contrast, much of the identification in DL incorporates the cross-state variation in the growing use of abortion following legalization in the middle to late 1970s.

These differences in approaches are important for assessing the way in which abortion access affects cohort quality. In this paper, we propose a method for comprehensively assessing the impact of abortion access on cohort outcomes. We introduce a formal model of selection, which we use to identify two distinct mechanisms by which changes in abortion access may alter cohort characteristics. We then relate this model to past empirical work, indicating how that past work relies on different subsets of these mechanisms. We also introduce an empirical model that incorporates both the legalization variation used by GLS and the variation in "taste" for abortions across states used by DL to credibly identify selection effects.

To preview our results, we find that there is evidence of selection effects of abortion on young adult outcomes. In particular, we find that lower costs of abortion led to an increase in the likelihood of college graduation, lower odds of welfare receipt, and lower odds of being a single parent. We are unable to sharply distinguish the mechanisms through which selection is occurring, but our results are robust to the empirical methodology employed. We also find that earlier results regarding the relationship between abortion and crime are generally robust to the different identification strategies described here, but that these findings may not reflect selection so much as the effects of lower cohort size.

Our paper proceeds as follows. Section II provides background on abortion legalization and reviews previous studies of its effects. Section III then provides a model of the mechanisms through which abortion affects selection of birth cohorts. Section IV describes the methodology for the current study, and Section V discusses the data which we analyze. Section VI presents the Census results, and Section VII presents the crime results. Section VIII 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. The abortion rate almost doubled between 1973 and 1980. This heightened prevalence of abortion came at the same time as an ongoing steep reduction in fertility rates. Because births had been falling precipitously even before the introduction of legalized abortion, however, 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 1970 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.

B. Abortion Legalization and Child Outcomes

GLS 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 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 afterwards. 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.

DL also estimated changes in children's outcomes as a result of abortion legalization.[2] 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 GLS. 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; the findings suggest that abortion legalization in the early 1970s can explain as much as half of the decline in crime observed in the 1990s.

The DL study has generated a great deal of controversy. Critiques by Joyce (2004a and 2004b) and Foote and Goetz (2005) have raised important questions regarding the empirical analysis, to which Donahue and Levitt (2004, 2006) have responded. Although it is beyond the

[2] There are two other contributions to the literature on abortion and children's outcomes other than crime of which we are aware. Charles and Stephens (2002) estimate the impact of abortion legalization on drug use and employ quasi-experimental methods in their estimation in much the same manner as GLS. They find that legalized abortion led to a significant reduction in drug use. Pop-Eleches (2005) estimates the impact of a sudden ban on abortions imposed in Romania. He finds that the ban had larger effects on births to urban and educated women, but conditional on the mother's characteristics the children born after the ban had worse economic outcomes as adults.

scope of this paper to fully elaborate upon all the points raised in this debate, we do want to focus on Joyce's criticism of their identification strategy. Joyce argues that including the abortion rate on the right hand side of the regression does not accurately gauge variation in unwanted births, since the abortion rate is endogenous, and only the variation in legalization is exogenous. Joyce re-estimated the Donahue and Levitt model using the double quasi-experiment implemented by Levine, et al. (1999) and GLS and obtained results that he argued were inconsistent with a causal interpretation.

Donohue and Levitt disputed this reading of Joyce's findings, however, and highlighted a potential weakness of the GLS identification strategy: abortion rates do not correspond well to the "experiment" proposed by GLS. This is illustrated in Figure 1, which plots the difference in birth rates and differences in abortion rates between repeal states and non-repeal states from 1965 to 1979.[3] Differences in birth rates correspond well with the natural experiment idea: birth rates in repeal states fell relative to non-repeal states in 1971, but the gap disappeared by the mid 1970s once abortion was legalized in non-repeal states. But abortion rates do not follow this pattern. Due to data limitations, we impose a zero difference in abortion rates prior to 1970. Afterwards we see abortion rates in repeal states jumped dramatically compared to the rest of the country in 1971, which is not surprising since that is the first full year in which abortion was legal in all repeal states (although women could travel to early repeal states to obtain an abortion). But the differences that emerge in the early 1970s never dissipated later in the decade, as women in repeal states continued to use abortion at much higher rates than women in the rest of the country.

[3] The data used to generate this figure are described subsequently.

III. A MODEL OF SELECTION

A. Setup

To clarify some of the empirical differences between past work and to motivate our econometric model, we introduce a theoretical model of selection describing the potential effects of changes in the cost of abortion. This simple model of decision-making under uncertainty is closely related to that introduced by Kane and Staiger (1997) and Levine and Staiger (2002), which were designed to examine how changes in the cost of abortion would affect fertility decisions of an individual woman. We extend that here to incorporate heterogeneity across individuals in the expected payoff to giving birth, to allow analysis of the impact of abortion costs on the characteristics of children born.

We begin by briefly summarizing our assumptions regarding the decision-making process. First, a woman makes her decision about pregnancy based on the expected payoff evaluated at the time of becoming pregnant, and this expected payoff varies across women. Second, decisions are made sequentially; a woman first chooses whether to become pregnant, and then after some time has elapsed a decision is made whether to abort or give birth. Third, a woman makes her decision about abortion based on the expected payoff evaluated at the time of the abortion, when she is better informed about the consequences of a birth than at the time of becoming pregnant. Thus, in our model, abortion differs from other methods of avoiding a birth (e.g., contraception or abstinence) because the abortion decision is made with more complete information than the pregnancy decision.[4] Finally, we assume that children's outcomes are directly linked to the payoff of giving birth. Intuitively, we assume that more "wanted" (i.e. higher payoff) births have better outcomes than less wanted births.

[4] In fact, there is considerable evidence that information obtained after becoming pregnant (e.g. support from parents or the father, health problems of mother or fetus) is an important determinant of the abortion decision (Bankole, Singh and Haas, 1998; Torres and Forrest, 1988).

B. A Simple Model

More formally, consider the following simple model. A woman initially chooses whether to become pregnant. If she does not become pregnant, she receives a payoff normalized to 0. The payoff should she give birth (X) is uncertain at the time of choosing to become pregnant, but has a known distribution, $X \sim N(\mu,\sigma^2)$, with a mean (μ) that varies across women in the population with distribution $f(\mu)$. If she becomes pregnant, she learns the value of X, and then can choose to give birth and receive a payoff of X, or have an abortion and receive a payoff of –A, where A represents the cost (both monetary and psychic) of an abortion and is assumed to be positive.[5] The woman's objective is to maximize her expected payoff.

The formal details of the solution to this model are presented in Appendix 1, but the main implications are intuitive. Within this simple model, lowering the cost of abortion affects fertility decisions on two margins: the abortion and pregnancy margin. Among the women who become pregnant, lowering the cost of abortion will increase the probability of abortion and reduce the probability of birth (since women give birth only if $X > -A$). The births that are eliminated on this margin we refer to as "marginal births."

Lowering the cost of abortion will also affect the pregnancy margin: more women will become pregnant because the down-side risk of a pregnancy is less costly should they want an abortion after becoming pregnant. The pregnancies that are added on this margin we refer to as "marginal pregnancies."

Note that these marginal pregnancies will result in some additional births. These women are indifferent about getting pregnant ex ante (hence a marginal pregnancy), but at the time of choosing an abortion some will have received positive news about the payoff to a birth and give

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[5]Since the main purpose of this model is to make positive, rather than normative, statements, we do not distinguish between private and social costs or benefits. More generally, one could allow for externalities associated with unwanted births or abortions.

birth ($X > -A$), while women who do not receive positive news will abort the pregnancy ($X < -A$). These marginal pregnancies are essentially paying a cost (the potential abortion cost) to buy an option, with the resulting births corresponding to options that are exercised.

The implications of this model for pregnancy and birth rates are illustrated in Figure 2. When the cost of abortion is very high (e.g. abortion is illegal) at the right hand side of the diagram, no women will choose to abort and all pregnancies will end in a birth, i.e. Pr(birth|pregnancy) = 1 and Pr(pregnancy) = Pr(birth). Lowering the cost of abortion will increase the pregnancy rate (through the addition of marginal pregnancies) and will reduce the proportion of pregnancies that end in a birth (through the elimination of marginal births by abortion). Thus, lowering the cost of abortion will unambiguously increase the number of pregnancies and abortions.

The net effect this has on the birth rate is ambiguous, as the elimination of marginal births is offset by the addition of births resulting from marginal pregnancies. We have chosen to draw Figure 2 so that lowering the cost of abortion results in lower birth rates, except at low costs of abortion where the birth rate is flat. This would be the case, for example, if there are relatively few marginal pregnancies until the cost of abortion becomes fairly low, e.g. women are unwilling to become pregnant in hopes of receiving positive news about the payoff to a birth *unless* the cost of abortion is quite low. When the abortion curve is much steeper than the pregnancy curve, the birth rate falls; when the abortion and pregnancy curve are equally steep, the birth rate is flat.

Figure 3 uses Figure 2 to provide a possible explanation for why birth rates in repeal and non-repeal states diverged from 1971-1973 but then converged by 1976, while abortion and pregnancy rates remained divergent throughout the 1970s (see the earlier discussion of Figure 1).

In the 1960s (we use 1965 as an example), the cost of abortion was very high in all states at A_{65}. Then, from the 1960s to the early 1970s (e.g. 1972), the cost of abortion fell dramatically in the early repeal states (to $A_{R,72}$), while the costs fell only modestly in the non-repeal states (to $A_{N,72}$), partly due to travel to the repeal states for abortion. From the early to mid 1970s (e.g. 1976), the cost of abortion continued to fall in both sets of states, through legalization in the non-repeal states and increased access in the repeal states. This led the costs to move to $A_{R,76}$ and $A_{N,76}$ respectively. Such a difference in abortion costs between states even when abortion is universally legal could result from differences in the social acceptability of the procedure, for example, which could lead to lower access to abortion providers or generalized stigma against the procedure.

This set of changes is consistent with the empirical evidence from Figure 1. After national legalization, pregnancy and abortion rates could still be higher in very low cost (repeal) states relative to low cost (non-repeal) states, but birth rates could have converged. In other words, if abortion costs remained lower in repeal states relative to non-repeal states even after Roe v. Wade, birth rates could look similar because the greater number of terminated marginal births in the repeal states was cancelled out by the greater number of marginal pregnancies that were subsequently carried to term.

C. Implications for Selection

Convergence of birth rates in repeal and non-repeal states by 1976, therefore, does not necessarily imply that selection into the birth cohorts had also converged. Higher abortion rates suggest that abortion costs were lower in repeal states, as does other evidence. Birth cohorts in repeal states may therefore have been made up of fewer marginal births and more births from marginal pregnancies. The impact on cohort selection then depends upon a comparison of the payoffs to marginal births relative to births that result from marginal pregnancies. If payoffs to marginal births are lower than payoffs to the births resulting from marginal pregnancies, then cohort "quality" in repeal states will continue to improve relative to that in non-repeal states in the mid to late 1970s despite the fact that differences in birth rates stabilized.

The implications of our model for the payoffs to marginal births and births from marginal pregnancies are illustrated in Figure 4. This figure displays the distribution of birth payoffs (X) for two women at the time of becoming pregnant. The top panel depicts the distribution of birth payoffs for an average pregnancy, with an expected payoff at the time of becoming pregnant of μ^A. These payoffs are known at the time of deciding on abortion, and women will abort whenever the payoff of a birth is below the cost of an abortion ($X < -A$). The dashed vertical line at $-A$ represents the cost of an abortion and is the truncation point: below this point all pregnancies will be aborted. The truncated distribution to the right of this line represents the distribution of payoffs, conditional on giving birth, for the average pregnancy. Thus, the mean payoff for a birth resulting from an average pregnancy is the mean of this truncated distribution, denoted by $E(X \mid \text{birth}, \mu^A)$.

Marginal births are births to women who are just indifferent between having an abortion and giving birth. Thus, the payoff to these births must equal the cost of an abortion ($X = -A$), and is denoted by the vertical line at $-A$. If the cost of abortion fell, the vertical line would shift to the right and these women would choose to abort rather than give birth. Since all other births have a higher payoff, these marginal births must have a payoff below the payoff to birth from the average pregnancy.

The bottom panel of Figure 4 depicts the distribution of birth payoffs for a marginal pregnancy. A marginal pregnancy is one with a zero expected payoff at the time of becoming pregnant (before observing X), so that the woman is just indifferent to becoming pregnant at the current level of abortion costs, i.e. pr(X>-A)*E(X|X>-A) + pr(X<-A)*-A = 0. If the cost of abortion falls incrementally, the expected payoff to a pregnancy must rise (since the payoff improves or stays the same for all values of X). Therefore, women with these marginal pregnancies will choose to become pregnant. With probability p(X>-A), these women will give birth and receive a positive expected payoff to the birth: E(X|X>-A) = E(X|birth, μ^M).

Thus, the expected payoff for births from the marginal pregnancy must be higher than the payoff for the marginal birth: E(X|birth, μ^M) > -A. Moreover, since the distribution of marginal pregnancies lies to the left of the distribution of average pregnancies ($\mu^A > \mu^M$), the births from marginal pregnancies must have an expected payoff that is below that resulting from an average pregnancy: E(X|birth, μ^A) > E(X|birth, μ^M). Thus, the model implies that births from marginal pregnancies will have an expected payoff that is above the payoff to a marginal birth but below the payoff to a birth resulting from an average pregnancy: E(X|birth, μ^A) > E(X|birth, μ^M) > -A.

In this model, cohort quality improves under two circumstances. If marginal births are reduced with little or no change in marginal pregnancies, then cohort quality will rise. This likely represents the experience at the time that abortion was legalized. But cohort quality may also rise if marginal births are replaced by an equal number of births resulting from marginal pregnancies. This may have reflected the experience in the years following abortion legalization.

IV. EMPIRICAL METHODOLOGY

Two empirical approaches have been used to document the relationship between the availability of abortion and child outcomes in a state-year birth cohort. Both approaches run regressions in which the dependent variable is the average outcome in a birth cohort (where cohorts are defined by year of birth and state of birth, for GLS, or state of residence, for DL), and control for a comprehensive set of variables including state and cohort fixed effects, state-specific trends, and other state-level factors. One approach (GLS) uses an IV regression of the average outcome in a birth cohort on the log of the birth rate in the state and year of birth, instrumenting for the birth rate with changes in the legal status of abortion within a state. As discussed in GLS, the coefficient on the log of the birth rate can be interpreted as the difference in outcomes between the marginal child (who was not born because of abortion legalization) and the average child. A second approach (LD) uses an OLS regression of the average outcome in a birth cohort on the ratio of abortions to births in the state and year of birth. In this section we propose a more general empirical framework that encompasses both approaches as a special case, and which separately estimates impacts of the marginal birth and the marginal pregnancy.

A. Specification

The equation estimated by GLS took the form:

$$OUTCOME_{st} = \alpha_1 * \ln(BIRTHRATE_{st}) + controls + \epsilon_{st} . \tag{1}$$

The dependent variable, $OUTCOME_{st}$, is a measure of the average outcomes of those born in state s in year t. For simplicity, we will define all outcomes so that they reflect lower socio-economic status of the cohort, e.g. the proportion of the cohort living in poverty, or the proportion of the cohort that did *not* graduate from college. The dependent variable is included in both logs and levels in different specifications. The key right hand side variable, $\ln(BIRTHRATE_{st})$, is the log of the birthrate to women of childbearing age in the cohort's year and state of birth. This specification includes generic controls for the multitude of otherwise unobservable differences that exist across regions or take place over time.

Taking the derivative of equation (1) with respect to the log of the birth rate implies that α_1 is an estimate of the gap between the outcome for the marginal birth and the "average birth" (defined from the model to be the expected payoff to birth from an average pregnancy) in the cohort.[6] Alternatively, 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. Our theoretical model suggests that α_1 should be positive; the marginal birth is more likely than the average birth to live in poverty, not graduate from college, etc.

In the context of the model presented earlier, equation (1) imposes an important restriction by not acknowledging the potential role of the marginal pregnancy: a decline in the cost of abortion could raise pregnancy rates and lower the birth ratio (births per pregnancy), leaving the birth rate unchanged. This possibility is indeed suggested by Figure 1, which shows a continuing rise in abortion rates in the early repeal states, despite the fact that there was no relative birth rate difference after abortion legalization. In equation (1), this change cannot affect the average outcome of the birth cohort, but in our theoretical model this would affect average outcomes in the cohort since outcomes of the marginal pregnancies differed from outcomes of the marginal births they are replacing. Thus, equation (1) implicitly assumes that outcomes are the same for the marginal birth and the marginal pregnancy. This is a potential problem for the identification strategy used by GLS.

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[6] See GLS for a derivation of this.

A more general specification estimates the impact of the marginal birth and marginal pregnancy separately:

$$OUTCOME_{st} = \beta_1 * \ln(PREGRATE_{st}) + \beta_2 * \ln(BIRTHRATIO_{st}) + controls + \epsilon_{st} . \qquad (2)$$

The variable PREGRATE is the number of pregnancies per woman of childbearing age in the cohort's year and state of birth, and BIRTHRATIO is the ratio of births to pregnancies. Using the same argument that led to the interpretation of α_1, β_1 is an estimate of the difference in outcomes between births resulting from the marginal pregnancy (equal to $E[X|birth, \mu^M]$ in the model) and the births resulting from the average pregnancy (equal to $E[X|birth, \mu^A]$), while β_2 is an estimate of difference in outcomes between the marginal birth (-A) and the average birth ($E[X|birth, \mu^A]$).[7] Thus, equation (1) imposed the restriction that the marginal pregnancy and the marginal birth had the same outcomes ($\beta_1 = \beta_2$). The theoretical model developed earlier suggested that outcomes from the marginal birth should be worse than the outcome for the marginal pregnancy. Thus, for a negative outcome such as living in poverty, the theoretical model implies that $0 < \beta_1 < \beta_2$.

Interestingly, equation (2) also includes the specification run by DL as a special case. To see this, note that one can rewrite the log of the birth ratio as:

$$\begin{aligned} \ln(BIRTHRATIO) &= \ln(births/pregnancies) = -\ln(pregnancies/births) \qquad (3) \\ &= -\ln((births+abortions)/births) = -\ln(1 + abortions/births) \\ &\approx -(abortions/births) \end{aligned}$$

[7] Note that we use the birth ratio, rather than the birth rate, since we are holding pregnancy rates constant in this specification. A change in the marginal birth holding pregnancy rates constant is the birth ratio. When we convert this to two equation versions of this specification in equations (6) and (7), we will use the more intuitive birth rate.

The final expression in equation (3) is the abortion ratio, the variable used by DL.[8] Thus, equation (2) can be approximately rewritten as:

$$OUTCOME_{st} = \beta_1 * \ln(PREGRATE_{st}) + -\beta_2 * ABORTRATIO_{st} + controls + \epsilon_{st} . \qquad (4)$$

DL estimated this equation restricting $\beta_1 = 0$. Their restriction implicitly assumed that the birth outcome for the marginal pregnancy was the same as the outcome for the average birth.

Thus, the difference between the DL specification and the specification estimated in GLS rests on what each assumed about the birth outcomes resulting from the marginal pregnancy. In the more general specification of equation (2) the impact of the marginal birth and marginal pregnancy are estimated separately, and the assumptions maintained in earlier papers can be tested directly.

B. Estimation

Estimates of equation (2) by OLS will be biased if much of the variation in pregnancy rates and birth ratios was not the result of changes in the cost of abortion. For example, transitory economic improvements or unobserved improvements in the expected outcome of births could generate increases in pregnancy rates or birth ratios, biasing OLS toward finding that the marginal birth or pregnancy would have been the same as – or even better off than – the average birth. DL estimated their model by OLS, implicitly assuming that the variation in the birth *ratio* was largely driven by changes in the cost of abortion. GLS instrumented for the birth rate using variation in the legal status of abortion across states, because variation in the birth *rate* was potentially driven by factors other than the cost of abortion.

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[8] DL refer to the ratio of abortions to births as the abortion rate, but the abortion ratio is the conventional name for this statistic. The abortion rate is typically defined as the number of abortions performed per 1,000 women of childbearing age.

While GLS used legalization alone as an instrument, the trends in Figure 1 suggest that even after legalization in all states there continued to be differences in abortion cost between repeal and non-repeal states; indeed, as we describe below, such differences exist even within the set of non-repeal states. We therefore extend our earlier instrumental variables strategy to explain the wider variation in the use of abortion across states and years (beyond legalization), along two dimensions. The first is the travel distance to the nearest state in which abortion was legal during the period in which it was only legal in some states. The notion that travel costs matter for abortion access is intuitive and is supported by the work of Levine et al. (1999) and Kane and Staiger (1996). Both papers find that the birth rate is affected by distance to the nearest legal state (in the 1970-1973 period) or to the nearest abortion provider (thereafter).

The second dimension that we exploit to explain state heterogeneity beyond legalization is the "latent cost" of abortion in each state. The legalization of abortion loosened a constraint on abortion demand. This constraint was more binding in places with low latent costs of abortion than in places with high latent costs, where abortions would not be heavily demanded even if legal. Such latent costs could be a function of many factors, but certainly an important one is social attitudes towards the use of abortion: higher social opprobrium on abortion use raises its psychic cost and reduces its use. In the states with greater latent costs, we expect the social costs of abortion to be higher after abortion is legalized; that is, latent cost is a proxy for the subsequent social costs of abortion.

The notion that social norms matter is strongly supported by our data. Figure 5 plots abortion rates in repeal and non-repeal states over time, dividing the non-repeal states into two groups: "socially liberal" states with at least 22% of the population reporting they were "liberal" in opinion polls from the 1960s, and "socially conservative" states with less than 22% "liberal".

The opinion data that were used to identify conservative and liberal states are described subsequently. Seven of the non-repeal states fall into the liberal category. As can be seen in Figure 5, repeal states had the highest abortion rates after 1973. Abortion rates were lowest in socially conservative states and socially liberal non-repeal states were in the middle.

The first stage equation for the pregnancy rate, therefore, includes the interactions between repeal status and year dummies used as instruments by GLS, plus additional interactions with travel distance and latent cost of abortion to capture the heterogeneity of abortion cost within non-repeal states after legalization:

$$\begin{aligned} \ln(\mathrm{PREGRATE}_{st}) = {} & \mathrm{D7173} * \mathrm{NonRepeal}_s * (\rho_1 + \rho_2 \mathrm{LC}_s + \rho_3 \mathrm{DIST}_s) \\ & + \mathrm{D7475} * \mathrm{NonRepeal}_s * (\rho_4 + \rho_5 \mathrm{LC}_s) \\ & + \mathrm{D7679} * \mathrm{NonRepeal}_s * (\rho_6 + \rho_7 \mathrm{LC}_s) + \text{controls} + \epsilon_{st}. \end{aligned} \tag{5}$$

$\mathrm{NONREPEAL}_s$ is a dummy for a cohort born in a non-repeal state; D7173, D7475 and D7679 are dummies for the eras 1971-1973, 1974-1975, and 1976-1979; DIST_s is the average straight-line distance from state s to the nearest repeal state; and LC_s is a measure of latent cost of abortions (operationally defined below). Both DIST and LC have been rescaled so that they range from 0 to 1: 0 represents a state with the lowest distance (the repeal states) or the lowest latent cost (New York), and 1 represents a state with the highest distance (Lousiana) or highest latent cost (Mississippi). All of the repeal states have a distance of zero and latent cost near zero, so there is no need to include interactions between repeal and these variables in the specification. Similarly, all states have a Distance of zero after 1974, so there is no need to include distance interacted with D7475 or D7679. The main effects of distance, latent cost, and repeal status are all absorbed by the state of birth fixed effects.

In 1971-1973, relative to women in repeal states, we expect that women in non-repeal states faced higher abortion costs, particularly in states that were far from repeal states (where travel to repeal states was unlikely) or states that had high latent cost (where social costs of getting an abortion were high). Since higher abortion costs are expected to lower pregnancy rates, this means that the coefficients on all three interactions with D7173 should be negative (ρ_1, ρ_2, $\rho_3 < 0$). After abortion is legalized in all states, we expect that women in states that had the highest latent costs were the least likely to have an abortion, implying that $\rho_5 < 0$ and $\rho_7 < 0$. Controlling for latent cost, we expect the difference in abortion costs between non-repeal and repeal states to decline during the transition years 1974-1975 as abortion providers gradually entered these states following Roe (implying that $\rho_1 < \rho_4 < 0$), and expect no difference in abortion costs between non-repeal and repeal states after 1976 (ρ_6=0). The first stage equation for the birth ratio is analogous to equation (5), except that the expected signs of the coefficients in this equation are the opposite of the pregnancy rate equation (since higher abortion costs reduce pregnancies but raise the birth ratio).

There is one important limitation in implementing this instrumental variables approach to estimate equation (2). Conceptually, the cost of abortion is a single variable that is being used to instrument for two different variables, leaving equation (2) unidentified. However, our model suggests that the effects of the cost of abortion may be very non-linear, and the non-linear effects will differ between the pregnancy rate and the birth ratio. For example, going from illegal abortion to legal abortion at a high cost may have a large effect on the birth ratio (as many unwanted births are aborted), but little effect on the pregnancy rate (because the abortion option is so costly). In contrast, going from high cost to low cost abortion may have a much larger relative effect on the pregnancy rate, as the down-side risk of a pregnancy is all but eliminated. Our use of a set of instruments provides identification by capturing this non-linearity.

Nevertheless, in practice we found that the identification these instruments provide were insufficient to separately estimate the impact of pregnancy rates and birth ratios in the same model. Regression results based on this approach were plagued by imprecision.

As a result, we report estimates from restricted versions of equation (2), which are consistent with past models. In particular, we estimate:

$$OUTCOME_{st} = \delta_1 * \ln(BIRTHRATE_{st}) + controls + \epsilon_{st} . \quad (6)$$

$$OUTCOME_{st} = \gamma_1 * \ln(BIRTHRATIO_{st}) + controls + \epsilon_{st} . \quad (7)$$

Equation (6) is identical to equation (1), and represents a restricted form of equation (2) in which $\beta_1 = \beta_2$ (the marginal pregnancy and the marginal birth have the same outcomes, analogous to GLS). Equation (7) is very similar to equation (4), but imposes the restriction on that equation that $\beta_1 = 0$ (the birth outcome for the marginal pregnancy is the same as the outcome for the average birth, analogous to DL). We estimate each of these models, where the dependent variable is measured in logs, separately as well as by OLS and IV, using the instruments described previously.

Although these restrictions make it impossible to test the full implications of the model, these specifications allow us to bound the true difference between the marginal birth and the average birth (β_2 in equation 2). That is, equation (6) leads to an *overstatement* of the difference between the marginal and average birth. Intuitively, the reason is that this model estimates the impact of changes in "net births," ignoring the fact that the overall birth effect includes offsetting changes in marginal births and births resulting from marginal pregnancies.[9] On the other hand,

[9] This is a direct application of omitted variable bias. To see this, rewrite Equation 2 as:
$Outcome_{st} = (\beta_1 - \beta_2)\ln(pregrate_{st}) + \beta_2 \ln(birthrate_{st}) + controls + \varepsilon_{st}$ [since ln(birthrate)=ln(pregrate)+ln(birthratio)]

equation (7), leads to an *understatement* of the difference between the marginal and average birth by ignoring marginal pregnancies.[10] Thus, while it is unfortunate that we cannot estimate equation (2), we will at least be able to bound the true selection effect for the marginal birth.

V. DATA

To estimate these models we use several different sources of data. Our main source of data is the 2000 decennial Census of the United States.[11] We use these data to measure a variety of outcomes for individuals born in a given state/year: whether the individual lives in a household that is below the poverty line, receiving welfare, or headed by a single parent; whether the individual dropped out of high school or did not graduate from college; and whether the individual is incarcerated or not employed.[12] The Census provides two important advantages over other data sets containing similar outcome measures. First, it identifies state of birth rather than merely state of residence. If migration from one's childhood state is related to other outcomes, then state of residence will be a biased measure of abortion availability at birth. Second, it offers large sample sizes. As noted in GLS, even large changes in the outcomes of the marginal child will result in relatively small changes in the outcome of the average child, which

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If we omit ln(pregrate) from this equation we have Equation 6. According to our model, $\beta_1 - \beta_2 < 0$ and cov(pregrate, birthrate) < 0 (unless the rise in pregnancy is so large that it offsets the decline in the birth ratio, which does not appear to be the case in our data). Therefore, the omitted variable bias is positive. Since $\beta_2 > 0$ (assuming the outcomes are negative), if we omit ln(pregrate), then the coefficient on ln(birthrate) in equation 6 will be overstated.

[10] This is also a direct application of omitted variable bias. Equation 7 estimates $\text{Outcome}_{st} = \gamma_1 * \ln(\text{birthratio}_{st})$ + controls + ε_{st} omitting the pregnancy rate term, $\ln(\text{pregrate}_{st})$, from equation 2. Since $\beta_1 > 0$, $\beta_2 > 0$ and cov(pregrate, birthratio) < 0, the OLS estimate of γ_1 will be a downward biased estimate of β_2.

[11] All Census data used were taken from the Minnesota Integrated Public Use Microdata Series (Ruggles and Sobek 2003); only unallocated observations were used.

[12] 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.

is our unit of observation. The five percent sample provides the precision to identify these small changes.

Our sample includes those born in the United States and observed in the 2000 Census at ages 21 to 35 in that year (born between 1965 and 1979).[13] 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. This data structure allows us to control for both age and state of birth fixed effects in our model.

One drawback to the use of the decennial Census is that we only observe each cohort once in adulthood, in the year 2000. As a result, we cannot separate age and cohort effects within states (national age effects are captured by a full set of age dummies). To address this problem, we allow quadratic variation in age effects by state in all specifications, as we did in GLS. That is, we assume that any state-specific age patterns are proxied by a quadratic trend in age, and that any remaining differences across states by cohort reflect true cohort effects.

Key explanatory variables in our model require data on pregnancies, abortions and births. Explicit measures of pregnancy rates are not available, so we follow the reasonably common practice of adding abortions and births to define pregnancies, implicitly assuming that miscarriages are exogenous. Birth rates by year and state of residence come from *Vital Statistics*. Abortion rates by year and state of residence come from the Alan Guttmacher Institute (AGI) beginning in 1973. These data are generally regarded as the best abortion data available since they reflect the results of surveys of large and small abortion providers that report the

[13] In models of college graduation and poverty status, we have restricted the sample to those between the ages of 24 and 35 in 2000 (born between 1965 and 1976). We did this because college graduation rates are very low for those in their early 20s and because poverty rates for those in their early 20s are much higher than those at older ages, making them difficult to capture using quadratic age trends.

counts of abortions performed at their site. We use AGI data for which an algorithm has been employed that converts data arranged by state of occurrence to one in which it is arranged by the mother's state of residence.[14] We then augment these data with additional data reported by the Center for Disease Control for the 1970 through 1972 period (Centers for Disease Control, 1971, 1972, and 1974). These data are collected through the Vital Statistics system and are believed to include some undercount of the total abortions performed (cf. Saul, 1998), but these are the best data available for this period. We impose an abortion rate of zero for years prior to 1970.[15]

We also use three different measures of the cost of acquiring an abortion as instrumental variables in our analysis. The first, intended to capture high real costs of access to abortion, is whether the cohort was born in one of the five early-repeal states (CA, NY, WA, AK, HI).[16] The second is the average straight-line distance to the nearest county in which abortion is legal (calculated for each county in a state and averaged, weighting by county population). Distance is zero after 1973 and in repeal states, and has been rescaled to go from zero to one in non-repeal states between 1971 and 1973 (a value of one corresponds roughly to 1000 miles, the distance from Louisiana to the nearest repeal state).[17]

Third, we use two different measures of the latent social cost of abortion. The ideal measure would be a measure of tastes for abortion in the pre-legalization period. This variable

[14] The procedure for doing so is described in Levine (2004).

[15] We have also undertaken an exercise designed to test the sensitivity of our results to imposing abortion rates of zero for the pre-1970 period. We took the retrospective data on illegal abortion that is described subsequently as one of our instrumental variables and used it to fill in the missing values of the abortion rate. Since, in this exercise, pre-1970 abortion is the dependent variable in the first stage regression, we cannot use it as an instrument, instead relying solely on our alternative pre-1970 fraction liberal instrument for that purpose. The results of this exercise are very similar to those reported below, with coefficient estimates generally 10-20% larger in magnitude.

[16] We have also considered other proxies for the real costs of obtaining abortion. One alternative measure is distance to the nearest state in which abortion is legal; another measure is distance to the nearest county with an abortion provider. When we replicate our analysis using either of these variables, the results are consistent with those reported here.

[17] Using AGI data on the location of counties with abortion providers, and calculating distance to the nearest abortion provider as in Kane and Staiger (1996), yields very similar results. Because of the potential concern about endogenous location of abortion providers, we do not report these results.

would capture the extent to which women were constrained by social costs when abortion was illegal and, therefore, the extent to which they might take advantage of it upon legalization. Unfortunately, such data are not available. Instead, we use two different measures of states' social climates prior to 1970 as proxies for this ideal measure.

Our first proxy instrument uses data on state political attitudes compiled from 1960s state-level voter surveys conducted by Louis Harris and Associates.[18] In 38 states, at least one Harris poll of a representative sample of voters was conducted in 1962, 1964, or 1966 that asked the question "What do you consider yourself—conservative, middle of the road, or liberal?" We use the fraction of self-identified liberals as one measure of latent costs: where the population is more liberal, the social costs of attaining a legal abortion post-repeal are lower.[19]

Our second proxy is a measure developed by Levine (2004) to capture illegal abortion rates by state before 1970: in places where there was more demand for illegal abortion, the latent costs of abortion were lower. He employs retrospective data from the 1982 and 1988 National Surveys of Family Growth and estimate abortion rates by state for the 1965-1969 period (one observation for each state, aggregating over years to help overcome small sample sizes). These data suffer from recall bias and the general reluctance to report abortions (perhaps even more so if performed while illegal). Nevertheless, they provide a gauge of the potential differences across states that may have existed in the years prior to abortion legalization. This variable is normalized to equal unity in the states in which no abortions were reported during this period and

[18] Historical Harris poll data are available from the data archive of the Odum Institute for Research in Social Science, at http://www.irss.unc.edu/data_archive/home.asp.
[19] In some later polls, the option "radical" was added to the survey question. Respondents who identified themselves as "radical" are grouped with those who identified as "liberal" for the purposes of our analysis.

zero in the state with the greatest number of reported abortions (so that latent abortion costs rise as the index rises).[20]

For each of our proxy measures, data are missing for some states. Twelve states had no survey data on political identification prior to 1970 and two small states did not have pre-1970 estimates of the abortion rate. For both of these measures, we replaced each missing state value with the average value for other states in its Census division (based on the nine Census divisions).[21]

In addition, to provide some controls for the state- and year-specific environment in which the birth or pregnancy decision was made, we use data on the economic and demographic conditions in the state and year of birth. These include per capita income, the crime rate, and the percent of the population that was white, all from the Statistical Abstract (various years). The insured unemployment rate comes from the United States Department of Labor, Employment and Training Administration (1983).

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[20] Nevada has the highest reported abortion rate, but that may also be an outlier due to its small sample size. Among the bigger states, New York had the highest abortion rate at 7 per 1,000 women of childbearing age. This compares to a current (year 2000) value of 36.

State of residence in the survey year is not available in the public use file of the NSFG. Researchers can access these data, however, by visiting the National Center for Health Statistics and conducting the analysis in their Research Data Center. For the purposes of this project, we assign the state of residence in the survey year to the respondent's residence in all preceding years, as Gruber, et al. (1999) have done.

[21] We have also considered other proxies for latent costs that have the advantage of greater state coverage, but the disadvantage of being available only post-repeal. These include state attitudes towards abortion itself, measured in either the General Social Survey (which covers some states beginning in 1972) or the DDB Needham Lifestyle Survey (starting in 1985). In addition, NARAL (2003) compiles recent state rankings of access to abortion. Each of these measures is strongly correlated with both the pre-1970 abortion rate and state political attitudes, and using each as an instrument yields similar results.

VI. CENSUS RESULTS

A. Reduced Form

We begin our analysis by presenting reduced form results based on the methodology used in GLS that lays out clearly the impact of abortion legalization on cohort outcomes. In particular, following GLS, we estimate models of the form:

$$\ln(OUTCOME_{st}) = \rho_1 Repeal_s{*}D7173 + \rho_2 Repeal_s{*}D7475 + \rho_3 Repeal_s{*}D7679 + controls + \epsilon_{st}. \quad (8)$$

where Repeal is a dummy variable for states that are early repealers of abortion restrictions, and the time dummies are as defined above. As with the other models described above, we include in this model state and cohort dummies, quadratic trends by state of birth, and controls for economic and demographic conditions in the state and year of birth. The dependent variables are all in logs, so that the coefficients can be interpreted in percentage terms.

This model serves to examine whether or not any gap in outcomes that emerged in 1971-1973 across these groups of states closed afterward when abortion was legal everywhere. That is, if abortion legalization improves average cohort outcomes through selection, then cohort outcomes should improve in the repeal states in 1971-1973 ($\rho_1 <0$, since outcomes are negative), but then "catch up" in other states after 1973 (ρ_2 , ρ_3 ~=0).

Table 1 reports the estimated impact of abortion legalization on key fertility outcomes. The first three columns report results for the log of the birth rate, pregnancy rate, and birth ratio (births/pregnancies). Similar to Levine, et al. (1999) and GLS, we estimate that birth rates declined by 5.7 percent in repeal states relative to non-repeal states in the 1971-1973 period, but that the gap shrunk after 1973 and had disappeared by 1976-1979. Both the initial decline and the "catch up" by the non-repeal states are strongly statistically significant. Estimates for the pregnancy rate and birth ratio are quite different. As expected, pregnancy rates increased by 12.9 percent in repeal states relative to non-repeal states in the 1971-1973 period, but there was no significant catch up and the gap remained at 10.9 percent by 1976-1979. Similarly, the birth ratio declined by 18.7 percent in repeal states relative to non-repeal states in 1971-1973, but by 1976-1979 this gap had only partially disappeared. Thus, while birth rates had converged by 1976, pregnancy rates had risen and birth ratios had fallen in repeal states relative to non-repeal states. This is the same pattern as was observed in Figure 1 and, as discussed earlier, suggests that abortion costs may have continued to be lower in repeal states after 1976. It also suggests that other outcome differences between repeal and non-repeal states may persist beyond the 1971-1973 period, since marginal pregnancies are replacing marginal births at a higher rate in the repeal states.

In column 4 of Table 1 we confirm that the fertility effects associated with abortion legalization that we observe in vital stats data are present 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 the 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.[22] Indeed, our estimates indicate that these constructed survival rates are just over 4 percent lower in repeal states relative to non-repeal states in cohorts born between

[22] In fact, we know that this assumption, taken in its strongest terms, is inaccurate. GLS show 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.

1971 and 1973 relative to that from earlier cohorts, and not significantly different in later years. These results mimic rather closely those in column 1, and verify that differences in cohort size in the 2000 data reflect the earlier impact of abortion legalization.

The final two columns of Table 1 document that legalized abortion resulted in selection at the time of birth. Moreover, they provide some evidence that the resulting gap in average characteristics of birth cohorts did not completely disappear after 1973. The dependent variables in these regressions are the percent of all infants born to minors (under the age of 18), and the percent born to nonwhite mothers, derived from Vital Statistics data from the Natality Detail files for each year between 1965 and 1979. Vital Statistics outcomes are based on all births, and tend to produce more precise estimates than outcomes based on the 5 percent sample from the 2000 Census. The percent of infants born to minors decreased by 8.5 percent in repeal states relative to non-repeal states in the 1971-1973 period, but there was no significant catch up and the gap remained at 11.5 percent by 1976-1979. The results for percent born to nonwhite mothers, on the other hand, are more consistent with catch up in the non-repeal states. Thus, despite a convergence in birth rates, some of the average characteristics of the birth cohorts did not converge in repeal and non-repeal states. This evidence suggests that changes in the birth rate alone may not be sufficient to identify selection into a cohort.

This conclusion is supported in Table 2, which reports the differential patterns in repeal and non-repeal states in children's outcomes as adults, including poverty status, welfare receipt, single parenthood, educational attainment, employment, and the likelihood of being incarcerated.[23] All outcomes are defined to be negative in terms of socio-economic status (e.g.,

[23] 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.

in poverty, not a college graduate) so that selection due to lower cost of abortion would predict a negative coefficient on repeal*1971-1973. The coefficient on repeal*1976-1979 should be zero (or less negative) if there is convergence in these outcomes following legalization in non-repeal states consistent with the convergence in birth rates. The failure to converge may signal continued differences in selection later in the 1970s beyond the change in the birth rate as marginal pregnancies replace marginal births.

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 GLS. The results on education are perhaps most striking, with a large negative effect on the percent of the cohort who did not graduate from college, indicating that abortion legalization shifted the distribution of education upward. But these results provide no evidence of convergence; coefficients on repeal*1974-1975 and repeal*1976-1979 are generally of the same sign and larger than the coefficient on repeal*1971-1973. Again, consider the results for college graduation. The coefficient on repeal*1971-1973 shows that the odds of not graduating from college fell by 2.7 percent in the early repeal states over the 1971-1973 period, relative to other states, but the coefficient on repeal*1976-1979 is larger yet, indicating that the odds of not graduating from college fell 3.8 percent in early repeal states even in the later period. These results provide further support for the possibility that marginal pregnancies continued to replace marginal births differentially in early-legalization states even after abortion was legalized nationally.

B. First Stage Estimates

This leads us to our updated methods for estimating selection effects based upon the model described in equations (6) and (7). Before presenting OLS and IV results from these models, we first report the results of the first stage regressions, which relate differences in proxies for abortion costs to the log of the birth rate and birth ratio. We also estimate first stage models where the log of the pregnancy rate is the dependent variable because of its importance in equation (2) and because we have unambiguous predictions regarding the impact of abortion costs on this outcome.

Table 3 reports two sets of first stage estimates, corresponding to our two instrumental variables strategies; the results are similar for both sets of instruments, and are consistent with our predictions. Columns 1 and 4 provide support for the prediction that higher abortion costs reduce the pregnancy rate. Non-repeal states had lower pregnancy rates during the 1971-1973 period and, among non-repeal states, those states with higher latent social costs of abortion (based on pre-1970 liberalism or on the illegal abortion rate) experienced lower pregnancy rates. The same pattern continues in the 1974-1975 and 1976-1979 periods. As expected, the negative relationship between latent costs and the pregnancy rate becomes more pronounced after legalization: once the legal constraints are removed, the underlying latent costs of abortion become the primary determinant. In the remainder of the table, we see that a higher cost of abortion (as proxied by non-repeal, latent cost of abortion, and distance) was associated with higher birth ratios (or equivalently, fewer abortions) and higher birth rates. The first stage F-statistics for each of these regressions are reasonably high, sufficiently large to rule out weak instrument problems.[24]

[24] IV estimation of the more general specification in Equation 2 requires instruments for both the pregnancy rate and the birth ratio. A generalization of the first-stage F-statistic to the case of two endogenous variables, described in Staiger and Stock (1997), indicated that our instruments were too weak to reliably estimate Equation 2. The instruments do not generate sufficient independent variation to reliably estimate the coefficient on each of the endogenous variables.

C. OLS and IV Results

Regression results based on equations (6) and (7) are reported in Tables 4 and 5, respectively. In each table, each panel represents regressions on a different outcome. There are three columns in each table, for OLS and our two IV strategies. For each IV regression, we also show the p-value for the Hausman test that OLS and IV outcomes are significantly different, and the p-value for the over-identification test of the instrument set.

In Table 4 (where the key independent variable is the log of the birth rate from equation 6), the pattern of coefficients is fairly consistent with selection, with positive IV coefficients for living in poverty, being a single parent, receiving welfare, being a high school dropout, and not graduating from college. The IV results are wrong-signed (negative) for being not employed and being incarcerated. The results for single parenthood and college graduation are statistically significant with either instrument, and for welfare receipt are marginally significant.

The findings are similar in Table 5: positive (expected sign) coefficients on education, welfare receipt, poverty rate, and single parenthood; negative (wrong-signed) for being not employed and being incarcerated. Once again, the results for single parenthood, welfare receipt and college graduation are all at least marginally significant with both instruments.

The results suggest sizeable effects of selection on outcomes. Recall that the estimates from the specifications in Table IV and Table V provide a bound on the difference between the marginal and average birth. Therefore, the estimates in the IV columns in each table suggest that the marginal birth is 23 to 69 percent more likely to be a single parent, 73 to 194 percent more likely to receive welfare, and 12 to 31 percent less likely to graduate college.

Thus, overall, we find evidence that is consistent with long-run selection effects through abortion. While the statistical significance of our findings depends on the particular outcome under consideration, it is robust to the choice of instrument, and the pattern is clear: when abortion costs are lowered, cohort outcomes improve.

VII. ABORTION AND CRIME REVISITED

While our Census analysis allows us to consider a wide variety of cohort outcomes, much of the attention in this literature has focused on the specific analysis of the relationship between abortion and crime. In this section, we apply the methods described earlier to re-examine DL's findings regarding the relationship between abortion and crime. The main practical improvement that we offer relative to their work is that we estimate IV models where changes in the birth ratio and birth rate are instrumented using changes in abortion policy.

The data on arrest rates used by DL do not just come from a single cross-section, as with our data on Census outcomes. Rather, their data—arrest rates for murder, violent crime, and property crime by age, year, and state, compiled from *Crime in the United States* (annual)—provide multiple observations for each cohort.[25] The data we use cover the years 1985 through 1996 and include the number of arrests for single cohort/state of residence cells born between 1965 and 1979 and observed at ages 15 to 24.[26]

These data offer econometric advantages relative to the 2000 Census data that we used earlier. Because multiple observations exist for those of each age in each state, we can control non-parametrically for age effects by state, and do not need quadratic trends. We can also

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[25] We are grateful to those authors for providing us with their data.

[26] DL actually use the 1961 to 1981 birth cohorts, but we have restricted our analysis to 1965-1979 for consistency with the rest of our analysis. We have replicated their results using this restricted cohort range and found that it did not alter them much. Our specifications also differ from theirs in other ways that have little impact on the findings. First, we include some explanatory variables in our models that vary by birth cohort and state to reflect differences in social circumstances in the year the child was born. Second, we do not use the District of Columbia in our analysis because some of these explanatory variables do not exist for DC. We also cluster our standard errors by state, rather than state/year. Finally, we weight our regressions by the state population in the year the cohort was born, while they weight their estimates by the state population in the year the crime was committed. None of these modifications substantively changes the results.

include fixed effects for age by year, state by year, and state by age, a stronger way to address concerns about the conflation of age and time effects within states.[27]

The results of our analysis are reported in Table 6. The top panel of this table presents estimates for specifications using the log of the birth ratio as the right hand side variable. This specification most closely resembles that estimated by DL, which used the ratio of abortions to births as the right hand side variable. Since the abortion ratio declines as the birth ratio rises, the coefficients in our specification will have the opposite sign but should otherwise be similar in magnitude. We present OLS and IV estimates for violent and property crime using two different methods of measuring crime. The first three columns use the log of the number of crimes as the dependent variable, which is the specification initially reported by DL. The second three columns use the log of the number of crimes *per capita* as the dependent variable, where population for each state-year-age cell was obtained from intercensal estimates available from the National Cancer Institute (as in Foote and Goetz, 2005). Crime per capita corresponds to the average outcome in each cohort, and therefore these estimates correspond more closely to the estimates presented in our earlier tables.

The first three columns of the top panel of this table replicate the results of DL using our data and specification, presenting OLS results for violent and property crime and using the log of the number of crimes as the dependent variable. Despite a number of differences in the exact data sources and control variables being used, our coefficient estimates are quite similar to the

[27] Foote and Goetz (2005) and DL (2006) debate the proper role of including all of these interaction terms. Our approach is to include all of them in every specification and to test the sensitivity of the results to our alternative identification strategies.

original DL estimates in both magnitude and significance.[28] The IV versions of these specifications yield similar estimates.

As noted recently by Foote and Goetz (2005), however, these specifications do not measure the selection effect alone, since the total number of crimes committed by a cohort depends on both the cohort size and selection into the cohort. Since the birth rate was estimated to fall by approximately 6 percent following legalization of abortion, much of the estimated impact on crime could be through cohort size rather than the selection effect.

The last three columns of the top panel of Table 6 use the log of the number of crimes *per capita* as the dependent variable. Crime per capita corresponds to the average outcome in each cohort, and therefore these estimates correspond more closely to the estimates presented in our earlier tables. Both the OLS and IV estimates for these specifications are noticeably smaller and none are statistically significant.

The second panel of Table 6 replicates this analysis using our alternative independent variable, the log of the birth rate. In the first three columns, point estimates are similar to those from the top panel, but standard errors are larger, making it difficult to detect statistically significant effects. In the last three columns regarding crime per capita, some of the estimates turn negative, but again the standard are large, making it difficult to draw firm conclusions.

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[28] We have replicated their original results and reconciled them with our estimates. Our specifications differ in a number of ways, but we found that the estimates were relatively robust to most of these differences. The major differences are (1) we included state-year fixed effects in our specification, which were inadvertently omitted by DL, (2) we used more complete data on abortions from CDC for the 1971-1973 period, while they assumed abortions were zero in non-repeal states during this period, (3) we restricted the sample to cohorts born 1965-1979, (4) we included some explanatory variables in our models that vary by birth cohort and state to reflect differences in social circumstances in the year the child was born, (5) we did not use the District of Columbia in our analysis because some of these explanatory variables did not exist for DC, (6) we clustered our standard errors by state, rather than state/year, (7) we weighted our regressions by the state population in the year the cohort was born, while they weight their estimates by the state population in the year the crime was committed, and (8) we used the log of the birth ratio in place of the ratio of abortions to births. Only the first two changes had any substantial impact on the estimates: including state-year fixed effects reduced the magnitude of the estimates, while using our more complete abortion data increases the magnitude of the estimates.

Overall, our analysis shows that Donohue and Levitt's results, that increased abortion access was associated with a decline in crime, are not sensitive to the alternative identification strategies that we describe. However, it is unclear how much of the decline in crime came through the selection effect.

VIII. CONCLUSIONS

In this paper, we have extended past analyses of abortion and selection in several dimensions. First, we have provided a new theoretical model that provides useful insights regarding the mechanisms that may result in selection associated with changes in the cost of abortion. The main insight there is that selection may still occur even if birth rates are unaffected since the composition of births may have changed. Second, we have introduced an econometric methodology that encompasses the differing approaches taken by past researchers. Although we are unable to implement this methodology directly due to limited power, we can identify what restrictions need to be imposed for the past methods to be consistent with our theoretical model. Since our findings are broadly consistent across these specifications, the impact of the different restrictions does not appear to be very dramatic. Third, we have updated the literature on abortion and selection to include outcomes in early adulthood. We found consistent evidence that changes in cohort composition that occurred in the 1970s that can be attributed to greater abortion access led to improved cohort outcomes, particularly in the form of higher rates of college graduation, lower rates of single motherhood, and lower rates of welfare receipt. Finally, we reconsidered the analysis of abortion and crime originally conducted by Donohue and Levitt to incorporate our updated methodological framework. The results of this analysis support the association between abortion and crime, but suggest that it is difficult to associate their finding with selection as opposed to the direct effect of cohort size.

Most importantly, taken together with earlier results (Gruber, et al., 1999), our findings suggest that the improved living circumstances experienced by the average child born after the legalization of abortion had a lasting impact on the lifelong prospects of these children. Children who were "born unwanted" prior to the legalization of abortion not only grew up in more disadvantaged households, but they also grew up to be more disadvantaged as adults. This conclusion is in line with a broad literature documenting the intergenerational correlation in income (Solon, 1999) and showing that adverse living circumstances as a child are associated with poorer outcomes as an adult (Haveman and Wolfe, 1995). Overall, our results provide further evidence that abortion is associated with differential selection and its impact is persistent.

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Table 1: Impact of Abortion Legalization on Fertility Outcomes

	(1) Log Birth Rate	(2) Log Pregnancy Rate	(3) Log Birth Ratio	(4) Log Survivor Rate	(5) Log % Born to Minor	(6) Log % Born to Nonwhite Mother
Repeal*1971-1973	-0.057	0.129	-0.187	-0.043	-0.085	-0.053
	(0.012)	(0.044)	(0.035)	(0.013)	(0.020)	(0.025)
Repeal*1974-1975	-0.018	0.145	-0.162	-0.000	-0.113	-0.024
	(0.019)	(0.055)	(0.041)	(0.029)	(0.019)	(0.025)
Repeal*1976-1979	0.006	0.109	-0.103	0.039	-0.115	-0.005
	(0.023)	(0.064)	(0.045)	(0.049)	(0.026)	(0.033)
Mean of Dependent Variable (Before Logs)	7.8%	8.8%	89.1%	5.3%	6.8%	17.8%
P-value for test of equality of coefficients on repeal*1971-1973 and repeal*1976-1979	0.00	0.40	0.00	0.04	0.22	0.01
P-value for test of joint significance of reported coefficients	0.00	0.00	0.00	0.00	0.00	0.01

Notes: Reported coefficients are for 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 specifications (1)-(4) 750 observations are available representing the 50 states and 15 years of birth cohorts (1965-1979). Specifications (5) and (6) are limited to 600 observations (1968-1979). All standard errors are clustered at the state level.

Table 2: Impact of Abortion Legalization on Adult Outcomes, 2000 Census

	(1) Log % in Poverty	(2) Log % Single Parent	(3) Log % on Welfare	(4) Log % HS Dropout	(5) Log % Not College Graduate	(6) Log % Not Employed	(7) Log Incarcerated per 1000
repeal*1971-1973	-0.036	-0.033	-0.148	-0.002	-0.027	0.018	0.007
	(0.044)	(0.038)	(0.056)	(0.053)	(0.009)	(0.020)	(0.154)
repeal*1974-1975	-0.085	-0.027	-0.215	0.014	-0.020	0.014	0.102
	(0.071)	(0.058)	(0.154)	(0.042)	(0.013)	(0.035)	(0.264)
Repeal*1976-1979	-.191	-0.008	-0.177	-0.018	-0.038	-0.016	-0.295
	(0.076)	(0.083)	(0.233)	(0.050)	(0.020)	(0.048)	(0.662)
Mean of Dependent Variable (Before Logs)	10.3%	9.0%	2.1%	7.4%	70.6%	22.5%	4.8
P-value for test of equality of coefficients on repeal*1971-1973 and repeal*1976-1979	0.00	0.64	0.88	0.83	0.44	0.32	0.58
P-value for test of joint significance of reported coefficients	0.01	0.39	0.04	0.69	0.01	0.24	0.50

Notes: Reported coefficients are for 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. All specifications include 750 observations representing the 50 states and 15 years of birth cohorts (1965-1979), with the following exceptions: Specifications (1) and (5) exclude 1977-1979 because % in poverty and % not college graduate increase sharply at these ages; specification (3) excludes one observation with 0 % in welfare; and specification (7) excludes 50 observations with 0 incarcerated. All standard errors are clustered at the state level.

Table 3: First Stage Regressions

	Latent Cost Based on Pre-1970 % Liberal			Latent Cost Based on Pre-1970 Abortion Rates		
	(1) ln(Pregnancy Rate)	(2) ln(Birth Rate)	(3) ln(Birth Ratio)	(4) ln(Pregnancy Rate)	(5) ln(Birth Rate)	(6) ln(Birth Ratio)
1971-1973 *						
Non-repeal	-0.099	0.021	0.120	-0.109	0.019	0.127
	(0.048)	(0.014)	(0.040)	(0.051)	(0.020)	(0.043)
Non-repeal*"latent cost"	-0.031	0.061	0.092	-0.021	0.035	0.056
	(0.031)	(0.026)	(0.023)	(0.031)	(0.023)	(0.030)
Non-repeal*distance	-0.027	0.018	0.045	-0.010	0.023	0.033
	(0.012)	(0.016)	(0.012)	(0.008)	(0.010)	(0.012)
1974-1975 *						
Non-repeal	-0.048	-0.024	0.023	-0.044	-0.042	0.001
	(0.064)	(0.024)	(0.050)	(0.077)	(0.035)	(0.062)
Non-repeal*"latent cost"	-0.205	0.090	0.295	-0.125	0.075	0.201
	(0.046)	(0.030)	(0.040)	(0.061)	(0.037)	(0.055)
1976-1979 *						
Non-repeal	0.011	-0.041	-0.053	0.034	-0.066	-0.101
	(0.076)	(0.031)	(0.059)	(0.090)	(0.046)	(0.071)
Non-repeal*"latent cost"	-0.267	0.079	0.346	-0.176	0.076	0.252
	(0.060)	(0.043)	(0.054)	(0.069)	(0.048)	(0.061)
# of Observations	750	750	750	750	750	750
First-Stage F-Statistic	14.9	32.0	37.1	9.9	33.4	13.9
p-value of F-test	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Models also 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. Distance and Latent Cost are scaled 0-1, with zero representing the state with the lowest cost and distance (highest pre-1970 % liberal or abortion rate). Standard errors and F-statistics account for clustering at the state level.

Table 4: Estimates of Relationship between Log Birth Rate and Adult Outcomes
(Standard Errors in Parentheses)

Outcome	OLS	IV 1	IV 2
% Living in Poverty	0.084	0.199	0.113
	(0.198)	(0.354)	(0.383)
p-value for Hausman test		0.731	0.938
p-value for over-ID test		0.458	0.620
% Single Parent	0.161	0.665	0.689
	(0.229)	(0.318)	(0.333)
p-value for Hausman test		0.166	0.142
p-value for over-ID test		0.720	0.646
% Receiving Welfare	0.179	1.939	1.720
	(0.539)	(0.936)	(0.956)
p-value for Hausman test		0.043	0.075
p-value for over-ID test		0.666	0.293
% High School Dropout	0.314	0.472	0.411
	(0.325)	(0.690)	(0.740)
p-value for Hausman test		0.787	0.877
p-value for over-ID test		0.832	0.721
% Not College Graduate	0.219	0.311	0.289
	(0.061)	(0.119)	(0.126)
p-value for Hausman test		0.306	0.477
p-value for over-ID test		0.324	0.591
% Not Employed	0.137	-0.112	-0.275
	(0.166)	(0.219)	(0.183)
p-value for Hausman test		0.259	0.037
p-value for over-ID test		0.597	0.856
Incarcerated per 1000	-0.648	-1.332	-2.819
	(1.050)	(1.490)	(2.144)
p-value for Hausman test		0.624	0.252
p-value for over-ID test		0.587	0.059

Notes: IV1 represents the specification that includes state pre-1970 fraction liberal as an instrument. IV2 represents the specification that includes state pre-1970 abortion rates as an instrument. Models also 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. Sample sizes are the same as those reported in Tables 1 and 2. Standard errors and test statistics account for clustering at the state level. The excluded instruments in the IV models are those reported in Table 3. For poverty and college graduate we only use 1965-1976, so the instruments used 1974-1976 interactions, rather than 1974-1975 and 1976-1979 interactions.

Table 5: Estimates of Relationship between Log Birth Ratio and Adult Outcomes
(Standard Errors in Parentheses)

Outcome	OLS	IV 1	IV 2
% Living in Poverty	0.125	0.019	0.030
	(0.091)	(.153)	(.158)
p-value for Hausman test		0.201	0.328
p-value for over-ID test		0.426	0.618
% Single Parent	0.123	0.240	0.232
	(0.128)	(.130)	(.123)
p-value for Hausman test		0.241	0.292
p-value for over-ID test		0.666	0.434
% Receiving Welfare	0.448	0.790	0.733
	(0.238)	(.241)	(.243)
p-value for Hausman test		0.067	0.158
p-value for over-ID test		0.536	0.269
% High School Dropout	0.260	0.144	0.098
	(0.139)	(.230)	(.265)
p-value for Hausman test		0.392	0.324
p-value for over-ID test		0.739	0.671
% Not College Graduate	0.112	0.120	0.120
	(0.023)	(.026)	(.028)
p-value for Hausman test		0.746	0.754
p-value for over-ID test		0.201	0.688
% Not Employed	-0.001	-0.062	-0.124
	(0.082)	(.082)	(.078)
p-value for Hausman test		0.241	0.060
p-value for over-ID test		0.584	0.879
Incarcerated per 1000	-0.490	-0.812	-1.346
	(0.432)	(.613)	(.878)
p-value for Hausman test		0.427	0.121
p-value for over-ID test		0.647	0.108

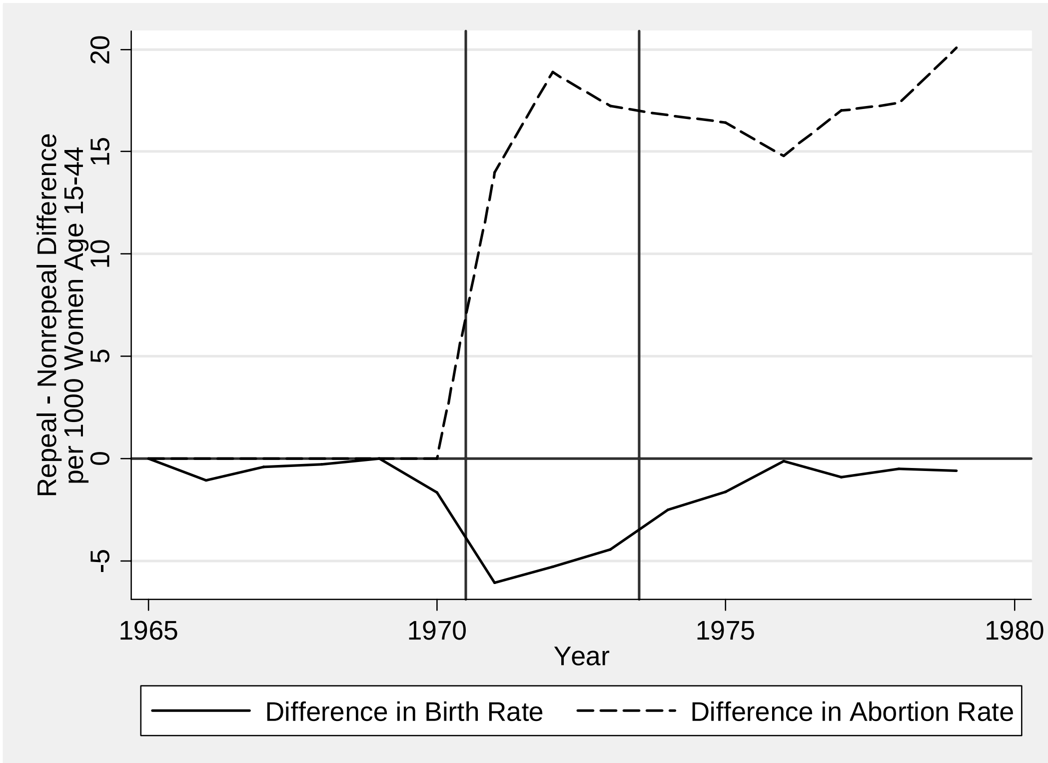
Notes: IV1 represents the specification that includes state pre-1970 fraction liberal as an instrument. IV2 represents the specification that includes pre-1970 abortion rates as an instrument. Models also include the insured unemployment rate, per capita income, crime rate, percent of the population that is nonwhite, a full set of state and year dummies, and state-specific quadratic trends. Sample sizes are the same as those reported in Tables 1 and 2. Standard errors and test statistics account for clustering at the state level. The excluded instruments in the IV models are those reported in Table 3. For poverty and college graduate we only use 1965-1976, so the instruments used 1974-1976 interactions, rather than 1974-1975 and 1976-1979 interactions.

Table 6: Estimates of Relationship between Log Birth Ratio or Log Birth Rate and Crime
(Standard Errors in Parentheses)

	Crime			Crime Per Capita		
	OLS	IV1	IV2	OLS	IV1	IV2
	Key Explanatory Variable: ln(Birth Ratio)					
Arrest Rate for Violent Crime	0.387	0.310	0.259	0.209	0.124	0.074
	(0.112)	(0.127)	(0.142)	(0.126)	(0.171)	(0.190)
p-value for Hausman test		0.278	0.129		0.317	0.159
p-value for over-ID test		0.365	0.351		0.146	0.130
Arrest Rate for Property Crime	0.249	0.306	0.308	0.070	0.119	0.123
	(0.094)	(0.099)	(0.104)	(0.104)	(0.094)	(0.100)
p-value for Hausman test		0.180	0.280		0.219	0.314
p-value for over-ID test		0.260	0.195		0.018	0.050
	Key Explanatory Variable: ln(Birth Rate)					
Arrest Rate for Violent Crime	0.387	0.307	0.383	-0.068	-0.077	0.000
	(0.287)	(0.427)	(0.529)	(0.283)	(0.519)	(0.582)
p-value for Hausman test		0.990	0.998		0.975	0.852
p-value for over-ID test		0.324	0.275		0.159	0.138
Arrest Rate for Property Crime	0.185	0.387	0.473	-0.269	-0.061	0.090
	(0.210)	(0.318)	(0.318)	(0.210)	(0.352)	(0.342)
p-value for Hausman test		0.302	0.188		0.327	0.122
p-value for over-ID test		0.209	0.037		0.107	0.112

Notes: IV1 represents the specification that includes pre-1970 fraction liberal as an instrument. IV2 represents the specification that includes pre-1970 abortion rates as an instrument. Models also include the insured unemployment rate, per capita income, crime rate, percent of the population that is nonwhite, and a full set of age-by-state dummies, year-by-state dummies, and year-by-cohort (e.g. year born) dummies. The excluded instruments in the IV models are those reported in Table 3. Reported standard errors are corrected for heteroskedasticity and for arbitrary forms of covariance across observations within the same state over time. Sample size is 5064. Four observations with zero violent crime were dropped from the log specification.

Figure 1. Difference Between Repeal and Non-Repeal States in the Birth Rate and Abortion Rate per 1000 women age 15-44, 1965-1979



Note: Differences in abortion rates prior to 1970 are set at zero due to lack of data for this period.

Figure 2. Relationship Between Cost of Abortion and Probability of Pregnancy and Birth

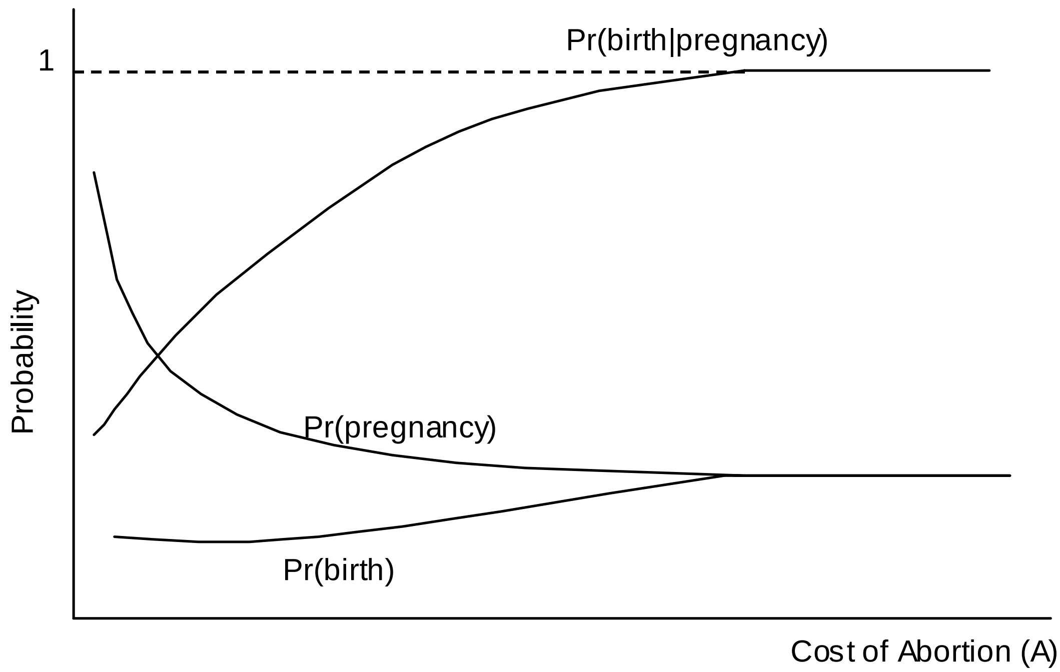


Figure 3. Implications for the Probability of Pregnancy and Birth, Given the Likely Cost of Abortion in Repeal (R) and NonRepeal (N) States in 1965, 1972, and 1976.

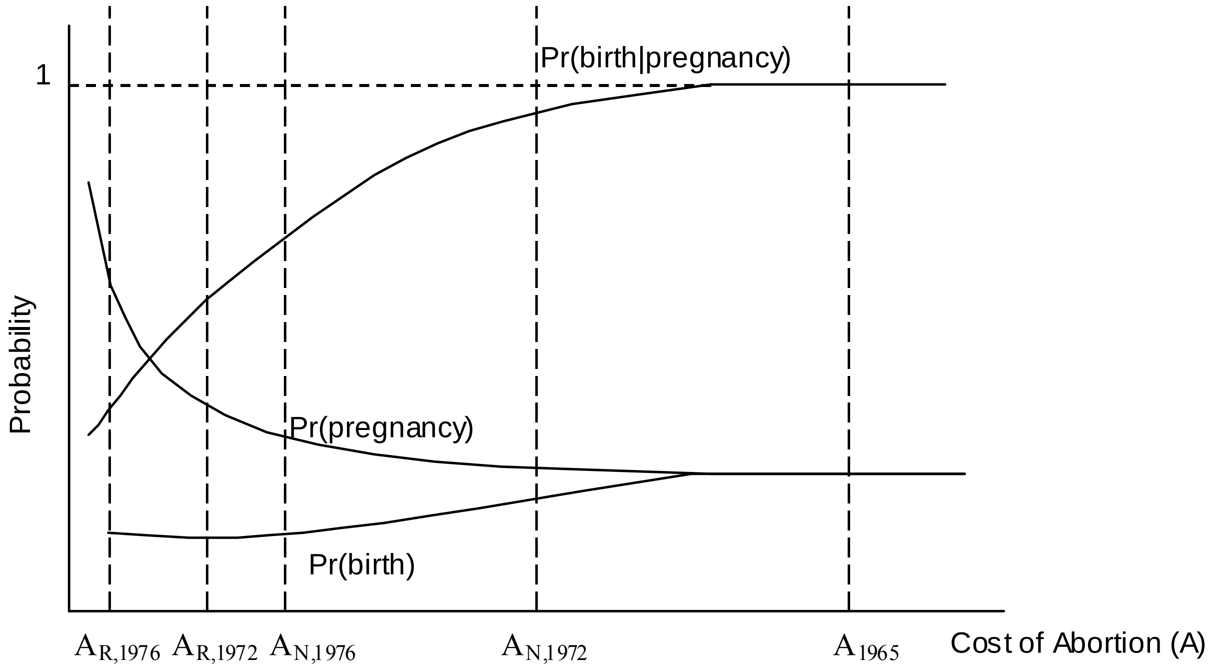


Figure 4: Relationship Between Marginal Pregnancy, Marginal Birth, and Average Birth

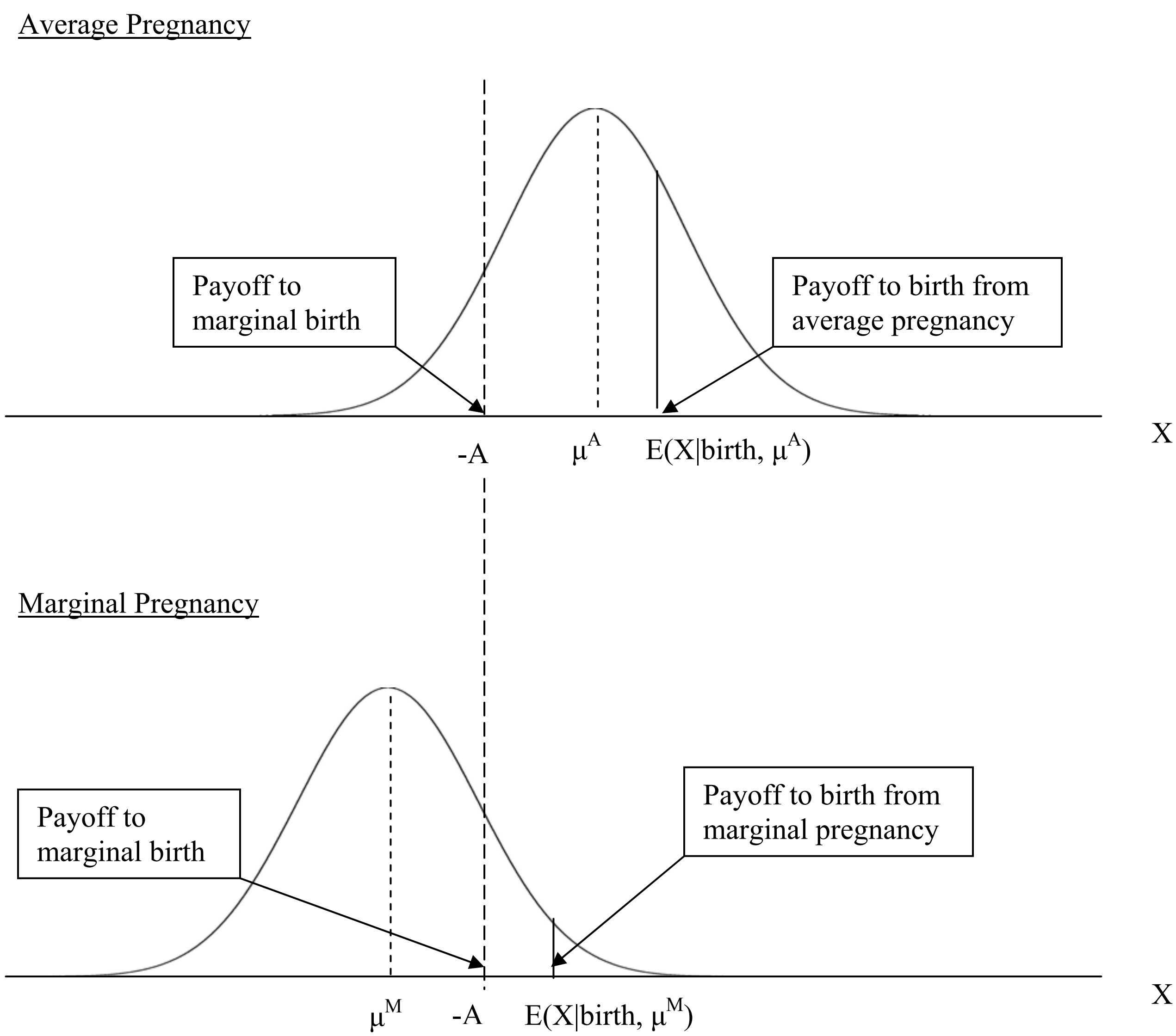
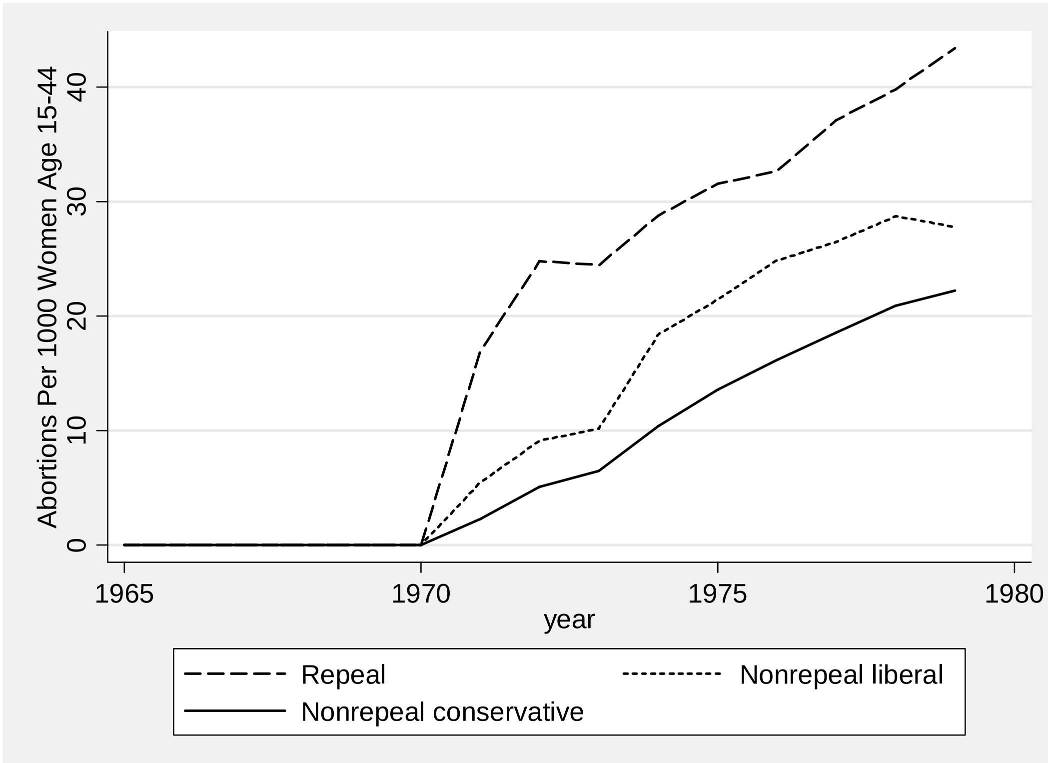


Figure 5. Trends in Abortion Rate per 1000 Women Age 15-44 in Repeal State and Liberal and Conservative Non-repeal States, 1965-1979



Note: Liberal states defined as states with at least 22% of survey respondents identifying themselves as liberal, based on survey data from the 1960s.

Appendix A: Formal Solution to Theoretical Model

A. Setup

Consider the following simple model. A woman initially chooses whether to become pregnant or not. If she does not become pregnant, she receives a payoff normalized to 0. If she becomes pregnant, she receives additional information regarding the payoff to giving birth, and then can choose to give birth and receive a payoff of X, or have an abortion and receive a payoff of –A, where A represents the cost (both monetary and psychic) of an abortion and is assumed to be nonnegative.[29] The woman's objective is to maximize her expected payoff.

The payoff that a woman receives if she gives birth (X) is known when she chooses between abortion and birth, but uncertain at the time of choosing to become pregnant. At the time of choosing to become pregnant, a woman knows that the distribution of X is $N(\mu,\sigma^2)$. Thus, μ represents each woman's expected payoff of a birth at the time of becoming pregnant, and σ^2 captures how much that expectation may change between the pregnancy and abortion decision because of new information. Since women in the population are likely to heterogeneous, we allow the expected payoff from giving birth to vary across women with distribution $f(\mu)$.

B. Solution

The solution to this model is straightforward, and is derived by working backwards. The decision between abortion and birth is made after learning the birth payoff (X). Thus, if the birth payoff is greater than the cost of abortion (X>-A), a woman will give birth and receive a payoff

[29]Since the main purpose of this model is to make positive, rather than normative, statements, we do not distinguish between private and social costs or benefits. More generally, one could allow for externalities associated with unwanted births or abortions.

of X. Otherwise, the woman will abort and receive a payoff of -A. Therefore, ex ante (prior to learning X) the probability that a pregnant woman will give birth is given by:

(1) $\Pr(\text{birth}|\text{pregnant}) = \Pr(X>-A) = \Phi[(\mu+A)/\sigma]$

where $\Phi[.]$ is the standard normal CDF. Equation (1) implies that the probability that a pregnant woman gives birth increases with both the cost of abortion (A) and the expectation she has about the birth payoff at the time of becoming pregnant (μ). This probability is conditional on μ, while the unconditional probability would be integrated over the distribution of μ among women who get pregnant.

Conditional on giving birth, the payoff that a woman expects to receive (prior to learning X) is simply $E(X|X>-A)$. Since X is normally distributed this is the mean of a truncated normal, which increases in both the mean and variance of X, and also in the truncation point (-A).

Each woman chooses to become pregnant if the expected payoff from getting pregnant is greater than zero (the payoff from not getting pregnant). At the time of choosing to become pregnant, the expected payoff from being pregnant is greater than zero when:

(2) $E(\text{payoff}|\text{pregnant}) > 0$

$\leftrightarrow E(\text{payoff}|\text{birth})*\Pr(\text{birth}|\text{pregnant}) + E(\text{payoff}|\text{abort})*\Pr(\text{abort}|\text{pregnant}) > 0$

$\leftrightarrow E(X|X>-A)*\Phi[(\mu+A)/\sigma] - A*\{1-\Phi[(\mu+A)/\sigma]\} > 0$

Equation (2) implies that a woman's decision to become pregnant is determined by μ and A. For any cost of abortion (A) there is an expected benefit of birth (μ^A) such that the expected payoff of pregnancy is exactly zero, and all women with $\mu > \mu^A$ will choose to become pregnant. Not surprisingly, it is straightforward to show that the expected payoff to pregnancy increases as the cost of abortion (A) declines and as the expected benefit of a birth (μ) grows. Thus, higher costs of abortion will lead to a higher value of μ^A, i.e. $\partial\mu^A/\partial A>0$.

C. Implications

In this model, a change in the cost of abortion affects behavior on both the pregnancy margin and on the birth margin. On the pregnancy margin, we have:

(3) $\Pr(\text{pregnant}) = \Pr(\mu>\mu^A)$

Lower costs of abortion (A) lead to a lower value of μ^A, and pregnancy rates will increase in the population. Moreover, when the cost of abortion declines the marginal pregnancy that occurs has a lower expected birth payoff (μ^A) than all other pregnancies (since $\mu>\mu^A$ for all other pregnancies). This implies that the births resulting from these marginal pregnancies will have lower expected payoffs than the average birth. Through the addition of these marginal pregnancies, lower cost abortion will reduce the expected payoff of the average birth.

On the birth margin, we have:

(4) $\Pr(\text{birth}|\text{pregnant}) = \int_{\mu>\mu A} \Pr(X>-A)f(\mu)d\mu = \int_{\mu>\mu A} \Phi[(\mu+A)/\sigma]\, f(\mu)\, d\mu$

Conditional on being pregnant, lower cost abortion (A) will reduce the probability of birth (and increase the probability of abortion) for all women, so the proportion of pregnancies that end in birth will fall in the population. The marginal births that are aborted will have lower expected benefits (X=-A) than the average birth (where X>-A). Through the elimination of marginal births, lower cost abortion will increase the expected benefit among the remaining births.