ESSAYS ON THE IMPACT OF LEGALIZED ABORTION AND THE PILL ON CHILDBEARING BEHAVIOR AND BIRTH OUTCOMES

by

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Abstract

ESSAYS ON THE IMPACT OF LEGALIZED ABORTION AND THE PILL ON CHILDBEARING BEHAVIOR AND BIRTH OUTCOMES.
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This dissertation is composed with two essays on reproductive behavior and related health outcomes. Recent studies on the "power of the Pill" have not adequately accounted for the role of abortion in the years between 1970 and 1973. In the first essay, I use rediscovered data on abortions performed in New York State in 1971 and 1972 by age, race and state of residence to demonstrate the remarkable impact of legal abortion services in New York on the fertility rates of young women as far away as Montana prior to *Roe v. Wade*. My results strongly suggest that laws enhancing access to legalized abortion more than the Pill caused birth rates of young women to fall in the early 1970s.

In the second essay, I explore the hypothesis that abortion circumstances faced by women of childbearing age affect the subsequent fertility behavior of their daughters when they reach the ages of 15 through 24. Total and out-of-wedlock birth rate among teenagers and young women in the U.S. declined remarkably in early 1990s. Previous studies suggested that the legalization of abortion in the 1970s might contribute to this decline. I examine the impact of exposure to legalized abortion *in utero* on women's childbearing behavior using extended abortion data and new empirical design. The results suggest legalized abortion between 1970 and 1979, as a supply shock, reduces birth rate among teenagers born in this period by 5-10% and reduces birth rate among young women born in this period by 3-8%, but it has nearly no impact on women's marital

status when they give birth. In the post legalizing period, 1980-1987, abortion exposure in utero is positively associated with teen birth rate. It implies abortion legalization may initialize the drop of early childbearing in 1990s, but it does not count for the continuous decline of teen fertility after 1994. I also show that the children of teenage girls and young women who give births in the 1990s are less likely to have health conditions such as low birth weight, pre-mature birth or abnormal birth if the mothers were born in year and state in which historical abortion ratio was high.

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Chapter 1

Changes in Birth Rates of Young Women Following Access to the Pill and Abortion in the Early 1970s

1.1 Introduction

The birth control pill (the Pill) recently celebrated its 50th anniversary. In commemorating the event, commentators and historians linked the Pill to the broadening of women's opportunities for schooling and work (Gibbs 2010; May 2010). The decline in birth rates in the 1960s and subsequent rise in female college attendance and labor force participation in the 1970s are consistent with this narrative. However, statistically identifying the contribution of the Pill to women's growth in higher education and the professions is challenging. The Pill was first available nationally in 1960 but was limited primarily to married women. Its use grew broadly and steadily over the decade, which makes distinguishing its impact from other changes associated with the evolving role of women in society exceedingly difficult (Bailey 2010).

In an effort to evaluate the Pill's role in women's marriage and career decisions, Goldin and Katz (2002) used variation in the age of majority and the expanded rights of minors across states and cohorts to identify its effect among young, single women. With a focus on college graduates, they found that access to the Pill among unmarried women was associated with a delay in marriage and a rapid increase of women in law and medicine. Goldin and Katz's work stimulated additional studies. Bailey (2006) extended Goldin and Katz's work by using cross-state and cross-cohort variation in access to the Pill among unmarried women to analyze age at first birth and labor force participation. Guldi (2008) used a similar identification strategy to evaluate the relative contribution of access to the Pill and legalized abortion on the birth rates of women 15 to 21 years of age. Ananat and Hungerman (forthcoming) have pushed the framework to evaluate the well-being of children born to women who first gained access to the Pill as teens. The general finding is that access to the Pill in the late 1960s and early 1970s had a significant impact on the reproductive, marital, educational and occupational choices of young women as well as the well-being of their offspring.

The estimated "power of the Pill" is obtained from the reduced-form association between state laws and policies regulating access to the Pill among young, unmarried women and outcomes related to the fertility and well-being of its users. In the language of experimental design, this is an estimate of the intention-to-treat (ITT). However, the credibility of any ITT estimate depends on whether the intervention affected treatment—in this case whether state policies increased use of the Pill. Unfortunately, evidence supporting a robust "first-stage" is lacking due to the paucity of data on sexual activity and contraception among teens in the late 1960s and early 1970s. But an even larger challenge to estimating the impact of the Pill on women's well-being is the role of legalized abortion. Most states expanded access to the Pill among single, young women between 1970 and 1973, a period of seismic change in access to legalized abortion. Each study that emphasizes the role of the Pill controls for whether abortion was legal in the state at the time a young woman gained access to the Pill. Based on this categorization, abortion was illegal in 45 states until *Roe v. Wade* in 1973. However, a simple indicator of legalized abortion fails to account for the astonishing number of young women who travelled from their state of residence to terminate their pregnancy in primarily New York and to a lesser extent California and the District of Columbia in the years prior to *Roe*. To illustrate, Figure 1 shows teen abortion rates by state of residence in 1971. These data are based solely on abortions performed in New York. To give one example, over 4,800 teens 15-19 years of age from Michigan traveled to New York in 1971 to terminate a pregnancy. The resident teen abortion rate in Michigan based solely on abortions obtained in New York was 10.9 per 1000 teens in 1971.[1] To appreciate the magnitude of Michigan's abortion rate at a time when abortion was illegal in the State, the teen abortion rate in Michigan in 2005 was 19.0 (Guttmacher Institute 2010).

In this study we re-examine the association between access to the Pill and legalized abortion on the birth and abortion rates of young women in the years before and just after *Roe*. The analysis proceeds in four parts. In the first, we review the relationship between access to the Pill and abortion and the use of each. We demonstrate the remarkable impact of legalized abortion services in New York on the abortion and birth rates of young women in states where abortion on demand remained unavailable.[2] The analysis is made possible by the re-discovery of data on induced abortions performed in New York by age, race and state of residence in 1971 and 1972. Although the two-year window is limited, the data provide compelling evidence that access to legalized abortion, as proxied by distance to New York, had a large and differential effect on age and race-specific abortion rates in the years prior to *Roe*. Second, we introduce distance to the nearest abortion provider in New York as a valid instrument for abortion in 1971-1972.

[1] Based on authors' calculations of New York State data described below.

[2] To appreciate the uniqueness of the data, it is important to realize that there exists no population-based data on induced abortions by age, race and state of residence in the US today. The Guttmacher Institute's survey of abortion providers reports abortion totals by state of occurrence in each state in selected years. Researchers at the Guttmacher Institute *estimate* abortion by state of residence. The CDC's annual surveillance reports are available by state of *occurrence* cross-tabulated by age or race but are not available by state, age and race. Some states make available individual-level records on induced abortions that can be aggregated into detailed cells (Joyce, Kaestner and Colman 2006). However, there is no reciprocal reporting agreement for induced abortions among states as there are with births. Thus abortions to residents of one state that occur in another are rarely reported back to the state of residence. The best that researchers can do is report the number of abortions to residents of a state that are performed in that state or collect abortion data from a cluster of states and assign each abortion to the state of residence regardless of the state in which it occurred (see Colman and Joyce 2009).

In the third part we follow Guldi (2008) and Ananat and Hungerman (forthcoming) and analyze the association between birth rates of young women and access to the Pill and legalized abortion in the years before and after *Roe*. We show that the reduced-form association between access to the Pill and the birth rates of young women depends on the choice of counterfactual. Both Guldi (2008) and Ananat and Hungerman (forthcoming) include interactions of state and year fixed effects in their birth rate regressions. In this specification they identify effects of liberalized Pill laws on birth rates by exploiting variation across age within each state and year. However, the level and trend in birth rates between minors and young women are so disparate as to undermine the credibility of cross-age comparisons. When we narrow the age group comparisons, the association between access to the Pill and birth rates is greatly diminished. The same is not true of abortion. The association between the birth rates of young women and distance to the nearest abortion provider is robust to stratification by age.

In the fourth part, we return to the New York State data to examine the direct association between lagged abortion rates and age- and race-specific birth rates in the years before *Roe*. We instrument abortion rates with distance to New York, but the estimates are no different from those obtained by ordinary least squares. Lagged abortion rates in this context may serve better as a proxy for access to legalized abortion services than as an endogenous determinant of fertility. Despite the ambiguity of this interpretation, these data are unique and they provide the first estimates of the direct association between birth and abortion rates in the early years of legalized abortion.

Our findings are significant because they raise questions regarding the burgeoning literature loosely labeled "the Power of the Pill." Our results do not refute the importance of the Pill to the well-being of women, but they challenge the appropriateness of the identification strategy supporting recent claims. The results also underscore the importance of access to legal abortion services in the years before *Roe* as teens traveled hundreds of miles to terminate an unwanted pregnancy. Even today, with vastly expanded contraceptive choices, induced abortion remains a significant form of fertility control. In 2006, for example, 27 percent of all teen pregnancies were voluntarily terminated (Guttmacher Institute 2010).

1.2 Association between Access and Use

In this section we review evidence linking the availability of the Pill and legalized abortion on the use of each among young women.[3] Except for Goldin and Katz (2002) none of the studies linking the reduced-form association between access to the Pill and downstream outcomes such as completed schooling, labor force participation or the well-being of children provide evidence that liberalized policies increased use of the Pill.

[3] Throughout we use the term young women to refer to those less than 21 years of age. Laws liberalizing access to the Pill can roughly be divided into those that lowered the age of majority from 21 to 18 and those based on the mature minor doctrine which affected teens less than 18 years of age. Following Guldi (2008) we analyze birth rates of 15- to 21-years olds. The birth rates of 21-year olds reflect changes in access to the Pill among 20-year olds.

Similarly, the association between the use and availability of legalized abortion services in the years before *Roe* has also been neglected due to a lack of data. In this section we address both these relationships.

1.2.1 The Pill

1.2.1.1 NSYW71

Goldin and Katz (2002) make a compelling case that use of the Pill before age 21 among college-educated women rose rapidly for cohorts born between 1945 and 1950. Whether there was more use of the Pill among states with more lenient laws regarding access is more difficult to demonstrate. The only micro-level data that can address the question is the National Survey of Young Women 1971 (NSYW71), a single cross-section of 4,611 teens 15 to 19 years of age interviewed about sexual activity, contraceptive use and abortion in 1971.[4] Goldin and Katz (2002) regress Pill use on a dichotomous measure of whether a state had lenient Pill use policies for women 16 years of age or less. The dependent variable is one if the teen answers yes to the question, "Did you ever use birth control Pills?" and zero otherwise. According to the survey, 6.5 percent of all teens 15-19 years of age and 24.5 percent of sexually active teens had ever used the Pill. Based on their regressions, Goldin and Katz find that Pill use was 2 percentage points greater among all teens and 8.1 percentage points greater among sexually active teens in the 12 states with lenient policies in which teens 16 years of age and older had access to the Pill relative to states in which access was limited to teens at

[4] There are 4,611 observations in the survey. If we drop ever-married teens the sample falls to 4,240. If we include only those with data on pill use we arrive at 4,211, the sample used by Goldin and Katz (2002). The prevalence of Pill use among the 4,211 never-married teens use is 6.5% weighted and 7.7% unweighted.

least 17 years of age. The estimates represent about a 33 percent increase and are robust to stratifying the sample by age and college attendance.

Goldin and Katz's results from the NSYW71 become an important point of departure in subsequent analyses of the Pill. Bailey (2006), Guldi (2008) and Ananat and Hungerman (forthcoming) all use the Goldin and Katz findings to justify their use of Pill access laws to identity effects of the Pill on fertility, marriage, educational attainment and child well-being. However, none re-estimate Goldin and Katz's (2002) regressions with the NSYW71, even though their coding of the laws/policies that liberalize access to the Pill among unmarried, young women differs. For instance, Goldin and Katz (2002) characterize 12 states as having lenient laws or policies that allow teens 16 years of age or older to obtain the Pill without parental involvement. Guldi's (2008) interpretation of these laws and policies suggests that teens in 14 states had access to the Pill without parental consent. Nine of Guldi's 14 states are the same as Goldin and Katz's. Ananat and Hungerman (forthcoming) use Guldi's coding of the laws with the NSYW71, but their measure of access varies by age and state whereas Goldin and Katz's measure varies by state only.[5] Finally, Bailey's (2006, 2009) measure of early legal access to the Pill treats any state that allowed unmarried women less than 21 to obtain the Pill without parental consent as liberal and all others as restricted. According to Bailey, 14 states could be characterized as liberal prior to 1971, of which only 3 states (in the NSYW71)

[5] Ananat and Hungerman's coding is based on which women had access to the Pill at age 16. Thus, a 19 year old in 1971 from the NSYW71 is considered as having access if her state had lenient policies in 1968 when she was 16 years of age. The coding represents a conservative measure of access. If a state allowed an unmarried 18-year old access to the Pill in 1970, then the same 19 year old in 1971 would be coded as living in a state with restrictive laws even though she had access to the Pill in 1970 and 1971. Ananat and Hungerman never regress Pill use on Pill access but instead regress use of abortion on this measure of Pill access. Goldin and Katz's (2002) coding does not vary by age but instead characterizes states that allowed unmarried girls 16 years or older to obtain the Pill in 1971 as lenient.

overlap with Goldin and Katz (2002). The differences matter. In Table 1 we replicate Goldin and Katz's regressions from columns 1 and 2 of Table 3 of their article.[6] Estimates in column (1) pertain to all teens whereas those in column (2) are restricted to sexually active teens. As reported above, the proportion of teens that have ever used the Pill is greater in states with lenient policies regarding access to the Pill for teens 16 years or older. In columns 3 and 4 we run the same regressions but substitute Guldi's 14 states with lenient laws in place of Goldin and Katz's 12 states; in columns 5 and 6 we show results using Bailey's (2006, 2009) coding and in columns 7 and 8 we use Ananat and Hungerman's variation on Guldi's coding. As is apparent in columns 3-8, there is no association between access to the Pill and its use with these variations in coding. Indeed, the coefficient is negative in all 6 instances.

The NSYW71 is but a single cross-section and provides only limited evidence as to the impact of state laws granting access to the Pill on its use. Nevertheless, the lack of a robust "first-stage" is inconsistent with the very large association between laws regulating access to the Pill and teen birth rates reported in recent studies. Ananat and Hungerman (forthcoming), for example, find that access to the Pill among unmarried teens is associated with a 10 percent decline in teen fertility rates, a decrease equivalent to that associated with the legalization of abortion (Levine et al. 1999). Guldi (2008) reports that access to the Pill is associated with an 8.5 percent decline in the birth rates of white women 15 to 21 years of age, but it is unassociated with birth rates of non-whites. Bailey (2006) finds that the access to the Pill among young unmarried women is associated with a 9 percentage point drop in the probability that a woman had a first birth

[6] We thank Claudia Goldin and Larry Katz for graciously sharing their data and programs.

before age 22, an 18 percent decline evaluated at the mean. However, Bailey had to retract her estimates because of coding errors. In her erratum, she finds that liberal laws are associated with only a 0.9 percentage point drop in the probability of a first birth before age 22 (Bailey 2009).[7]

1.2.1.2 Control for Abortion Legalization

A second concern is that 30 states increased access to the Pill for young women from 1970 to 1972, a period of rapid growth in legalized abortion (Guldi 2008). The overlap presents a major challenge to identifying the separate effect of each. To illustrate, Figure 4 shows the years in which access to the Pill for unmarried women less than 21 years of age changed in the 48 coterminous states using Guldi's (2008) coding. The lightest shaded states changed before 1970, the darkest states changed after 1972 and the remaining states changed from 1970 to 1972. The circles show the resident teen abortion rate averaged over 1971-1972. We show two versions: one that uses only data on abortions obtained in New York State (Figure 4) and the other that uses our estimates of resident teen abortion rates in all 48 states (Figure 5). Recall that Goldin and Katz (2002), Bailey (2006), Guldi (2008), Ananat and Hungerman (forthcoming) have coded abortion as illegal in all 48 states but California, New York and Washington.[8] If the actual abortion rate in years before *Roe* is a good proxy for pregnancies that would have been carried to term in absence of legalized abortion in California, New York and

[7] Bailey (2009) argues that the probability of a first birth before age 21, 20 or 19 is a more appropriate outcome with which to assess the effect of access to the Pill on fertility. In these regressions, she finds that liberal laws are associated with a 1.4 percentage point decline (Table III).

[8] Note that Washington State had a residency requirement and based on data from the CDC there appears to have been few abortions to non-residents prior to 1973 (Center for Disease Control 1974, 1975, 1976). Also, Bailey (2006) and Guldi (2008) assume abortion was legal in New Jersey and Vermont in 1972.

Washington, then some portion of the decline in teen birth rates associated with access to the Pill is likely attributable to abortion. We explore this in more detail next.

1.2.1.3 Effects of the Pill and others

Beginning with Goldin and Katz, all of the authors in the "power of the pill" literature looking at downstream outcomes consider that early legal access to the Pill (ELA) can change women's outcomes oven among those who do not use the Pill (indirect effect of the Pill). In particular, ELA changes marriage markets by delaying average age at marriage. Therefore, evidence that ELA is associated with sharp changes in completed scholling, labor force participation and the well-being of children is not undercut by weaker changes in pill use (direct effect of the Pill).

However, We contend that without a direct effect of access to the Pill on Pill use and pregnancy rates or delay, there can be no indirect effect. In other words, the Pill increases age at first birth, age at first marriage and decreases teen birth rates by diminishing the risk of an unintended pregnancy. We see the direct effect as a necessary condition for the indirect effect through thicker marriage markets. If the Pill had no substantive effect on the risk of an unintended pregnancy, then it would suggest that couples could have delayed birth and marriage without the Pill. Thus, the stronger the effects of Pill access on Pill use and fertility, the greater the potential indirect effects. The converse is equally likely. An analogous argument can be made for abortion legalization. Without a fall in birth rates or a delay in childbearing associated with legalization, any association with downstream outcomes would be unconvincing (see Angrist and Evans 1999). In fact, in both the Bailey (2006) and Ananat and Hungerman (forthcoming), the authors built the conceptual framework and conclusion around the impact of the Pill on fertility, which is the direct effect of the Pill.[9]

1.2.2 Legalized Abortion

Five states and the District of Columbia effectively legalized abortion between 1969 and 1970.[10] Sklar and Berkov (1974) estimated that abortion reform in 1970 reversed an upward trend in non-marital fertility as well as a short up-tic in marital fertility that had occurred between 1969 and 1970. However, changes in fertility were not limited to states that reformed their laws or legalized abortion outright. Fertility rates also fell after 1970 in states that made no changes to their abortion laws. Levine et al. (1999) showed that the closer a woman lived to a state that legalized abortion before *Roe*, the greater the decline in the birth rate of that state between 1971 and 1973. The teen abortion rates in Figures 1, Figure 2 and Figure 5 are consistent with this finding.

[9] For instance, Bailey writes, "The paper's evidence, thus far, suggests that the mechanism responsible for changes is women labor-force participation is the delay in childbearing…The absence of an effect (*on labor force participation*) among women who gave birth before age 22 across ages provides strong evidence for the delay of childbearing as the mechanism linking increased labor-force participation to the Pill" (p. 313). Ananat and Hungerman (forthcoming) also focus on the direct effect of access to the Pill on fertility as the key mechanism. They write, "In sum, analysis of the relevant economic research implies that the net effects of Pill diffusion on children depend on whether: 1) there were short-term declines in fertility; 2) these short-term declines represented delays in fertility or permanent reductions; 3) reductions, if any, occurred on the intensive margin, the extensive margin or both; 4) eventual fertility was concentrated among women who differed from the average…." (p. 6-7)

[10] The California Supreme Court case in *People v. Belous* (September, 1969) resulted in *de facto* legalization in California. This decision was followed by repeals in Hawaii (effective March 1970), New York (July, 1970), Alaska (July, 1970) and Washington State (December 1970). Abortions became available at outpatient clinics in Washington DC in 1971 following the decision in *US v. Vuitch* (April 1971). For details, see Garrow (1998) and Lader (1973).

Moreover, data from the Centers for Disease Control (CDC) underscore the importance of abortion services in New York to non-residents of the state prior to *Roe*. In 1971-72 there were 921,092 legal abortions with known residences performed in the US. However, 396,403 were to women who obtained an abortion outside their state of residence. Seventy-nine percent or 314,929 abortions to non-residents were performed in New York, 33,272 (8.4%) were performed in California and 27,500 (6.9%) in Washington, DC (CDC 1972, 1974).[11]

As noted previously, in none of the recent studies on the impact of the Pill on the well-being of women were the authors able to adequately control for abortions to residents from states in which abortion remained illegal (Goldin and Katz 2002; Bailey 2006; Guldi 2008; Ananat and Hungerman forthcoming). Goldin and Katz (2002) use a dichotomous indicator of whether abortion is legal in the state or the actual abortion rate in their analysis of age at first marriage. Goldin and Katz (2002) finds that legalized abortion lowers the likelihood that a college woman will marry before age 23 but the estimates are not robust to the inclusion of state-linear trends. Bailey (2006) also includes a dummy variable for whether the state legalized abortion. However, Bailey's coding

[11] The importance of D.C. as a location for legal abortions prior to *Roe v. Wade* has not been appreciated by many researchers (Levine et al. 1999; Angrist and Evans 1999; Donohue and Levitt 2001, 2004). The Preterm abortion clinic in Washington D.C. began performing abortions in March of 1971. According to its Medical Director, Jane Hodgson, they performed approximately 60 abortions per day or over 12,000 annually in the first two years that she was in charge (Joffe 1995,p.18). Published analyses of complication rates at the Preterm clinic attest to the caseload (Margolis et al. 1974; Hodgson and Portman 1973; Hodgson 1975).

differs somewhat from Goldin and Katz's (2002). She assumes that New Jersey and Vermont legalized abortion in 1972, the year before *Roe*.[12]

Coding a state's abortion policy as illegal does not necessarily mean the abortion rate is zero. It is problematic because this way of coding equals the period in 1970-1972 during which abortion was legal in NY, DC and CA with the period before 1970 when abortion is illegal national wide. We make a similar assumption for 1968 and 1969, but we believe with greater justification. We assume that there was "no legal abortion on demand" even though the CDC recorded over 12,000 legal abortions in 1969. Prior to 1970, most legal abortions were obtained in states that had reformed their statutes. Most reforms required women seeking an abortion to obtain approval from a panel of physicians at a local hospital. There is little evidence that these reforms had any substantive impact on fertility (see Levine et al. 1999). I also agree that women obtained illegal abortions instead of traveling to DC, New York or California before 1973. However, if every woman substituted a legal abortion in New York for an illegal one in their own state, there would be no decline in birth rates, which is counter to literature and my own results in this manuscript.

Guldi (2008) treats women as having access to abortion in state j and year t if abortion is legal and if there is no parental consent requirement for a girl of a specific age. It means that Guldi (2008) bases access to abortion on two conditions: legality and a lack of parental consent. If abortion was illegal, or if the state enforced a parental

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[12] Both states passed legislation that legalized abortion in 1972, but their impact was minimal (see Garrow 1998). According to the CDC there were no reported legal abortions performed in New Jersey in 1972, but 10,047 one year later. In Vermont, there were 193 abortions in 1972 and 1,401 the following year (Centers for Disease Control 1974, 1975).

involvement law or the state prohibited minors from obtaining medical treatment without parental consent, then Guldi considers abortion to be inaccessible to minors. As such she equates a regime under which there is no effective legal abortion in the entire country with one constrained only by parental consent in the years after *Roe*.[13] She finds a strong association between abortion access and the birth rates of white women 15 to 21 years of age, but a weak association among nonwhites, a pattern at odds with many previous studies of abortion legalization (Sklar and Berkov 1974; Joyce and Mocan 1990; Levine et al. 1999; and Angrist and Evans 1999). However, Figure 6 shows our estimate of resident teen abortion rates stratified by years and states in which a teen 17 years or less could obtain an abortion without parental consent. There is little evidence that parental consent requirements were binding. This finding motivated us to look deeper into her coding of abortion access since the impact of legalized abortion on nonwhite birth rates is a robust and well-established finding.

[13] To give a concrete example, consider a 17-year old in Massachusetts in 1968, 1972 and 1974. In all three years Guldi considers the minor to have no legal access to abortion. This is obvious in 1968 as abortion is effectively illegal nationally. But access to abortion is very different in 1972 and again in 1974. For instance, the abortion rate of Massachusetts' residents 15-17 years of age was 9.4 per 1000 in 1972 based solely on terminations performed in New York. By 1974, the abortion rate was undoubtedly greater given the availability of legal services in Massachusetts, but Guldi still considers abortion unavailable to minors in the state. There is also little evidence to suggest that parental consent laws for minors seeking an abortion were binding especially in the early years of legalized abortion (Dennis et al. 2009). For instance, 60 percent of minors involve their parents in their decision to abort in states that have no consent or notification requirements (Henshaw and Kost 1992). In other words, only 40 percent of minors on average would be affected by a law that required parental consent for an abortion. Moreover, many minors who did not involve their parents obtained an abortion in a nearby state. The seminal study of Massachusetts' parental consent law revealed that abortions to minors obtained in Massachusetts fell 43 percent after enforcement in 1981 but that there was no change in abortions to minors when measured by state of residence. To avoid parental involvement, minors from Massachusetts went primarily to New Hampshire, New York and Rhode Island to terminate their pregnancies (Cartoff and Klerman 1986).

1.2.3 The 1971-72 window and Distance as measure of access

The legalization of abortion in New York in July of 1970 provides a unique opportunity to analyze the effect of an unanticipated increase in abortion availability on abortion and birth rates. New York's abortion law passed by one vote in the state assembly and was unexpected by the opposition and almost certainly unanticipated by young women who resided in states other than New York (see footnote 10). Thus, distance to New York offers a plausibly exogenous change in abortion services. This is not true of the period after national legalization. Abortion markets developed rapidly after *Roe.* The assumption that the number of abortion providers in a county or state is exogenous to the demand for abortions is difficult to defend (Blank et al.1996; Kane and Staiger 1996). To illustrate, we regress the number of abortion providers per capita from 1973-79 in the 45 states that legalized abortion with *Roe* on the average abortion rate for all women in these same states from 1970-72, a period in which abortion on demand was illegal. The point is to show that abortion providers were more likely to locate where latent sentiment for abortion was more favorable. The number of abortion providers is from the Guttmacher Institute's survey of providers which was fielded annually from1973 to 1979. Residence abortion rates from 1970-72 are from the CDC (1971, 1972, 1974). Specifically, we estimate versions of the following regression:

$$\text{(1)}\quad \text{Prov}_{jt} = \alpha(Abor70-72_j * Trend_t) + \mathbf{X}_{jt}\boldsymbol{\beta} + \mu_j + \tau_t + e_{jt}$$

Prov $_{jt}$ is the number of abortion provider per 100,000 women 15 to 44 years of age in state j from 1973 to 1979; $Abor70\text{-}72_j$ is the average abortion rate in state j from 1970-72; $Trend_t$ is a linear trend term; $\mathbf{X}_{jt}$ is a matrix of state variables that include the insured unemployment rate, per capita income and the percent of population that is nonwhite; μ and τ are state and year fixed effects. Because the abortion rate in 1970-72 varies only by state, we show two specifications: one with and without state fixed effects. We also analyze all abortion providers and then non-hospital providers separately. In 1973, 48 percent of all abortions were obtained in non-hospital facilities; by 1979 non-hospital facilities provided 76 percent of all abortions (Forrest, Sullivan and Tietze 1979; Henshaw and Van Vort 1992). The rapid changes suggest that non-hospital providers offer a better indication of the endogeneity of provider location. The results are displayed in Table 2. In the models without fixed effects (columns 1 and 3), the abortion rate in 1970-1972 is strongly correlated with the total number as well as the number of non-hospital abortion providers in a state in the years after *Roe*. The second specification is based on equation (1). The estimate of α shows whether the trend in abortion providers is positively correlated with the magnitude of the abortion rate in the years prior to *Roe* adjusted for state and year fixed effects. Even controlling for time-invariant factors within each state, the coefficient is positive and statistically significant in the case of non-hospital providers. The point of this exercise is to underscore the unique but limited window provided by the 1971-72 period with which to analyze the impact of an exogenous change in abortion services.

Given the large number of women from all other states who travel to New York to have abortion service in 1970-1972, we consider distance to nearest legal abortion provider as a measure of access to abortion services in this period of time. In Figure 7 we show estimates of resident teen abortion rates against distance measure in hundreds of miles in 1972 and then again in 1974.[14] We show the predicted lines from regressions of abortion rates on a linear measure of distance and the natural logarithm of distance. The log of distance better captures the non-linear association between teen abortion rates and distance as measured by the coefficient of determination.[15] Distance, however, is undefined prior to 1970, and only varies by state between 1970 and 1972. Distance in the years pre and post-*Roe* changed dramatically. Average distance to a legal abortion provider drops from 502 miles in 1972 to 29 miles in 1973 and continues to decline to 17 miles by 1979.

The legalization of abortion in New York in July of 1970 had the greatest impact on the availability of services to nonresidents around the country. The large number of abortion providers in the state, its proximity to the population centers in the east and Midwest, and the lack of a residency requirement greatly facilitated access for those who did not live in New York, DC or CA. Another advantage of the pre-*Roe* period is that women had to travel substantial distances, which aides in identifying effects of access on abortion.

We begin with plots of the bivariate relationship between the natural logarithm of distance to the nearest abortion provider and teen abortion rates by state of residence for the years 1971-1972 based on national data (Figure 8). There is an obvious negative association between teen resident abortion rates and distance from nearest abortion provider. The R-square for 1971-1972 (Figure 8A) is much larger than those for 1973-1974 (Figure 8B) and 1975-1976 (Figure 8C)

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[14] We used the New York State data on abortions and CDC surveillance data and data from the Guttmacher Institute to estimate resident teen abortion rates. A detailed explanation is available upon request.

[15] A quadratic in distance or the reciprocal of distance also fit the data better than a linear model. We chose the logarithm specification given its parsimony and ease of interpretation.

1.2.4 Weak first-stage.

The lack of an association between Pill use and access in a single survey is not definitive evidence of a weak first-stage. But we argue that laws liberalizing access to the Pill provide a less sharply-defined shock to availability than did legalized abortion. Unlike abortion, legal use of the Pill was widespread among married women since 1960 and was only limited to women less than 21 who lacked parental consent (Massachusetts and Wisconsin being exceptions). It is plausible that many women less than 21 years of age could have acquired the Pill before the age of majority was lowered or policies liberalized. As Paul, Pipel and Wechsler (1974) note, not one physician was ever prosecuted for dispensing the Pill to a minor 15 years or older. This suggests that the "black market" in Pill acquisition may have been relatively benign. This would tend to weaken an association between laws liberalizing access to the Pill and use of the Pill among young women. We also show that there is no visual evidence of a break or discontinuity in the time-series of teen birth rates associated with policies liberalizing access to the Pill (see further discussion about Figure 10 and Figure 12).

We are not arguing that the Pill was unimportant to the well-being of women. Nor are we arguing that the Pill had no affect on secular trends in birth rates and other outcomes. Rather we are making the point that the laws affecting *unmarried* women less than 21 may not be a good source of identifying variation in these studies.

Many women 19 and 20 years of age may have obtained access to the Pill with parental consent, at college or through marriage (see Edlund and Machado 2011). As we note in the conclusion, the late 1960s and early 1970s were a period of great social change (feminism, civil rights, the Vietnam War, drug use, and sexual mores). Identification of a particular factor amidst this tumult requires a sharp break in policy or technology that leads to discernable changes along a logical pathway. The legalization of abortion was also a product of its time, but passage of New York's abortion law in 1970 was a great surprise as detailed by Garrow(1998) and Lader (1973). More importantly, New York's law had a large effect on the abortion rates of young women in states still adamantly opposed to abortion. The suddenness of its passage and its plausibly exogenous impact on women in other states makes it a credible shock. Our point is that we can link this change in access to abortion to a change in fertility more convincingly than linkages to access to the Pill not because data on Pill use are unavailable, but because the change in abortion access caused by legalization was more abrupt than liberalized access to the Pill to young women.

To sum up, the problematic coding of abortion legalization causes at least two issues. First, abortion legalization is not well-control as it should be, since the number of abortion was large in 1970-1972. Second, it leads to a difference-in-difference-in-difference design of identification strategy when comparing the effect of legalization of abortion and early access of the Pill. When applying this DDD strategy, state-year effect was included and it leads the estimation replying on comparison across different age groups. As we will show in section V, the results in favor of the ELA weaken evidently when we separate age groups and redo the estimation.

In the previous discussion we argue that evidence from NSYW71 of a first stage is important because of the large reduced form effects of Pill access on birth rates and the overlap between laws liberalizing access to the Pill with the legalization of abortion in the 1970-1972 period. We then segue into the first-stage evidence for abortion with the New York data.

1.3 Introducing Distance as an Instrument: Identification

1.3.1 New York State Data and Distance to Abortion Service

To eliminate confounding from unmeasured access to abortion, Guldi (2008) and Ananat and Hungerman (forthcoming) include interactions of state- and year-fixed effects in their preferred specifications. These controls absorb all variation by state and year and thus eliminate distance to the nearest abortion provider and as well as state abortion rates as potential confounders. However, state-year fixed effects will not capture variation in abortion by state, year and age. To illustrate we take advantage of data on abortions performed in New York State from 1971-72 as collected by the New York State Department of Health. New York was not only the most frequented destination for women seeking an abortion, but the state recorded the patient's age and state of residence for each termination performed in the state. The age breakdown includes women 15-17, 18-20, 21-24, and 25 years and older. We also have abortions by age and race for whites and nonwhites, but the age-breakdown is not as refined: women less than 20, 20-29, and

30 years and older. [16] We create abortion rates by dividing abortions in each group by the number of women in the state, year, age and racial group. Population is from the Surveillance Epidemiological and End Results (SEER) from the National Cancer Institute.

To proxy the availability of abortion services we computed the straight line distance in miles from the population centroid in each state to nearest of Buffalo, New York or New York City. We also limit the sample to states in which 70 percent or more of all abortions to residents of the state were obtained New York. Our sample consists of the 28 states east of the Mississippi River plus Minnesota, Iowa, Missouri, Arkansas and Louisiana, but excluding Delaware, the District of Columbia, Maryland and Virginia.[17] The goal is to include states for which New York was the most likely destination for a resident of that state who sought an abortion.

Figure 9 shows the relationship between age-specific abortion rates by state of residence and distance to New York in hundreds of miles. The fitted line in each panel is from a regression of the abortion rate on the natural logarithm of distance. The logarithm of distance provides a superior fit to the data than distance entered linearly. There is an obvious negative association between resident abortion rates and distance from New York for each age group. The slopes are roughly similar among women 18-

[16] The New York State Department of Health would not make available race-specific data in more detailed age breakdowns. As to reporting, only 2.13 % of cases were missing age and 1.8% were missing place of residence.

[17] The number of abortions to non-residents in Washington, DC exceeded the number in California in 1972. However, unlike New York, the District of Columbia did not report the distribution of abortions by state of residence. We consider DC to be the primary market for women in Delaware, Virginia and Maryland. Anecdotal support for this comes from Lader (1973), but the data on abortion rates provides additional evidence. For example, the abortion rate for 18-19 year olds in 1972 based only on abortions obtained in New York was 1.7 per thousand in the District of Columbia, 1.4 in Maryland and 3.1 in Virginia, but 18.8 in Michigan, 8.9 in Missouri, 9.4 in Tennessee and 6.8 in Kansas (authors' tabulations based on New York State data).

20 and 21-24 years of age (Panels B & C), which in turn are almost three times as steep as those for minors and women 25 years and older (Panels A & D). The R-square in all four regressions exceeds 70 percent.

We provide a more formal test of the association between the use and availability of abortion by estimating equation (2) below for our sample of age-specific abortion rates in the 28-state sample

$$(2) \quad \text{Abrate}_{ajt} = \alpha_0 LnDis_j + \alpha_1 Pill_{ajt} + \sum \varphi_a A_a + \sum \delta_a (A_a * LnDis_j) + \mathbf{X\beta} + \lambda_j + \tau_t + e_{ajt}$$

Specifically, let $Abrate_{\text{ajt}}$ be the abortion rate for age group *a* in state *j* and year *t*; let $LnDis_{\text{j}}$ be the natural logarithm of distance to New York which varies only by state. Let $Pill_{\text{ajt}}$ be one if age group *a* had access to the Pill in state j and year t (Guldi 2008). Note that Pill access varies by age, state and year. Let A_{a} be a set of age dummies (15-17, 18-20, 21-24) with women 25 and older as the omitted category. The next set of variables, A_a*$LnDis_{\text{j}}$ are interactions between age and distance to New York followed by three controls for state characteristics(**X**): the insured unemployment rate, per capita income and the percent of the population that was nonwhite. Finally, we estimate models with and without state-fixed effects. In models with fixed effects, the main effect of distance is absorbed by the fixed effects. The interaction terms still reveal the relative impact of distance on abortion rates between age groups. If we assume that inclusion of fixed effects reduces the main effect of distance to zero, then the coefficient on the interaction term represents absolute effect of distance on the abortion rates of the specific age group.

Estimates of equation (2) are displayed in Table 4. The first two columns are for all women and the next four columns contain race-specific estimates. Note the more aggregated age breakdown in the race-specific regressions. For each grouping we show estimates with and without state-fixed effects. Consider results from the specification that excludes fixed effects (column 1). There is a strong, negative association between distance and abortion rates of women 18 to 24 years of age relative to adults 25 years and over (the omitted category). Every unit increase in distance, or 100 miles, is associated with a decline in the abortion rate of 18-20 year olds of 1.12 abortions per 1000 population.[18] The same holds approximately for 21-24 year olds. In the specification with fixed effects the interpretation is less straightforward (column 2). If we assume that inclusion of fixed effects reduces the main effect of distance to zero, then the abortion rate of 18-20 year olds would be expected to decline by 0.67 abortions per 100 miles from New York (-3.22/4.83). Thus, we view estimates in columns (1) and (2) as upper and lower bounds. In neither of these specifications is access to the Pill associated with changes in the abortion rate.[19]

Not unexpectedly, results for whites (columns 3 & 4) are similar to those of all women. An increase of 100 miles is associated with a decrease of 0.48 abortions per 1000 teens less than 20 years of age.[20] The effect of distance among nonwhites is much greater (columns 5 & 6). A 100 mile increase in distance to New York is associated with

[18] Distance is measured in logs. Thus $\delta y/\delta \ln x = (\delta y/\delta x)*x$. To obtain $\delta y/\delta x$ we divide the marginal effect by mean distance. Using the coefficients for 18-20 year olds in column (1) of Table 4 a one unit change of distance, or 100 miles, is associated with a decline of 1.12 abortions per 1000 population i.e., [(-2.22+-3.21)/4.83)] where the denominator is the mean of distance in hundreds of miles.

[19] An apparent anomaly is the positive and statistically significant coefficient on the interaction between distance and minors 15-17 years of age. This indicates that the slope of distance for minors is less steep than the slope for the omitted category, women 25 and older. As shown in Figure 4, the coefficient on the log of distance is -2.12 for minors but -2.82 for women 25 and older. The difference, 0.70 is almost identical to the slope for minors in Table 2, column (1).

[20] As before, we use the results from the models without state fixed effects. For white teens, we compute the effect of an increase of 100 miles on abortion rates as follows: (-1.17+-1.15)/4.83=0.48, where 4.83 is the mean distance from New York State in hundreds of miles.

decline of 1.93 abortions per 1,000 nonwhite teen population. These estimates pertain to models without fixed effects. If we assume the main effect of distance is zero, then white and nonwhite abortion rates for teens fall 0.24 and 1.05, respectively, per 100 miles from New York. The race-specific estimates are consistent with evidence that the legalization of abortion had a bigger impact on the fertility rates of nonwhites than whites, since whites had greater access to hospital committees or private physicians willing to perform illegal abortions.

1.3.2 First Stage of Abortion Use, the Role of the Pill?

The results in Table 4 represent a reduced form. We do not include and do not have a measure of Pill use for these states and these years. We follow the literature and use access to the Pill as proxied by laws/policies permitting young, unmarried women to obtain the Pill without parental consent. We use distance to New York as the proxy for access to abortion. If one accepts that policies granting access to the Pill and distance to New York are exogenous, then there is no simultaneity bias. Omitted variable bias is always a threat. We hope by improving the specification of abortion access prior to *Roe*, we have lessened that threat somewhat.

We only have two years of data so we don't think state*specific trends are a major source of confounding.

The distance*age interactions show the association between abortion rates and distance to New York by age group *relative to the omitted category of women 25 and* *older.* The simple relationship between the abortion rate and distance for teens 15-17 years of age is negative as shown in Figure 9. The slope is -2.12 and the R-square is 0.75. But the slope is the smallest among the 4 groups and thus *positive* relative to women 25 and older, the omitted category in the regressions.

We did not interact the Pill with age because unlike distance to New York, access to the Pill varies by state, year and age. For the same reason we did not interact the lagged abortion rate in Table 7, because it also varied by state, year and age.

The seven panels in Figures 10A and 10B display birth rates by single year of age from 1968 to 1979. In each panel we group birth rates by states based on years in which access to the Pill was liberalized. For 19- to 21-year olds there are three periods: before 1970, 1970-1972 and 1973 and after. For the younger teens we divide the years after 1972 into two separate periods. As we can observe, there is no evidence of any discontinuity of birth rates associated with increased access to the Pill. The level and trend in birth rates among 19 to 21 in states that reformed their Pill laws from 1970-1972 are almost identical to those that reformed their laws prior 1970 or after 1972.[21] The same is true for younger teens. Regardless of when access to the Pill was liberalized, birth rates among 17-year olds, for example, peak around 1970, remain flat for the next three years, and then decline after 1973.

Figure 1.12A-F are plots of birth rates by single year of age stratified by the year the state changed access to the Pill for young, unmarried women. The results for the women less than 19 years of age are noisy because the policy changes are not as bunched.

[21] The lack of a discontinuity is even more evident in plots that group states by the individual year in which states liberalized access to the Pill and are available upon request.

However, the plots for 19- to 20-year olds (Figures 12E and 12F), reflecting decisions predominately by women 18 to 19 years of age are even more striking than those in our Figure 10A. There is simply no evidence of any discontinuity associated with access to the Pill. Consider 19-years olds. A 10 percent decrease in birth rates—the estimated impact of access to the Pill on birth rates obtained by Guldi (2008) and Ananat and Hungerman (forthcoming)—represents a decline in the level of the birth rate of approximately 13 per 1000 women from the 1969 baseline birth rate of 130. Moreover, the decline should be staggered. Birth rates should fall in 1971 in Kansas, followed in 1972 for the states that revised access in 1971, in 1973 those that revised their laws in 1972, etc. Instead we observe a coincident decline in all the states that revised their laws from 1970 to 1974. The same is true for 20- and 21-year olds.

A key theme, the laws allowing access to the Pill among young women without parental consent may not be a convincing way to demonstrate the power of the Pill.

1.3.3 The exogeneity of distance to New York

A key assumption underlying the results in Table 4 is that distance to New York is a plausibly exogenous measure of the availability of abortion services. Several factors support this assumption. First, the legalization of abortion in New York in July of 1970 was unanticipated; it passed by one vote in a dramatic legislative session.[22] Second, none of the other 28 states in our sample followed New York. Indeed, "abortion on demand"

[22] The law passed only after a representative switched his vote from negative to positive after an emotional conversation with his son. The vote was not only close but many considered its passage implausible. The Catholic Church, for example, had been preparing to contest a much less liberal bill and complete legalization caught the Church by surprise (Garrow 1998; Lader 1973).

remained unpopular, and yet, the legalization of abortion in New York can had a profound impact on the availability of abortion services to non-residents of New York from the 28 states.

Third, distance to New York is only a determinant of state abortion rates in the period before *Roe* and irrelevant afterwards. In other words, it's an unanticipated, transitory increase in the availability of abortion services. As an illustration, Figure 3 replicates the map of teen abortion rates from Figure 1 but for the year 1975, two years after *Roe*. The numbers in each state show resident teen abortion rates but only for abortions *obtained in New York*. The comparison illustrates clearly that distance to New York only mattered in the years before *Roe*. As soon as abortion services became available in each state, few teens traveled to New York. In other words, legalization of abortion in New York had a dramatic but transitory impact on access to abortion services for women in other states. The other advantage of distance to New York is that it virtually eliminates policy endogeneity. Women in states other than New York gained access to abortion even though voters in their own states were unwilling to legalize abortion. Consider once again, Michigan. The resident abortion rate was 10.9 per 1000 teens in 1971 but it falls to 0.2 per 1000 by 1975. In absolute numbers, 4,889 teens from Michigan obtained an abortion in New York in 1971, but only 87 did so in 1975. We should also note distance to New York has never has been included as an independent covariate in studies of state abortion and birth rates in the period after *Roe* (see Matthews, Ribar and Wilhelm 1997; Kane and Staiger 1996; Levine et al. 1996; Blank et al. 1996).

The exogeneity of distance after *Roe*, especially after 1975 as we concern, is problematic. Distance is essentially our only real instrument and thus, over-identification tests are not possible. We still believe that distance to a state in which abortion was legal prior to *Roe* (AK, CA, DC, HI, NY, WA) is plausibly exogenous, though we are less confident about distance after *Roe*. Therefore, we limit the analysis of birth rates to young women and distance to 1968-1975 in an effort to lessen the potential endogeneity of provider location (see Table 5). Given that distance is included with a lag, we include distance only through 1974. This enables us to exploit two dramatic changes in distance to the nearest abortion provider that occurred during this period: from 1969-1970 and 1972-1973. As stated in the text, the average distance to the nearest legal abortion provider fell from 502 miles in 1972 to 29 miles in 1973. Such a large, abrupt change provides a potentially useful source of identifying variation.

Tests of Exogeneity

There is no way to test for the exogeneity of an instrument in a just-identified model. One has to rely on a theoretical or conceptual argument. As explained in details above, distance to New York represents an unanticipated, transitory increase in the availability of abortion services. The contrast between 1971 in Figure 1 and 1975 in Figure 3 is dramatic. Resident teen abortion rates performed in New York fall by over 90 percent by 1975. The exceptions are Connecticut and New Jersey which is most likely related to their proximity to New York City. The remaining small rates of abortion performed in New York by non-residents could be for students attending college in New York. We also want to point out that there are no studies of birth or abortion rates post-*Roe*, that included distance to New York as a covariate.

Some may refer to distance to a college as a similar type instrument. We think New York's early legalization of abortion is substantively different. The decision to locate in a "college town" is not exogenous as families are drawn to the intellectual and cultural amenities. The construction of new colleges as used by Currie and Morreti (2003) may overcome this to some extent, but colleges can take years to construct. Second, many colleges draw students who live out of state because of the quality of the school or its special offerings. Thus, a new college that opens in Pennsylvania, for instance, may have no effect on students attending Columbia, NYU, Cornell, Syracuse, etc. This is not true of abortion services. As the map in 1975 (Figure 3) makes clear, the opening of local services virtually eliminated travel to New York.

Contrast distance to New York with laws granting access to the Pill. These were state-specific policies that may have reflected potentially endogenous sentiment towards greater rights for individuals less than 21 years of age. Moreover, passage of these laws was largely concentrated in the years 1970-72 suggesting a bandwagon effect. We would argue that the potential for policy endogeneity is greater with the Pill than early legalization of abortion.

1.3.4 More Details on Traveling to NY

Driving through D.C.

A woman might not drive though DC to get to New York if the legalization of abortion in DC was as well-established as New York's in 1970-1972. Abortion was more accurately characterized as *de facto* legal in DC before *Roe*. The U.S. Supreme Court ruling in the *United States v. Vuitch* in April of 1971 was ostensibly a defeat for legalized abortion, since it upheld the old DC statute. But the ruling allowed physicians to make decisions to abort based on the whether the life or health of the patient was at risk without threat of prosecution. Importantly, the ruling expanded health to include psychological health (see Greenhouse 2005 as well as Garrow 1998). The Preterm clinic in DC under the medical direction of Dr. Jane Hodgson used the ruling to greatly expand its services as we note in the text. The Preterm clinic performed approximately 1000 abortions per month in the year before *Roe* (see footnote 8 on page 8). But the DC abortion market serviced women primarily in DC, Maryland and Virginia, perhaps based on references by local clinicians (Lader 1973, p. 115). It is also important to note that our characterization of DC as having legalized abortion *de facto* is at odds with Levine et al. (1999), Gruber, Levine and Staiger (1999), Donohue and Levitt (2001), Goldin and Katz (2002), Bailey (2006) and Guldi (2008), all of whom consider abortion illegal in DC prior to *Roe*. Thus, a woman in, say, Florida deciding to seek an abortion would probably choose New York given the large number of providers and the lack of any ambiguity as to its legality.

Albany

We ignored Albany because it would only affect New Englanders and even then only a subset. New York State gave us information on whether the abortion was performed in New York City (NYC) or the rest of the state (ROS). The ROS included Long Island.

Table 1.3 shows the location of abortions performed in New York to teen residents of the six New England states. Sixty-five percent were performed in NYC but even this is an underestimate since women undoubtedly when to Long Island.

New York Data as a Natural Experiment

As we discuss before, distance to New York represents an exogenous shock to abortion availability. There are only two years of data, but the natural experiment is unique. Unlike with access to the Pill, policy endogeneity is not a real threat to internal validity. For non-residents of New York, the legalization of abortion is external to the politics of their state. Its passage was unanticipated and yet its impact was immediate. In addition, distance to New York becomes irrelevant by 1975, which provides further evidence of its exogenous but transitory nature. Finally, the association between distance and abortion rates in New York is consistent with the use of distance and birth rates in Table 6.

In summary, there is little evidence of an association between laws and policies liberalizing access to the Pill and state variation in its use. Clearly, the lack of data on Pill use by state and age impedes a more definitive assessment. However, unlike the legalization of abortion, access to the Pill for young women did not change abruptly. The Pill had been in widespread legal use for 10 years and parental consent was the primary legal barrier to unauthorized use among young women. There is also little evidence of sanctions against clinicians for prescribing the Pill to underage women. The survey data that exist on Pill use and Pill laws suggest a fragile association in 1971. Moreover, as we show in the next section, there is no visual discontinuity in plots of age-specific birth rates associated with access to the Pill. In contrast, the association between access to legalized abortion and its use is compelling. The sudden legalization of abortion in New York affected women in states that had no intention of legalizing abortion. Moreover, non-resident teens stopped coming to New York for an abortion as soon as local services became available. This suggests that distance to New York in the years before Roe was a plausibly exogenous but temporary determinant of abortion rates. Despite this evidence, the New York "experiment" is limited to only two years and only affected women in a sub-sample of states. Thus, we next analyze the birth rates of young women in all 50 states and over a longer period of time in an effort to further elucidate the role of access to the Pill and legalized abortion.

1.4 Analysis of Birth Rates

In this section we associate birth rates by race and single year of age for women 15 to 21 with access to The Pill and legalized abortion from 1968 to 1979 in all 50 states and the District of Columbia. The analysis proceeds in two steps. We first replicate Guldi (2008) and then evaluate the sensitivity of her estimates to a different identification strategy. In the next part we substitute distance to the nearest legal abortion provider instead of her measure of abortion legality as a measure of abortion availability.

1.4.1 Data

In all analyses we use vital statistics on births by state, year, age and race from the National Vital Statistics System of the National Center for Health Statistics. Population by state, year, age, gender and race is from the Surveillance Epidemiological and End Results (SEER) of the National Cancer Institute. Since the series begins in 1969, we use population for that year for 1968. Our measure of abortion availability takes into account three distinct legal regimes that characterize the years 1968-1975. Specifically, we assume that there was no legal access to abortion on demand in 1968-69.[23] To proxy access to legal abortion services from 1970-1972, we use distance from the population centroid of a woman's state of residence to the nearest legal abortion provider. Since we are analyzing birth rates in the entire US we measure availability as the nearest distance to either New York City, Buffalo, New York, Los Angeles or San Francisco in 44 of the 48 lower states. We ignore distance to the state of Washington in the pre-*Roe* years despite the legalization of abortion in December of 1970 because the state had a 90-day residency requirement for an abortion (CDC 1971). We use distance from the population centroid of Delaware, Maryland and Virginia to Washington, DC instead of New York for those three states from 1970-72 because of the relatively large number of legal abortions performed in Washington, DC prior to *Roe.* From 1973 to 1975 we use the Guttmacher survey of abortion providers by county and year. We measure distance from the population centroid of each county to the county of the nearest abortion provider regardless of whether the provider was in the state of residence or in a neighboring state.

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[23] In 1969, there were 12,584 legal abortions reported to the CDC. This relatively small number of abortions is not associated with any substantive impact on birth rates (Levine et al. 1999). The second phase is from 1970 to1972 in which abortion became *de facto* legal on demand in California and the District of Columbia and *de jure* legal in Alaska, Hawaii, New York and Washington. In 1970, there were 180,119 legal abortions reported to the CDC, an order of magnitude more than in the previous year. By 1972 the total number of abortions had risen to 586,760 (Centers for Disease Control 1972;1974).

We assume distance is zero if the county had an abortion provider. To obtain a summary measure at the state level, we average the distance for each county in the state weighted by the population of women 15 to 44 years of age in the county.

We limit the analyses using distance to the nearest legal abortion provider to the years 1968-75 in an effort to lessen the endogeneity bias associated with distance. The location of abortion providers after *Roe* reflects the interplay of supply and demand within each state. At the same time, we try to exploit the dramatic change in the availability of abortion services generated by early legalization in 1970 and then national legalization in 1973. For instance, average distance (unweighted) to the nearest legal abortion provider dropped from 502 miles in 1972 to 29 miles in 1973 and 18 miles by 1975. This also provides state and year variation in the distance measure which enables us to include state and year fixed effects. Nevertheless, the endogeneity of distance after *Roe* is an important caveat.

1.4.2 Results

1.4.2.1 Time-series plots

As mentioned in section 3, the seven panels in Figures 10A and 10B display birth rates by single year of age from 1968 to 1979. Several observations stand out. First, there is no evidence of any discontinuity of birth rates associated with increased access to the Pill. The second observation is that younger teens are a questionable comparison group for changes among older teens and vice-versa. This becomes important for identification because regressions with only state- and year-fixed effects use cross-state changes within age as well cross-age changes within each state to identify effects of laws governing access to the Pill on birth rates. In contrast, a model with state-year interactions eliminates variation from cross-state changes and relies exclusively on variation across age within each state and year. The source of identifying variation becomes pivotal in this case since access to the Pill can be broadly divided between changes that lowered the age of majority from 21 to 18, which would affect the birth rates of 19- to 21-year olds, and those policies that allowed teens less than 18 to obtain contraception without parental consent. The practical consequence is that models with state-year interactions use differences in birth rates between women 19 to 21 and teens 15 to 18 years of age within each state and year to identify effects of the Pill. But as Figures 10A and 10B make clear both the level and pre-change trends in births between these two groups appear too disparate to credibly compare. Consider the birth rates of 16- and 20-year olds. In 1971 the birth rate of 20-year olds is approximately 140 per thousand and falling in each of the three prior years. The birth rate of 16-year olds is about 42 per thousand and rising in each of the three prior years. We question using birth rates of 16-year olds as the counterfactual for changes in liberalized access for 20-year olds.

1.4.2.2 Birth rate regressions

To illustrate the sensitivity of an identification strategy that uses only variation across age within state and year, we follow Guldi (2008) and estimate regressions of the following form:

$$(3) \qquad LnBrate_{ajt} = \alpha_0 Abor_{ajt-1} + \alpha_1 Pill_{ajt-1} + \sum \varphi_a A_a + \delta_{jt} + \lambda_j + \tau_t + e_{ajt}$$

where $LnBrate_{ajt}$ is the natural logarithm of the birth rate of age a, in state j and year t. $Abor_{ajt-1}$ and $Pill_{ajt-1}$ are lagged measures of access to abortion and the Pill that vary by age, state and year; A_a represents a set of age dummies and δ_{jt} are interactions of state and year fixed effects. Estimates of equations (3) are shown in Table 5. In column (1) we replicate Guldi's (2008) results which served as her baseline specification.[24] The specification in column (2) uses state and year fixed effects instead of interactions between states and years. For whites, estimates in column (2) are somewhat smaller than those in column(1), but the magnitudes are still substantial and both are statistically significant. There is essentially no change in the estimates for nonwhites between the specifications in columns (1) and (2). The comparisons suggest that state-year shocks may not be an important source of confounding. However, results for whites change fundamentally as soon as we allow estimates to vary by age. There is no association between access to the Pill and birth rates of 15-18 year olds (column 3) and estimates for women 19 to 21 (column 4) are negative but only a third of their magnitude from those in column (2).

We do not include state-year interactions in birth rates of 15-18 and 19-21 year olds because there is too little variation in the Pill and abortion policies within these age groups with which to obtain robust estimates. However, we can contrast the birth rates of 18 and 19 years olds since they represent the outcome of policies directed at 17- and 18-year olds, respectively. To see this, we have plotted the proportion of 17- and 18-year olds exposed to policies liberalizing access to the Pill and abortion by year in Figure 11A and the same for 19- and 20-year olds in Figure 11B. There is substantial variation in exposure between 17- and 18-year olds but none for 19- and 20-year olds. The overlap is

[24] We thank Melanie Guldi for sharing the coding of the laws. Our results differ slightly. We did the aggregation of births ourselves and we used the SEER population data instead of the census data, which affects both the rates and the weights slightly.

so complete among the latter that only two lines are visible, one for the Pill and the other for abortion. Returning to Table 5, we show estimates from a model with state-year interactions but limited to 18- and 19-year olds in column (5). In column (6) we show estimates from the same sample but with state and year fixed effects instead of their interactions. In columns (7) and (8) we run regressions for each age group separately, which forces the identifying variation to come from cross-state changes within age. Comparing estimates of access to the Pill across columns (5)-(8) suggests a relatively weak association regardless of the source of the identifying variation[25]. This is true for both whites and nonwhites. By contrast, access to abortion has a relatively robust association. This is especially notable for nonwhites since Guldi (2008) had reported no association, a finding at odds with the literature (Sklar and Berkov 1974; Joyce and Mocan 1990; Levine et al. 1999; and Angrist and Evans 1999).

The important point from the results in Table 5 is that the more appropriate the comparison group, the less sensitive the estimates to the source of identifying variation. As Figure 10 demonstrates, differences in the level and trends of birth rates between minors and young women may be too great to use with a difference-in-difference strategy.[26] We prefer to use variation within age to identify effects of the Pill because

[25] This null finding for subgroups is consistent with different age patterns having been confounders in previous papers, which the authors conclude is the case. It is also consistent with greater noisiness of subgroup estimates, a possibility the authors ignore. However, In Tables 5 and 6, standard errors on the coefficients for Pill access are the same or smaller when we stratify by age as compared to when we pool the age groups. Thus, we do not think the weaker association between Pill access and birth rates in the sub-group analysis is due to noise, but the result of a better counterfactual.

[26] The inclusion age fixed effects does not eliminate disparities between age groups. Large differences in baseline birth rates between age groups reflect variation in sexual activity, education, marital status and labor force participation to name a few factors. Propensity score matching provides a useful analogy. If a researcher was trying to match 20-year olds exposed to a new form of hormonal contraceptive, she would be hard-pressed to find 16-year olds whose baseline characteristics were similar to those of 20-year olds.

the level and trend in birth rates between those exposed and unexposed to liberalized Pill laws are more similar than comparisons across age within each state and year. Even with the 18- and 19-year olds, the birth rate of the latter is still 30 to 40 percent greater than those of 18-year olds before 1970. The argument in favor of state-year interactions is that they eliminate state-year shocks. However, a comparison of results in columns (1) and (2) and columns (5) and (6) of Table 5 suggests this is not a major source of bias in this context.

1.4.2.3 Distance to the nearest abortion provider

As noted previously, Guldi's (2008) coding of abortion access assumes that a regime in which abortion is illegal is equivalent to one in which abortion is legal but teens are required to obtain parental consent. As an alternative, we modify the specification in Table 5 by substituting distance to the nearest legal abortion provider as a proxy for access to abortion. We also limit the analysis to the year 1968-75 in an effort to lessen the endogeneity of abortion providers' location. The new regression is as follows:

$$(4)\ LnBrate_{ajt} = \alpha_0(LnDis_j * Y7173) + \alpha_1(LnDis_{jt-1} * Y7475) + \alpha_2 Pill_{ajt-1} + X\beta + \sum \varphi_a A_a + \lambda_s + \pi_t + e_{ajt}$$

We interact the natural logarithm of distance in hundreds of miles with periods that reflect different legal regimes for abortion because of the dramatic change in availability. We assume distance in year t-1 affects birth rates in year t. Note also that we include

In other words, it would be difficult to achieve balance among observable characteristics, let alone the unobserved ones.

state- and year-fixed effects, but not their interactions since distance only varies by state and year.

Access to the Pill is negatively associated with white birth rates when we pool all the age groups (Table 6, column 1). However, the association is weakened considerably once we stratify by age (columns 2-4) and there is no association between access to the Pill and the birth rates of 18- or 19-year olds if we force identification to come from cross-state changes within age (columns 5-6). In contrast, distance to the nearest abortion provider has a robust association across models. Since this is a double-log specification, the coefficients on distance can be interpreted as elasticities. Consider 18-year old teens, a 50 percent increase in distance, approximately 250 miles in 1970-72, is associated with a 0.9 percent increase in white birth rates and 1.3 percent increase in nonwhite birth rates (Table 6, column 5). The absolute increase in birth rates is larger among nonwhites given greater mean birth rates.

The association between distance and birth rates increases after 1972, even though distance to the nearest abortion provider falls radically. Thus, a 50 percent increase in distance in 1975, approximately 10 miles, is associated with a 1.2 percent increase for white and 1.6 percent increase for non-white birth rates of 18-years olds (Table 6, column 5). We are skeptical of this association since the supply of abortion providers, and thus distance, is potentially endogenous and we have no credible means by which to control for the simultaneity. Distance is more plausibly exogenous prior to 1973 and thus we are more confident of these estimates.

1.4.3 Identification Strategy Debate: FE vs. Comparison Group

As we note in the text, we do not interpret the weak association as evidence that the Pill was not important to fertility, but rather that the laws may not be a good way to identify the effect of the Pill.

We are better able to demonstrate the impact of various identification strategies on estimates of access to the Pill and abortion in the birth rate regressions of young women in Table 5. As the Figure 11 shows, there is substantial variation in the timing and coverage of Pill laws between women 17 and 18 years of age which will affect the birth rates of 18- and 19-year olds. There is no variation among women 19 and 20 years of age. This enables us to estimate regressions for 18- and 19-year olds that include state-year interactions. We contrast estimates from this specification with a model that uses only state and year fixed effects and then with models that use only cross-state changes within age. The results are shown in columns 5-8 of Table 5. The association between birth rates and access to the Pill is now relatively weak and more consistent across specifications. It is also important to note that the association between access to abortion and birth rates is essentially unchanged across models in columns 5-8 of Table 5. In addition, estimates for nonwhites 19 years of age and older are more consistent with the literature as the legalization of abortion had a greater impact on the birth of nonwhites relative to whites (see columns 4 and 8).

Very large baseline differences in outcomes between those in a treatment group (in our case those exposed to a liberal Pill law) and the controls (those unexposed to the Pill law) often are suggestive of differences both observed and unobserved that are not eliminated by including dummy variables for basic characteristics (see Meyer 1995). The birth rates of 20-year olds in 1969 are approximately 200 percent greater than those of 16 years olds. This reflects large differences in education, marital status, sexual activity, labor force participation, parental characteristics, etc. The appeal of propensity score matching (PSM) over the past 10 years is that it balances observable differences between those in the treatment and control groups. Put differently, PSM limits comparison to those who are on the same support of the covariates. It is in this spirit that we are skeptical of using 16-year olds as a comparison group for 20-year olds even with age fixed effects. For instance, it would be unlikely to find a 16-year old who has the same set of observables as a 20-year old. If one also considers unobserved differences between the two age groups, then 16-year olds become an even more questionable comparison group for 20-year olds and vice-versa.

Another argument for not comparing the birth rates of 16- and 20-year olds is the difference in pre-law trends. The key to a credible difference-in-difference strategy is that the pre-intervention trends between those exposed and unexposed to the intervention of interest be similar (see Chapter 6 in Angrist and Pischke 2009; Meyer 1995). Our Figures 10 demonstrates very different trajectories. As we note in the text, the inclusion of state-year fixed effects forces the identifying variation to come from differences in birth rates across age within each state and year. We believe comparisons in birth rates between women of the same age across states provide a better counterfactual.

The inclusion of state-year interactions does not change Guldi's comparison group. Indeed we believe it actually worsens it. She uses the birth rates of teens 18 years and less who had no legal access to abortion as the counterfactual for the birth rates of women 19 to 21 who gained access with legalization. In reality, there are no age differences with respect to legalized abortion. Parental consent in 1975 is a very different constraint than no legal abortion in 1968. We believe the inclusion of state-year fixed effects (state-year interactions) worsens the bias in her estimates because it restricts the identifying variation to comparisons across age within state and year and eliminates variation within age across states. If Guldi believes that parental consent for an abortion is a meaningful constraint (which we do not), then she should include it as a separate covariate. However, in that specification, state-year interactions would absorb all the variation in abortion legalization. Alternatively, she could include state-year interactions but then she would have to interact a legalization dummy with age.

If one has a good comparison group, then the average change in the outcome associated with the intervention captures variation in both the level and trend. This is why the similarity in pre-intervention trends is so important. We believe the comparison of 18- and 19-year olds comes closer to this ideal than comparisons across all age groups. We still prefer comparisons within age groups because the level as well as the trend in birth rates are similar. Nevertheless, the analysis of 18- and 19-year olds enables us to include state-year interactions and thereby demonstrate the similarity of estimates across models in Table 5.

We can't prove *a priori* the superiority of one identification strategy versus another, but as noted in the introduction to the editor, we now present a way to test for the consistency of estimates based on different sources of identifying variation. We show that when we narrow the age group comparisons to 18- and 19-year olds, model with state-year interactions yields estimates of the effects of access to the Pill on birth rates that are relatively small in magnitude and similar to models with only state and year fixed effects as well as estimates from models that use only cross-states change within age (Table 1.5 columns 5-8). We conclude that with a more appropriate comparison group, the association between Pill access and birth rates is more stable but greatly diminished.

However, we believe we can address the concerns regarding the inclusion of state-year interactions (state*year fixed effects) while demonstrating the importance of narrowing the age-group comparisons. The key is that access to the Pill varies substantially between 17- and 18-year olds. The latter gained access to the Pill largely by lowering the age of majority from 21 to 18 between 1970 and 1972. Those less than 18 years of age gained access to the Pill primarily through mature minor statutes that tended to occur later (see Figure 11 in text). Access to the Pill among 17-year olds affects the birth rates of 18-year olds one year later whereas access to the Pill at age 18 affects the birth rates of 19-year olds with the same lag. Herein lies the test. Birth rate regressions that include 18- and 19-year olds enable us to compare models with and without state-year interactions while narrowing the age difference between those exposed and unexposed to laws liberalizing access to the Pill. We present four models in Table 2, columns 5-8. The first uses 18- and 19-year olds and includes state-year interactions; the second includes 18- and 19-year olds but contains state and year fixed effects and not their interactions; the third uses only 18-year olds with state and year fixed effects and the fourth uses 19-year olds again with only state and year fixed effects. The identifying variation is different across models but we show that estimates are essentially the same. By improving the counterfactual we improve the consistency of the estimates. Based on this test we find at best weak support for a negative association between increased access to the Pill and lower birth rates.

We can do this same comparison for other age groups less than 19 (15 vs. 16, 17 vs. 18, etc.) but there is much less variation in Pill access between 15- and 16-year olds, for instance than between 18- and 19-year olds. Nevertheless, our general point is clear: the closer the age group comparison, the smaller the impact of access to the Pill on birth rates. We have attached two tables with these results that illustrate this point (Table1.10 and 1.11). In these tables all regressions include state-year interactions. The heading of each column shows the age groups included. Thus, in Panel A, column 1 of Table 1.10, we use only 15- and 21-year olds. The coefficient on the Pill Access is -0.15. Moving horizontally we narrow the age groups included. In the last column we use 15- and 16-year olds only. In only a few states do 15-year olds have access to the Pill when 14-year olds do not. Nevertheless, the coefficient is now only -0.04 and marginally significant. Panel B compares 16-year olds to older age groups. Again, the coefficient on Pill access falls as the age groups narrow. The same holds for 17- and 18-year olds.

The general point is that state-year interactions are less important than narrowing the age comparisons. The better the counterfactual, the less sensitive are the results to the specification of fixed effects (Table 1.5, 1.10 and 1.11). In fact, we would argue that including state-year interactions and forcing identification to come from comparisons across age, within each state and year introduces bias because of the large disparity in levels and trends between those exposed and unexposed to the laws (see Figure 10 and Figure 12).

1.4.4 Sensitive Tests

The overall findings in Tables 5 and 6 are largely insensitive to whether the regressions are estimated with or without population weights or if the dependent variable is expressed in levels rather than in logs (results available upon request). One result from the sensitivity analysis is noteworthy. The association between distance to the nearest abortion provider and birth rates in the years after *Roe* (1973-1975) is weaker in models without population weights. We do not read too much into this result given the endogeneity of provider location in years after *Roe*.

To illustrate the sensitivity of an identification strategy that uses only variation across age within state and year, we estimate regressions of the form of equation (3) by single year of age groups. The results are shown in Table 1.8.

$$\text{(3)} \quad \text{Ln}Brate_{ajt} = \alpha_0 Abor_{ajt-1} + \alpha_1 Pill_{ajt-1} + \sum \varphi_a A_a + \lambda_{st} + e_{ajt}$$

In these models we rely completely on cross-state changes in the policies within age for identification while allowing state and year fixed effects to vary by age. The results in columns (1)-(7) of Table 8 provide no evidence that increased access to the Pill was associated with birth rates of whites and no robust evidence among nonwhites. The coefficients on abortion access are all negative for both white and nonwhites and six of the 14 coefficients are statistically significant.

Then we modify the specification in Table 9 by substituting distance to the nearest abortion provider as a proxy for access to abortion and use the sample in 1968-1979 instead of 1968-1975 to test the sensitivity of this model regarding different sample periods. The new regression is as follows:

$$(5)\quad \mathrm{Ln}Brate_{ajt} = \alpha_0(LnDis_j * Y7072) + \alpha_1(LnDis_{jt} * Y7378) + \alpha_2 Pill_{ajt-1} + \mathbf{X}\beta + \sum \varphi_a A_a + \lambda_s + \pi_t + e_{ajt}$$

Average distance to a legal abortion provider dropped from 502 miles in 1972 to 29 miles in 1973 and continued to decline more modestly to 17 miles by 1978. We assume distance in year t-1 affects birth rates in year t. Note also that we include state and year fixed effects, but not their interactions since distance only varies by state and year.

There is no association between access to the Pill and birth rates once we force identification to come from cross-state changes within age (columns 4-10). In contrast, distance to the nearest abortion provider has a robust association in the same models. Since this is a double-log specification, the coefficients on distance can be interpreted as elasticities. Consider 18-year old teens. A 50 percent increase in distance, approximately 250 miles in 1970-72, is associated with a 1.1 percent increase in white birth rates and 1.2 percent increase in nonwhite birth rates (Table 9, column 7). The absolute decline in birth rates is larger among nonwhites given greater mean birth rates.

The association between distance and birth rates increases after 1972, even though distance to the nearest abortion provider falls radically. Thus, a 50 percent increase in distance in 1975, approximately 10 miles, is associated with a 1.8 percent increase in both the white and non-white birth rates for 18 years olds (Table 9, column 7). We are skeptical of this association since the supply of abortion providers, and thus distance, is endogenous and we have no credible means by which to control for the simultaneity. Distance is more credibly exogenous prior to 1973 and thus we are more confident of these estimates.

In summary, two findings stand out. There is no consistent association between access to the Pill and birth rates when identification is based on cross-state changes in laws within age. The regression estimates by single year of age in Tables 8 and 9 largely confirm the lack of any apparent discontinuities associated with changes in access to the Pill in Figures 10A and 10B. These same plots also may explain the strong association between Pill access and birth obtained by Guldi (2008) and Ananat and Hungerman (forthcoming). In numerous states, access to the Pill among 18-20-year olds preceded access to minors. Consider specifications in which the identifying variation is limited to cross-age comparisons within states. In this instance, the rising birth rates of minors between 1968 and 1972 serves as the counterfactual for the declining birth rates of women 19-21. Given the discrepancy in the levels and trends of birth rates across age we prefer cross-state comparisons within age. The second finding is the robust association between distance to the nearest abortion provider and birth rates of young women. We are more confident of the findings before *Roe,* which are consistent with the strong relationship between abortion rates and distance to New York in Table 4.

1.5 Abortion and Birth Rates: NY data 1971-72

In this section we analyze the relationship between birth and abortion rates by age and race in the 28 states for which New York State was the probable source of legalized abortion services in the period preceding *Roe*. Despite the limited sample, these data and this period are unique. Not only are detailed data on abortions prior to *Roe* rare, but abortions today are no longer collected by age, race and state of residence. In addition, the sudden legalization of abortion in New York in July of 1970 provides an opportunity to instrument for lagged abortion rates in a birth rate regression. However, distance is a crude proxy for the availability of abortion services and that lack of variation overtime requires interactions by age and year in models with state fixed effects. Abortion rates, on the other hand, vary by age, year, state and race and given the plausibly exogenous change in the availability of legalized abortion, the lagged abortion rates can also be viewed as more detailed proxies for the availability of abortion services than is distance.[27] With this interpretation, estimates obtained by ordinary least squares are interesting in their own right.

The results in Table 7 are based on the following regression of birth rates by age, state and year on resident abortion rates for the same, age, state but lagged one year ($Arate_{ajt-1}$). The additional regressors are the same as in equation (1) above.

$$(4)\quad Brate_{ajt} = \alpha_0 Arate_{ajt-1} + \alpha_1 Pill_{ajt-1} + \sum \varphi_a A_a + X_{jt}\beta + \lambda_j + \tau_t + e_{ajt}$$

In models of all women, an increase in the abortion rate of 1 per 1000 age-specific women is associated with a decrease in birth rates of between 1.16 – 1.39 births per 1000 age-specific women (columns 1 and 2). Although the 95% confident intervals include -1.0, the point estimates should not exceed -1.0 in absolute value unless we have underestimated resident abortion rates or abortion rates capture other changes in fertility control practices that are omitted. The coefficients on the abortion rates in the race-

[27] Goldin and Katz (2002) also use abortion rates as a proxy for the availability of abortion services.

specific regressions are closer to -1.0 in the case of whites and less than -1.0 in absolute value among nonwhites. Among the latter, an increase in the abortion rate of 1.0 per 1000 is associated with decline in birth rates of 0.53 per 1000. There is no difference between estimates instrumented by distance and those obtained by ordinary least squares. Taken literally, this suggests that lagged abortion rates are an exogenous determinant of birth rates in the years before *Roe*. However, calibrating the exact relationship between abortion and birth rates may not be possible, as evidenced by the magnitude of the coefficients. Thus, a more conservative interpretation is that the OLS estimates capture the strong association between access to abortion and birth rates.

In all models, the association between increased access to the Pill and age-specific birth rates is positive and statistically significant in the regressions of all women and whites, but small and statistically insignificant among nonwhites. Even if we estimate the models without the lagged abortion rate, the coefficient on Pill access is similar (results not shown). The unexpected association may be due to the limited sample period and states, but the finding for nonwhites is consistent with the results from the national data in Tables 5 and 6.

Using distance to New York or a legal abortion provider as a regressor is frustrating because it lacks variation by age and time. One of the great advantages of the New York data is the availability of abortions by state, year, age and race. As noted in the text, we use the lagged abortion rate much like Goldin and Katz as a less crude proxy for access than distance in our case and a legalization dummy in theirs. Goldin and Katz (2002) write, "Since an indicator variable for state legalization of abortion is likely to be a crude indicator of access to abortion, we also explore the impact of a continuous measure: the average abortion rate (abortion/live births) in an individual's state of birth when the individual was18-21 years of age" (p. 755).

We agree that a coefficient on the abortion rate greater than 1 in absolute value suggests that each abortion prevents more than one unintended birth and that abortion is picking up other factors including an incomplete count of abortions. Nevertheless, we are not trying to estimate the full structural model of fertility. Our more modest goal is to use actual abortion rates to show the importance of legalized abortion during a time dramatic change. To offset some of the potential simultaneity, we lag the abortion rate. If one accepts distance as a legitimate instrument, then the similarity of the OLS and IV estimates of lagged abortion in the birth rates regressions provides some support for the OLS estimates.

1.6 Conlusion

Recent studies on the "Power of the Pill" have linked increased access to the Pill among young, unmarried women in the late 1960s and early 1970s to growth in their educational, marital and occupational opportunities (Goldin and Katz 2002; Bailey 2006; Guldi 2008; Ananat and Hungerman forthcoming). These reduced-form associations rest critically on whether state laws and policies liberalizing access to the Pill did in fact increase use of the Pill, which in turn led to decreases or delays in fertility.[28] In this paper we argue that these "first-stage" relationships have not been adequately demonstrated. There is no visual evidence of any break or discontinuity in the time-series of birth rates associated with policies liberalizing access to the Pill and regression estimates are sensitive to the comparison group. By contrast, evidence linking access to legalized abortion and state abortion rates is compelling. Using recently discovered data on abortions by age, race, and state of residence in 1971 and 1972 we document the remarkable travel by young women to New York for an induced termination. We show that distance to New York is inversely associated with abortion rates in years before *Roe.* These findings are consistent with the robust association between distance to an abortion provider and increased birth rates of young women in the national panel that covered a longer time period.

Strong conclusions about the role of abortion or contraceptive services in the early 1970s must be tempered by the limited data and evolving social changes during this period. Advances in civil rights, the women's movement, the Vietnam War, the Pill,

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[28] Goldin and Katz (2002) present a model in which the Pill also may have an indirect effect on women who did not use the Pill by the "thickening" of the marriage markets at older ages.

legalized abortion, changing sexual mores, and the increased use of recreational drugs all interacted in complicated ways that may have affected reproductive outcomes. Identification of a particular causal factor amidst this cauldron of change requires a sharp break in policy or technology that leads to discernable changes along a logical pathway. The legalization of abortion comes closest to providing the desired natural experiment. Despite the evidence presented, our results do not refute the importance of the Pill to the well-being of young women, but they call into question the identification strategy supporting recent claims.

Table 1.1: Pill Access and Pill Use in 1971 by Various Coding of Pill Policies±

	Goldin and Katz (2002)		Guldi (2006)		Bailey (2006)		Ananat and Hungerman (2010)	
	All	Sexually Active	All	Sexually Active	All	Sexually Active	All	Sexually Active
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pill access[#]	0.023**	0.081**	0	-0.002	-0.020*	-0.055	-0.006	-0.03
	(0.009)	(0.028)	-0.009	-0.029	(0.010)	(0.032)	(0.013)	(0.037)
Observations	4211	1314	4211	1314	4211	1314	3214	1142
R-squared	0.085	0.113	0.084	0.108	0.085	0.109	0.074	0.088

± Source: National Survey of Young Women (NSYW 1971) *p<0.05, ** p<0.01.
Guldi's (2008) coding differs from that of Goldin and Katz (2002). Guldi treats Kansas, Maryland, Ohio, Pennsylvania and Virginia as having lenient policies towards the pill. Goldin and Katz (2002) consider policies toward the pill in Arkansas, California and Michigan as lenient. Guldi (2008) and Goldin and Katz (2002) are concordant on 9 states: Alabama, D.C., Georgia, Illinois, Mississippi, New Hampshire, New York, Oregon, and Tennessee. Bailey's (2006) coding characterizes 10 states in NSYW71 as allowing early legal access to the pill among young, unmarried women less than 21 years of age prior to 1971. Only 3 out of 10 states are consistent with Goldin and Katz (2002): Arkansas, Georgia and Mississippi. Seven other states are treated as lenient in Bailey (2006): Kansas, Kentucky, Maryland, Nevada, Ohio, Oklahoma and Utah. Annat and Hungerman(2010)'s coding were based on the data from Guldi (2008). Their measure of access to the pill is a lagged indicator for whether women had access at the age of 16.

Table 1.2: Association between Pre-Roe. Abortion Rates and Abortion Providers by State and Year, 1973-1979

	All Abortion Providers per 100,000 women		Non-Hospital Providers per 100,000 women	
	(1)	(2)	(3)	(4)
Abortion rate$_{70\text{-}72}$	0.429**		0.072**	
(mean=5.00)	(0.036)		(0.018)	
Abortion rate$_{70\text{-}72}$ x trend		0.004		0.011*
		(0.008)		(0.005)
N	315	315	315	315
R-sq	0.40	0.88	0.22	0.79
Year_FE	Yes	Yes	Yes	Yes
State_FE	No	Yes	No	Yes
Mean Dependent Variable$_{1973}$	2.32		0.33	
Mean Dependent Variable$_{1979}$	3.92		1.56	

Notes: Abortion Rate$_{70\text{-}72}$ is the total abortion rate per 1,000 women age 15-44 by state of residence averaged from 1970-1972 in the 45 states that legalized abortion with *Roe* (CDC 1971, 1972, 1974). The first dependent variable is the number of all types of abortion providers per 100,000 women from age 15-44, and the second dependent variable is non-hospital abortion providers per 100,000 women from age 15-44. * p<0.05, ** p<0.01.

Table 1.3: The number and percent of teen abortions performed in New York City (NYC) or the Rest of the State (ROS) by State of Residence in 1971-72

State	**NYC 1971-72**	**ROS 1971-72**	**% NYC**
Connecticut	3537	626	85.0%
Maine	890	301	74.7%
Massachusetts	4061	3651	52.7%
New Hampshire	775	247	75.8%
Rhode Island	951	244	79.6%
Vermont	205	333	38.1%
Total	10419	5402	65.9%

Table 1.4: Regressions of Age-Specific Abortion Rates on the Natural Log of Distance: 28 States 1971-72

	All Women		Whites		Nonwhites	
	(1)	(2)	(3)	(4)	(5)	(6)
Ln distance 00 miles	-2.22**		-1.17**		-4.25**	
	(0.40)		(0.24)		(0.84)	
Ln distance *Ages 15-	0.67*	0.66*				
(abortion rate=5.1)$^{\pm}$	(0.28)	(0.29)				
Ln distance *Ages 18-	-3.21**	-3.22**				
(abortion rate =12.7)	(0.76)	(0.81)				
Ln distance *Ages 21-	-3.28**	-3.28**				
(abortion rate =9.7)	(0.15)	(0.16)				
Ln distance *Age<20			-1.15*	-1.15*	-5.07**	-5.08**
(WA=8.6; NWA=12.6)			(0.45)	(0.50)	(0.71)	(0.78)
Ln distance *Age 20-29			-2.54**	-2.54**	-7.54**	-7.54**
(WA=7.8; NWA=12.6)			(0.13)	(0.15)	(0.49)	(0.53)
Access to Pill	-0.63	-0.87	-0.60	-0.37	1.99	1.77
	(0.85)	(0.55)	(0.91)	(0.57)	(2.46)	(2.15)
Observations	224	224	168	168	168	168
R-sq	0.91	0.96	0.87	0.95	0.80	0.89

[#]Estimates of equation (1) in text. Abortions performed in New York State in 1971-72 are by age, race and state of residence. All regressions include insured unemployment rate, per capita income and percent of nonwhite population. There 224 observations among all women (4 ages *2 years *28 states) and 168 by race (3 ages * 2years * 28 states). The reference group for age is women 25 and over among all women and 30 and older in the race-specific regressions. $^{\pm}$ Mean abortion rate by age. “WA” and “NWA” are the mean abortion rates for whites & nonwhites, respectively. The proportion of women with access to the Pill in 1971-72 by age is as follows: 0.36 for 15-17; 0.62 for 18-20; 1.0 for 21 and over. Mean distance is 4.83 in hundreds of miles. *p<.05, ** p<.01. Robust standard errors, clustered at state level.

Table 1.5: Association between Access to the Pill and Abortion and Age and Race-specific Birth Rates, 1968-79

Age	15-21 (1)	15-21 (2)	15-18 (3)	19-21 (4)	18 & 19 (5)	18 & 19 (6)	18 (7)	19 (8)
					Whites			
Abortion Access	-0.116*	-0.087**	-0.029	-0.015	-0.059**	-0.059**	-0.039*	-0.043*
	(0.050)	(0.023)	(0.021)	(0.014)	(0.018)	(0.010)	(0.018)	(0.017)
Pill Access	-0.092*	-0.064*	-0.002	-0.023	-0.035	-0.019	0.012	-0.020
	(0.040)	(0.026)	(0.025)	(0.016)	(0.018)	(0.014)	(0.023)	(0.018)
Observations	4284	4284	2448	1836	1224	1224	612	612
Adj. R-sq	0.97	0.97	0.98	0.95	0.96	0.95	0.96	0.97
Mean Birthrate	72.1	72.1	39.9	115.1	86.3	86.3	74.3	98.3
					Nonwhites			
Abortion Access	-0.031	-0.033	-0.008	-0.061**	-0.039*	-0.051**	-0.042*	-0.086**
	(0.061)	(0.029)	(0.023)	(0.015)	(0.016)	(0.015)	(0.018)	(0.017)
Pill Access	0.023	-0.001	-0.036	0.007	0.014	-0.004	-0.030	0.000
	(0.057)	(0.031)	(0.025)	(0.018)	(0.019)	(0.017)	(0.025)	(0.023)
Observations	4284	4284	2448	1836	1224	1224	612	612
Adj. R-sq	0.93	0.93	0.94	0.90	0.92	0.89	0.90	0.90
Mean Birthrate	139.07	139.07	101.1	189.4	170.3	170.3	159.3	181.3
State*Year FE	Yes	No	No	No	Yes	No	No	No

Notes: Estimates of equation (2) in text. Estimates in each column within race are from a separate regression. The dependent variable is the natural logarithm of the birth rate of age group "a", in state "j" and year "t". Measures of access to abortion and the Pill are lagged by one year. Each model includes state and year fixed effects except where noted. There are 51 states, 12 years and 7 age groups (N=4284). Regressions are weighted by age-specific population and clustered at the state level. Efforts to replicate Guldi's regression in column (1) are not exact. Guldi's estimates (standard errors) for whites were -0.100 (0.054) and -0.085 (0.0411) for abortion and Pill access, respectively, and -0.030 (0.058) and 0.009 (0.051) for nonwhites. We aggregated births from the national natality files and used the SEER population data instead of census data, which may explain the differences. Robust standard errors, clustered at state level. * $p<0.05$, ** $p<0.01$.

Table 1.6: Association between Access to the Pill and Abortion (Distance) and Age and Race-specific Birth Rates, 1968-75

Age	15-21 (1)	15-18 (2)	19-21 (3)	18&19 (4)	18 (5)	19 (6)
			Whites			
Ln distance *Y71-73	0.016**	0.020**	0.012**	0.017**	0.018**	0.016**
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Ln distance *Y74-75	0.025**	0.031**	0.018**	0.021**	0.024**	0.019**
	(0.005)	(0.006)	(0.006)	(0.004)	(0.006)	(0.005)
Pill	-0.112**	-0.046*	-0.004	-0.034**	-0.009	0.001
	(0.023)	(0.019)	(0.010)	(0.011)	(0.014)	(0.013)
Observations	2856	1632	1224	816	408	408
Adj. R-sq	0.97	0.98	0.95	0.96	0.97	0.97
			Nonwhites			
Ln distance *Y71-73	0.026**	0.029**	0.020**	0.025**	0.025**	0.025**
	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)	(0.002)
Ln distance *Y74-75	0.032**	0.042**	0.017**	0.024**	0.031**	0.018*
	(0.008)	(0.009)	(0.005)	(0.008)	(0.011)	(0.007)
Pill	0.009	-0.034	0.015	-0.005	-0.032	0.010
	(0.026)	(0.019)	(0.016)	(0.016)	(0.020)	(0.021)
Observations	2856	1632	1224	816	408	408
Adj. R-sq	0.93	0.95	0.88	0.87	0.89	0.89

Notes: Estimates of equation (3) in text. Estimates in each column within race are from a separate regression. The dependent variable is the natural logarithm of the birth rate of age group “a”, in state “j” and year “t”. Log distance is lagged one year. Each model includes state and year fixed effects. See notes to Table 2. * $p<0.05$, ** $p<0.01$.

Table 1.7: Regressions of Age-Specific Birth Rates on Lagged Abortion Rates in 28 States 1972-73#

	All Women		Whites		Nonwhites	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Abortion Rate	-1.39**	-1.16**	-1.05**	-0.99*	-0.53**	-0.53*
	(0.34)	(0.33)	(0.39)	(0.41)	(0.18)	(0.24)
Pill Access	6.49*	6.81**	9.10**	9.11**	0.29	0.29
	(2.69)	(2.46)	(3.12)	(2.77)	(5.14)	(4.59)
Observations	224	224	168	168	168	168
R-sq	0.94	0.94	0.96	0.96	0.92	0.92
Partial F, 1^{st}-stage		142.3		84.4		100.4
Partial R-sq, 1^{st}		0.74		0.63		0.40

#See notes to Table 2. First-stage estimates for the abortion rate are from columns 2, 4 and 6 of Table 2 for all women, whites and nonwhites, respectively. The partial F and R^2 pertain to distance-age interactions in Table 2, the instruments for the abortion rate. Robust standard errors.

Table 1.8: Association between Access to the Pill and Abortion and Age and Race-specific Birth Rates, 1968-79

Age	15	16	17	18	19	20	21
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Whites			
Abortion Access	-0.003	-0.025	-0.040	-0.039*	-0.043*	-0.003	0.003
	(0.029)	(0.024)	(0.025)	(0.018)	(0.017)	(0.013)	(0.017)
Pill Access	0.007	0.020	0.002	0.012	-0.020	-0.029	-0.029
	(0.029)	(0.024)	(0.030)	(0.023)	(0.018)	(0.017)	(0.016)
Observations	612	612	612	612	612	612	612
Adj. R-sq	0.93	0.95	0.95	0.96	0.97	0.97	0.97
Mean Birthrate	9.5	26.2	49.6	74.3	98.3	115.7	131.4
				Nonwhites			
Abortion Access	-0.018	-0.000	-0.032	-0.042*	-0.086**	-0.048*	-0.048**
	(0.027)	(0.024)	(0.022)	(0.018)	(0.017)	(0.021)	(0.017)
Pill Access	-0.039	-0.052	-0.048	-0.030	0.000	0.013	-0.002
	(0.035)	(0.036)	(0.030)	(0.025)	(0.023)	(0.020)	(0.021)
Observations	612	612	612	612	612	612	612
Adj. R-sq	0.86	0.88	0.88	0.90	0.90	0.92	0.91
Mean Birthrate	42.6	81.6	121.1	159.3	181.3	192.2	194.8

Notes: Estimates of equation (2) in text. Estimates in each column within race are from a separate regression. The dependent variable is the natural logarithm of the birth rate of age group "a", in state "j" and year "t". Measures of access to abortion and the Pill are lagged by one year. There are 51 states, 12 years and 7 age groups (N=4284). Regressions are weighted by age-specific population and clustered at the state level. Efforts to replicate Guldi's regression in column (1) are not exact. Guldi's estimates (standard errors) for whites were -0.100 (0.054) and -0.085 (0.0411) for abortion and Pill access, respectively, and -0.030 (0.058) and 0.009 (0.051) for nonwhites. We aggregated births from the national natality files and used the SEER population data instead of census data, which may explain the differences. Robust standard errors, clustered at state level. * $p<0.05$, ** $p<0.01$.

Table 1.9: Association between Access to the Pill and Abortion (Distance) and Age and Race-specific Birth Rates, 1968-79

Age	15-21 (1)	15-18 (2)	19-21 (3)	15 (4)	16 (5)	17 (6)	18 (7)	19 (8)	20 (9)	21 (10)
						Whites				
Ln distance *71-73	0.021**	0.025**	0.013**	0.028**	0.023**	0.026**	0.022**	0.018**	0.011**	0.010**
	(0.003)	(0.003)	(0.002)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.002)
Ln distance *74-79	0.035**	0.041**	0.029**	0.045**	0.039**	0.042**	0.037**	0.032**	0.026**	0.028**
	(0.006)	(0.008)	(0.006)	(0.013)	(0.009)	(0.010)	(0.007)	(0.006)	(0.008)	(0.007)
Pill	-0.075**	-0.010	-0.014	-0.010	0.010	0.002	0.008	-0.011	-0.021	-0.019
	(0.024)	(0.020)	(0.015)	(0.025)	(0.020)	(0.023)	(0.018)	(0.017)	(0.015)	(0.015)
Observations	4284	2448	1836	612	612	612	612	612	612	612
Adj. R-sq	0.97	0.98	0.95	0.94	0.96	0.96	0.96	0.97	0.97	0.97
						Nonwhites				
Ln distance *71-73	0.023**	0.026**	0.018**	0.027**	0.026**	0.027**	0.024**	0.023**	0.014**	0.015**
	(0.003)	(0.004)	(0.002)	(0.007)	(0.005)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)
Ln distance *74-79	0.038**	0.050**	0.016**	0.058**	0.061**	0.044**	0.036***	0.023**	0.012*	0.013*
	(0.009)	(0.011)	(0.005)	(0.015)	(0.012)	(0.014)	(0.009)	(0.008)	(0.005)	(0.005)
Pill	-0.001	-0.038	0.015	-0.043	-0.056	-0.044	-0.030	0.011	0.018	0.006
	(0.030)	(0.022)	(0.017)	(0.033)	(0.032)	(0.024)	(0.020)	(0.023)	(0.020)	(0.021)
Observations	4284	2448	1836	612	612	612	612	612	612	612
Adj. R-sq	0.93	0.95	0.90	0.87	0.89	0.90	0.91	0.91	0.92	0.91

Notes: Estimates of equation (3) in text. Estimates in each column within race are from a separate regression. The dependent variable is the natural logarithm of the birth rate of age group "a", in state "j" and year "t". Log distance is lagged one year. See notes to Table 3. * $p<0.05$, ** $p<0.01$.

Table 1.10: Association of Abortion and Pill Access with Birth Rates in Models with State-Year Interactions Stratified by Binary Age Groupings, Whites 1968-79

			Whites			
			Panel A: Age 15			
	(1)	(2)	(3)	(4)	(5)	(6)
Age	15 vs 21	15 vs. 20	15 vs. 19	15 vs 18	15 vs 17	15 vs 16
Abortion Access	-0.19	-0.19*	-0.18*	-0.07	-0.09	-0.03**
	(0.11)	(0.09)	(0.07)	(0.05)	(0.06)	(0.00)
Pill Access	-0.15	-0.15	-0.14*	-0.05	-0.08	-0.04
	(0.09)	(0.08)	(0.07)	(0.08)	(0.06)	(0.02)
St*Yr FE	Yes	Yes	Yes	Yes	Yes	Yes
			Panel B: Age 16			
	(1)	(2)	(3)	(4)	(5)	
Age	16 vs 21	16 vs. 20	16 vs. 19	16 vs 18	16 vs 17	
Abortion Access	-0.13	-0.13	-0.12	0.02	-0.10**	
	(0.10)	(0.08)	(0.06)	(0.03)	(0.02)	
Pill Access	-0.11	-0.10	-0.09	-0.03	-0.02	
	(0.07)	(0.06)	(0.05)	(0.06)	(0.02)	
St*Yr FE	Yes	Yes	Yes	Yes	Yes	
			Panel C: Age 17			
	(1)	(2)	(3)	(4)		
Age	17 vs 21	17 vs. 20	17 vs. 19	17 vs 18		
Abortion Access	-0.10	-0.10	-0.08	0.04*		
	(0.09)	(0.07)	(0.05)	(0.02)		
Pill Access	-0.10	-0.09	-0.07	0.00		
	(0.08)	(0.06)	(0.05)	(0.00)		
St*Yr FE	Yes	Yes	Yes	Yes		
			Panel D: Age 18			
	(1)	(2)	(3)			
Age	18 vs 21	18 vs. 20	18 vs. 19			
Abortion Access	-0.12*	-0.10**	-0.06**			
	(0.04)	(0.03)	(0.02)			
Pill Access	-0.05	-0.05	-0.04			
	(0.04)	(0.03)	(0.02)			
St*Yr FE	Yes	Yes	Yes			
No. Obs	1224	1224	1224	1224	1224	1224

Table 1.11: Association of Abortion and Pill Access with Birth Rates in Models with State-Year Interactions Stratified by Binary Age Groupings, Non-Whites 1968-79

			Non-Whites			
			Panel A: Age 15			
	(1)	(2)	(3)	(4)	(5)	(6)
Age	15 vs 21	15 vs. 20	15 vs. 19	15 vs 18	15 vs 17	15 vs 16
Abortion Access	-0.05	-0.05	-0.05	0.15**	-0.05*	-0.01**
	(0.13)	(0.12)	(0.10)	(0.04)	(0.02)	(0.00)
Pill Access	0.08	0.05	0.03	-0.10**	-0.01	-0.03
	(0.15)	(0.12)	(0.09)	(0.03)	(0.04)	(0.02)
St*Yr FE	Yes	Yes	Yes	Yes	Yes	Yes
			Panel B: Age 16			
	(1)	(2)	(3)	(4)	(5)	
Age	16 vs 21	16 vs. 20	16 vs. 19	16 vs 18	16 vs 17	
Abortion Access	-0.05	-0.05	-0.05	0.10**	0.03	
	(0.10)	(0.10)	(0.08)	(0.04)	(0.02)	
Pill Access	0.06	0.03	0.02	-0.07	-0.03	
	(0.12)	(0.09)	(0.07)	(0.04)	(0.03)	
St*Yr FE	Yes	Yes	Yes	Yes	Yes	
			Panel C: Age 17			
	(1)	(2)	(3)	(4)		
Age	17 vs 21	17 vs. 20	17 vs. 19	17 vs 18		
Abortion Access	-0.03	-0.02	-0.03	0.07**		
	(0.08)	(0.07)	(0.05)	(0.02)		
Pill Access	0.01	-0.01	-0.01	0.00		
	(0.10)	(0.07)	(0.05)	(0.00)		
St*Yr FE	Yes	Yes	Yes	Yes		
			Panel D: Age 18			
	(1)	(2)	(3)			
Age	18 vs 21	18 vs. 20	18 vs. 19			
Abortion Access	-0.06	-0.05*	-0.04*			
	(0.03)	(0.02)	(0.02)			
Pill Access	0.02	0.02	0.01			
	(0.05)	(0.03)	(0.02)			
St*Yr FE	Yes	Yes	Yes			
No. Obs	1224	1224	1224	1224	1224	1224

Figure 1.1: Resident Teen Abortion Rates For Abortions Performed In NY, 1971
Abortions per 1000 teens 15-19

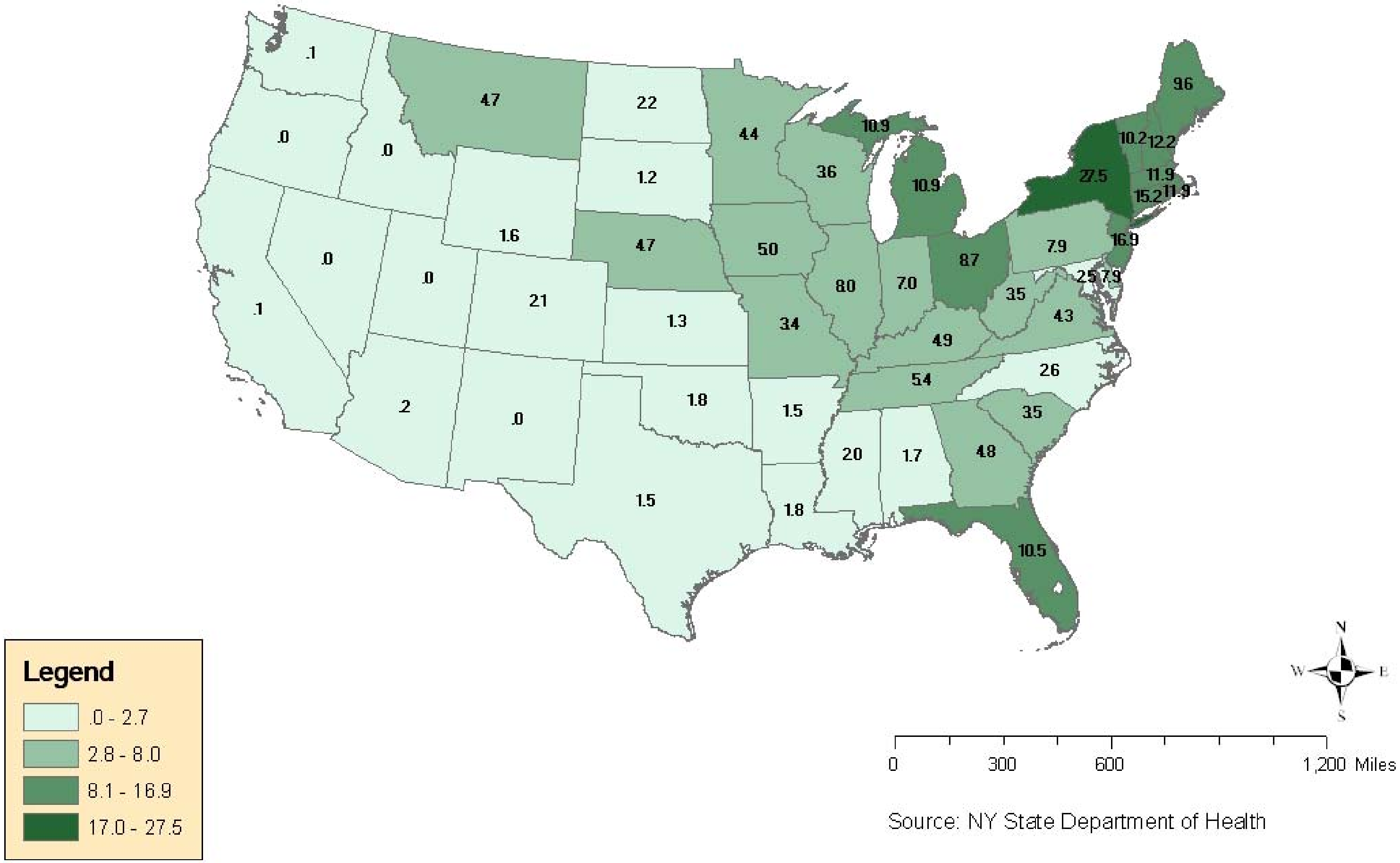
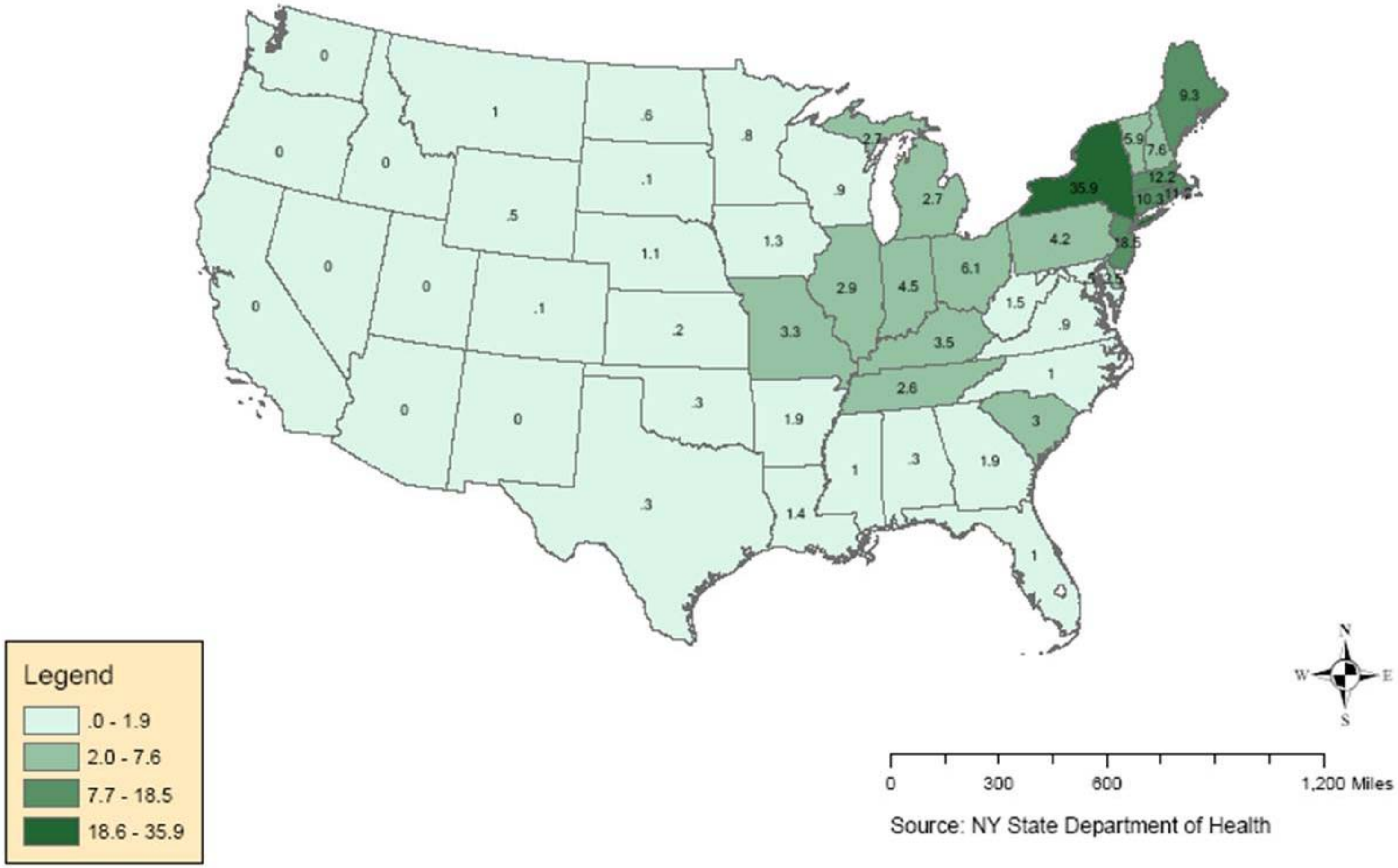
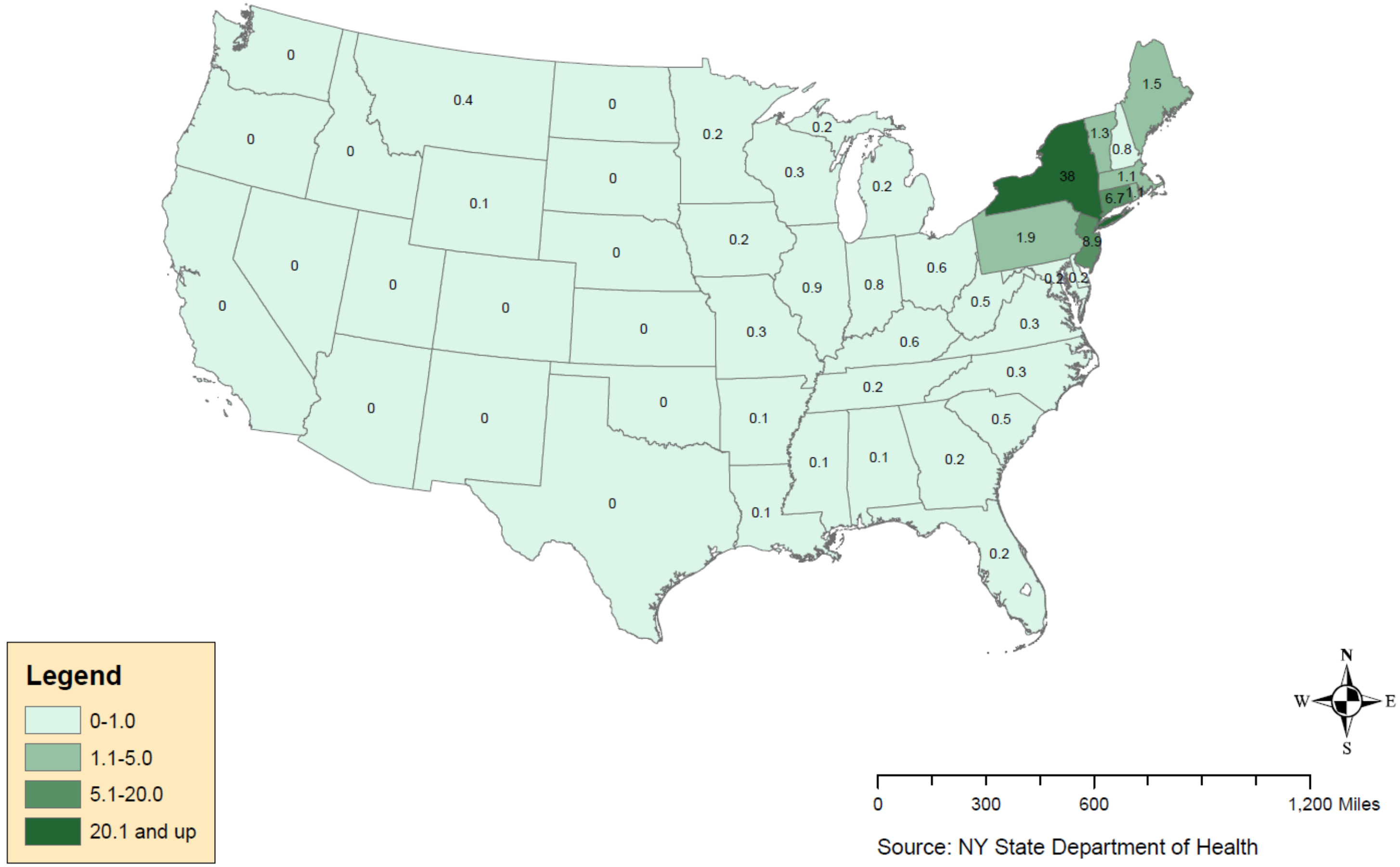


Figure 1.2: Resident Teen Abortion Rates for Abortions Performed in NY, 1973
Abortions per 1000 teens 15-19



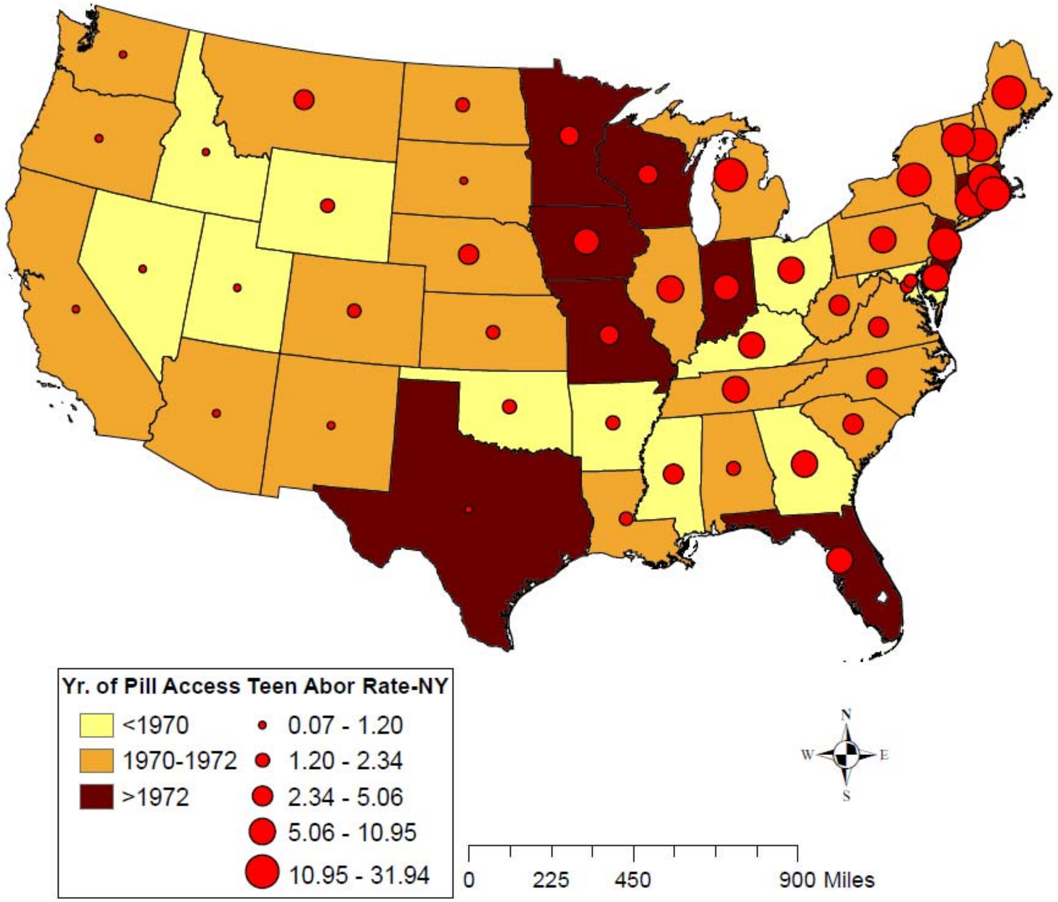
Source: NY State Department of Health

Figure 1.3: Resident Teen Abortion Rates for Abortions Performed in NY, 1975
Abortions per 1000 teens 15-19



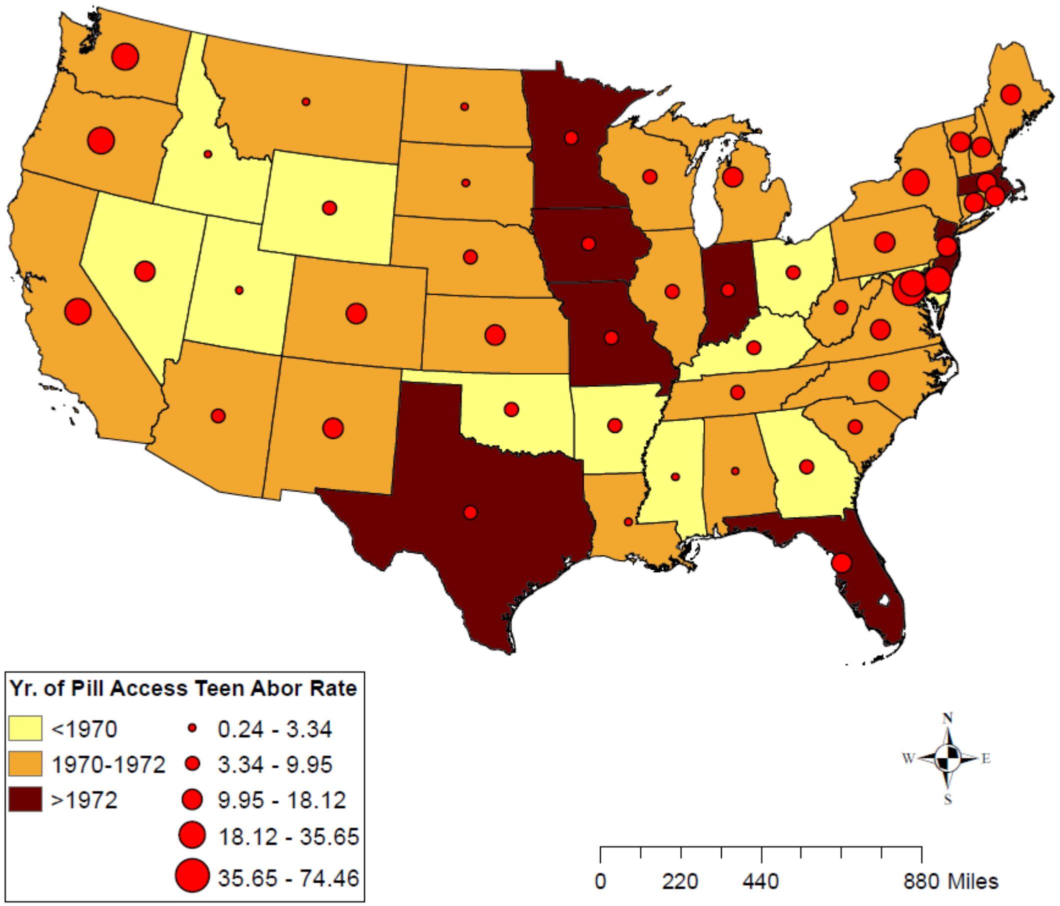
Source: NY State Department of Health

Figure 1.4: Year Access to the Pill Provided to Those Younger than 21 Years of Age and Teen Abortion Rate in 1971-72; NY*



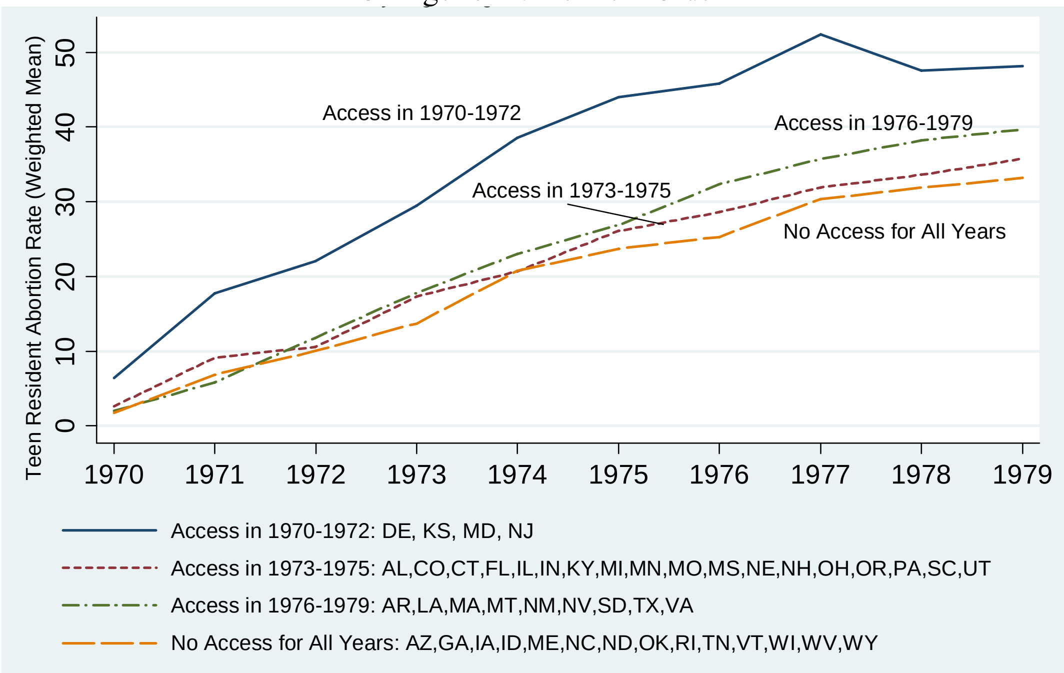
* Abortion rate by states of residence for abortions obtained only in NY state.

Figure 1.5: Year Access to the Pill Provided to Those Younger than 21 Years of Age and Teen Abortion Rates in 1971-72



Source: Centers for Disease Control, NY State Department of Health, Guldi(2008)

Figure 1.6: Teen Resident Abortion Rate (Age 15-19) by Timing of Abortion Access Change for 15-17 Year Olds



*Early Legalizing States AK, CA, DC, HI, NY and WA not included

Figure 1.7: Teen Resident Abortion Rates in 1972 and 1974 in All States by Distance to Nearest Legal Abortion Provider

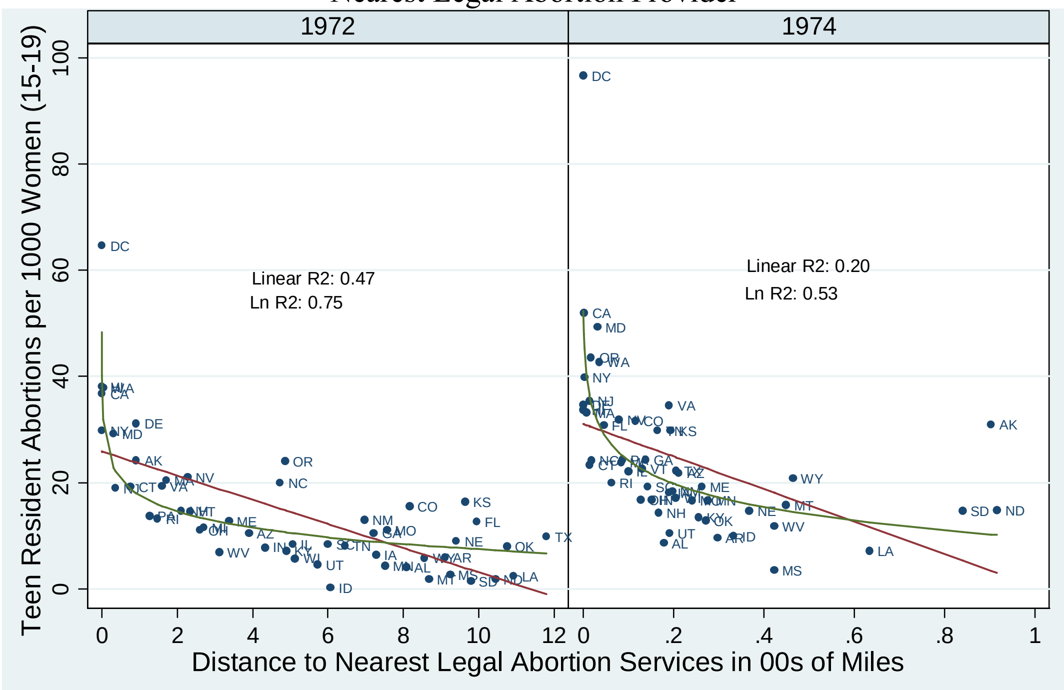


Figure 1.8: Teen Resident Abortion Rates by Distance to Nearest Legal Abortion Provider, 50 States + DC

Figure 1.8A Teen Resident Abortion Rates in 1971 - 1972

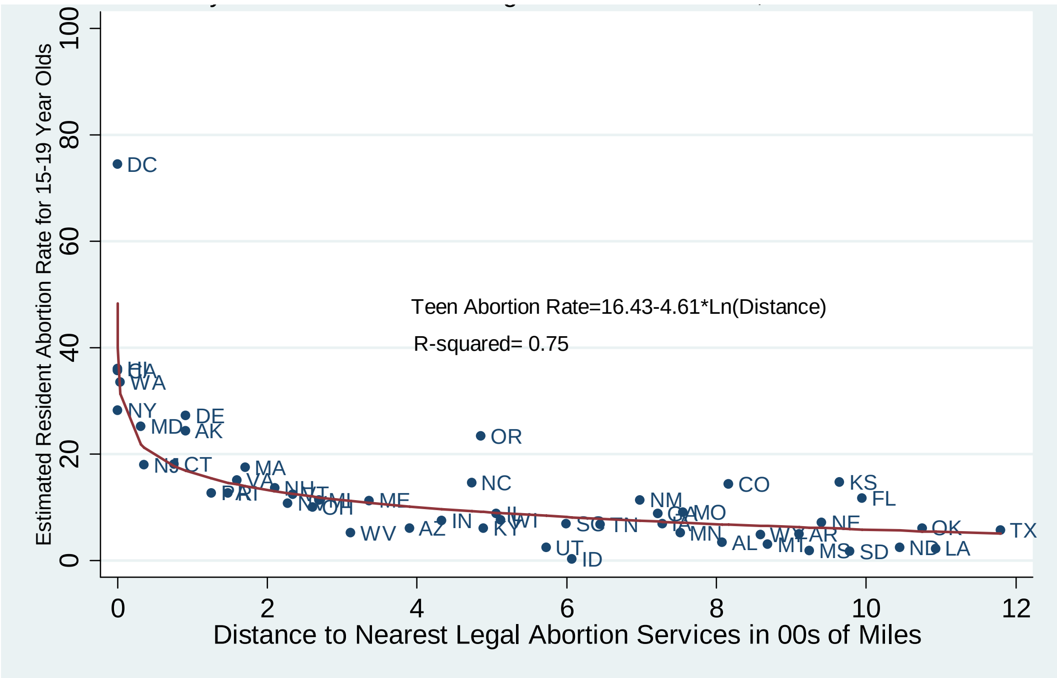


Figure 1.8B Teen Resident Abortion Rates in 1973 - 1974

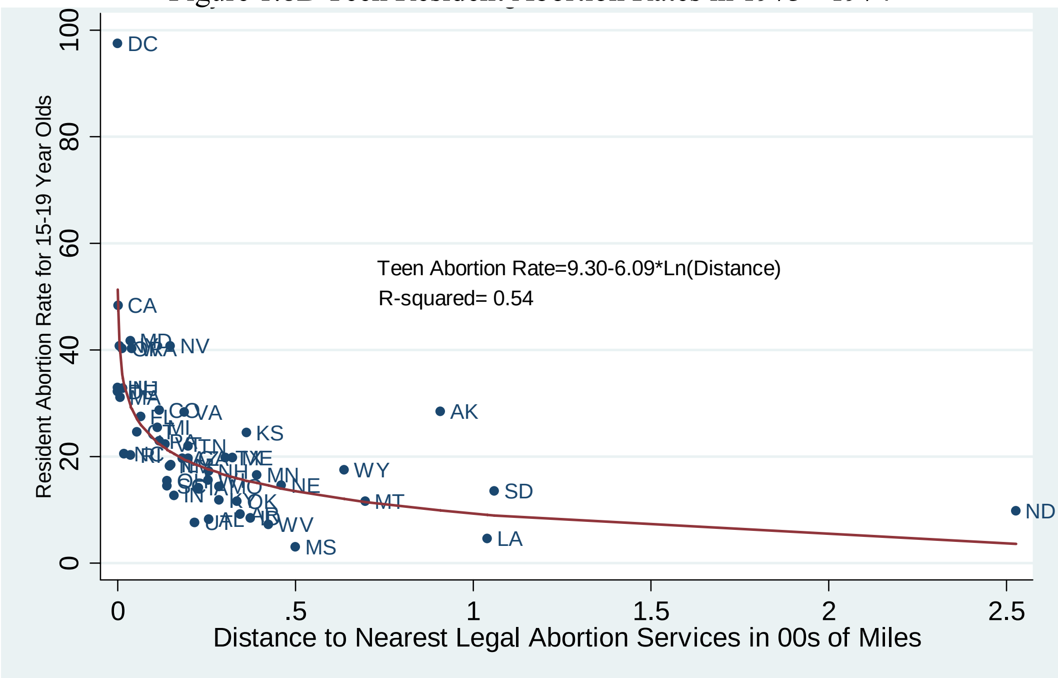


Figure 1.8C Teen Resident Abortion Rates in 1975 - 1976

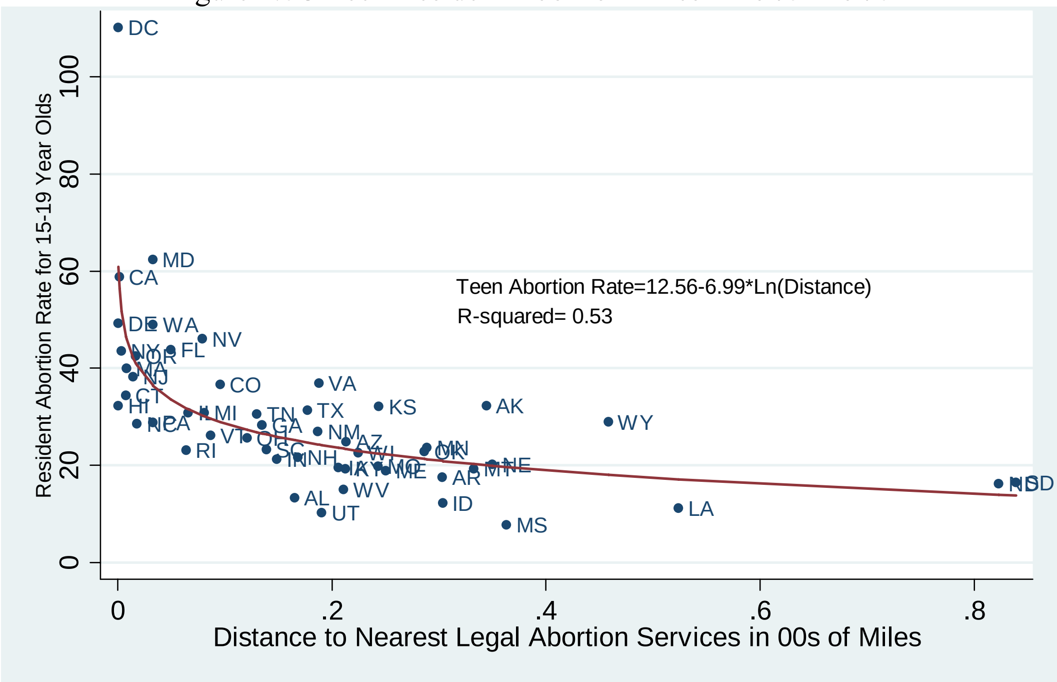


Figure 1.9: Resident Abortion Rates in 1971 - 72 in 28 states by Distance to Nearest New York Legal Abortion Provider

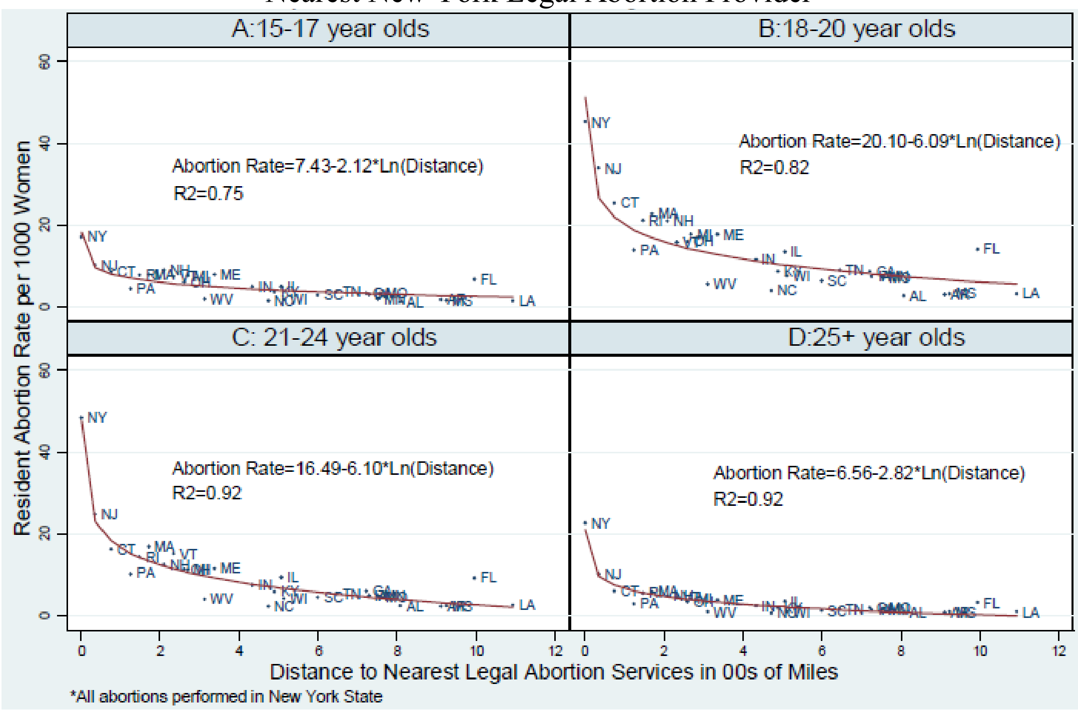


Figure 1.10: Birth Rate of 19-21 Year Olds by Year of Access to the Pill without Parental Consent*

Figure 1.10A Birth Rate of 19-21 Year Olds by Year of Access to the Pill

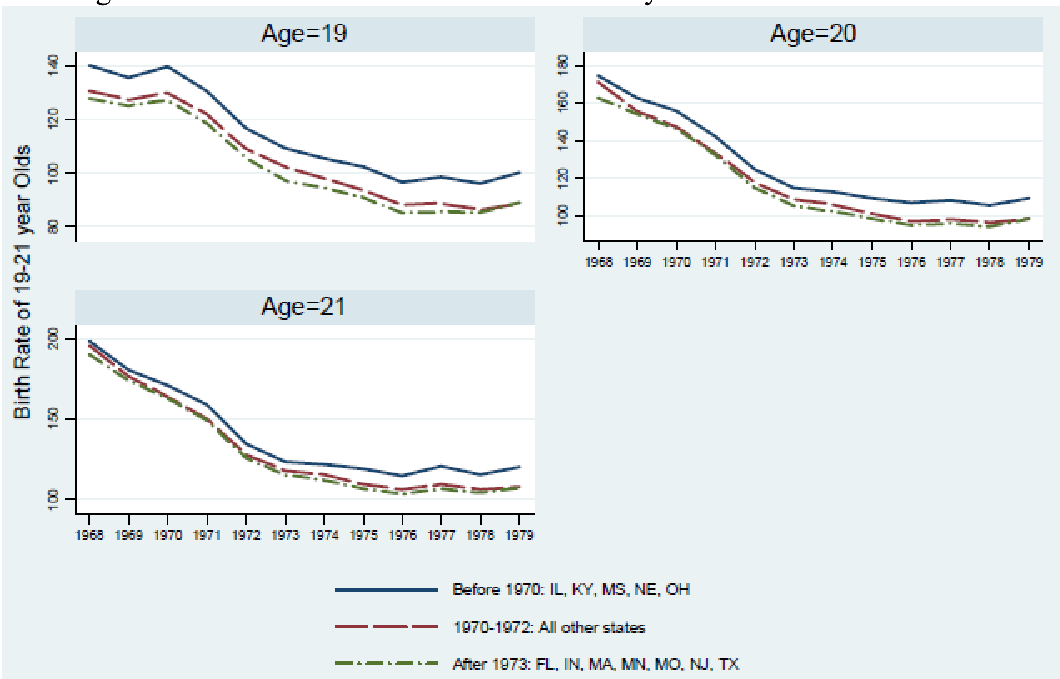
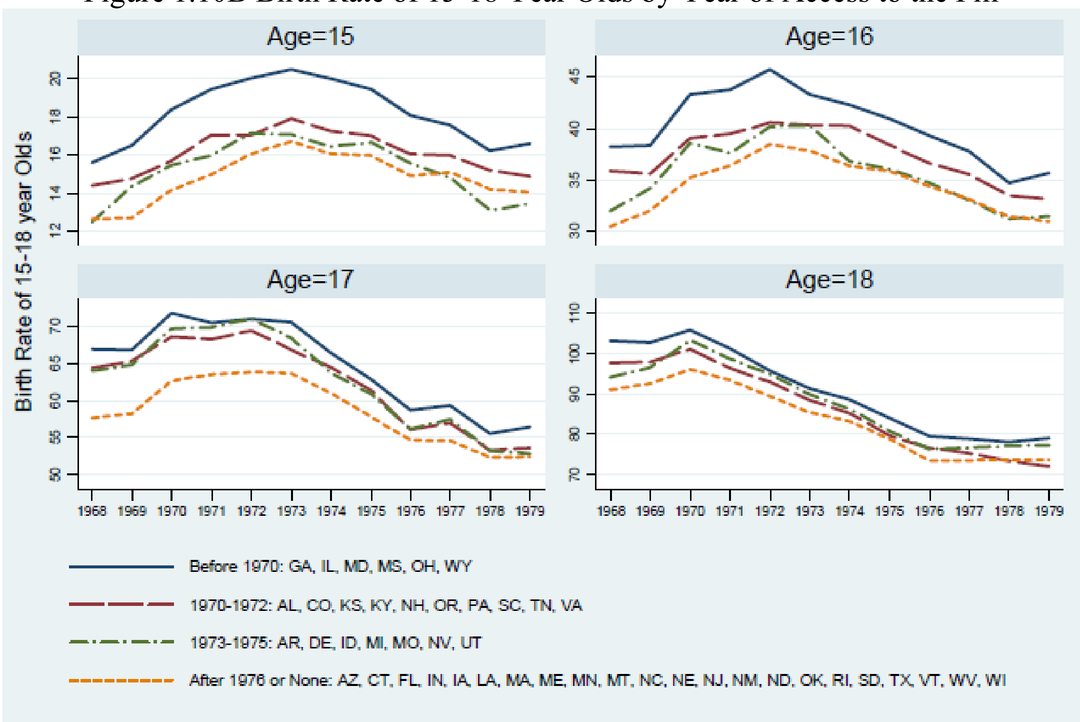


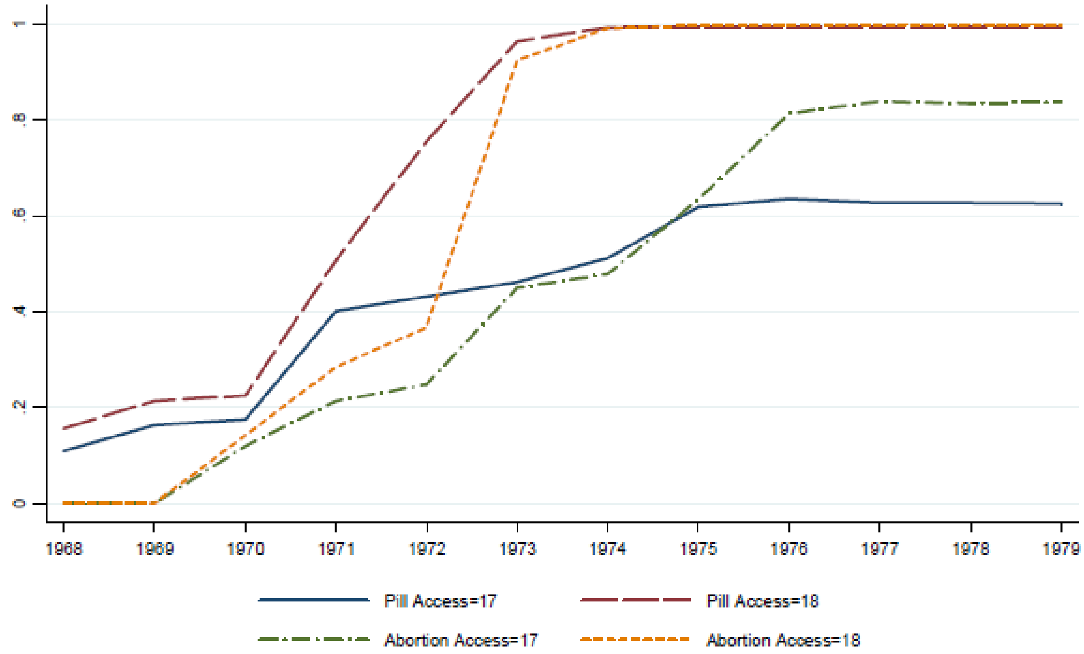
Figure 1.10B Birth Rate of 15-18 Year Olds by Year of Access to the Pill



*Early Legalizing States AK, CA, DC, HI, NY and WA not included

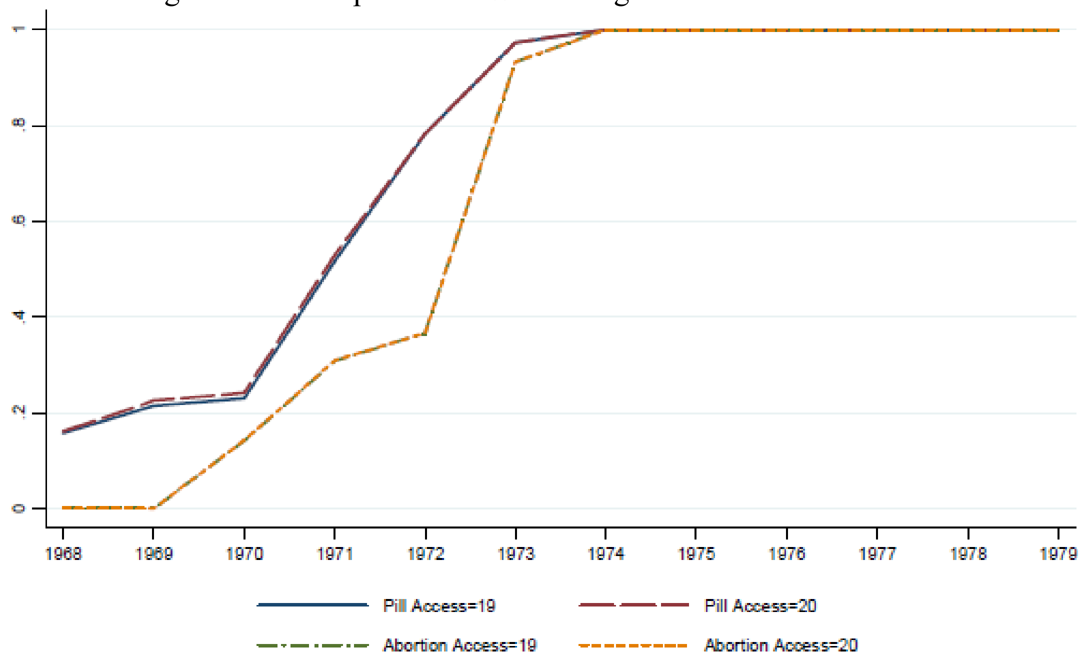
Figure 1.11: Proportion of Women Aged 17 and 18 with Liberal Access to Pill and Legalized Abortion by Age and Year*

Figure 1.11A Proportion of Women Aged 17 and 18 with Access



*Source: Guldi(2008). The Proportion is weighted by population

Figure 1.11B Proportion of Women Aged 19 and 20 with Access



*Source: Guldi(2008). The Proportion is weighted by population

Figure 1.12: Birth Rate of 15 Year Olds by Year of Access to the Pill without Parent Consent*

Figure 1.12A Birth Rate of 15 Year Olds by Year of Access to the Pill

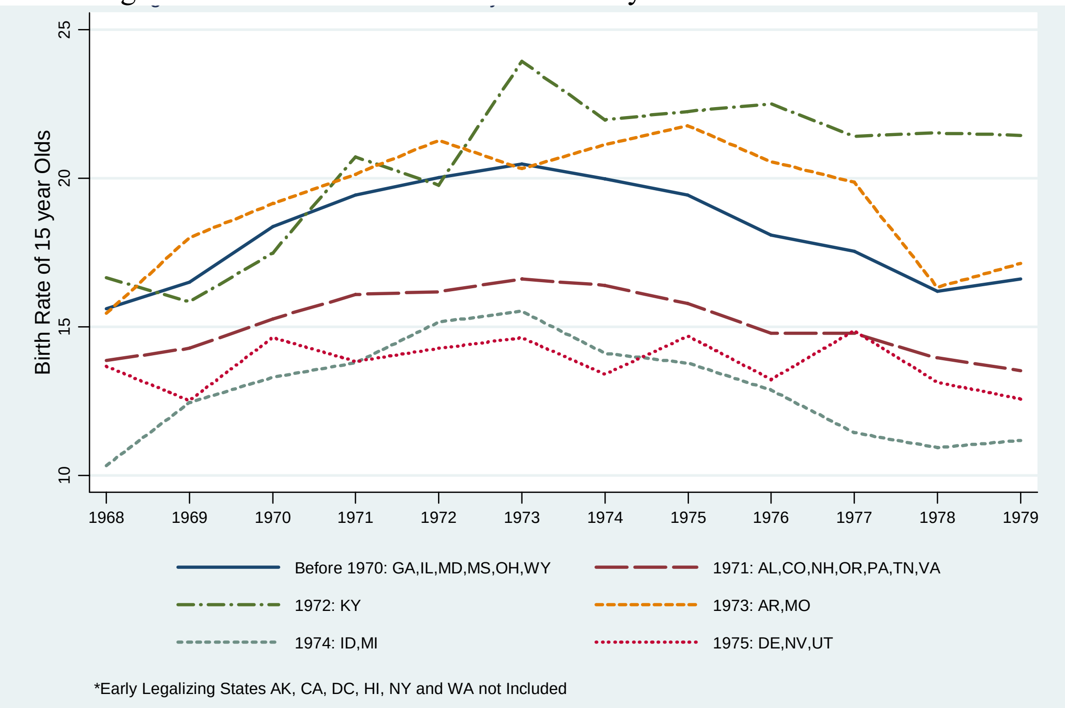


Figure 1.12B Birth Rate of 16 Year Olds by Year of Access to the Pill

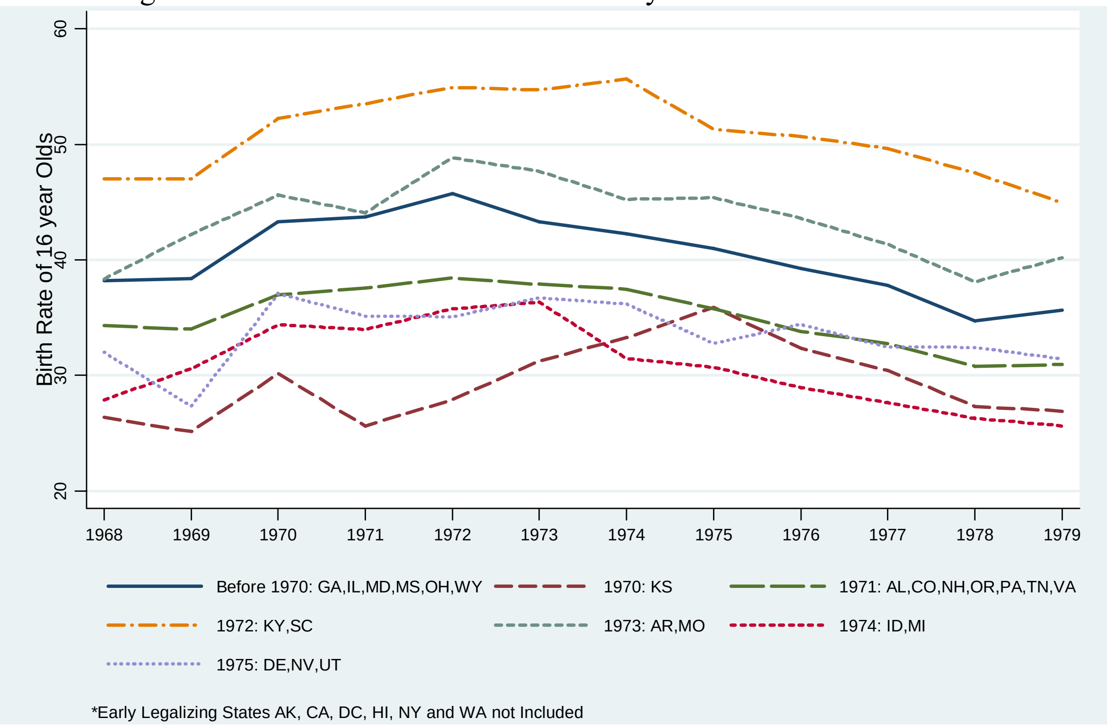


Figure 1.12C Birth Rate of 17 Year Olds by Year of Access to the Pill

Figure 1.12D Birth Rate of 18 Year Olds by Year of Access to the Pill

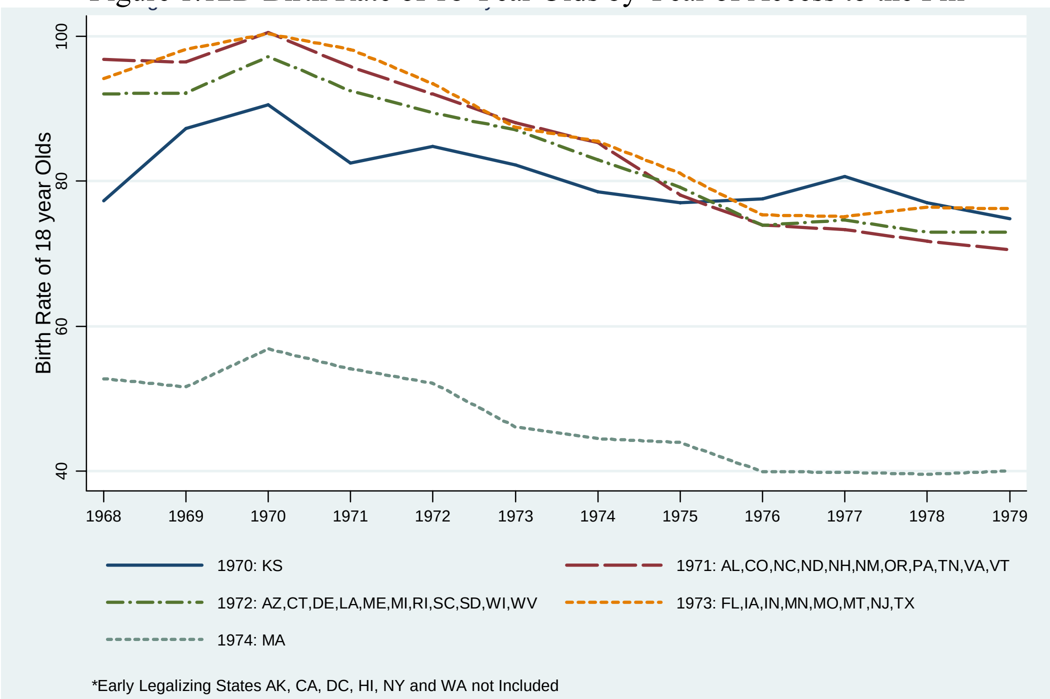


Figure 1.12E Birth Rate of 19 Year Olds by Year of Access to the Pill

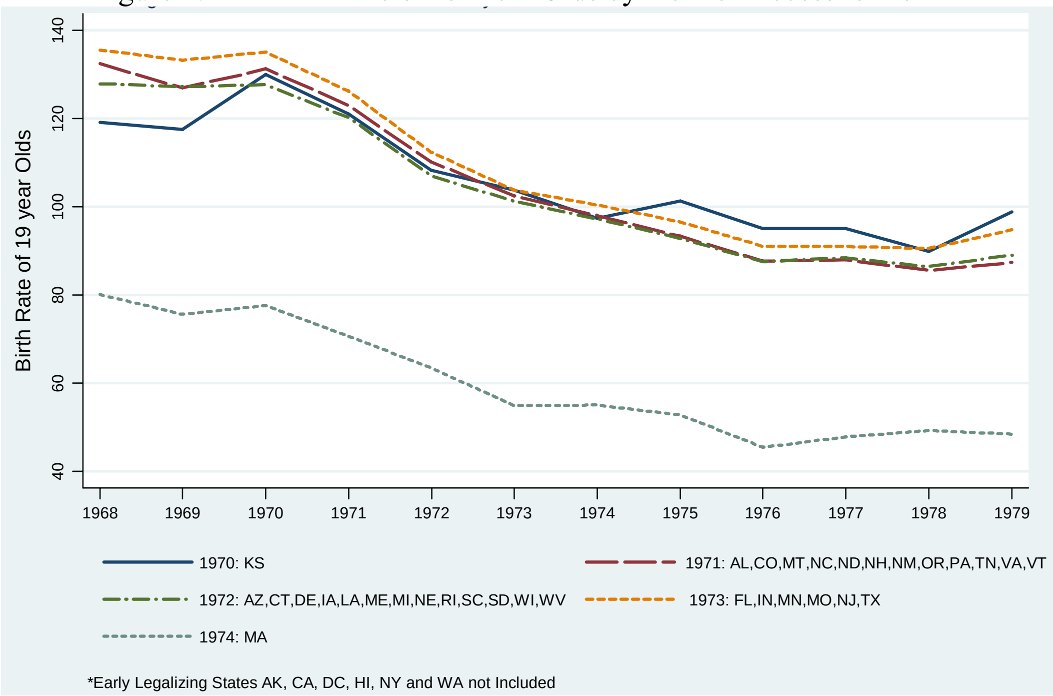
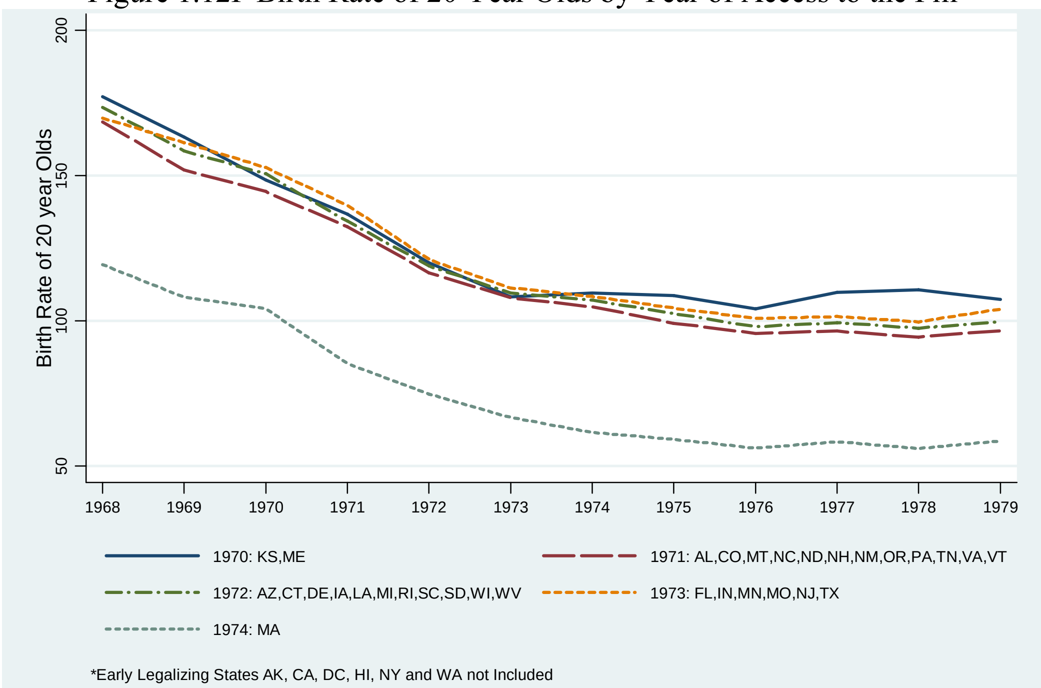


Figure 1.12F Birth Rate of 20 Year Olds by Year of Access to the Pill



Chapter 2

The Impact of Legalized Abortion on Early Childbearing and Health Outcomes in the Next Generation

2.1 Introduction

A few unexpected demographic turns of events appeared among U.S. teenagers and young women in the early 1990s. The teen birth rate and the rate of first births among teens decreased sharply, starting in 1992, after a rapid increase during the late 1980s; teen out-of-wedlock births began to fall in 1994 after four decades of consecutive increase; the teen abortion rate decreased by 21% from 1991 to 1995 (Centers for Disease Control (CDC), 2006). For young women in their early 20s, total birth rate, first birth rate and abortion rate also declined in early 1990s and stabilized at a lower level in the late 1990s; lastly, the increase in the unmarried birth rate slowed down. Two explanations are offered for this impressive set of changes. One focuses on contemporaneous changes regarding sexual behavior and childbearing decisions during the 1990s (Santelli et al., 2004; Santelli et al., 2009; Abma and Sonenstein, 2001). The other emphasizes intergenerational impact and cohort effects based on the fact that the drop in the birth rate starts with cohorts born in the early 1970s, a period of revolutionary change in family planning in legalized abortion (Donohue, Grogger and Levitt, 2009). In this paper, I follow the latter path and ask whether abortion circumstances faced by women of childbearing age affect the subsequent fertility behavior of their daughters when they reach the ages of 15 through 24.

Abortion was legalized nationally in 1973 with the Supreme Court decision in *Roe v. Wade*. The decision enabled women to control their childbearing with greater certainty and lower cost. Consider the fertility decisions made by U.S. women in the period from 1960s through 1980s. With the choice of abortion, women might have terminated unintended pregnancy, had fewer children in unfavorable circumstance, and invested more in each child to whom she did give birth or in her own human capital than she did otherwise. Girls born in this regime may have an improved childhood circumstance and be less likely to give birth when they become teenagers and young adults than those born in the regime in which abortion is illegal. This suggests that abortion legalization and availability may lower the overall risk of early childbearing faced by certain cohorts. However, if abortion is fully interacted with sexual activity and contraceptive use as a substitute of other birth control methods, it may have no impact on the number and experience of children who are actually born. It is important to understand which one is the case and to what extent legalized abortion is associated with the drop of birth rate among youth in 1990s and 2000s.

I explore this hypothesis by comparing the rate of teen childbearing among cohort born before and after the legalization of abortion. More specifically, I link the availability of legalized abortion services to when women were *in utero* with their subsequent fertility decisions as teens 15-19 or young adults ages 20-24 in the period from 1982 through 2002. The key variable of interest is the historical or *in utero* abortion exposure, measured by the number of abortions per thousand births in the state and year in which a woman was born. Along the same lines, the dependent variable pertains to births per thousand women by state of mother's birth. The question can be addressed as, for example, whether the childbearing behavior of 18 year old women in 1990 born in New York in 1972, regardless of where they resided in 1990, is correlated with the resident abortion ratio in New York in 1972.

The issue that I investigate in this paper is the same one in Donohue, Grogger, and Levitt (2009; hereafter, DGL). My study differs from theirs in three major aspects.

First, their outcomes are birth rates by mother's state of residence rather than mother's state of birth. Therefore, they have to estimate the historical abortion ratio as a weighted average of the abortion ratios faced by all women, for example, who were 18 in 1990 and resided in New York in that year. The weights are the fractions of New York residents born in each state of the U.S. DGL adjust for migration based on the Census of population data in 1980, 1990 and 2000 on cross-state mobility by age. This adjustment largely diminishes the variation and accuracy of abortion data and introduces significant measurement error into their key independent variable. In this paper, birth rates are calculated using birth data and population data aggregated by women's state of birth.

Second, DGL assume that the historical abortion ratio is zero for each state in which abortion was illegal prior to the Supreme Court decision in *Roe v. Wade* in 1973. While they account for early legalization by six states prior to 1973, they ignore abortions obtained by residents of neighboring states in the early legalizing states (Joyce et al., 2010). I will correct this bias using extended data of abortion from original CDC Abortion Surveillances in the early 1970s.

Third, I obtain separate estimates for the cohorts born between 1968 and 1979 and for the cohorts born between 1980 and 1987 as well as estimates for the entire period. On the other hand, DGL focus on results for the entire period. DGL include daughters of women born as early as 1958 and as late as 1987 in one sample when they examine the fertility behavior of women in 1982-2002. Abortion was illegal in all states of the U.S. prior to 1967. Inclusion of the period from 1958 through 1967 may rise to misleading results if state-specific trends in birth rates in this period differ from those after 1967. The early period of abortion legalization is characterized by dramatic changes in the legal status and utilization of abortion. It includes the period from 1970 through 1972 during which only a number of states reformed their abortion laws and the years of rapid increase in abortion in 1973-1979 in all states. In the 1980s, abortion utilization became stable nationally. Therefore, it is crucial to allow the effect of abortion to be different in different sample periods.

The estimates in this paper show that abortion exposure *in utero* does have a significant impact on early childbearing behavior, but the effect is in opposite direction in the legalizing period and the post legalizing period. In the legalizing period, rapid increase of abortions between 1970 and 1979, as a response to the supply shock, reduces birth rate in the next generation by 5-10% among women aged 15-19 who were born in 1968-1979 and 3-8% among women aged 20-24 who were born in the same period. But it has nearly no impact on women's marital status when giving birth. In the post legalizing period, 1980-1987, historical abortion ratio is positively associated with teen birth rate 15-19 years later. It implies abortion legalization initializes the drop of early childbearing in 1990s, but it does not count for the continuous decline after 1994. It also suggests that adjusting assumptions about demand side of abortion market for different periods is important for studying the impact of abortion.

In the following sections, I first explore the potential causal link between abortion legalization and early childbearing behavior in theory. Next, I analyze the empirical reasoning and concerns for modeling the impact of abortion exposure. I then provide descriptive and formal econometric evidence showing the relationship between abortion exposure *in utero* and various birth rates among teen and young women. Results from alternative specifications and further tests are presented in the end.

2.2. Abortion Exposure in utero and Early Childbearing

2.2.1. The Legalization of Abortion

Abortion legalization in the U.S. started in the late 1960s. From 1967 to 1969, nine states reformed their abortion statues as proposed by the American Law Institute.[29] Abortion was considered legal if performed by licensed physician under certain conditions, such as rape, incest, grave defect of the fetus and the case that women's physical or mental health would be severely impaired. The number of abortions was relatively small[30] and it is not associated with any substantive impact on birth rates (Levine et al. 1999). Illegal abortion did exist, but there was little data on it. I assume that there was no legal access to abortions on demand in general before and in 1969. From 1970 to1972, abortion became *de facto* legal on demand in California and the District of Columbia and *de jure* legal in Alaska, Hawaii, New York and Washington. In other states, although abortion was illegal, women could go to the five early legalizing states (Washington had resident restriction) to have abortion (CDC, 1972; CDC 1974). In 1970, there were over 180,000 legal abortions reported to the CDC, an order of magnitude much more than in the previous year. By 1972 the total number of abortions had risen to 780,000 (CDC, 1975). Starting January of 1973, abortion became legal nationally with the historical ruling of the United States Supreme Court in *Roe v Wade*. The number of

[29] Arkansas, California, Colorado, Delaware, Georgia, Maryland, New Mexico, North Carolina and Oregon (CDC, 1975).

[30] In 1969, about 22,000 legal abortions were reported to the CDC (1971).

abortions continuously increased every year, peaked around 1979 and stabilized in the 1980s. While the cohorts born in the early 1970s, affected by these changes of laws, composed the majority of the teenagers in 1991-1995 and the population in their early 20s in 1996-2002.

What has not been appreciated in the literature is that the legalization of abortion around 1970 dramatically increased the availability of legal abortion services not just for resident of the six early legalized states but for all women across the country.[31] New York was clearly the most important destination for non-residents of the state. Nationally, 79% of 314,929 of abortions to non-residents in 1971-1972 were performed in New York, 33,272 (8.4%) were performed in California and 27,500 (6.9%) in Washington, DC (CDC, 1972; CDC, 1974). Abortion rate among residents of states other than the six early legalized states increased to 11.00 in 1973 from 6.88 in 1972 (CDC, 1975), instead of jumping up from zero as most previous studies assumed. As a result, fertility rates also fell in 1970-1973 in states that made no changes to their abortion laws (Levine et al. 1999). Cross state variation was large in terms of taking advantage of the availability of legal abortion service before the *Roe v Wade* and depended on the cost of traveling to NY, CA and DC[32], and therefore, should be taken account.

2.2.2. From Legalized Abortion to Childbearing of Offspring

The immediate short-term effect of abortion legalization is to reduce size of the cohorts born in the 1970s, though the net magnitude of the effect is still debatable (Joyce

[31] The importance of access to legalized abortion in the early 1970s on the birth rates of teens is not a new finding (Sklar and Berkov 1974; Joyce and Mocan 1990; Levine et al. 1999; Angrist and Evans 1999).
[32] Levine et al. 1999 showed that the closer a woman lived to a state that legalized abortion before Roe, the greater the decline in the birth rate of that state between 1971 and 1973.

and Mocan, 1990; Levine et al., 1999; Angrist and Evans, 1999). The potential causal link between abortion legalization and subsequent decline in early childbearing of offspring relies on the possibility that abortion disproportionately reduces the number of children at high risk of giving births at early age when they grow up (a composition effect; Donohue and Levitt, 2001).

Numerous studies show that early childbearing behavior, especially teenage childbearing, associates directly with unfavorable family background and childhood circumstances or indirectly through women's own socioeconomic disadvantage which closely relates to family factors. First, women who live in poverty, live in a household receiving welfare, grow up in single parent family or experience parental marital separation are more likely to have teen pregnancy and teen births (Alan Guttmacher Institute, 1994; Gruber et al., 1991; Kahn and Anderson, 1992). Second, teen childbearing is correlated across generations—daughters having teen mothers or receiving low maternal education are at high risk of engaging in early childbearing (Newcomer and Udry, 1984; Kahn and Anderson, 1992; An et al., 1993). Third, teenage childbearing is associated with low educational expectations, poor school performance and risk taking behavior which correlate with early life circumstance (Alan Guttmacher Institute, 1994; Horwitz et al, 1991).

The availability of abortion may improve average level of birth outcome and childhood living condition by reducing the number of children that would be born under unfavorable circumstance. This composition change can be partly reflected by the ratio of number of abortions to births. A number of studies show that abortion contributes in this way at least during the years immediately after legalization. Hayes (1987) reported that teenagers counted for 30% of women who received abortions in the years after legalization. Levine et al. (1999) found that teen childbearing fell by 12% in the early 1970s with the introduction of legalized abortion, compared to a decline of only 5% for non-teens. This decrease in teen fertility could have an echo-effect a generation later. Meanwhile women who were unmarried, in poverty or nonwhite were heavily overrepresented in population receiving abortion service than in the general population (Alan Guttmacher Institute, 1994). The proportion of children born in a single parent family, in poverty or receiving welfare declined significantly after abortion legalization (Gruber et al., 1999).

Another channel through which abortion legalization may contribute to the average level of childhood circumstance for cohorts born in the 1970s is by increasing women's education and labor market consequences (Angrist and Evans, 1999; Ananat et al. 2007). Assume that most women use abortion to postpone childbearing for a few years instead of permanently avoiding abortion. When these women do give births, their children will have relative better living conditions and lower likelihood of engaging in teenage childbearing. This postponing effect cannot be captured by the historical abortion ratio when these women received abortion service. But during a period when abortion continuous increase annually, it can be partly reflected by the historical abortion ratio in the year they give births.

2.2.3. Why Re-Visit the Decline in Early Childbearing in 1990s

First of all, there are several other hypotheses offered regarding this remarkable decline in birth rates, but the discussion is far from complete. Levine (2001) summarized that greater use of long-term contraceptives and condoms led by the AIDS epidemic, decreased welfare generosity and a strong economy associated with increased job opportunities raised the "costs" of becoming pregnant and giving birth at early age. However, increased use of condoms and long-term contraceptives may serve more as a substitute for oral contraceptive pill instead of an overall increased use of contraceptive methods (Abma and Sonenstein, 2001; Piccinio and Mosher, 1998). Explanations based on welfare reform and business cycles often lack empirical evidence. Levine (2001) suggested that these increased costs combined could only explain less than half of the decline in teen fertility in the 1990s. Sexual activity among teenagers decreased by 8% in the 1990s (Brener et al., 2002), but it can hardly account for the 30% decline in birth rate. Besides, this decline in sexual activity itself needs to be explained.

This paper is motivated by DGL (2009), which proposed a causal relation between abortion legalization in the 1970s and the drop of out-of-wedlock teen birth rate in the 1990s. Following the abortion and crime literature, they considered teen out-of-wedlock childbearing as a female counterpart of crime for young male, though this argument is still under debate (Donohue and Levitt, 2001, 2004, 2008; Joyce, 2004, 2009; Foote and Goetz, 2008). They found significant and negative effects for out-of-wedlock teen birth rate, but insignificant and inconsistent results for total teen birth rate. It is puzzling because total birth rate declines more than unmarried birth rate during the 1990s and the percentage of out-of-wedlock births in total births to teenagers and young women surely increases in the whole 1990s. Based on their results, it is ambiguous whether teenagers and young adults in the 1990s are less likely to give birth in general or more likely to get married before giving birth. It is also strange that their results become weaker when they restricting sample period to cohorts 1971-1975, when legalization was in process.

A closer examination on DGL's empirical work raises the following concerns: First, they used birth rate and historical abortion ratio by state of residence as their dependent and independent variables respectively. Since inter-state mobility is high in the U.S., this empirical setting conflicts with the essential hypothesis that birth year and place can change characteristics of a cohort and therefore change childbearing behavior of women in this cohort in the future. To estimate historical abortion ratio by women's current state of residence, DGL took weighted average of original historical abortion ratio across all states. The problem is the weighted averaged historical abortion ratio cannot accurately reflect a woman's exposure to legalized abortion as defined by their year and state of birth. Meanwhile, it creates measurement error in key independent variable, which is often considered worse econometrically than having random measurement error in dependent variable.

Second, DGL (2009) made the assumption that there is no abortion before 1973 in states where abortion is illegal. Though this is a common assumption in research regarding abortion legalization, ignoring availability of abortion service in early legalizing states before 1973 for other states may be highly relevant in a study utilizing abortion rates or ratios by cohort and birth rates by single year of age.

Third, DGL (2009) estimated their results using an entire sample of 15-19 and 20-24 years old women in 1982-2002 which covers women born from 1958 to 1987. It may be problematic for the following reasons. First, the results might be upward biased by including many cohorts before abortion legalization as early as 1958 and assigning them zero abortion exposure *in utero*, because the early cohorts have higher birth rate. Second, abortion exposure *in utero* is considered as measurement of unwantedness in DGL (2009), but assuming unwantedness to be zero with no variation for any cohorts before 1970 does lack theoretical and empirical evidence. Though it is ambiguous how this assumption will affect the estimates, having nearly 30% of observations with no variation in independent variable may magnify the potential bias. Third and most importantly, DGL (2009) ignore the possibility that abortion exposure *in utero* may affect early childbearing behavior in different ways for women born in different sample periods. With the dramatic development of abortion market and other contraceptive methods, it is necessary to allow different interaction between abortion utilization and other aspects of childbearing decisions.

Another commonly raised concern is whether the causal relation between abortion exposure and early childbearing can be estimated using a straightforward non-parametric model, such as difference-in-difference-in-difference (DDD) model. Ozbeklik (2007) used DDD model to estimate the effect of the legalizations on the birth rate without relying on abortion data. He reported similar results on out-of-wedlock birth rate as DGL (2009) and suggested that the impact for black appeared earlier than that for whites. Moreover, he also used the birth rate by mother's state of residency which does not match the woman's abortion environment when she was born. For another thing, a DDD design relies on the assumption that abortion is strictly illegal in states other than the six early legalized states, which is not true. Moreover, a DDD design requires accurate information on women's year of birth and the time when they were conceived, which is not available in most birth data and population data. When the unit of study is by single year of age, the period when women could be exposed to abortion *in utero* are three years. Since the DDD design relies on variation of abortion policy in exactly three years, 1970-1972, bias due to this error in birth year can be large.

2.3. Empirical Framework for Understanding the Effect of Abortion Circumstance

2.3.1. Empirical Design and Data Source

Early childbearing feature of a cohort should be described as a combination of total fertility, age at first birth and marital status when women give births. In this paper, I used total birth rate, first birth rate, unmarried birth rate and married birth rate as dependent variables. Birth rate is defined as the number of births in certain category per 1,000 women as in equation (1) where *s* stands for state of birth, *y* stands for year and *a* stands for age. For example, unmarried birth rate is the number of births to women who are unmarried when giving birth per 1,000 women, regardless of whether they were unmarried or married.

(1) $$Birthrate_{s,y,a} = No.ofBirths_{s,y,a} / (FemalePop_{s,y,a} / 1000)$$

To estimate the abortion circumstance faced by a cohort of women right before they were born, I employed the concept of "abortion exposure *in utero*" brought up in the abortion and crime literature (Donohue and Levitt, 2001). There are two commonly concepts used to measure the level of abortion utilization in year *y-a* and state *s*—abortion ratio and abortion rate, which are defined separately as:

(2) $$HistoricalAbortionRatio_{s,y-a} = No.ofAbortions_{s,y-a} / (No.ofLiveBirths_{s,y-a} / 1000)$$

(3) $$HistoricalAbortionRate_{s,y-a} = No.ofAbortions_{s,y-a} / (FemalePop15-44_{s,y-a} / 1000)$$

Donohue and Levitt (2001) used abortion ratio, number of abortions per 1,000 live births in the state and year that a woman was conceived, as their measure of "abortion exposure *in utero*." It directly describes the change of composition of a cohort caused by abortion, since the live births in year *y-a* is the whole population of cohort *y-a* and the number of abortion represents reduced number of children at high risk of early childbearing. Since birth rate can be considered as the ratio of the number of women who gave birth and the total number of women, it is reasonable to use the abortion ratio to predict birth rate 15-24 years later. However, Joyce (2004) pointed out that abortion ratio was more endogenous than abortion rate as a measurement of abortion exposure. Since the decision of having abortion or carrying to term is made simultaneously, the number of abortion surly has a direct impact on the number of live births in the following year. Abortion rate, defined as the number of abortion per 1,000 women at fertility age (15-44), is a less endogenous measurement of abortion circumstance, though it does not directly represent the change of cohort composition.

To take account of the debate described above, I first use abortion ratio when a woman was conceived (referred as historical abortion ratio) in this paper. Then I used abortion rate when a woman was conceived (referred as historical abortion rate) as independent variable for alternative analysis.

The empirical specification is the following:

(4) $$Birthrate_{s,y,a} = \alpha \ H\_Abratio_{s,y-a,a} + \delta_{s,a} + \lambda_{y,a} + \tau_{s,y} + e_{s,y,a}$$

where *s* is women's state of birth, *y* is the year when the women give birth and *a* is women's age by single year. $Birthrate_{s,y,a}$ presents the annual number of births per 1000 women at age *a* in year *y* who were born in state *s*. $H\_Abratio_{s,y-a,a}$ is the historical abortion ratio defined as the abortion ratio in year *y-a* and state *s*, when and where the woman was herself *in utero*. $\delta_{s,a}$, $\lambda_{y,a}$ and $\tau_{s,y}$ represent state-age, year-age and state-year fixed effect respectively.

Since birth rate and historical abortion ratio vary by year, state and single year of age, I am able to include all three sets of interactions between year, state and age as control variables. I rely exclusively on fixed effects to control for omitted variables that may affect fertility among youth for the following reasons. First, most social-economic, legal and policy factors only vary at state level by year or the data of these factors are only available at state-year level, therefore the effect of them will be fully absorbed by year-state fixed effect. For example, current abortion rate and sexual activity among young women can surely vary across ages and affect birth rate, but data are only available by state and year. Second, the state-age and year-age fixed effect will control for age trend within state and age specific year trend. Third, the very few potential variables (e.g., parental notification laws), which are relevant to current fertility and vary by age within state and year, are only available by state of residency. They cannot match the dependent and independent variable in my empirical specification.

Aggregated number of births and female population are needed for calculating birth rate by year, women's state of birth and single year of age. Data on birth come from the National Vital Statistics Natality Detail Files collected by the National Center for Health Statistics. These micro-level data provide demographic information, including mothers' state of birth, age and marital status, drawn from birth certificates for all births in each year in the U.S. In some early years, mother's marital status is imputed for a few states. I present the results from the sample including all states and years, since the results are not sensitive to this condition. But I exclude these state-year observations when comparing relevant results with DGL's as they did in DGL (2009). Throughout this study, only women born in the U.S. and births to women born in the U.S. are counted.

Two independent sources of population data are used in this paper. One is from the U.S. Census. The other source of population data is the historical Natality Detail Files. The Vital Statistics Natality Detail Files provides an accurate estimate of cohort size by state of birth, because female population at age *a* in year *y* who were born in state *s* is the number of births in year *y-a* and state *s*[33], assuming cohort survival rate to be 100% and migration from the U.S. to other countries (emigration rate) to be zero[34]. The natality population tends to slightly overestimate the real population and therefore underestimate real birth rate. But it serves as a source of sensitivity analysis complementing estimates from the CENSUS population data and a sensitivity analysis, since the measurement errors in these two population data are from different sources and in different nature. The Natality Files started 1968, so as to population estimation created based on natality data. I also used population data from the Surveillance, Epidemiology, and End Results (SEER) Program of the National Cancer Institute to do robust tests (See section 2.5).

In the remainder of this paper, I will refer the birth rate calculated using population data from CENSUS as CENSUS birth rate and the corresponding results as CENSUS results. And I will refer the birth rate calculated using population data from

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[33] More accurately, since the Natality Files are by calendar year, women who report to be *a* years old in year *y* could be born in year *y-a* or *y-a-1*, the population estimate from the Natality Files will be an average of the number of births in year *y-a* and *y-a-1*.

$Vpop_{s,y,a} = 0.5 * No.ofFemaleBirths_{s,y-a-1,a} + 0.5\ No.ofFemaleBirths_{s,y-a,a}$

[34] The cohort survival rate until age 24 for female born in 1970 is about 99.4%. It means among all births in 1970, 99.4% of them are still alive in 1994 (Arias, 2007). Emigration rate in the U.S. in 2000-2002 is 0.8 per 1,000 populatoin as calculated by the United Nations Development Programme (UNDP). It ranks 173 out of the 182 countries in the world included in the Human Development Report 2009 from UNDP.

historical Natality Files as Natality birth rate and the corresponding results as Natality results.

Abortion data are obtained from two sources: (1) the Alan Guttmacher Institute's (AGI) estimates of the number of abortion by year and women's state of residence in 1973-1987; (2) the Centers for Disease Control and Prevention data on abortion by year and women's state of residence in 1970-1972. Since the birth data are aggregated by calendar year and women's precise year of birth is not available, a woman who give birth in year *y* and was recorded as age *a* might be born in year *y-a* or *y-a-1*. Abortion is often taken within 6 months of pregnancy. A woman born in year *y-a* or *y-a-1* would expose to abortion in the later half of year *y-a-2* through the former half of *y-a*. Therefore, I measure the level of abortion exposure *in utero* as a weighted average of abortion ratios in *y-a*, *y-a-1* and *y-a-2*.

To illustrate the major change in the dependent and independent variables of interest in time-series, I plot the trend of birth rate for teenagers and women in early 20s by year and by state groups with different timing of abortion legalization in Figure 1A and Figure 1B. Though birth rates are aggregated by two five years age groups, Figure 1A and Figure 1B show that women born in early legalized states between 1970 and 1973 tend to experience moderate increase in birth rates when they become 15-24 years old. In Figure 2, I show the average abortion exposure *in utero* by year for the 15-19 age group and the 20-24 age group. The significant increases of abortion exposure represent the consequence of abortion legalization.

The health outcome variables are also from the National Vital Statistics Natality Detail Files.

2.3.2. Prediction

The direct impact of abortion on birth rate in next generation mainly depends on how abortion circumstance affects the number of births. Assume that "marginal children[35]" are always more likely to give birth as teenagers.

Begin with the identity

(6) $B \equiv P - A,$

where B stands for births, P stands for pregnancies, and A stands for abortions. Specify a behavioral relationship that relates P to A and a vector of variables X:

(7) $P = \alpha + \beta A + \gamma X$

Substitute equation (7) into equation (6) to get

(8) $B = \alpha + (\beta - 1)A + \gamma X.$

Presumably, β is positive. An increase in A due to a reduction in its money price or an increase in its availability leads to a reduction in the use of conventional methods of

35 By "marginal children," I mean the children who would have been born but were not due to abortion, or the children who would not have been born but were actually carried to term because of the change of abortion circumstance.

birth control. This effect can be referred to as moral hazard. Joyce et al. (2010) fit equation (8) and get an estimate of (β-1) that is approximately equal to -1 for whites which means β=0 for the early period of abortion legalization. It implies that abortion had small impact on pregnancy (P) in early 1970s. Then increase of abortion leads to decrease of births. The aborted children are considered to have at higher risk of early childbearing (not necessary, but more likely to be unwanted or from poor family). In this sense, large abortion ratio will reduce the share of children at high risk of early childbearing, which implies the coefficient α in equation (4) would be negative.

In post-legalization period, β may have gotten larger so that variations on A had smaller effects on B for the mothers of the teenagers and young women who may give birth in 1990s and 2000s. Therefore, the historical abortion ratio should have had an even smaller effect on the fertility of teenagers and young women in the sample period I focus on. If β=1, abortion has no impact on the number of births in a cohort, and therefore should have no impact on childbearing behavior of women in this cohort when they grow up. If β>1, the moral hazard is so strong that one unit change of abortion responds to more than one unit change of pregnancy and some adverse and unintended pregnancies are carried to term. This can be interpreted as a selection effect. The referred population may have strong moral hazard, underestimate the real cost of abortion heavily and engage in risky behavior easily. They are more likely to be those with disadvantaged social-economic status, and their children will be at higher risk of early childbearing. In these two cases, the coefficient α in equation (4) will be either zero or positive.

Therefore, it is a rationale to allow the effect of legalized abortion to be different in different period depending on whether the supply shock due to sudden legalization has been absorbed so that demand for abortion has adjusted to equilibrium. Abortion rate and ratio rapidly increased in the 1970s and leveled off in the 1980s, which naturally forms two distinguish periods of the development of abortion utilization. Hence, I will do the regressions for cohorts 1968-1979 and cohorts 1980+ separately.

The combined data set covers 15-24 years old women in 1983-2002 in all states, who were born in 1968-1987. Unit of observations is by year, women's state of birth and single year of age. The summary of statistics are presented in Table 1 for the whole sample, age group 15-19 (cohorts 1968-1987) and age 20-24 (cohorts 1968-1982). For women aged 15-19, mean of total birth rate is 47.06; for women aged 20-24, the total birth rate is 104.41. Abortion ratio increased from 0 in 1968 to 356.02 in 1979. The average level of abortion exposure in utero is 244.53 for teenagers and 203.07 for young women. Eliminating cohorts before 1968 makes the mean of birth rate smaller and the mean of historical abortion ratio larger than those in DGL's paper.

2.4. Empirical Evidence and Results

2.4.1. Descriptive Analysis of Cohort Effect

It is a major challenge to present the impact of abortion exposure *in utero* on birth rate among teenagers and young women, which is a cohort effect, using simple time-series plots of age-specific nationally averaged birth rate. For one thing, there is an evidently profound period and age effect on birth rate in the 1980s and 1990s. Birth rate tremendously increased in the late 1980s and decreased in the early 1990s among young females at any ages (See Figure 3 for 15-19 years olds). The timing and pattern of decrease is different for teenagers and young women. In this sense, it is difficult to show the potential cohort effect visually without removing year and age specific period effect. For another thing, because of the absence of accurate data on year of birth, the abortion exposure *in utero* of women in sequential single age groups overlaps with each other to a large extent. For example, women aged *a* in year *y* may expose to abortion in year *y-a*, *y-a-1* and *y-a-2*; women aged *a-1* in year *y* may expose to abortion in year *y-a-1*, *y-a-2* and *y-a-3*. The two years overlap makes it hard to distinguish a cohort effect by single year of age.

However, it is possible to detect cohort effect using annual percentage change of birth rate for teenagers by age, as showed in Figure 4. Though total birth rates for each single year of age peak in 1991 according to the time series plot in Figure 4, the grow rate of birth rate peaks in different years for girls at different ages (Figure 5). As marked in Figure 5, the 1971 cohort nearly always has the highest growth rate in ten years between 1985 and 1995, except for 16 years olds. Even for the 16 years olds, cohort 1971 has the second highest annual percentage change, while the peak in year 1989 reflects the strong period effect shared by all age groups.

2.4.2. Empirical Results for Early Childbearing

I first use the same sample period as that in DGL (2009), which covers all girls and women aged 15-24 in 1982-2002 to construct a comparison. The results are in Table 2.1 and Table 2.2. In Table 2.1, Coefficients in Panel A are from DGL's paper. Panel B represents the results when I use birth rate by mother's birth state as dependent variables and the historical abortion ratio (nearly zero for illegal states before 1973) as in DGL (2009). The major conclusive point of DGL's results is that they find negative effect of abortion exposure *in utero* on unmarried birth rate, especially for 15-19 years old.

However, the coefficients on overall birth rate are positive and insignificant. When we use birth rate by mother's birth state as dependent variable, the results on overall birth rate are still positive, and the results on unmarried birth rate for 15-19 years olds become insignificant. The way birth rate constructed is not the reason that DGL have positive sigh for overall birthrate, though the results of unmarried birth rate for 15-19 years old is sensitive to the change of dependent variable.

Then I run the same set of regression for the same sample period, but use birth rate by mother's state of birth and updated data of historical abortion ratio (Table 2.2) Panel A represents the results if I use CENSUS population data to calculate birth rates. The major difference is that DGL's results on overall birth rate are positive and insignificant, but the CENSUS ones are negative and significant. DGL find a negative effect of abortion exposure *in utero* on unmarried birth rate (their conclusive point) and a positive effect on married birth rate. The CENSUS results on unmarried birth rate are negative, insignificant for the 15-19 age groups and significant for the 20-24 age groups. The effect on married birth from the CENSUS sample for 15-19 is opposite to that for 20-24. DGL's sample and the CENSUS sample both include 15-19 years olds born since 1963 and 20-24 years olds born since 1958. Outcomes in Table 2.2 suggest that adding abortion data for years 1970-1972 affect the estimates.

The main results for teenagers aged 15-19 and young women aged 20-24 born between 1968 and 1979 are presented in Table 2.4. All regressions are weighted by the number of women. In panel A, the dependent variable, birth rate, is calculated using population data from CENSUS; in panel B, birth rate is calculated using population data from historical Natality Detail Files. Each column represents a separate regression with different birth rate measurement for 15-19 age group and 20-24 age group respectively. The results reveal that an increase in historical birth ratio is significantly associated with a decrease in birth rate among teenagers and young women in 15-24 years later. For teenagers, if historical abortion ratio increases from 0 to the mean level of the whole sample 244.53 (See Table 2.3 for summary of statistics), the CENSUS results imply a reduction of 2.91 births per 1000 teenage girls (a 6.1% fall from the mean of teen total birth rate). If we consider abortion ratio increase from 0 in 1968 to its peak level 356.02 in 1979, the Census results imply a reduction of 4.2 births per 1000 teen per year, which associates with a 8.7% fall relative to the mean of birth rate. Note the Census sample is composed by cohorts born before 1979.

The Natality results imply a reduction of 1.79 births (4% fall) if historical abortion ratio increases from 0 to 244.53. The Natality sample is composed by cohort in 1968-1979, since Natality data start from 1968. The share of women who started childbearing as teenagers (first birth rate) is also reduced by 4-5% measured at mean. Out-of-wedlock birth rate is negatively and significantly associated with historical birth rate similarly (fall by 4-6%). The magnitude of unmarried birth rate estimates is close to that of total birth rate, because the share of out-of-wedlock births was continuously increasing the whole 1980s and 1990s and reached approximately 80% in 2002.

For young women, the increase of abortion ratio in the legalization period also reduced birth rate in all dimensions. Increasing historical abortion ratio from 0 to 203.07 will lead to a 2.1 less births per 1000 young women, which implies 2% decrease in total birth rate among women aged 20-24 if Natality birth rate is used as dependent variable and a 2% decrease in first birth rate. Different from teenagers, CENSUS and Natality results are not as consistent with each other.

Abortion exposure *in utero* affects teenagers' birth rate after 1995 in an opposite manner. In the top rows of table 4, I report the results for teenage girls born in 1980-1987. The corresponding years will be 1995-2002. The coefficients on the historical abortion ratio are consistently positive and statistically significant for overall birth rate and first birth rate, no matter in the CENSUS panel or the Natality panel. The significance of results for birth rate by marital status is less stable, but the coefficients are still all positive. It implies that high abortion exposure in the 1980s decelerates the decline of teen birth after 1994. Teenagers born in a state and year with high abortion ratio in the 1980s are more likely to give births. However, the mean value of abortion ratio actually decreased from 382.12 abortions per 1,000 live births in 1980 to 360.90 in 1987, which means the positive effect of abortion exposure *in utero* would not show up as an increasing year trend of birth rate. For 20-24 age group, there is no significant result, since the data only covers three years from 1999 to 2002 which is too short to capture any change in birth rate.

2.4.3. Empirical Results for Health Outcomes

(9) $$LBRatio_{sya} = \alpha_0 + \alpha_1 H\_Abratio_{s,y-a,a} + \alpha_2 \delta_{sa} + \alpha_3 \lambda_{ya} + \alpha_4 \gamma_{sy} + u_{ijt}$$

(10) $$EarlyRatio_{sya} = \alpha_0 + \alpha_1 H\_Abratio_{s,y-a,a} + \alpha_2 \delta_{sa} + \alpha_3 \lambda_{ya} + \alpha_4 \gamma_{sy} + u_{ijt}$$

(11) $$RiskyMomRatio_{sya} = \alpha_0 + \alpha_1 H\_Abratio_{s,y-a,a} + \alpha_2 \delta_{sa} + \alpha_3 \lambda_{ya} + \alpha_4 \gamma_{sy} + u_{ijt}$$

(12) $$AbnormalRatio_{sya} = \alpha_0 + \alpha_1 H\_Abratio_{s,y-a,a} + \alpha_2 \delta_{sa} + \alpha_3 \lambda_{ya} + \alpha_4 \gamma_{sy} + u_{ijt}$$

(13) $$HardLaborRatio_{sya} = \alpha_0 + \alpha_1 H\_Abratio_{s,y-a,a} + \alpha_2 \delta_{sa} + \alpha_3 \lambda_{ya} + \alpha_4 \gamma_{sy} + u_{ijt}$$

The dependents variables on health outcomes of new births and new mothers are defined as the following. Low birthweight ratio (LBRatio) is the number of births under 2500gm per 1,000 live births. Pre-mature birth ratio (EarlyRatio) is the No. of births born under 37 weeks per 1,000 live births. Mother's risky behavior ratio (RiskyMomRatio) is the number of births born mothers who smoked or drank during pregnancy per 1,000 live births. Abnormal birth ratio (AbnormalRatio) is the number of abnormal births per 1,000 live births. Complicated labor process ratio (HardLaborRatio) is the number of births whose mother experienced complicated labor per 1,000 live births.

As shown in Table 2.8, for 15-19 years olds born before 1979, a 200 (mean of historical abortion ratio) unit increase of historical abortion ratio is associated with a reduction of 2.8(=-0.0138*200) pre-term births per 1000 live births per year, and a reduction of 3.2 (=-0.0166*200) cases of smoking and drinking during pregnancy per 1000 live births. These results imply a 1.5% fall of pre-mature ratio measured at mean level (Mean Pre Ratio=139) and a 1.3% fall of mother's risky behavior (Mean Risky Behavior Ratio=247)

For 20-24 years old new mothers born before 1979, a 200 (mean of historical abortion ratio) unit increase of historical abortion ratio is associated with a reduction of 2.7(=-0.0133*200) Low Birthweight births, a reduction of 2.8 (=-0.0142*200) pre-term births, and a reduction of 1.7 (=-0.0083*200) abnormal births. Mean of Low Birthweight ratio is 71, and the result implies a 4% fall from mean level. Mean Pre-mature Ratio is 102, the result associated with a 3% fall from mean; Mean Abnormal Births Ratio is 76, and is associated with 2% fall.

2.5. Further Tests

2.5.1 Alternative Specification with Abortion Rate

Since abortion rate is considered as a less endogenous measurement of abortion exposure, I use historical abortion rate, instead of abortion ratio as independent variable to test the sensitivity of results presented in last section. The empirical model is as the following:

(14) $$Birthrate_{sya} = \alpha\ H\_Abrate_{s,y-a,a} + \delta_{sa} + \lambda_{ya} + \tau_{sy} + e_{sya}$$

Here $H\_Abrate_{s,y-a,a}$ is the number of abortion per 1,000 women aged 15-44, which measured the level of abortion utilization among all women at fertility age. Results are reported in Table 6 for cohorts 1968-1979 and in Table 7 for cohorts 1980-1987. The results for health outcomes are presented in Table 9. They confirm the findings in Table 4, Table 5 and Table 8. For women born in the legalizing period, high abortion exposure *in utero* is associated with low likelihood of early childbearing. While for cohorts born in the 1980s, girls exposed to high abortion rate *in utero* are more likely to give birth when they become teenagers. The interpretation remains the same as that for historical abortion ratio results.

2.5.2. Preference in Marital Status

Though the results on unmarried birth rate are similar to those on total birth rate, it is important to know whether there is a change in women's preference in marital status or the likelihood of being married when they give birth. To understand this issue, I regress historical abortion ratio on “unmarried birth ratio.” I calculated the “unmarried birth ratio” as number of births to unmarried women per 1,000 live births. It is essentially the proportion of out-of-wedlock births in total births. This set of regressions is solely based on birth data from the Vital Statistics and has nothing to do with population data. It is weighted by number of births, instead of female population. Results are represented in Table 10. There is no significant results for any sub-samples (1968-1979 and 1980+), no matter whether the state and year of observations with imputed marital status are included. Accordingly, historical abortion does not have much impact on women’s marital status at the time of giving birth, though inconsistent results occasionally show up for the whole sample.

2.5.3. Without DC

Table 12-18 presents the sensitive test for results in Table 4-10 with data from DC in the sample. I eliminate DC from the main sample because the abortion data for DC is problematic in an obvious way (abortion ratio is over 1000 abortions per 1000 live births, which is impossible). Even so, the results with DC do not differ from the results I have from the sample without DC.

2.5.4. Use SEER population data

In this section, I will use population data from the Surveillance, Epidemiology, and End Results (SEER) Program of the National Cancer Institute to calculate birth rate. The SEER data represent population estimates on July 1 produced by the US Census Bureau's Population Estimates Program by year, age, sex, state of residency. It is essentially the same population data from the U.S. Census used in DGL (2009). To estimate female population born in the U.S. by state of birth, I use cross-state mobility information by age from the Decennial Census to weight and aggregate the original SEER population data. Specifically, let r be mother's state of current residency, s be mother's state of birth, I generate $d_{r,y,a}$, the fraction of population born in US, and weight $k_{sr,y,a}$ (s = 1,...,51; r = 1,...,51) using the Decennial Census. Thus, the SEER female population state of birth is:

(15) $$SEERpop_{s,y,a} = \sum_{r=1}^{51} k_{sr,y,a} * d_{r,y,a} * SEERpop_{r,y,a}$$

The results for regression with historical abortion ratio as independent variable are in Table 2.19, and the results for regression with historical abortion rate as independent variable are in Table 2.20. The results are basically consistent with those in last section obtained from Census and Natality population data.

2.6. Conclusion

To conclude, abortion circumstance can change early childbearing decision among the next generation by changing the share of children born in adverse nurturing environment and hence the average quality of childhood experience. However, the direction of the effect depends on how abortion exposure affects number of births on the margin in short-run at the first place. An increase in abortion utilization driven by relatively exogenous supply shock with lowered cost of having abortion, such as the abortion legalization, reduces the portion of children at high risk of early childbearing, and therefore reduces birth rate among teens and young women 15-24 years later. If the equilibrium level of abortion is reached due to a shift of the demand curve of abortion, the correlation between abortion exposure *in utero* and early childbearing will be ambiguous, depending on elasticity of birth to abortion.

Empirically, abortion legalization in 1970s lowers birthrate among both 15-19 years olds and 20-24 years old. Total teen birthrate fall by 5% for cohorts 1968-1979, and by 11% fall for cohorts 1970-1976 due to abortion legalization in 1970s. First teen birth rate and unmarried teen birthrate fall by 4-10% due to abortion legalization. Abortion legalization in 1970s may also lead to a 2-3% decrease of low birthweight, early term births, risky behavior during pregnancy and abnormal birth among offspring born in 1990s. The impact on health outcomes is even larger impact on 20-24 group.

The results in this paper show that abortion legalization help accelerate the remarkable decline or moderate the increase in birth rate among teens and young women in the 1990s. However, this impact of abortion exposure is limited to cohorts born around the years when legalization took place (considered 1968-1979 in this paper). For cohorts born in the 1980s, high abortion exposure in utero may lead to high teen birth rate 15-19 years later. Abortion exposure in utero accounts for roughly 30% of the decline in birth rate among teenagers before 1995, but it has no contribution to the consecutive decline of teen birth rate in 1995 and 2002. The impact of abortion legalization performs by reducing total fertility and postponing the age of first birth, but does not alter women's preference in marital status when giving birth in a consistent way.

Finally, it is worthy to point out that though high teen birth rate is a serious concern of public policy in the U.S., giving birth in early 20s cannot be presumed as negative behavior of the mothers that causally related to disadvantaged childhood environment. Further study is needed for understanding the association between abortion exposure *in utero* and childbearing decision for young women; the relevant findings in this paper should be interpreted with caution.

Table 2.1: The Impact of Historical Abortion Ratios on Birth Rates by Age and Marital Status for Year 1982-2002, DGL

	Panel A			Panel B		
		DGL Original			DGL Census by Mother's Birth State	
	Overall BR	Unmarried BR	Married BR	Overall BR	Unmarried BR	Married BR
Sample	All	Limit	Limit	All	Limit	Limit
Ages 15-19						
Historical Abortion Ratio	0.023	-0.064*	0.006	0.026	-0.024	0.035**
	(0.032)	(0.028)	(0.015)	(0.014)	(0.014)	(0.009)
N				5355	4730	4730
R-sq				0.997	0.994	0.993
Ages 20-24						
Historical Abortion Ratio	0.066	-0.082**	0.101**	0.078**	-0.027*	0.065**
	(0.043)	(0.031)	(0.036)	(0.022)	(0.013)	(0.016)
N				5355	4730	4730
R-sq				0.984	0.991	0.991
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the year-state level in parentheses. Regressions are run separately for women who are 15-19 years old and 20-24 years old at the time of giving birth, and are weighted by the number of women. The unit of observation is year-state-single year of age. In DGL (Panel A), state is women's state of residency; in CENSUS, state is women's state of birth. DGL's and my CENSUS sample contain data from the 50 states and Washington DC from 1982 through 2002 for women between the age of 15 and 24. The samples labeled as "All" contain all states and years. The samples labeled as "Limit" exclude the state-years with imputed marital status, which are California (15 years), Connecticut (17), Maryland (7), Michigan (21), Montana (7), Nevada (15), New York (21), Ohio (7), and Texas (12). The "Limit" sample is emphasized for regressions on unmarried and married birth rates in DGL, but it is not relevant to total birth rate. The historical abortion ratio is a weighted average of abortion ratios in year y-age, y-age-1 and y-age-2. * and ** denote statistical significancy at the 0.05 and 0.01 levels.

Table 2.2: The Impact of Historical Abortion Ratios on Birth Rates by Age and Marital Status for Year 1982-2002

	Panel A My Census			Panel B My Natality		
	Overall BR	Unmarried BR	Married BR	Overall BR	Unmarried BR	Married BR
Sample	All	Limit	Limit	All	Limit	Limit
Ages 15-19						
Historical Abortion Ratio	-0.026	-0.046*	0.032**	-0.005	-0.062**	0.062**
	(0.016)	(0.018)	(0.010)	(0.012)	(0.013)	(0.009)
N	5355	4730	4730	4590	4100	4100
R-sq	0.997	0.994	0.993	0.998	0.996	0.995
Ages 20-24						
Historical Abortion Ratio	-0.022	-0.034*	0.048*	-0.102**	0.002	-0.062**
	(0.022)	(0.016)	(0.020)	(0.016)	(0.014)	(0.015)
N	5355	4730	4730	3315	3032	3032
R-sq	0.984	0.991	0.991	0.992	0.991	0.994
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the year-state level in parentheses. Regressions are run separately for women who are 15-19 years old and 20-24 years old at the time of giving birth, and are weighted by the number of women. The unit of observation is year-state-single year of age. In DGL (Panel A), state is women's state of residency; in CENSUS, state is women's state of birth. DGL's and my CENSUS sample contain data from the 50 states and Washington DC from 1982 through 2002 for women between the age of 15 and 24. The samples labeled as "All" contain all states and years. The samples labeled as "Limit" exclude the state-years with imputed marital status, which are California (15 years), Connecticut (17), Maryland (7), Michigan (21), Montana (7), Nevada (15), New York (21), Ohio (7), and Texas (12). The "Limit" sample is emphasized for regressions on unmarried and married birth rates in DGL, but it is not relevant to total birth rate. The historical abortion ratio is a weighted average of abortion ratios in year y-age, y-age-1 and y-age-2. * and ** denote statistical significancy at the 0.05 and 0.01 levels.

Table 2.3: Summary of Statistics

Variable		No. of Obs.	Mean	sd	Census Mean	Census sd	Natality Mean	Natality sd
All Ages:		7,750						
	Birth rate				72.18	39.92	70.16	38.48
	First birth rate				42.74	18.86	41.61	18.24
	Unmarried birth rate				40.42	18.95	39.37	18.43
	Married birth rate				31.76	28.73	30.79	27.69
	Historical abortion ratio		227.14	184.28				
	Historical abortion rate		15.24	11.30				
	Abortion ratio in 1979		356.02	151.93				
	Abortion rate in 1979		23.87	8.02				
Ages 15-19:		4,500						
	Birth rate				48.21	32.32	47.08	31.37
	First birth rate				37.68	21.48	36.81	20.86
	Unmarried birth rate				35.60	20.62	34.79	20.07
	Married birth rate				12.61	14.31	12.29	13.86
	Historical abortion ratio		244.53	185.32				
	Historical abortion rate		16.37	11.33				
Ages 20-24:		3,250						
	Birth rate				105.36	21.40	102.13	20.22
	First birth rate				49.73	11.16	48.24	10.73
	Unmarried birth rate				47.09	13.82	45.71	13.52
	Married birth rate				58.27	21.76	56.41	20.78
	Historical abortion ratio		203.07	180.10				
	Historical abortion rate		13.68	11.07				

Notes: The unit of observation is year-state of birth-single year of age. The samples contain data from the 50 states for women between the age of 15 and 24 who were born in or after 1968. With this cohort constrain, the samples cover the period 1983-2002. All states and years are included, disregards whether or not marital status is imputed.

Table 2.4: The Impact of Historical Abortion Ratios on Birth Rates by Age and Marital Status for Cohorts<=1979

	Census				Natality			
	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate
Ages 15-19	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Historical Abortion Ratio	-0.119**	-0.056**	-0.053**	-0.065**	-0.073**	-0.056**	-0.091**	0.018
	(0.019)	(0.014)	(0.020)	(0.014)	(0.016)	(0.012)	(0.019)	(0.015)
N	4000	4000	4000	4000	3000	3000	3000	3000
Ages 20-24								
Historical Abortion Ratio	-0.008	0.092**	-0.091**	0.083**	-0.104**	-0.050**	-0.014	-0.091**
	(0.029)	(0.022)	(0.017)	(0.025)	(0.021)	(0.014)	(0.019)	(0.020)
N	5200	5200	5200	5200	2950	2950	2950	2950
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the year-state level in parentheses. Regressions are run separately for women who are 15-19 years old and 20-24 years old at the time of giving birth, and are weighted by the number of women. The unit of observation is year-state of birth-single year of age. The samples contain data from the 50 states for women between the age of 15 and 24 who were born in or after 1968. With this cohort constrain, the samples cover the period 1983-2002. All states and years are included, disregarding whether or not marital status is imputed. The historical abortion ratio is a weighted average of abortion ratios in year y-age, y-age-1 and y-age-2. * and ** denote statistical significance at the 0.05 and 0.01 levels.

Table 2.5: The Impact of Historical Abortion Ratios on Birth Rates by Age and Marital Status for Cohorts 1980+

	Census				Natality			
	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate
Ages 15-19	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Historical Abortion Ratio	0.150**	0.133**	0.101*	0.050*	0.057	0.065*	0.025	0.033
	(0.044)	(0.034)	(0.039)	(0.023)	(0.035)	(0.028)	(0.031)	(0.023)
N	1500	1500	1500	1500	1500	1500	1500	1500
Ages 20-24								
Historical Abortion Ratio	-0.143	-0.220	-0.027	-0.116	0.079	-0.076	0.121	-0.043
	(0.442)	(0.334)	(0.406)	(0.197)	(0.368)	(0.280)	(0.345)	(0.192)
N	300	300	300	300	300	300	300	300
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the year-state level in parentheses. Regressions are run separately for women who are 15-19 years old and 20-24 years old at the time of childbirth, and are weighted by the number of women. The unit of observation is year-state of birth-single year of age. The samples contain data from the 50 states for women between the age of 15 and 24 who were born in or after 1980. With this cohort constrain, the samples cover the period 1995-2002 for 15-19 age group and 2000-2002 for 20-24 age group. All states and years are included, disregarding whether or not marital status is imputed. The historical abortion ratio is a weighted average of abortion ratios in year y-age, y-age-1 and y-age-2. * and ** denote statistical significance at the 0.05 and 0.01 levels.

Table 2.6: The Impact of Historical Abortion Rates on Birth Rates by Age and Marital Status for Cohorts<=1979

	Census				Natality			
	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate
Ages 15-19	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Historical Abortion Rate	-1.844**	-0.975**	-0.935**	-0.903**	-1.091**	-0.848**	-1.241**	0.150
	(0.281)	(0.207)	(0.309)	(0.241)	(0.226)	(0.175)	(0.293)	(0.236)
N	4000	4000	4000	4000	3000	3000	3000	3000
Ages 20-24								
Historical Abortion Rate	-0.834	0.653*	-1.011**	0.175	-1.780**	-0.960**	0.002	-1.782**
	(0.429)	(0.316)	(0.301)	(0.376)	(0.289)	(0.174)	(0.312)	(0.311)
N	5200	5200	5200	5200	2950	2950	2950	2950
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the year-state level in parentheses. Regressions are run separately for women who are 15-19 years old and 20-24 years old at the time of childbirth, and are weighted by the number of women. The unit of observation is year-state of birth-single year of age. The samples contain data from the 50 states for women between the age of 15 and 24 who were born in or after 1968. With this cohort constrain, the samples cover the period 1983-2002. All states and years are included, disregarding whether or not marital status is imputed. The historical abortion ratio is a weighted average of abortion ratios in year y-age, y-age-1 and y-age-2. * and ** denote statistical significance at the 0.05 and 0.01 levels.

Table 2.7: The Impact of Historical Abortion Rates on Birth Rates by Age and Marital Status for Cohorts 1980+

	Census				Natality			
	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate
Ages 15-19	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Historical Abortion Rate	2.127**	1.927**	1.781**	0.345	0.713	0.903	0.650	0.063
	(0.776)	(0.600)	(0.625)	(0.379)	(0.586)	(0.466)	(0.477)	(0.362)
N	1500	1500	1500	1500	1500	1500	1500	1500
Ages 20-24								
Historical Abortion Rate	-1.577	-2.855	0.357	-1.934	1.681	-0.730	2.449	-0.768
	(7.390)	(5.828)	(6.524)	(3.453)	(6.252)	(4.988)	(5.689)	(3.244)
N	300	300	300	300	300	300	300	300
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the year-state level in parentheses. Regressions are run separately for women who are 15-19 years old and 20-24 years old at the time of childbirth, and are weighted by the number of women. The unit of observation is year-state of birth-single year of age. The samples contain data from the 50 states for women between the age of 15 and 24 who were born in or after 1980. With this cohort constrain, the samples cover the period 1995-2002 for 15-19 age group and 2000-2002 for 20-24 age group. All states and years are included, disregarding whether or not marital status is imputed. The historical abortion ratio is a weighted average of abortion ratios in year y-age, y-age-1 and y-age-2. * and ** denote statistical significance at the 0.05 and 0.01 levels.

Table 2.8: The Impact of Historical Abortion Ratios on Children's Health Outcome by Mother's Age and Marital Status

	Ages 15-19					Ages 20-24				
	Low Birthweight	Pre-Mature	Mom's risk Behavior	Abnormal Birth	Complicated Labor	Low Birthweight	Pre-Mature	Mom's risk Behavior	Abnormal Birth	Complicated Labor
Cohort<=1979	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Historical Abortion Ratio	-0.049	-0.197**	-0.109	-0.004	-0.244*	-0.126**	-0.129**	-0.099	-0.074*	-0.072
	(0.055)	(0.072)	(0.083)	(0.069)	(0.105)	(0.035)	(0.040)	(0.062)	(0.034)	(0.074)
N	4000	4000	2000	2000	2000	5200	5200	3200	3200	3200
R-sq	0.873	0.935	0.993	0.949	0.974	0.912	0.950	0.993	0.957	0.982
Cohort>=1980										
Historical Abortion Ratio	0.254	0.002	0.166	0.109	-0.190	0.501	-0.370	-0.165	-1.370	0.803
	(0.199)	(0.267)	(0.279)	(0.186)	(0.353)	(0.977)	(1.369)	(2.282)	(1.200)	(2.225)
N	1500	1500	1500	1500	1500	300	300	300	300	300
R-sq	0.878	0.915	0.991	0.947	0.978	0.979	0.975	0.998	0.990	0.996
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the year-state level in parentheses. Regressions are run separately for women who are 15-19 years old and 20-24 years old at the time of childbirth, and are weighted by the number of women. The unit of observation is year-state of birth-single year of age. The samples contain data from the 50 states for women between the age of 15 and 24 who were born in or after 1980. With this cohort constrain, the samples cover the period 1995-2002 for 15-19 age group and 2000-2002 for 20-24 age group. All states and years are included, disregarding whether or not marital status is imputed. The historical abortion ratio is a weighted average of abortion ratios in year y-age, y-age-1 and y-age-2. * and ** denote statistical significance at the 0.05 and 0.01 levels.

Table 2.9: The Impact of Historical Abortion Rates on Children's Health Outcome by Mother's Age and Marital Status

	Ages 15-19					Ages 20-24				
	Low Birthweight	Pre-Mature	Mom's risk Behavior	Abnormal Birth	Complicated Labor	Low Birthweight	Pre-Mature	Mom's risk Behavior	Abnormal Birth	Complicated Labor
Cohort<=1979	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Historical Abortion Rate	-0.593	-2.359*	-1.785	-0.064	-2.929*	-1.685**	-1.688**	-0.787	-0.939	-1.585
	(0.809)	(1.144)	(1.183)	(1.042)	(1.469)	(0.547)	(0.630)	(0.854)	(0.491)	(1.093)
N	4000	4000	2000	2000	2000	5200	5200	3200	3200	3200
R-sq	0.873	0.935	0.993	0.949	0.974	0.912	0.950	0.993	0.957	0.982
Cohort>=1980										
Historical Abortion Rate	1.626	0.831	1.142	0.130	-3.581	6.952	-7.812	-2.632	-19.709	1.420
	(2.834)	(4.072)	(4.187)	(2.738)	(5.102)	(15.038)	(22.019)	(36.686)	(19.040)	(35.823)
N	1500	1500	1500	1500	1500	300	300	300	300	300
R-sq	0.878	0.915	0.991	0.947	0.978	0.979	0.975	0.998	0.990	0.996
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the year-state level in parentheses. Regressions are run separately for women who are 15-19 years old and 20-24 years old at the time of childbirth, and are weighted by the number of women. The unit of observation is year-state of birth-single year of age. The samples contain data from the 50 states for women between the age of 15 and 24 who were born in or after 1980. With this cohort constrain, the samples cover the period 1995-2002 for 15-19 age group and 2000-2002 for 20-24 age group. All states and years are included, disregarding whether or not marital status is imputed. The historical abortion ratio is a weighted average of abortion ratios in year y-age, y-age-1 and y-age-2. * and ** denote statistical significance at the 0.05 and 0.01 levels.

Table 2.10: The Impact of Historical Abortion Rates on Unmarried Birth Ratio

	Ages 15-19						Ages 20-24					
	All Cohorts		Cohorts<=1979		Cohorts>=1980		All Cohorts		Cohorts<=1979		Cohorts>=1980	
	All	Limit	All	Limit	All	Limit	All	Limit	All	Limit	All	Limit
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Historical Abortion Ratio	-0.537**	-0.240	-0.920**	-0.862**	-0.118	-0.206	-0.438**	-0.323**	-0.619**	-0.431**	0.837	1.105
	(0.130)	(0.145)	(0.169)	(0.224)	(0.290)	(0.289)	(0.120)	(0.124)	(0.123)	(0.138)	(2.303)	(2.256)
N	5500	4830	4000	3413	1500	1417	5500	4830	5200	4542	300	288
R-sq	0.992	0.992	0.993	0.992	0.995	0.995	0.995	0.995	0.995	0.995	0.998	0.998
Historical Abortion Rate	-6.770**	-3.673	-9.104**	-7.556*	-3.381	-4.268	-4.062*	-5.004*	-6.437**	-6.438**	15.939	15.886
	(1.985)	(2.321)	(2.465)	(3.366)	(4.558)	(4.535)	(2.002)	(1.986)	(2.080)	(2.189)	(37.099)	(36.537)
N	5500	4830	4000	3413	1500	1417	5500	4830	5200	4542	300	288
R-sq	0.992	0.992	0.993	0.992	0.995	0.995	0.995	0.995	0.995	0.995	0.998	0.998
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the year-state level in parentheses. Regressions are run separately for women who are 15-19 years old and 20-24 years old at the time of childbirth, and are weighted by the number of live births. The unit of observation is year-state of birth-single year of age. The samples contain data from the 50 states for women between the age of 15 and 24 who were born in or after 1968. The samples labeled as "All" contain all states and years. The samples labeled as "Limit" exclude the state-years with imputed marital status, which are California (15 years), Connecticut (17), Maryland (7), Michigan (21), Montana (7), Nevada (15), New York (21), Ohio (7), and Texas (12). With this cohort constrain, the samples cover the period 1983-2002. Unmarried birth ratio is defined as number of out-of-wedlock births per 1,000 live births. The historical abortion ratio is a weighted average of abortion ratios in year y-age, y-age-1 and y-age-2. * and ** denote statistical significance at the 0.05 and 0.01 levels.

Table 2.11: Age-Cohort-Year Chart

Age	15	16	17	18	19	20	21	22	23	24
Year										
1981	1966	1965	1964	1963	1962	1961	1960	1959	1958	1957
1982	1967	1966	1965	1964	1963	1962	1961	1960	1959	1958
1983	1968	1967	1966	1965	1964	1963	1962	1961	1960	1959
1984	1969	1968	1967	1966	1965	1964	1963	1962	1961	1960
1985	1970	1969	1968	1967	1966	1965	1964	1963	1962	1961
1986	1971	1970	1969	1968	1967	1966	1965	1964	1963	1962
1987	1972	1971	1970	1969	1968	1967	1966	1965	1964	1963
1988	1973	1972	1971	1970	1969	1968	1967	1966	1965	1964
1989	1974	1973	1972	1971	1970	1969	1968	1967	1966	1965
1990	1975	1974	1973	1972	1971	1970	1969	1968	1967	1966
1991	1976	1975	1974	1973	1972	1971	1970	1969	1968	1967
1992	1977	1976	1975	1974	1973	1972	1971	1970	1969	1968
1993	1978	1977	1976	1975	1974	1973	1972	1971	1970	1969
1994	1979	1978	1977	1976	1975	1974	1973	1972	1971	1970
1995	1980	1979	1978	1977	1976	1975	1974	1973	1972	1971
1996	1981	1980	1979	1978	1977	1976	1975	1974	1973	1972
1997	1982	1981	1980	1979	1978	1977	1976	1975	1974	1973
1998	1983	1982	1981	1980	1979	1978	1977	1976	1975	1974
1999	1984	1983	1982	1981	1980	1979	1978	1977	1976	1975
2000	1985	1984	1983	1982	1981	1980	1979	1978	1977	1976
2001	1986	1985	1984	1983	1982	1981	1980	1979	1978	1977
2002	1987	1986	1985	1984	1983	1982	1981	1980	1979	1978

Table 2.12: The Impact of Historical Abortion Ratios on Birth Rates by Age and Marital Status for Cohorts<=1979, With DC

	Census				Natality			
	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate
Ages 15-19	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Historical Abortion Ratio	-0.076**	-0.026	-0.025	-0.050**	-0.054**	-0.046**	-0.073**	0.018
	(0.021)	(0.016)	(0.019)	(0.013)	(0.016)	(0.011)	(0.017)	(0.013)
N	4080	4080	4080	4080	3060	3060	3060	3060
Ages 20-24								
Historical Abortion Ratio	-0.003	0.065**	-0.080**	0.077**	-0.092**	-0.052**	-0.011	-0.080**
	(0.024)	(0.019)	(0.016)	(0.020)	(0.019)	(0.011)	(0.017)	(0.018)
N	5304	5304	5304	5304	3009	3009	3009	3009
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: See note of Table 4.

Table 2.13: The Impact of Historical Abortion Ratios on Birth Rates by Age and Marital Status for Cohorts 1980+, With DC

	Census				Natality			
	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate
Ages 15-19	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Historical Abortion Ratio	0.146**	0.116**	0.097**	0.049**	0.078*	0.065**	0.040	0.037*
	(0.036)	(0.030)	(0.031)	(0.018)	(0.030)	(0.022)	(0.026)	(0.018)
N	1530	1530	1530	1530	1530	1530	1530	1530
Ages 20-24								
Historical Abortion Ratio	-0.150	-0.241	-0.027	-0.123	0.063	-0.102	0.114	-0.051
	(0.436)	(0.329)	(0.402)	(0.195)	(0.360)	(0.274)	(0.339)	(0.190)
N	306	306	306	306	306	306	306	306
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: See note of Table 5.

Table 2.14: The Impact of Historical Abortion Rates on Birth Rates by Age and Marital Status for Cohorts<=1979, With DC

	Census				Natality			
	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate
Ages 15-19	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Historical Abortion Rate	-1.255**	-0.602**	-0.531	-0.724**	-0.809**	-0.716**	-0.979**	0.169
	(0.316)	(0.231)	(0.302)	(0.213)	(0.228)	(0.163)	(0.276)	(0.202)
N	4080	4080	4080	4080	3060	3060	3060	3060
Ages 20-24								
Historical Abortion Rate	-0.654	0.405	-0.860**	0.206	-1.525**	-0.922**	0.078	-1.603**
	(0.380)	(0.265)	(0.288)	(0.319)	(0.290)	(0.160)	(0.288)	(0.275)
N	5304	5304	5304	5304	3009	3009	3009	3009
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: See note of Table 6.

Table 2.15: The Impact of Historical Abortion Rates on Birth Rates by Age and Marital Status for Cohorts 1980+, With DC

	Census				Natality			
	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate	Overall birth rate	First birth rate	Unmarried birth rate	Married birth rate
Ages 15-19	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Historical Abortion Rate	2.199**	1.816**	1.796**	0.403	1.006	0.937*	0.819	0.187
	(0.690)	(0.550)	(0.558)	(0.332)	(0.550)	(0.415)	(0.432)	(0.321)
N	1530	1530	1530	1530	1530	1530	1530	1530
Ages 20-24								
Historical Abortion Rate	-1.491	-2.612	0.355	-1.847	1.823	-0.452	2.495	-0.672
	(7.361)	(5.796)	(6.501)	(3.445)	(6.224)	(4.958)	(5.662)	(3.236)
N	306	306	306	306	306	306	306	306
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: See note of Table 7.

Table 2.16: The Impact of Historical Abortion Ratios on Children's Health Outcome by Mother's Age and Marital Status, With DC

	Ages 15-19					Ages 20-24				
	Low Birthweight	Pre-Mature	Mom's risk Behavior	Abnormal Birth	Complicated Labor	Low Birthweight	Pre-Mature	Mom's risk Behavior	Abnormal Birth	Complicated Labor
Cohort<=1979	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Historical Abortion Ratio	-0.068	-0.138*	-0.166*	0.008	-0.131	-0.133**	-0.142**	-0.076	-0.083**	-0.081
	(0.047)	(0.065)	(0.071)	(0.066)	(0.138)	(0.031)	(0.036)	(0.055)	(0.030)	(0.066)
N	4080	4080	2040	2040	2040	5304	5304	3264	3264	3264
R-sq	0.872	0.934	0.993	0.949	0.973	0.913	0.950	0.992	0.957	0.982
Cohort>=1980										
Historical Abortion Ratio	0.386*	0.273	0.042	0.065	-0.275	0.429	-0.436	-0.149	-1.434	0.696
	(0.176)	(0.257)	(0.233)	(0.148)	(0.295)	(0.975)	(1.369)	(2.265)	(1.202)	(2.227)
N	1530	1530	1530	1530	1530	306	306	306	306	306
R-sq	0.878	0.914	0.991	0.948	0.978	0.979	0.975	0.998	0.990	0.996
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: See note of Table 8.

Table 2.17: The Impact of Historical Abortion Rates on Children's Health Outcome by Mother's Age and Marital Status, With DC

	Ages 15-19					Ages 20-24				
	Low Birthweight	Pre-Mature	Mom's risk Behavior	Abnormal Birth	Complicated Labor	Low Birthweight	Pre-Mature	Mom's risk Behavior	Abnormal Birth	Complicated Labor
Cohort<=1979	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Historical Abortion Rate	-0.900	-1.794	-2.575*	-0.088	-1.490	-1.820**	-1.897**	-0.743	-0.991*	-1.504
	(0.698)	(0.993)	(1.026)	(0.982)	(1.887)	(0.489)	(0.582)	(0.748)	(0.449)	(0.998)
N	4080	4080	2040	2040	2040	5304	5304	3264	3264	3264
R-sq	0.872	0.934	0.993	0.949	0.973	0.913	0.950	0.992	0.957	0.982
Cohort>=1980										
Historical Abortion Rate	3.614	3.436	0.084	-0.150	-4.898	7.568	-7.106	-2.761	-18.890	2.372
	(2.766)	(3.941)	(3.820)	(2.434)	(4.692)	(15.018)	(21.897)	(36.485)	(19.019)	(35.693)
N	1530	1530	1530	1530	1530	306	306	306	306	306
R-sq	0.878	0.914	0.991	0.948	0.978	0.979	0.975	0.998	0.990	0.996
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: See note of Table 9.

Table 2.18: The Impact of Historical Abortion Rates on Unmarried Birth Ratio, With DC

	Ages 15-19						Ages 20-24					
	All Cohorts		Cohorts<=1979		Cohorts>=1980		All Cohorts		Cohorts<=1979		Cohorts>=1980	
	All	Limit	All	Limit	All	Limit	All	Limit	All	Limit	All	Limit
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
Historical Abortion Ratio	-0.468**	-0.244*	-0.786**	-0.668**	-0.051	-0.111	-0.473**	-0.423**	-0.627**	-0.513**	0.894	1.162
	(0.107)	(0.103)	(0.142)	(0.157)	(0.225)	(0.223)	(0.104)	(0.101)	(0.107)	(0.108)	(2.290)	(2.243)
N	5610	4940	4080	3493	1530	1447	5610	4940	5304	4646	306	294
R-sq	0.992	0.992	0.993	0.992	0.995	0.995	0.995	0.995	0.995	0.995	0.998	0.998
Historical Abortion Rate	-5.904**	-3.313	-7.887**	-5.956*	-2.285	-2.983	-4.399*	-5.544**	-6.488**	-6.741**	15.255	15.140
	(1.691)	(1.703)	(2.095)	(2.360)	(4.052)	(4.029)	(1.789)	(1.734)	(1.862)	(1.878)	(37.017)	(36.454)
N	5610	4940	4080	3493	1530	1447	5610	4940	5304	4646	306	294
R-sq	0.992	0.992	0.993	0.992	0.995	0.995	0.995	0.995	0.995	0.995	0.998	0.998
Age*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: See note of Table 10.

Table 2.19: The Impact of Historical Abortion Ratios on Birth Rates by Age and Marital Status Using SEER Population

	Panel A: Cohorts 1968-1979				Panel B: Cohorts 1980+			
	Overall Birth Rate	First Birth Rate	Unmarried Birth Rate	Married Birth Rate	Overall Birth Rate	First Birth Rate	Unmarried Birth Rate	Married Birth Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ages 15-19								
Historical Abortion Ratio	-0.0199**	-0.0157**	-0.0189**	-0.0010	0.0200**	0.0172**	0.0132*	0.0068*
	(0.0032)	(0.0025)	(0.0029)	(0.0024)	(0.0060)	(0.0049)	(0.0060)	(0.0028)
N	3000	3000	3000	3000	1500	1500	1500	1500
Ages 20-24								
Historical Abortion Ratio	-0.0419**	-0.0195**	-0.0149**	-0.0270**	-0.0038	-0.0158	0.0078	-0.0110
	(0.0043)	(0.0024)	(0.0030)	(0.0030)	(0.0921)	(0.0669)	(0.0818)	(0.0349)
N	2950	2950	2950	2950	300	300	300	300
Dummies for:								
age*year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state*age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state*year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the year-state level in parentheses. Regressions are run separately for women who are 15-19 years old and 20-24 years old at the time of giving birth, and are weighted by the number of women. The unit of observation is year-state of birth-single year of age. The samples contain data from the 50 states for women between the age of 15 and 24 who were born in or after 1968. With this cohort constrain, the samples cover the period 1983-2002. All states and years are included, disregarding whether or not marital status is imputed. The historical abortion ratio is a weighted average of abortion ratios in year y-age, y-age-1 and y-age-2. * and ** denote statistical significance at the 0.05 and 0.01 levels.

Table 2.20: The Impact of Historical Abortion Rates on Birth Rates by Age and Marital Status Using SEER Population

	Panel A: Cohorts 1968-1979				Panel B: Cohorts 1980+			
	Overall Birth Rate	First Birth Rate	Unmarried Birth Rate	Married Birth Rate	Overall Birth Rate	First Birth Rate	Unmarried Birth Rate	Married Birth Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ages 15-19								
Historical Abortion Rate	-0.3235**	-0.2513**	-0.2780**	-0.0453	0.4973**	0.4092**	0.3975**	0.0992*
	(0.0452)	(0.0372)	(0.0427)	(0.0389)	(0.0984)	(0.0838)	(0.0892)	(0.0437)
N	3000	3000	3000	3000	1500	1500	1500	1500
Ages 20-24								
Historical Abortion Rate	-0.6101**	-0.3011**	-0.1745**	-0.4365**	0.4372	0.0441	0.6212	-0.1843
	(0.0634)	(0.0320)	(0.0498)	(0.0446)	(1.7079)	(1.2827)	(1.3985)	(0.6778)
N	2950	2950	2950	2950	300	300	300	300
Dummies for:								
age*year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state*age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
state*year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors clustered at the year-state level in parentheses. Regressions are run separately for women who are 15-19 years old and 20-24 years old at the time of childbirth, and are weighted by the number of women. The unit of observation is year-state of birth-single year of age. The samples contain data from the 50 states for women between the age of 15 and 24 who were born in or after 1968. With this cohort constrain, the samples cover the period 1983-2002. All states and years are included, disregarding whether or not marital status is imputed. The historical abortion ratio is a weighted average of abortion ratios in year y-age, y-age-1 and y-age-2. * and ** denote statistical significance at the 0.05 and 0.01 levels.

Figure 2.1: Birth Rates for Teenagers, by Timing of Abortion Legalization

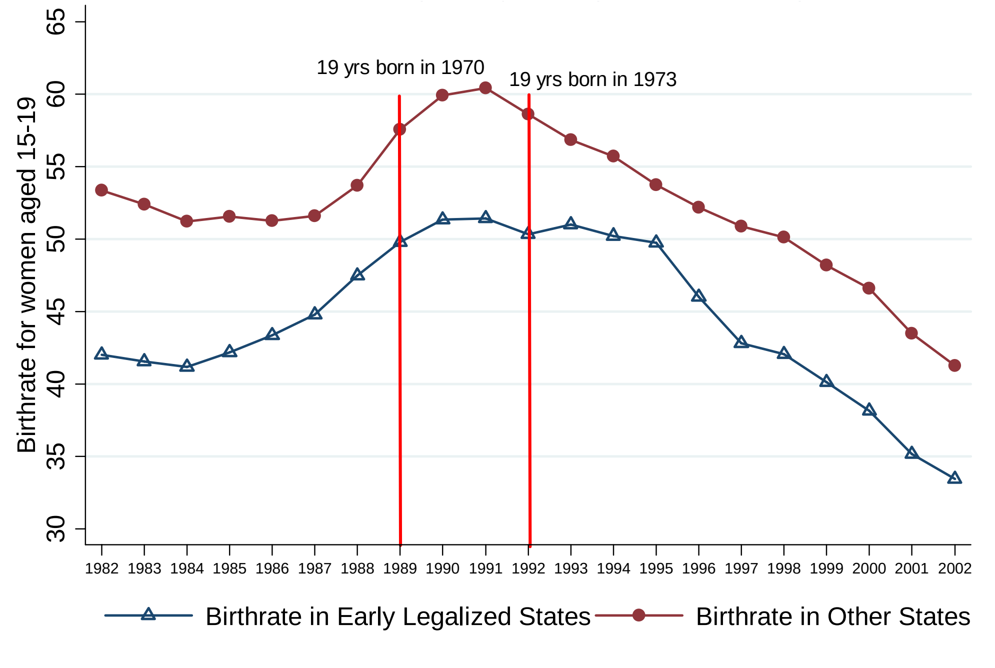
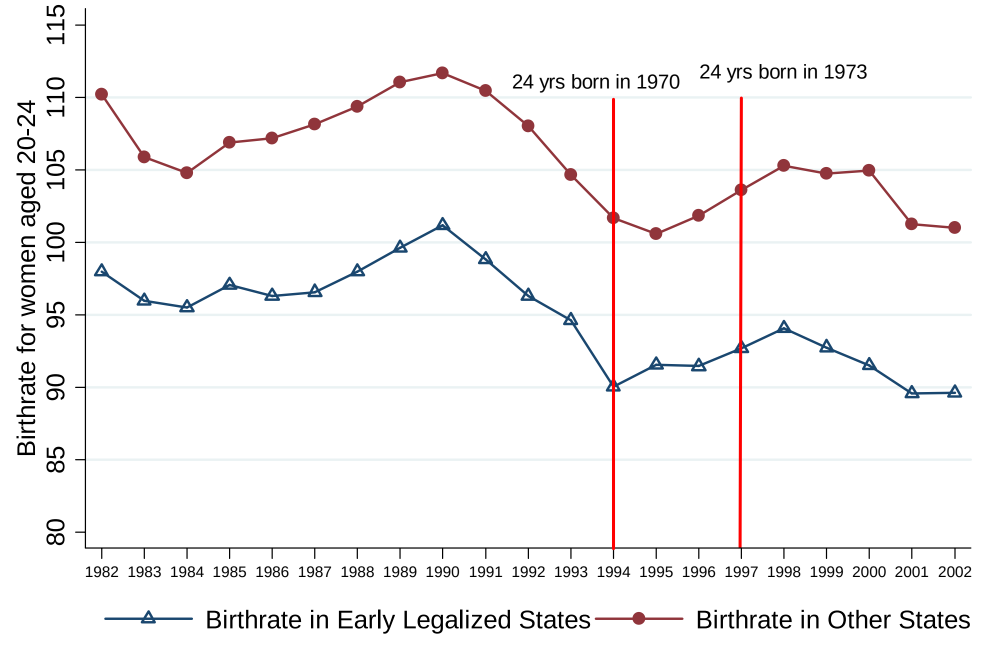
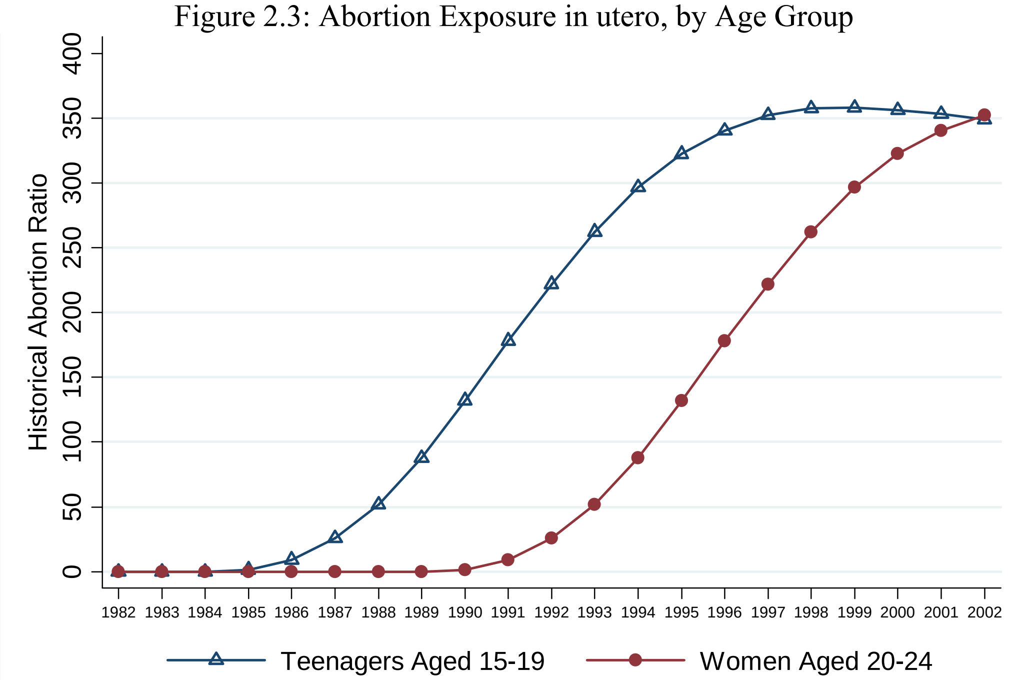


Figure 2.2: Birth Rates for Young Women, by Timing of Abortion Legalization



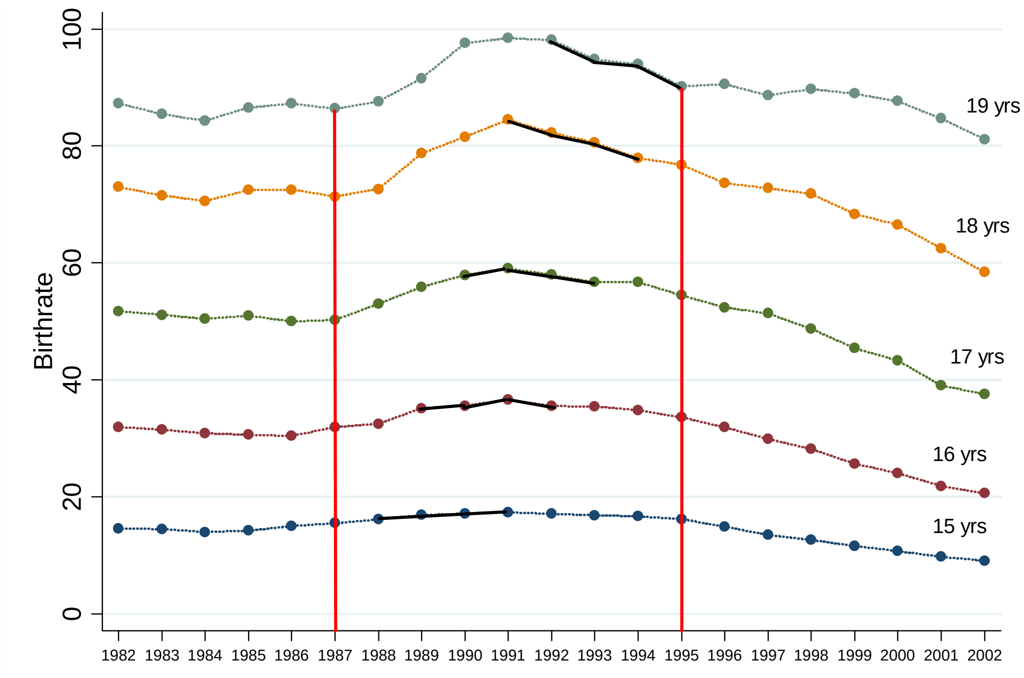
Source: National Vital Statistics Natality Detail Files and population from Surveillance, Epidemiology, and End Results (CENSUS), 1982-2002

Figure 2.3: Abortion Exposure in utero, by Age Group



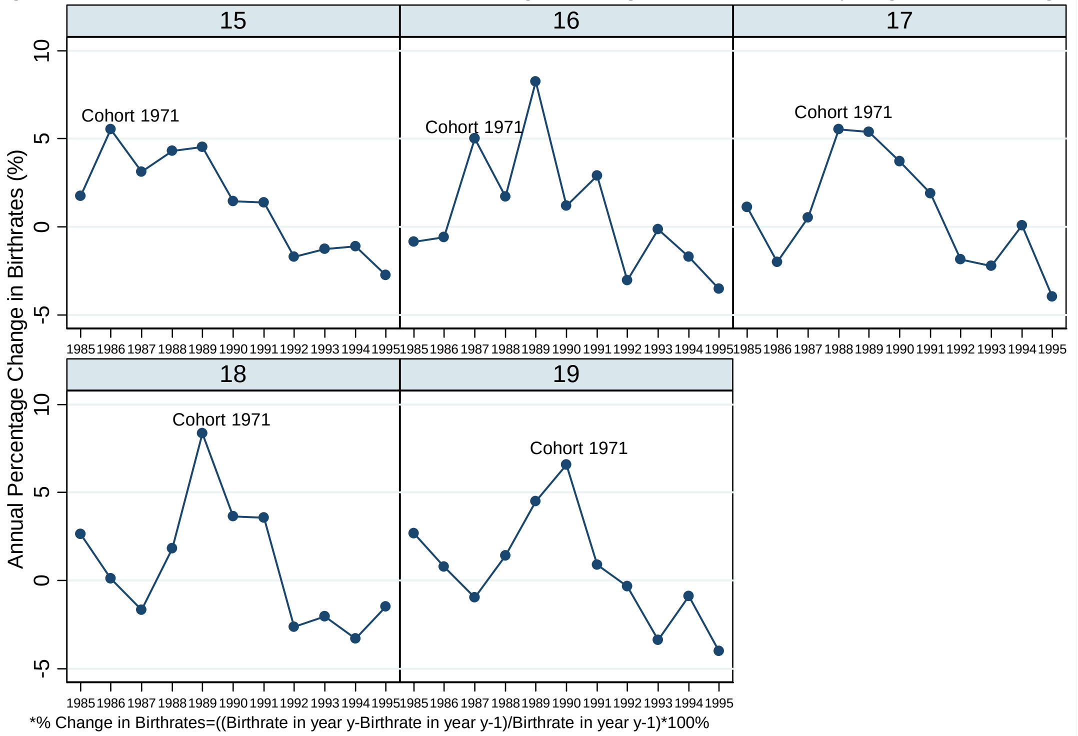
Source: Alan Guttmacher Institute's (AGI) for cohorts 1973-1987 and the Centers for Disease Control and Prevention for cohorts 1970-1972. Averaged by age groups for each year.

Figure 2.4: Birth Rates for Teenagers by Single Year of Age (15-19)



Source: National Vital Statistics Natality Detail Files and population from Surveillance, Epidemiology, and End Results (CENSUS), 1982-2002

Figure 2.5: Cohort Effect: Annual Percentage Change in Birthrates by Age for Teenagers



Source: National Vital Statistics Natality Detail Files and population from Surveillance, Epidemiology, and End Results (CENSUS), 1984-1995

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