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

Ted Joyce, Ruoding Tan and Yuxiu Zhang

Abstract

Recent studies on the "power of the Pill" have not adequately accounted for the role of abortion in the years between 1970 and 1973. We 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*. Our results strongly suggest that laws enhancing access to legalized abortion more than policies increasing access to the Pill caused birth rates of young women to fall in the early 1970s.

Acknowledgements

This study has been supported by a grant from the National Institute of Child Health and Human Development (NICHD) to the Research Foundation of the City University of New York (1RO3HD064760-01). We thank Martha Bailey, Melanie Guldi, Claudia Goldin, and Larry Katz for sharing data and programs. We also thank Michael Grossman and seminar participants at Georgia State, Baylor, Georgetown, Indiana University, University of Southern California and the National Bureau of Economic Research for comments.

Ted Joyce
NBER-NY&
Baruch College, CUNY
365 Fifth Ave, 5th Floor
NY, NY 10016
Theodore.joyce@baruch.cuny.edu

Ruoding Tan
Department of Economics
Graduate Center, CUNY
365 Fifth Ave, 5th Fl
NY, NY 10016
ruoding.tan@gmail.com

Yuxiu Zhang
Department of Economics
Graduate Center, CUNY
365 Fifth Ave, 5th Fl
NY, NY 10016
yuxiu909@gmail.com

I. 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 three 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*.

In the second 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

[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).

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 third 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).

II. 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. 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.

II.A. The Pill

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. Goldin and Katz (2002) regresses Pill use on a dichotomous measure of whether a state had lenient Pill use policies for women 16 years of age or less. They 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 younger had access to 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.

relative to states in which access was limited to teens at least 17 years of age. Goldin and Katz's (2002) 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's (2002) findings to justify their use of Pill access laws to identify 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 differ. We re-estimated Goldin and Katz's (2002) regressions with NSYW71 but we used the coding of laws liberalizing access to the Pill as specified by each of the aforementioned authors. In none of the regressions was access to the Pill associated with Pill use.[4]

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 absence of a robust "first-stage" is a potentially important limitation since it is unclear how effectively these policies differentiated Pill use between states. Most of these policies took effect after 1969. By then, the Pill had been legal for 10 years and was used widely by married women. Use by unmarried women was limited to women less than 21 years of age who lacked parental consent (Massachusetts and Wisconsin being exceptions). It is plausible that many unmarried women less than 21 acquired the Pill without parental involvement before the age of majority was lowered or policies were liberalized in their state. As Paul, Pipel and Wechsler (1974) note, not one physician was ever prosecuted for dispensing the Pill to an underage woman.[5]

[4] A table with the results from this exercise is available from the authors.
[5] The contrast with access to abortion is instructive. Abortion on demand was illegal before 1970. Although there were many illegal abortions, the risk of arrest and prosecution to clinicians and abortionists was substantial (see Lader 1973; Joffe 1995; Garrow 1998).

Evidence of a robust first-stage is also important as corroborating evidence for the very large association between laws regulating access to the Pill and birth rates of young women. Ananat and Hungerman (forthcoming), for example, find that access to the Pill among unmarried teens is associated with a 10 percent decline in the birth rates of young women, 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. 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 before age 22, an 18 percent decline evaluated at the mean.[6]

Finally, evidence of a robust association between access and use of the Pill is needed because of the overlap between policies liberalizing access to the Pill and the legalization of abortion. Thirty-one states increased access to the Pill for young women from 1970 to 1972, a period of rapid growth in legalized abortion (Guldi 2008). To illustrate, Figure 2 shows the years in which access to the Pill for unmarried women 18 to 20 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. Previous researchers have coded abortion as illegal in the lower 48 states except for California, New York and Washington. If the actual abortion rate in years before *Roe* represents a portion of pregnancies that would have been carried to term in absence of legalized abortion, then some of the decline in the birth rates of young women associated with access to the Pill is likely attributable to abortion. We explore this in more detail next.

[6]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).

II.B. Legalized Abortion

Five states and the District of Columbia effectively legalized abortion between 1969 and 1970.[7] 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 and 2 are consistent with this finding. 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).[8]

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

---

[7] 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).

[8] 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).

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 differs somewhat from Goldin and Katz's (2002). She assumes that New Jersey and Vermont legalized abortion in 1972, the year before *Roe*.[9] 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. 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*.[10] 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).

---

[9] 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).

[10] 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).

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. [11] 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. 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.[12] 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.

---

[11] 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.
[12] 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

Figure 3 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 18-20 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 (1) below for our sample of age-specific abortion rates in the 28-state sample

$$(1) \quad \text{Abrate}_{ajt} = \alpha_0 LnDis_j + \alpha_1 Pill_{ajt} + \sum \varphi_a A_a + \sum \delta_a (A_a * LnDis_j) + \mathbf{X\beta} + \lambda_j + \tau_t + e_{ajt}$$

Specifically, let $Abrate_{\mathrm{ajt}}$ be the abortion rate for age group a in state j and year t; let $LnDis_{\mathrm{j}}$ be the natural logarithm of distance to New York which varies only by state. Let $Pill_{\mathrm{ajt}}$ 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*$LnDis_{\mathrm{j}}$ 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

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).

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 (1) are displayed in Table 1. 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.[13] 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.[14]

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

[13] 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.

[14] 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 years of age.[15] The effect of distance among nonwhites is much greater (columns 5 & 6). A 100 mile increase in distance to New York is associated with 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.

A key assumption underlying the results in Table 1 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.[16] Second, none of the other 28 states in our sample followed New York. Indeed, "abortion on demand" 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 4 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*. Consider once again, Michigan. The resident abortion rate

---

[15] 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.

[16] 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).

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).

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.

III. 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.

III.A 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.[17] 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

[17] 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).

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. 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.

III.B. Results

Time-series plots

The seven panels in Figures 5A and 5B 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. Several observations stand out. First, 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.[18] 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.

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 5A and 5B 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

[18] 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.

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.

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:

$$(2) \qquad LnBrate_{ajt} = \alpha_0 Abor_{ajt-1} + \alpha_1 Pill_{ajt-1} + \sum \varphi_a A_a + \delta_{jt} + \lambda_j + \tau_t + e_{ajt}$$

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 (1) are shown in Table 2. In column (1) we replicate Guldi's (2008) results which served as her baseline specification.[19] 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).

[19] 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.

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 6A and the same for 19- and 20-year olds in Figure 6B. There is substantial variation in exposure between 17- and 18-year olds but none for 19- and 20-year olds. The overlap is so complete among the latter that only two lines are visible, one for the Pill and the other for abortion. Returning to Table 2, 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. 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 2 is that the more appropriate the comparison group, the less sensitive the estimates to the source of identifying variation. As Figure 5 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.[20] We prefer to use variation within age to identify effects of the Pill because 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 2 suggests this is not a major source of bias in this context.

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 2 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:

$$(3) \quad LnBrate_{ajt} = \alpha_0(LnDis_j{*}Y7173) + \alpha_1(LnDis_{jt-1}{*}Y7475) + \alpha_2 Pill_{ajt-1} + X\beta + \sum \varphi_a A_a + \lambda_s + \pi_t + e_{ajt}$$

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

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

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.

Access to the Pill is negatively associated with white birth rates when we pool all the age groups (Table 3, 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 3, 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 3, 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.

The overall findings in Tables 2 and 3 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*.

IV. 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.[21] With this interpretation, estimates obtained by ordinary least squares are interesting in their own right.

The results in Table 4 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.

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

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

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-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 2 and 3.

IV. Conclusion

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.[22] 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, 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

[22] 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.

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.

References

Ananat, Elizabeth Oltmans and Daniel M. Hungerman. 2008. "The Power of the Pill for the Marginal Child: Oral Contraception's Effects on Fertility, Abortion, and Maternal & Child Characteristics," http://www.nd.edu/~ dhungerm/Power_of_Pill. pdf.

Angrist, Joshua D. and William N. Evans. 1999. "Schooling and Labor Market Consequences of the 1970 State Abortion Reforms," *Research in Labor Economics*, XVIII:75-111.

Bailey, Martha J. 2006. "More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply." *Quarterly Journal of Economics* 121(1): 289-320.

Bailey, Martha J. 2009."Errata and Addendum: More Power To The Pill: The Quarterly Journal of Economics. February 2006." http://wwwpersonal.umich.edu/~baileymj/Bailey_Erratum.pdf

Bailey, Martha J. 2010. "Momma's Got the Pill: How Anthony Comstock and *Griswold v. Connecticut* Shaped U.S. Childbearing." *American Economic Review*100(1):98-129.

Blank, Rebecca M., Christine C. George and Rebecca A. London. 1996. "State Abortion Rates the Impact of Policies, Providers, Politics, Demographics, and Economic Environment." *Journal of Health Economics* 15(5):513-53.

Cartoff, Virginia G. and Lorraine V. Klerman.1986. "Parental Consent for Abortion: Impact of the Massachussetts Law." *American Journal of Public Health* 76(4):397-400.

Centers for Disease Control: Family Planning Evaluation:1971. December 1972.

Centers for Disease Control: Abortion Surveillance:1972. April 1974.

Centers for Disease Control: Abortion Surveillance:1973. May 1975.

Centers for Disease Control: Abortion Surveillance:1974. April 1976.

Colman, Silvie and Theodore J. Joyce. 2009. "Minors' Behavioral Responses to Parental Involvement Laws: Delaying Abortion until Age 18." *Perspectives on Sexual and Reproductive Health* 41(2):119-26.

Dennis, Amanda, Stanley K. Henshaw, Theodore J. Joyce, Lawrence B. Finer and Kelly Blanchard. 2009. "The Impact of Laws Requiring Parental Involvement for Abortion: A Literature Review." *New York: Guttmacher Institute.*

Donohue III, John J. and Steven D. Levitt. 2001. "The Impact of Legalized Abortion on Crime." *Quarterly Journal of Economics* 116(2):379-420.

Donohue III, John J. and Steven D. Levitt. 2004. "Further Evidence That Legalized Abortion Lowered Crime: A Reply to Joyce." *Journal of Human Resources* 39(1):29.

Garrow, David J. 1998. *Liberty and Sexuality: The Right to Privacy and the Making of Roe V. Wade*. Berkeley, CA: University of California Press.

Gibbs, Nancy. 2010. "The Pill at 50: Sex, Freedom and Paradox," *Time*, May 3.http://www.time.com/time/health/article/0,8599,1983712,00.html

Goldin, Claudia and Lawrrence F. Katz. 2002. "The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions." *Journal of Political Economy* 110(4): 730-70.

Guldi, Melaine. 2008. "Fertility Effects of Abortion and Birth Control Pill Access for Minors."*Demography* 45(4):1-11.

Guttmacher Institute. 2010. "U.S. Teen Pregnancy, Birth and Abortions: National and State Trends and Trends by Race and Ethnicity." http://www.guttmacher.org/pubs/USTPtrends.pdf.

Henshaw Stanley and Katherine Kost. 1992, Parental involvement in minors' abortion decisions. *Family Planning Perspectives*, 24(5):196-207.

Hodgson, Jane E. and K.C. Portmann. 1973. "First Trimester Abortion." *Minnesota Medicine* 56(10):887.

Hodgson, Jane E. 1975. “Major Concentrations of 20,248 Consecutive First Trimester Abortions,” *Advances In Planned Parenthood* 9: 52-59.

Joffe, Carole. 1995. *Doctors of Conscience: The Struggle to Provide Abortion Before and After Roe V. Wade*. Boston: Beacon Press.

Joyce, Theodore J., Robert Kaestner and Silvie Colman. 2006. "Changes in Abortions and Births and the Texas Parental Notification Law." *The New England Journal of Medicine* 354(10):1031.

Joyce, Theodore J. and Naci H. Mocan. 1990. "The Impact of Legalized Abortion on Adolescent Childbearing in New York City." *American Journal of Public Health* 80(3): 273.

Kane, Thomas J. and Douglas Staiger. 1996. "Teen Motherhood and Abortion Access." *The Quarterly Journal of Economics*, 467-506.

Lader, Lawrence. 1973. *Abortion II: Making the Revolution*. Boston: Beacon Press.

Levine, Phillip B., Amy B. Trainor and David J. Zimmerman. 1996. "The Effect of Medicaid Abortion Funding Restrictions on Abortions, Pregnancies and Births." *Journal of Health Economics*, 15(5), 555-78.

Levine, Phillip B., Douglas Staiger, Thomas J. Kane and David J. Zimmerman. 1999. "Roe v. Wade and American Fertility." *American Journal of Public Health* 89(2):199-203.

Margolis, Alan, Ronald Rindfuss, Phyllis Coghlan and Roger Rochat. 1974. "Contraception after Abortion." *Family Planning Perspectives* 6(1):56-60.

May, Elaine T. 2010. *America and the Pill: A History of Promise, Peril, and Liberation*.New York: Basic Books.

Matthews, Stephen, Ribar, David and Mark Wilhelm. 1997. "The Effect of Economic Conditions and Access to Reproductive Health Services on State Abortion Rates and Birthrates." *Family Planning Perspectives*. 29(2):52-60.

Paul, E. Pipel, H. and N. Wechsler. 1974. Pregnancy, Teenagers and the Law, 1974. *Family Planning Perspectives.*6(3):142-147.

Sklar, June and Beth Berkov. 1974. "Abortion, Illegitimacy, and the American Birth Rate." *Science* 185(4155), pp. 909-915.

Figure1: Resident Teen Abortion Rates for Abortions Performed in NY, 1971

Abortions per 1000 teens 15-19

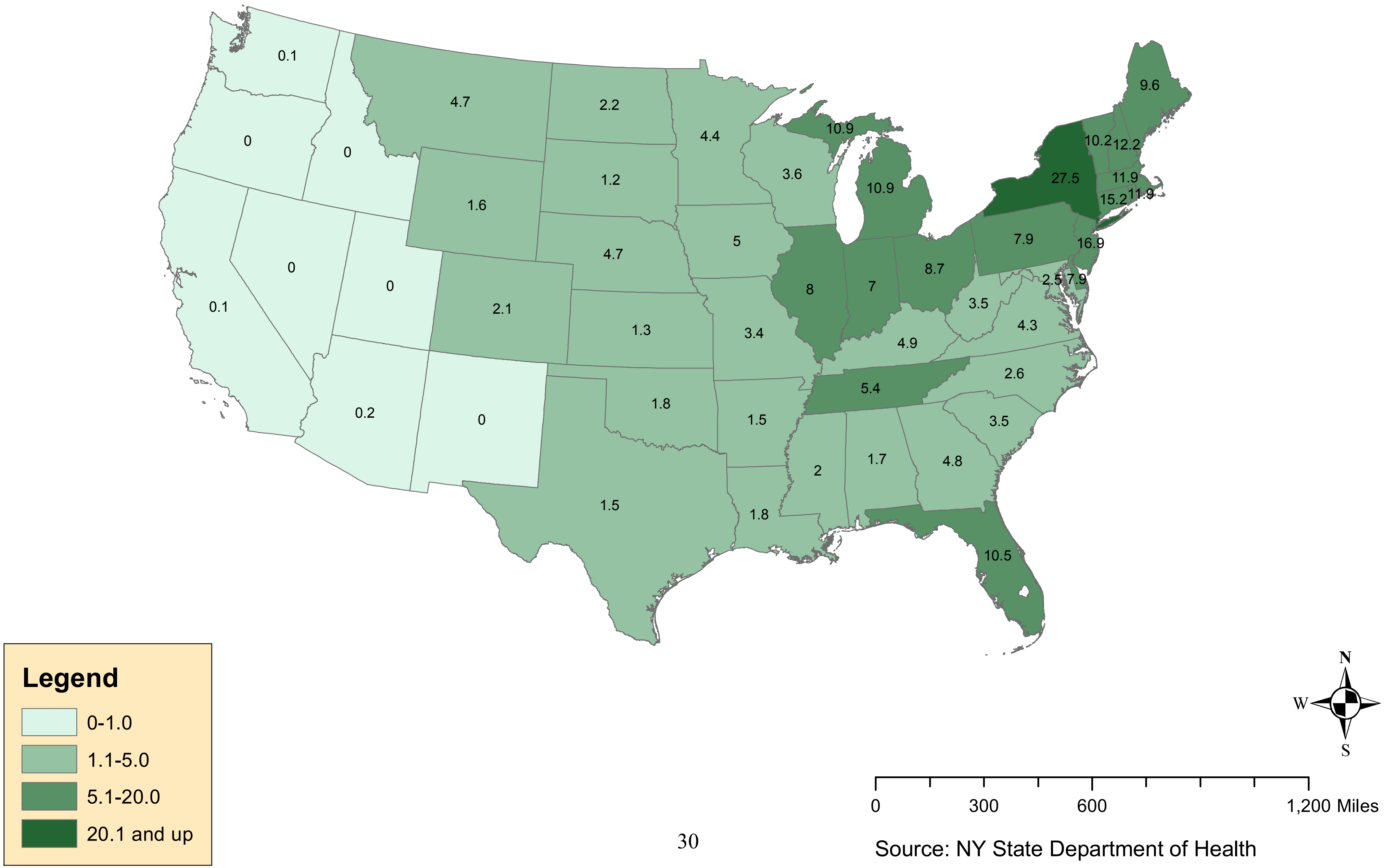
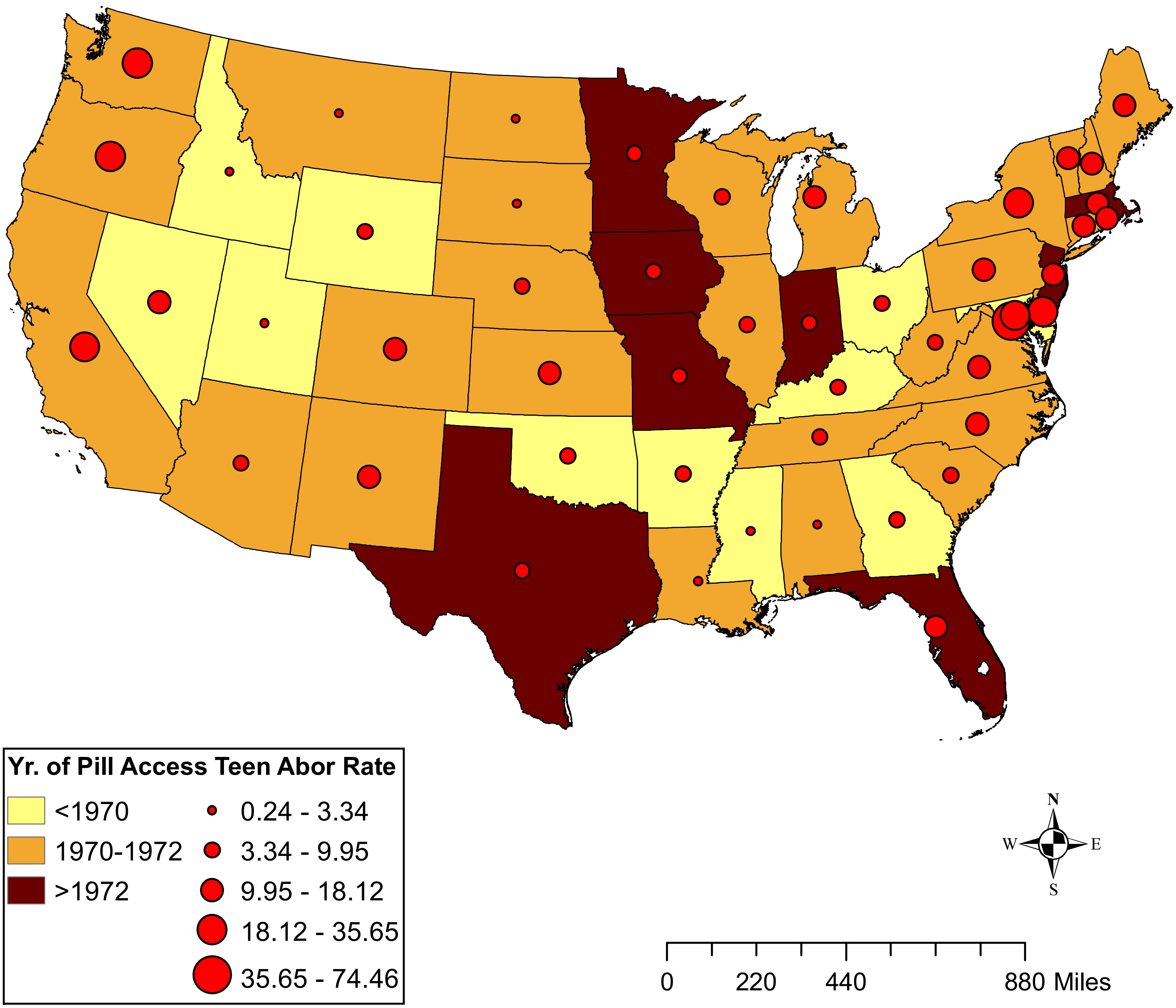


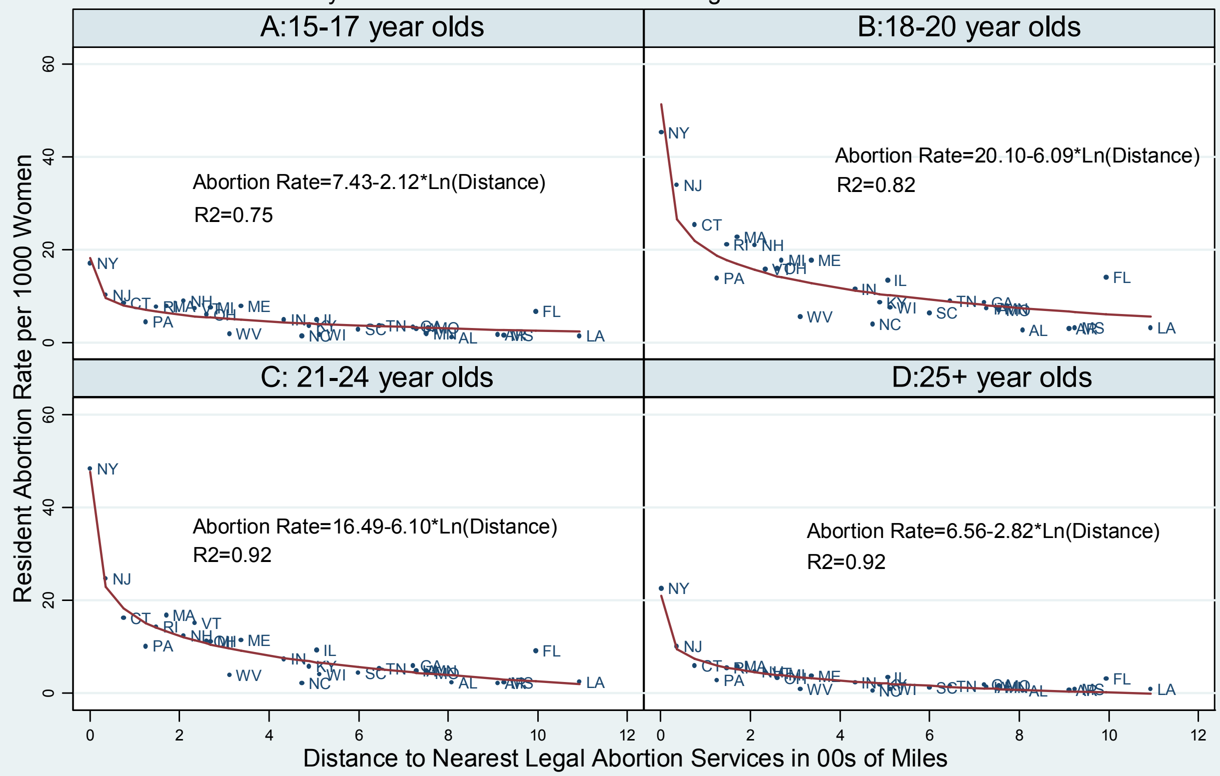
Figure 2: Year Access to Pill Provided to Those Younger than 21 Years of Age and Teen Abortion Rates in 1971-72



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

Figure 3: Resident Abortion Rates in 1971-72 in 28 States

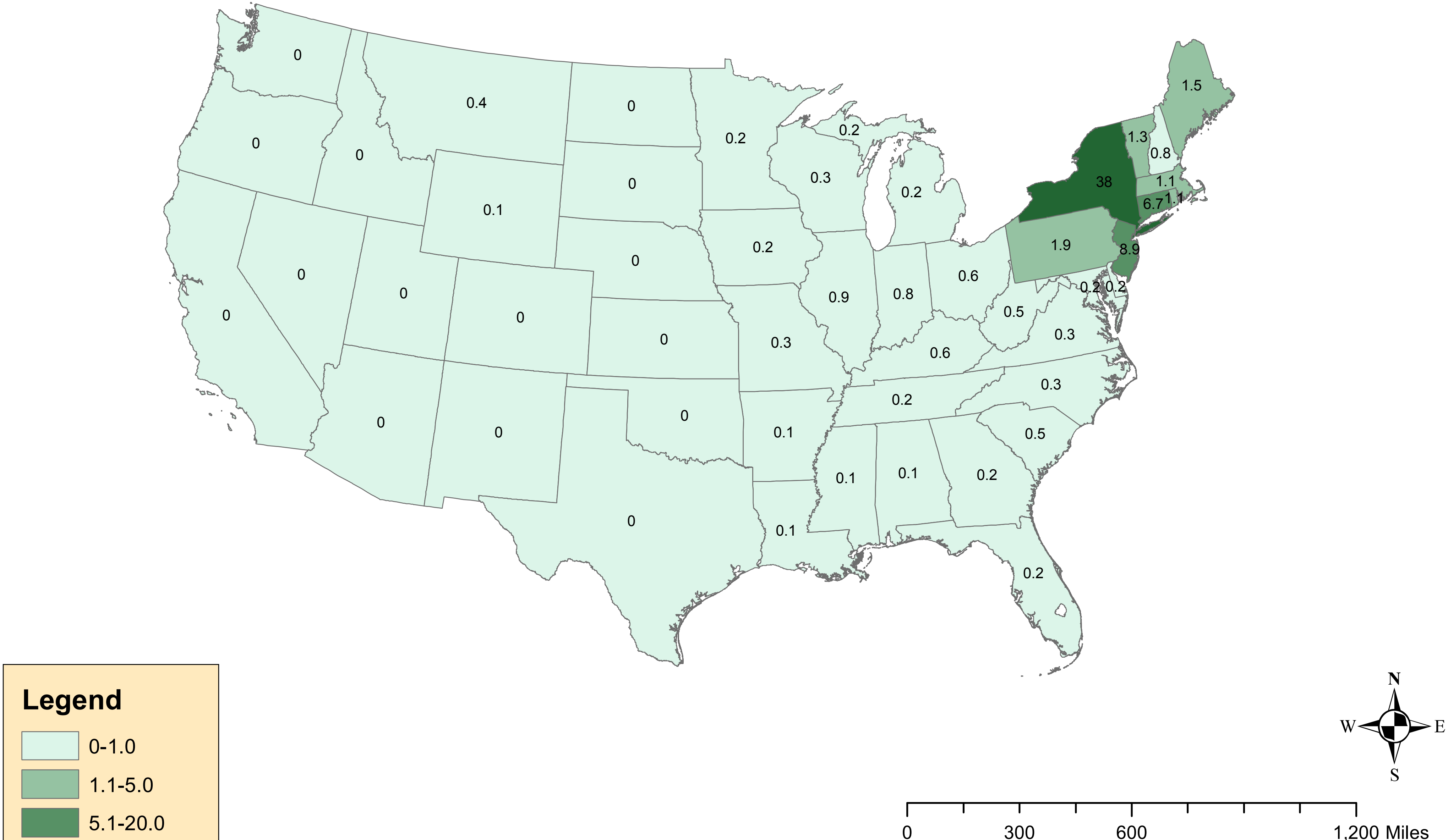
by Distance to Nearest New York Legal Abortion Provider



*All abortions performed in New York State

Figure 4: Resident Teen Abortion Rates for Abortions Performed in NY, 1975

Abortions per 1000 teens 15-19



Source: NY State Department of Health

Figure 5A: Birth Rate of 19-21 Year Olds by Year of Access to the Pill without Parental Consent
Early Legalizing States AK, CA, DC, HI, NY and WA not Included

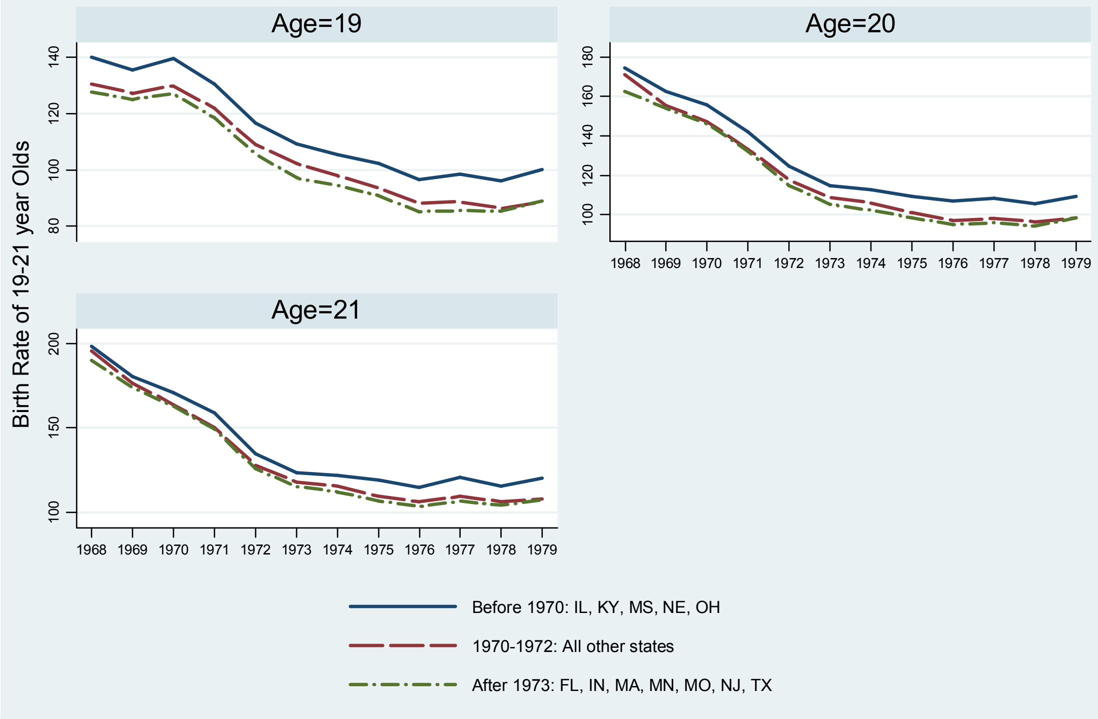


Figure 5B: Birth Rate of 15-18 Year Olds by Year of Access to the Pill without Parental Consent
Early Legalizing States AK, CA, DC, HI, NY and WA not Included

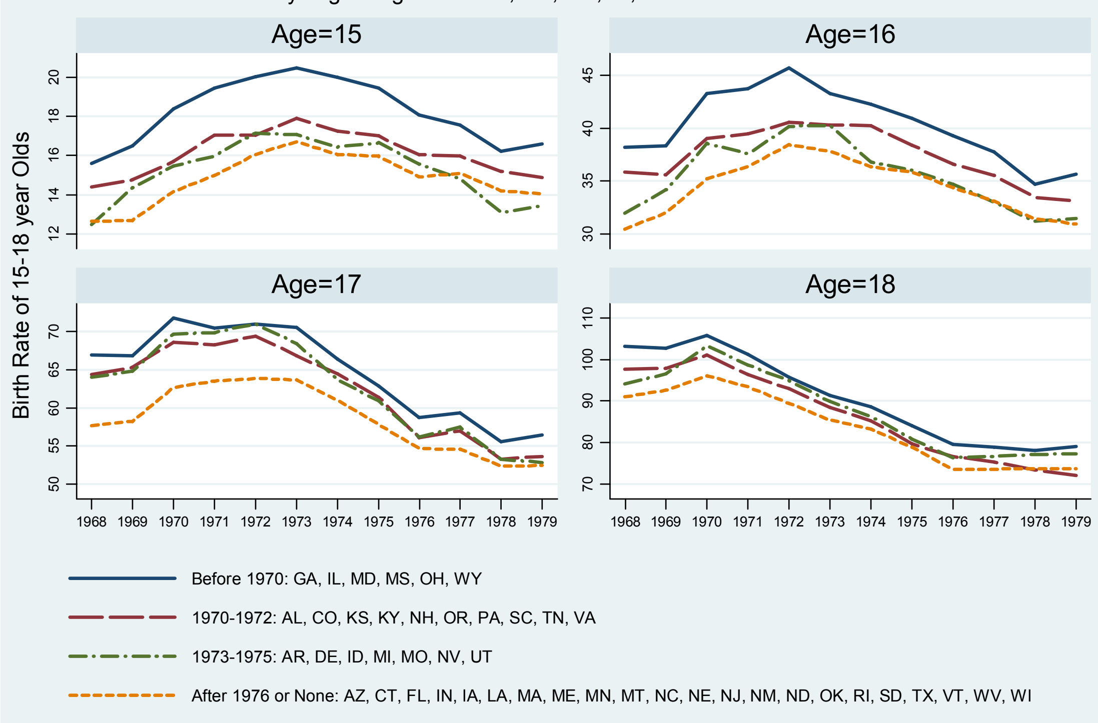
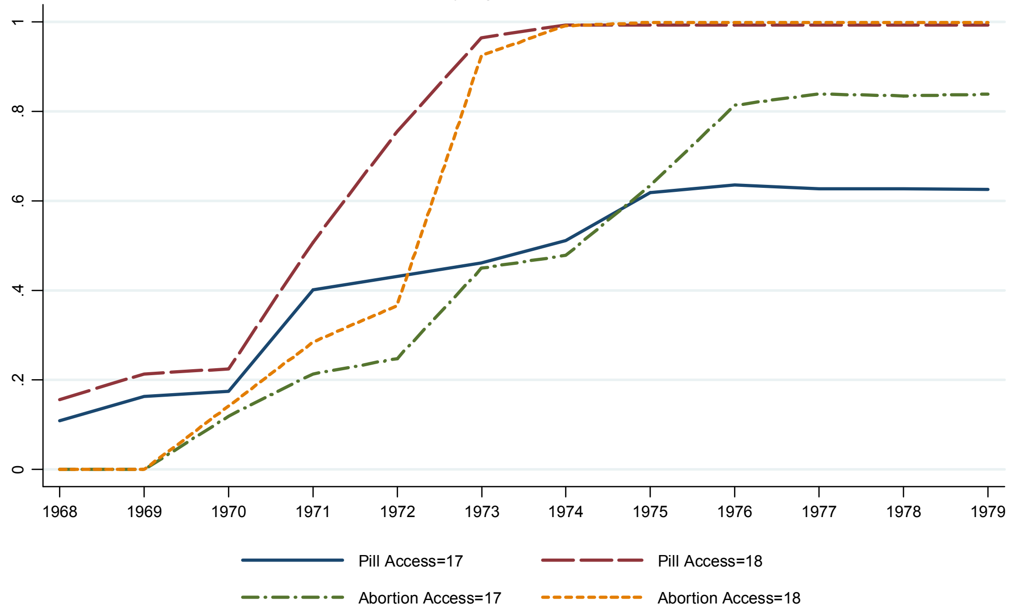
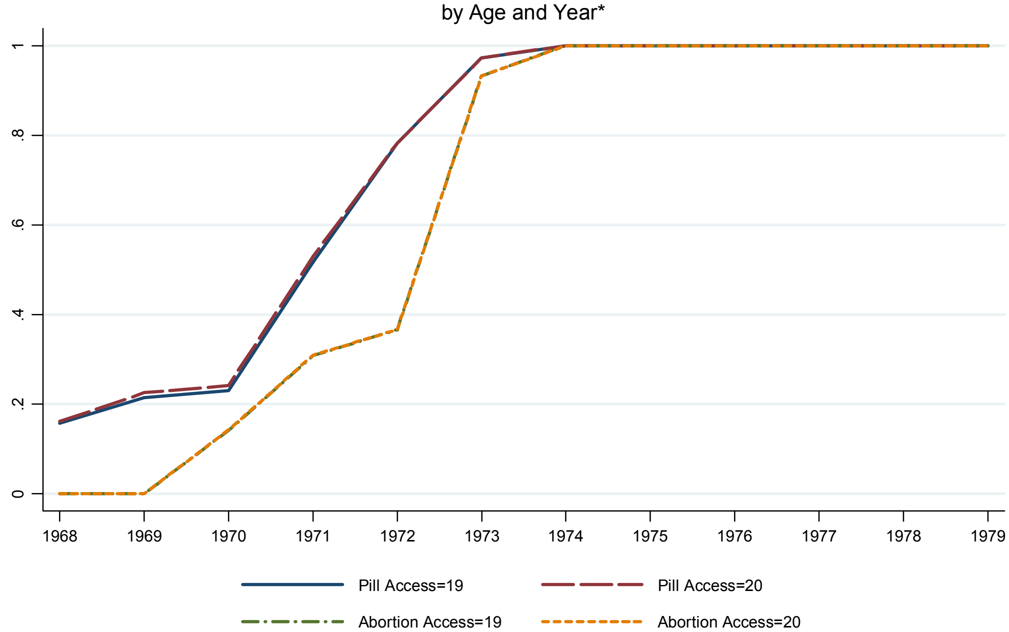


Figure 6A: Proportion of Women Aged 17 and 18 with Liberal Access to Pill and Legalized Abortion by Age and Year*



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

Figure 6B: Proportion of Women Aged 19 and 20 with Liberal Access to Pill and Legalized Abortion by Age and Year*



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

Table 1: 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 15-17	0.67*	0.66*				
(abortion rate=5.1)[±]	(0.28)	(0.29)				
Ln distance *Ages 18-20	-3.21**	-3.22**				
(abortion rate =12.7)	(0.76)	(0.81)				
Ln distance *Ages 21-24	-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. [±] 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 2: Association between Access to the Pill and Abortion and Age and Race-specific Birth Rates, 1968-79

	Whites							
Age	15-21	15-21	15-18	19-21	18 & 19	18 & 19	18	19
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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 3: Association between Access to the Pill and Abortion (Distance) and Age and Race-specific Birth Rates, 1968-75

			Whites			
Age	15-21	15-18	19-21	18 & 19	18	19
	(1)	(2)	(3)	(4)	(5)	(6)
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 4: 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, 1st-stage		142.3		84.4		100.4
Partial R-sq, 1st stage		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.