THREE ESSAYS ON REPRODUCTIVE HEALTH POLICIES AND THE ECONOMICS OF FERTILITY AND MARRIAGE

by

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Abstract

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Adviser: Dr. Theodore J. Joyce

This dissertation is composed with three essays, each of which empirically examines the effects of reproductive health policy on marriage, fertility and risky sexual behavior. The first essay provides rigorous tests of the Akerlof, Yellen and Katz's (1996) hypothesis that legalization of abortion in the early 1970s changed young women's marriage decisions by making shotgun marriage unnecessary in the event of premarital pregnancy. The essay empirically investigates the role of greater abortion access in explaining changes in marriage rates, the age at first marriage and the probability of a shotgun marriage. The essay finds that the increase in abortion availability during the 1970s significantly reduced teen marriage rates and raised the age at first marriage. Empirical evidence also lends support to the Akerlof et al.'s hypothesis that legalization of abortion caused teenage women to be less likely to marry in response to premarital pregnancy.

The second essay uses unique data on abortions performed in New York State from 1971-1975 to analyze the impact of legalized abortion in New York on abortion and birth rates of non-residents. The essay demonstrates that women travelled hundreds of miles for a legal abortion before *Roe*. Abortion rates declined by 12.2 percent for every

hundred miles a woman lived from New York in the years before *Roe.* Each abortion was associated with approximately 0.60 fewer births among residents in states nearest to New York. The results suggest that if recent legislative policies were to eliminate abortion providers in some states, the change in population measures of birth and abortion rates would be small, but that they would have more substantial effects on the birth rates of teens and less advantaged women.

The third essay tests whether the easier pharmacy access to emergency contraception (EC) induced teenager and young unmarried women to change their sexual risk-taking behavior in a way that leads to an increase in sexually transmitted diseases (STD) and abortions. Using synthetic control method, the essay evaluates the causal effect of easier access to EC on rates of gonorrhea and abortions in Washington State. The State approved pharmacy sell of EC in 1998 as part of a pilot program ten years prior to FDA's decision. The results indicate that Washington's pilot program had little effect on the prevalence of STD and abortion.

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1.1 Introduction

This year marks the 40th anniversary of the U.S. Supreme Court decision of *Roe v. Wade,* the decision that legalized abortion in the United States. It is hard to overstate its divisive impact on U.S. politics broadly and on the abortion service industry specifically. Abortion clinics have been firebombed, physicians murdered, and abortion patients frequently walk a gauntlet of protesters on their entrance to a facility (Jacobson and Royer 2011). And yet despite the controversy and violence, and despite the advances in hormonal contraceptives, morning after pills, and the wider use of condoms among youth, induced abortion remains a common method of fertility control. The number of abortions peaked at approximately 1.61 million per year in 1990 but there were still over 1.21 million abortions in 2008 (Jones and Kooistra 2011). Approximately 43 percent of unintended pregnancies are voluntarily terminated (Finer and Zolna 2006).

But there is uncertainty as to the future status of legalized abortion in the U.S. Since *Roe*, many states have passed laws that reflect a widespread antipathy towards abortion on demand.[1] Numerous analysts believe that the Supreme Court is but one vote away from overturning *Roe*. But even if *Roe* is not overturned in the near future, the availability of abortion services is evolving rapidly. Since 2010, there has been an unprecedented increase in the regulations of abortion services (Gold and Nash 2012).

[1] These include financing restrictions, parental consent for minors, mandated counseling and waiting periods, required ultrasounds, unnecessary building codes for clinics, requirements of local admitting privilege for physicians, declaration that life begins at conception or when a fetal heartbeat is heard, and gestational limits based on when a fetus can presumably feel pain.

The lone abortion provider in Mississippi and North Dakota, for instance, may each close under regulatory pressure. Virginia recently enacted legislation that would require existing abortions clinics meet the building standards of newly constructed hospitals. Texas may soon follow. Similar regulatory requirements in Kansas and Missouri have been temporarily enjoined. If these supply-side policies are enforced, many abortion clinics will close and the distance women will have to travel to terminate a pregnancy will increase substantially in many parts of the south and Midwest (Joyce 2011).

And yet, the impact of changes in the supply of abortion providers is not well known. In the years since *Roe* economists have estimated the association between abortion rates and the availability of abortion services. In each analysis abortion rates are regressed on the number or presence of the abortion providers in a county or state (Matthews, Ribar, Wilhelm 1997; Blank, George and London 1996; Haas-Wilson 1996). The maintained assumption is that the availability of abortion services is exogenous to use. In the one exception, researchers instrumented the natural log of abortion providers with the log of hospitals and non-OBGYNs in a state (Blank, George and London 1996). However, the exclusion restrictions were questionable by current standards and use of log physicians and hospitals instead of per capita measures was vulnerable to spurious scale effects.

The best evidence as to the effect of dramatically increasing the supply of abortion services comes from changes in birth rates before and after legalization in the early 1970s (Levine et al. 1999; Levine 2004; Angrist and Evans 1999). Results from these influential studies have proven to be robust and the difference-in-difference research design has been the basis for much subsequent work. And yet, without data on abortions in the pre-*Roe* era, it has not been possible to know the impact of early legalization on abortion rates, the relationship between abortion and birth rates, or even the total effect of legalization on fertility. The latter holds because the effect of legalized abortion on birth rates extended well beyond states in which abortion became available on demand. Literally tens of thousands of women traveled to New York for an abortion in the years before *Roe.* This movement is dramatically illustrated by the map in Figure 1. The number in each state is the abortion rate for residents of the state that were *performed in New York* in 1971-1972, two years before the Supreme Court decision in *Roe*. For instance, there were 7.6 abortions to residents of Michigan per 1000 women 15-44 obtained in New York. In absolute numbers, 29,227 women traveled from Michigan to New York for an abortion in 1971-72.[2]

In this study we return to the period just before and after *Roe* to analyze changes in the availability of abortion services on use. The legalization of abortion in New York in July of 1970 provides a plausibly exogenous change in the availability of abortion services to non-residents of the State moderated in part by distance. A second supply shock occurred with *Roe* in January of 1973 as abortion providers became available in every state obviating most travel to New York. We exploit both these changes to identify the effect of access to abortion in New York on use. We also estimate the direct relationship between abortion and birth rates focusing on residents of states within the New York "catchment" area. The analysis is made possible by re-discovered data on abortions performed in New York State by age, race, year and state of residence in the years before *Roe*. Although the analysis is limited geographically, the data are matchless and provide new insights as to the impact of legalized abortion on

[2] Authors' tabulations of data from the New York State Department of Health. See Table 1.

the abortion and birth rates of women from states where abortion remained illegal. Because similar abortion data are not available nationally, we take a less direct but broader approach to the question of abortion availability and use by examining the association between age- and race-specific birth rates with distance to nearest legal abortion provider in any state from 1968 to 1975. We use the results from the three analyses to provide a more detailed assessment than has been previously possible of the effect of legalized abortion on abortion and birth rates in the U.S. in the years just before and after *Roe.*

We find a robust association between distance to New York and resident abortion rates in the years before *Roe*. Abortion rates as measured by abortions performed in New York fell 12.2 percent for every hundred miles a woman lived from the state. The decline was greater for nonwhites than for whites. We also find that abortion rates are inversely related to birth rates in the years before *Roe*. The story that emerges from the national data is that early legalization of abortion in years before *Roe* had a much larger impact on fertility than the years after *Roe.* Marginal decreases in distance to the nearest abortion provider in the years immediately after *Roe* were associated with only a modest reduction in birth rates. We conclude that recent efforts by states to limit the supply of abortion providers will have only minor effects on population measures of birth and abortion rates, but they will impact younger women and those without resources to adjust.

1.2 Background

1.2.1 Impact of legalized abortion

Early studies on the impact of legalized abortion were largely descriptive, limited to one or a few states, or they did not account for ongoing trends in fertility (Melton et al. 1972; Smith et al. 1973; Paktar et al. 1973; Sklar and Berkov 1974; Quick 1978; Joyce and Mocan 1990). Levine et al. (1999) and Angrist and Evans (1999) were the first to provide a more comprehensive analysis of legalized abortion on fertility rates across all 50 states and over a longer period. Both studies used a difference-in-difference (DD) framework by comparing variation in fertility rates in states that legalized abortion or reformed their abortion laws in the years before *Roe* relative to states in which abortion remained illegal. Levine et al. (1999) analyzed changes among all women and then separately by age whereas Angrist and Evans (1999) focused on changes in teen fertility. Both studies found that birth rates declined by approximately 4 percent more in the early legalizing or reform states relative to the states in which abortion did not become legal until *Roe*. Both studies also found that birth rates of nonwhites fell more than those of whites. Neither study analyzed changes in abortion rates directly due to a lack of data. However, Levine et al. (1999) reported that birth rates fell less among women who lived more than 750 miles relative to women who lived within 750 miles of an early legalizing state. The association suggested that travel distance and abortion rates were inversely related.

The difference-in-difference estimator employed by Levine et al. (1999) and Angrist and Evans (1999) provides unbiased estimates of the *relative* changes in birth rates in states that legalized or reformed abortion laws relative to states in which abortion remained illegal. But the DD cannot estimate the absolute decline in birth rates in the non-legalizing states induced by legislation in New York or California. The limitation of the DD in this context is illustrated in Figure 2. Birth rates for women 15 to 44 years of age are plotted from 1968 to 1975, a period roughly two years before and after the legalization of abortion in the U.S. Birth rates are stratified as follows: those in which abortion was legally available on demand prior to *Roe* (Alaska, California, District of Columbia, Hawaii, New York and Washington, the repeal states); the 12 states within the New York "catchment" area; states that had reformed their abortion laws prior to *Roe*; and the remainder. All four series move in tandem. Birth rates are relatively flat from 1968 to 1970 and then decline rapidly to 1973 at which point the rate of decline flattens or moderates. Estimates from previous studies that used a DD design treated all non-repeal states as the control group for repeal states from 1971 to 1973 (see Gruber Levine and Staiger 1999). But the coincident decline in birth rates among women in the non-repeal states, as shown in Figure 2, suggests that trends in birth rates in the non-repeal states may not provide the desired counterfactual. Data on abortions to non-residents of New York performed in the state not only confirm this conjecture, but they provide an alternative means of estimating the broader impact of legalized abortion on fertility in the U.S. in the years before *Roe*.

In fact, access to abortion services in the years before *Roe* was more extensive and at the same time more variable than is captured by a zero-one indicator of legality or reform. For example, none of the aforementioned studies considered the District of Columbia (DC) as an early legalizing or reform state.[3] And yet, in 1972 there were 38,868 reported legal abortions in the District, the most of any state after New York and California. Moreover, there were more abortions to non-residents performed in DC (21,101) than there were to non-residents performed in California (20,201). In addition,

[3] The exceptions are Joyce (2004; 2009) and Myers (2012).

states that reformed their abortion laws before *Roe* but were not considered early legalizing states also varied greatly in the number of abortions that were performed and the proportion obtained by non-residents. Maryland and Georgia both reformed their abortion statutes and yet the abortion ratio in Maryland in 1972 (178 abortions per 1000 live births) was over five times greater than in Georgia (29 abortions per 1000 live births). The abortion ratio in Kansas, another reform state, was double that of Maryland (369 vs. 178), but 63 percent of abortions in Kansas were to non-residents, whereas only 2 percent of abortions in Maryland were to non-residents (Center for Disease Control 1974).

In summary, the patchwork of legal abortion services in the years before *Roe* has made it difficult to isolate the effect of legalization on reproductive outcomes. New York and California were not the only jurisdictions in the continental US where non-residents could obtain abortions. [4] As a result, previous studies may have underestimated the change in fertility associated with early legal access. In this study, we first estimate the impact of abortion legalization in New York on the abortion rates of non-residents obtained in New York from the period before and after *Roe*. We then focus on a subset of states for which New York was the likely source of legal abortion services in the period before *Roe* and estimate the direct association between birth and abortion rates We use these estimates to demonstrate the impact of legalized abortion in New York on the birth rates of women in non-repeal states.

1.2.2 Abortion availability and use

[4] Washington, an early legalizing state, had a residency requirement which greatly limited access to non-residents.

As noted above, the standard analysis of abortion availability and use has included regressions of abortion rates on the number of abortion providers per capita, the logarithm of abortion providers, or the percent of women in counties with an abortion provider (Matthews, Ribar, Whilhelm 1995; Blank, George and London 1996; Haas-Wilson 1996). In each case the coefficient on abortion availability from OLS regressions was positive and highly significant ($p<.001$). But interpretation of these estimates is hampered by the simultaneous determination of supply and demand. It is unclear, for example, whether abortion rates would fall if the number of abortion providers in New York suddenly declined by, say, 25 percent.[5] One recent study used quasi-experimental evidence to analyze changes in abortion rates given a sudden change in availability. In 2004, Texas required that all abortions after 15 weeks be performed in a hospital or ambulatory surgical center. At the time, not one of the 54 free-standing clinics met the standard. Abortions after 15 weeks fell by 64 percent to residents of the State in the first year of the law despite a threefold increase in Texas residents obtaining late abortions in nearby states (Colman and Joyce 2011). However, less than five percent of abortions to resident of Texas were performed at 16 weeks or more gestation. And although increased distance to the nearest late-term abortion provider appears to have been a deterrent, the generalizability of the findings to the much broader population of pregnant women is unclear.

In this study we use the two supply shocks following early legalization of abortion in New York and then national legalization with *Roe* to identify the effect of distance to a legal abortion provider on abortion rates. A strength of the design is that these shocks

[5] Jacobson and Royer (2011) show that abortions are largely unchanged when an abortion provider is closed due to arson or violence.

mitigate issues of policy endogeneity. Many state legislatures, for example, had no intention of legalizing abortion on demand in 1970. And yet, New York's sudden passage meant that women in non-legalizing states had unanticipated access to legal abortion services. With the U.S. Supreme Court decision in *Roe*, travel to New York for an abortion diminished rapidly, as we show below, and it fell more precipitously the further a woman resided from New York. However, after 1972 we can only test whether non-resident abortions were less likely to be performed in New York and not whether the local availability of abortion services caused resident abortion rates to rise.

There exist no national data on abortions by age, race and state of residents in the years before or even after *Roe*. Thus, we associate age- and race-specific births rates from 1968 to 1975 with distance to nearest legal abortion provider so as to compare findings from New York, albeit indirectly, with those based on national natality data.

In sum, we return to the legalization of abortion in the early 1970s for several reasons. First, *de facto* and *de jure* legalization of abortion in the repeal states may have had a greater impact on fertility nationally than has previously been estimated. Second, re-discovered data on abortions performed in New York provide new insights as to the association between the availability and use of abortion services and its direct impact on fertility. And lastly, the relationship between locally provided abortion services and abortion rates is of renewed interest as numerous states try to severely restrict the supply of abortion services.

1.3 Empirical Implementation

1.3.1 Data

1.3.1.1 Abortions performed in New York

Data on abortions performed in New York come from the New York State Department of Health. Analysts form the State provided aggregate data on abortions performed in New York from 1971 to 1975 by state of residence, age (<20, 20-24, 25+), race (white, nonwhite) and year. However, the age categories differed slightly stratified by race (<20, 20-29, 30+). To appreciate the exceptionality of these 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 Centers for Disease Control and Prevention (CDC) annual surveillance summaries report abortion by state of *occurrence* cross-tabulated by age or race but not by state, age and race. The Guttmacher Institute's survey of abortion providers collects data on the total number of abortions by state of occurrence in selected years. The Guttmacher Institute *estimates* the distribution of abortions by state of residence and age based on data from the CDC. 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. As a result, abortions to residents of one state that occur in another are rarely reported back to the state of residence. In sum, the New York State abortion data are matchless not only because they pre-date *Roe*, but because they are even more detailed than abortion data currently collected.

The focus on New York is driven only partly by the availability of data. New York was the overwhelming destination for women wishing to terminate a pregnancy in the pre-*Roe* years. In 1971, for example, abortion on demand was effectively available in

Alaska, California, the District of Columbia, Hawaii, New York and Washington. [6] Eighty-seven percent of the 480,259 reported legal abortions in the U.S. were performed in these 6 jurisdictions, but 84 percent of all known abortions obtained outside a woman's state of residence were performed in New York. Table 1 lists the number of abortions by state of residence as reported by the Centers for Disease Control in 1971 and 1972. The second column under each year shows the number and the third column the proportion of abortions to residents of the state obtained in New York. With relatively few exceptions, if the state had not legalized or reformed its abortion laws, then the vast majority of abortions to residents of the state were performed in New York. The exceptions have plausible explanations. For instance, Iowa, Missouri, Nebraska and Oklahoma all border Kansas, a reform state in which 63 percent of abortions were to non-residents. There are also changes between 1971 and 1972. Texas, for instance, reported 2,558 abortions in 1971, 92 percent of which were obtained in New York. In the next year, there were 16,022 reported abortions to residents of Texas but only 7 percent were obtained in New York.[7]

We use the detailed data on abortions from New York in two ways. First, we associate resident abortion rates performed in New York with distance to New York

[6] 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). By 1970, 11 states, AR, CO, DE, GA, KS, MD, NM, NC, OR, SC and VA had reformed their laws following guidelines outlined by the American Law Institute (ALI) which allowed women to terminate a pregnancy even if the mother's life was not endangered (Centers for Disease Control 1971). Evidence suggests these ALI reforms had no significant impact on birth rates (Levine et al. 1999).

[7] Another anomaly occurs when the CDC reports fewer abortions to residents of a state than does New York. We cannot explain this discrepancy since the CDC surveillance is ostensibly collecting data from all reporting states on abortions performed in a state and assigning women to their state of residence (CDC 1972, 1974). For instance, there were no reported legal abortions to residents of Michigan obtained in Michigan in 1972. The CDC reports 14,626 abortions to residents of Michigan obtained in other states in 1972. However, the New York State Department of Health alone reports 15,522 abortions to residents of Michigan obtained in the state.

from 1971-75. This exploits two exogenous changes in the availability of abortion services. Legalization in New York prior to *Roe* induced many women to come to the State to terminate their pregnancies. But then national legalization with *Roe* rendered such travel largely unnecessary. The change in these flows before and after *Roe* helps calibrate the importance of travel distance on the use of abortion services.

We then use the data from New York to associate age and race-specific birth rates with age and race-specific abortion rates. However, in these analyses we limit the sample to 1971-72 because abortions performed in New York are primarily relevant to resident birth rates in the years before *Roe* among residents of states in the New York "catchment" area.[8]

1.3.1.2 Distance to Abortion Provider

To proxy the availability of abortion services in New York we compute the straight line distance in miles from the population centroid in each county to the nearer of Buffalo, New York or New York City. We then average the county distances within each state weighted by the population of women 15 to 44 years of age in the county to arrive at the average distance to the nearest abortion provider in the state. For residents of New York, we compute the average distance from the population centroid of the county of residence to nearest county with an abortion provider based on the distribution of abortion providers in 1973 within the state. That was the first year the Guttmacher Institute collected data on the number of abortion providers by county. We average the county-level distances weighted by the county-level population of women 15 to 44 years

[8] For example, distance from New York is associated with lower abortion rates but it could also be associated with lower birth rates as women beyond the New York "catchment area" are more likely to obtain abortions in their own or other states. This is clearly true following *Roe*.

of age to arrive at the average distance to the nearest abortion provider within New York.

The average distance in hundreds of miles from each state to the nearest of either Buffalo, New York or New York City is displayed in Figure 3. We have organized states into three groupings. We assume that women from the 13 darkest-colored states were the most likely to obtain legal abortions in New York prior to *Roe.* This is based on proximity and the proportion of all known resident abortions obtained in New York (see Table 1). The mean distance to New York was 233 miles among the 12 states excluding New York ranging from a low 35 miles in New Jersey to a high of 506 miles in Illinois. The lightest colored states are states that either repealed or reformed their abortion laws prior to *Roe* (Arkansas, California, Colorado, Delaware, District of Columbia, Georgia, Kansas, Maryland, New Mexico, North Carolina, Oregon, South Carolina, Virginia and Washington). For women in the remaining states, New York was the most likely destination for an abortion but not the only source of legal services (see for example Iowa and Minnesota in Table 1).

We also compute distance to the nearest legal abortion provider in any state from 1970 to 1975. From 1970 to 1972 we compute distance from a woman's county of residence for those in the continental US to the nearest of either Los Angeles or San Francisco in California, and Buffalo or New York City in New York. We assumed residents of Maryland, Virginia and Delaware accessed services in Washington DC.[9] From 1973 to 1975 we used the Guttmacher Institute's abortion provider survey which details the counties in each state and year that had an abortion provider. We then

[9] Washington State had a 90-day residency requirement rendering services essentially inaccessible to non-residents. Alaska and Hawaii were relevant primarily for their own residents. The District of Columbia mainly served residents from the surrounding states (see Lader 1973, p. 115).

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.[10] 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.

1.3.1.3 Birth and other data

Data on births are from the National Center for Health Statistics national natality files.[11] We generate age- and race-specific birth rates by dividing the number of births by the number of women in the relevant state, year, age and racial group. We compute annual births rates based on the number of births from July of year "t" to June of year "t+1" in order to align them by year of conception with abortion rates that are measured by calendar year.[12] Thus, the birth rate for 1970 is births from July 1970 to June 1971 divided by the population in 1970. Population is from the Surveillance Epidemiological and End Results (SEER) from the National Cancer Institute as measured on July 1 of each year. We also include state controls for the per capita income, the percent of nonwhite females 15 to 44 years of age and the unemployment rate as well as indicators of whether states allowed women less than 21 to obtain the contraceptive pill without parental consent.[13]

[10] Delaware and the District of Columbia had providers in every county and thus zero distance. We substituted the minimum distance (0.62 miles) from the sample so as not to lose these observations when using logarithms.
[11] Natality vital statistics were obtained from the website of the National Bureau of Economic Research. http://www.nber.org/data/vital-statistics-natality-data.html
[12] Ninety percent of all abortions with known gestation in 1971 were performed within the first four months of pregnancy (CDC 1972), approximately six months earlier than birth from the same conception cohort.
[13] We thank Phil Levine for the state-level covariates and Melanie Guldi for sharing her coding on access to the pill (see Guldi 2008).

1.3.2 Statistical models

1.3.2.1 Abortion in New York before and after Roe

We estimate two models with New York data. In the first we analyze the association between resident state abortions performed in New York and distance to either Buffalo or New York City from 1971-75. In the second set of regressions we estimate the direct effect of abortion rates on birth rates in the two years before *Roe.*

We first regress abortion rates by state of residence in the 48 continental states and the District of Columbia on distance to New York from 1971-1975. We include only abortions performed in New York. As shown in Figure 1, the further women resided from New York, the lower the abortion rate. Exceptions include states in which abortion was legal on demand or states in which reforms of abortion statutes permitted hospital committees to approve induced terminations under selected circumstances (see Table 1). After *Roe* in 1973, travel to New York for an abortion fell off rapidly. Figures 4 and 5 show resident abortion rates for abortions obtained in New York in 1973 and 1975, respectively. However, even in 1973, proximity to New York mattered. The closer a woman lived to New York, the more likely she was to obtain abortion services in the State. For instance, the rate of abortions obtained in New York for residents of Connecticut fell from 9.8 in 1972 to 6.0 in 1973, a decline of 39 percent. In Michigan, by contrast, the rate fell from 8.0 to 1.4, an 83 percent decline (Figure 4). At the same time, the total resident abortion rate in Michigan rose to 18.3 in 1973 (Forrest, Sullivan and Tietze 1979). By 1975, virtually no one that resided in a state that did not border New York obtained an abortion in the state (Figure 4). To capture these changes in access on abortion rates, we estimate the following regression.

$$\text{(1)} \quad \text{Abrate}_{jt} = \alpha_1 D_j * 7172 + \alpha_2 D_j * Ref * 7172 + \alpha_3 D_j * \text{Rep} * 7172 + \alpha_4 D_j * 7374 + \alpha_5 D_j * Ref * 7374 + \alpha_6 D_j * \text{Rep} * 7374 + \mathbf{X}_{jt}\boldsymbol{\beta} + \lambda_j + \tau_t + e_{jt}$$

Let *Abrate*$_{jt}$ be the abortion rate for a specific age or racial group in state *j* and year *t* that were performed in New York; let D_j be distance to an abortion provider in New York; let *7172* be one if the year is 1971 or 1972 and zero otherwise*; 7374* is the equivalent dummy for 1973 and 1974; the omitted year is 1975. We use both the natural log of distance as well as distance entered in hundreds of miles.[14] *Ref* is 1 if the state had reformed its abortion laws and *Rep* is one if the state had repealed its abortion laws. We include three controls for state characteristics (**X**): the insured unemployment rate, per capita income, the percent of the population that was nonwhite. The **X** matrix also includes the full set of first-order interactions.[15] We also include a dichotomous indicator of whether state policies allowed women less than 21 years of age to obtain the contraception pill without parental consent (Guldi 2008). The last two terms capture the main state (λ_j) and year (τ_t) fixed effects.

Distance to New York does not vary over time and can only be included as an interaction term in models with state fixed effects. Thus, the coefficient, α_1 captures the difference in abortion rates among non-residents of New York in non-repeal and non-reform states in 1971-72 relative to 1975. We expect α_1 to be negative. The association between distance and abortion rates in reform states in 1971-72 relative to 1975 ($\alpha_1 + \alpha_2$) should also be negative but the sum should be less than α_1 in absolute value since there was some access to legal services in reform states which may attenuate the effect of distance to New York. The association between distance and

[14] We use 1975 as the reference year since by then, travel to New York for an abortion would have reached its post-*Roe* steady state, given the growth in local abortion providers.

[15] These include Ref*7172, Rep*7172, NY*7172, Ref*7374, Rep*7374, NY*7172, and NY*7374.

abortion rates in repeal states in 1971-72 relative to the 1975 ($\alpha_1 + \alpha_3$) is ambiguous and may be statistically insignificant since women in repeal states would have had no need to travel to New York. The coefficient on α_4 captures the association between distance to New York and abortions to non-residents in non-repeal and non-reform states performed in New York after national legalization relative to 1975. There would be no association if non-residents of New York obtained all abortions in their own state or a state other than New York. However, this depends on the speed with which abortion providers outside of New York were able to offer services after *Roe*. As noted previously women who resided in states that border New York continued to travel to New York after *Roe* while the market for abortion services developed locally (see Figure 4). However, by 1975 travel to New York for an abortion was rare except for women in Connecticut and New Jersey (Figure 5). The sum of coefficients, $\alpha_4 + \alpha_5$ and $\alpha_4 + \alpha_6$ show the association between distance and abortion rates performed in New York in 1973-74 relative to 1975 among residents of reform and repeal states, respectively.[16]

1.3.2.2 Association between Abortion and Birth Rates: Evidence from New York

Unlike distance, the abortion rate varies by state, year age and race and may be a better proxy for the availability of abortion services than distance in the pre-*Roe*

[16] We view distance to New York as the correct price for the cost of obtaining abortions in New York. To the extent that New York was the nearest legal abortion provider in 1971 and 1972, then we capture the effect of distance on resident abortion rates. After 1972, New York is no longer the nearest legal abortion provider for almost all non-residents, but it remains the correct price for abortions obtained in New York. Thus, coefficient on distance in 1973-74 remains negative as we show below since the further a woman lived from New York the less likely she was to use providers in New York. However, the coefficient on distance after 1972 no longer captures the effect of distance on resident abortion rates.

years.[17] Thus, we also estimate the direct effect of the abortion rate on the birth rate in 1971 and 1972 as follows:

(2) $$Brate_{ajt} = \alpha_0 Abrate_{ajt} + \sum \varphi_a A_a + \mathbf{X}_{jt}\boldsymbol{\beta} + \lambda_j + \tau_t + e_{ajt}$$

where $Brate_{ajt}$ is the resident birth rate of age group a, in state j and year t for and $Abrate_{ajt}$ is the resident abortion rate of abortions obtained in New York in also by age, state and year.[18] We estimate equation (2) for all three age groups, two years and 13 states (n=78) for which we consider New York the relevant "catchment area" in the years before *Roe* (See Figure 3). We also estimate equation (2) for whites and nonwhites separately.

Each abortion should replace less than one birth given fetal loss, renewed exposure to pregnancy, and substitution of legal for illegal abortions (Joffe 1995; Tietze 1973). Thus, we expect the coefficient on the abortion rate, α, to be less than 1.0 in absolute value. However, not all legal abortions were obtained in New York prior to *Roe* even in the 12 states for which New York was the most relevant market (Table 1). Another challenge is the potential measurement error in abortions. Some legal abortions were not reported to the CDC as surveillance systems were being developed. As a result, estimates of α could be greater than one in absolute value if the number of reported legal abortions obtained in New York was only a proportion of all the new legal abortions that occurred in 1971-72, especially for states . Finally, the latent demand for births and abortions are likely related and imbedded in a more complex model that includes determinants of sexual activity, contraception and even marriage. One approach to the potential measurement error and endogeneity concern is to use

[17] Goldin and Katz (2002) also use the abortion rate as a proxy for the availability of abortion services in the 1970s as an alternative to a dichotomous indicator of legalized abortion.
[18] Note birth rates are constructed with a six-month lag (see Section III.4).

distance to New York as an instrument for the abortion rate. Yet, distance can only be entered interacted with other covariates in models with state fixed effects. The limited variation with only two years of data yielded implausible results in a number of specifications (see Joyce, Tan and Zhang 2012, Table 3). Thus, we present estimates obtained by ordinary least squares in which the coefficient on the abortion rates is best interpreted as proxy for the availability of abortion services.

1.3.2.3 Association between Abortion Availability and Birth Rates

We cannot use distance to New York to explain birth rates nationally since women could obtain legal abortions not only in other repeal states but to a lesser extent in reform states prior to *Roe* and in their own state after *Roe*. Thus, increasing distance from New York should result in fewer abortions *performed in New York*, but it could also result in fewer births as women sought abortions closer to their state of residence. Thus, to estimate the effect of abortion availability on birth rates, we regress age- and race-specific birth rates on distance to the nearest legal abortion provider in any state from 1968 to 1975 as follows:

$$
\begin{aligned}
\text{(3)}\quad \mathrm{Brate}_{jt} = {} & \alpha_1 D_j * 7172 + \alpha_2 D_j * R\mathrm{ef} * 7172 + \alpha_3 D_j * \mathrm{Rep} * 7172 + \\
& \alpha_4 D_j * 7375 + \alpha_5 D_j * R\mathrm{ef} * 7375 + \alpha_6 D_j * \mathrm{Rep} * 7325 + \mathbf{X}_{jt}\boldsymbol{\beta} + \lambda_j + \tau_t + e_{jt}
\end{aligned}
$$

The specification is the same as in equation (1) but with two differences. First, birth rates are available electronically since 1968. The extra years yield more power. And second, distance to the nearest abortion provider varies by state and year from 1973 to 1975. As before, distance to the nearest provider (D) is entered in logs and interacted with dummy variables for the years 1970 to 1972 and then 1973 to 1975. The omitted years are 1968 to 1969. We do not weight the regressions by inverse of the female population, but use robust standard error procedures clustered at the state level to correct the standard errors.

1.4 Empirical Results

1.4.1 Graphical analysis

Figures 6A and 6B show the relationship between resident abortion rates in 1971-72 and distance to New York in hundreds of miles in the 48 states and the District of Columbia (left panel) and for the 13-state subsample (right panel). The data include only abortions performed in New York. The fitted line in Figure 6A is from a simple regression of abortion rates on the natural logarithm of distance in hundreds of miles; the fitted line in Figure 6B includes distance in hundreds of miles as the sole covariate. The logarithmic specification provides a tighter fit.[19] The R-squares in Figure 6A are twice as large as their linear counterparts in 6B. Another observation is that coefficients on the log of distance are almost the same in the 49- and 13-state samples (-3.35 vs. -3.57, respectively), but the slopes differ substantially between the two samples in the linear specification (-0.48 vs. -2.90). We also believe the logarithmic specification is conceptually superior. The linear specification forces a constant marginal effect. Thus, a 100-mile increase in travel distance for women who live 900 miles from New York imposes the same additional costs as does a 100-mile increase for women who live 50 miles from New York. However, the cost of flying, travel time, and outlays for an overnight stay should be relatively similar for women that reside 900 or 1000 miles from New York, which is better captured by the logarithmic specification.

[19] A quadratic in distance yielded similar estimates but was less parsimonious than the log specification for it substantially increased the number of interaction terms.

The last observation is that New York appears as a distinct data point. We estimate the average distance to the nearest abortion provider to be less than a mile in New York in 1971-72, which is an order of magnitude smaller than New Jersey at 35 miles, the state with the next smallest distance to an abortion provider. Consequently, we include separate interaction terms between New York and year in the regressions below. As a sensitivity check we estimate models without New York.

1.4.2 Regression analysis

1.4.2.1 Abortion performed in New York

Table 2 presents results from the estimation of equation (1). Each column is from a separate regression. The dependent variable is the age or race-specific resident abortion rate for abortions performed in New York from 1971 to 1975. The coefficient on *Ln Distance*1971-72* shows the change in abortion rates per unit change in the natural log of miles from the nearest abortion provider in New York and pertains to women who resided in a state other than New York but who did not live in a state that had repealed or reformed its abortion laws prior to *Roe* (see Figure 2). To demonstrate the marginal effect of distance in 100-mile increments, we compute the difference in abortion rates for women that resided 283 versus 183 miles from the nearest abortion provider in New York (Table 2, row2). The midpoint, 233 miles, is the average distance to New York in the 13-state sample excluding New York. The mean distance for the full sample is 828 miles. We compute the marginal effect of a 100-mile increase centered on that distance in row 3. As a point of comparison, we also include the constant marginal effect of distance from a separate regression in which distance is entered as the number of miles (in hundreds) instead of in logs (Table 2, row 4).

The overall abortion rate falls by 1.02 abortions per 1000 women 15-44 years of age when distance increases from 183 to 283 miles (column 1, row 2). This represents a decline of 12.2 percent (-1.02/8.37) evaluated at the mean abortion rate of the 12 states for which New York is the most likely site for legal abortions in the years before *Roe*. The change in abortions per 100 miles evaluated at the mean distance for the 49-state sample is -0.28 abortions per thousand women 15-44, a 6.6 percent decline at a mean abortion rate of 4.16. We contrast these changes with those based on a regression in which distance is entered linearly (row 4). The linear estimates are approximately one-third the magnitude of those based on logarithmic specification for women that lived within 280 miles of New York.

Another result of note is that the gradient for nonwhites is over double that of whites. For example, the abortion rate among nonwhites fell by 2.1 abortions per 1000 women or 15.1percent given a mean abortion rate of 13.9. The comparative change among whites was a decline of -0.95 abortions, an 11.6 percent decline evaluated at the mean (Table 2, row 3, columns 5 and 6). To the extent that race captures gross differences in socioeconomic status, then less advantaged women appear more sensitive to the costs associated with travel distance. The other coefficients shown in Table 2 conform to expectations. The association between abortion rates performed in New York and distance to the State falls substantially in 1973-74 as abortion services became available locally with national legalization (Table 2, row5). The map in Figure 4 suggests that distance still mattered somewhat in 1973 (relative to 1975 the omitted category) especially for women in states nearest New York, but by 1975 abortion services in New York were largely irrelevant to non-residents (see Figure 5). The same is true for women in repeal states prior to *Roe* as there is no meaningful association between distance to New York and abortions to residents of California, the District of Columbia and Washington State performed in New York (Table 2, row 8). The association between distance to New York and abortions obtained in New York is somewhat stronger for women who resided in reform states prior to *Roe* (Table 2, row 6).

In summary, estimates in Table 2 make several points: 1) the sudden availability of legal abortion services in New York in 1970 induced many women to travel to the State to terminate their pregnancies; 2) the further a woman lived from New York, the less likely she was to terminate her pregnancy in the State; and 3) the availability of local abortion services starting in 1973 dramatically reduced the likelihood that a woman travelled to New York for an abortion. What is not clear from Table 2 is whether the availability of legal abortion services in New York simply replaced abortions that would have been performed illegally or in some other location. Put differently, would pregnancies or some proportion of pregnancies that were terminated in New York have been carried to term had legal abortion services not been available in New York? One way to address this question is to regress birth rates on resident abortion rates performed in New York. If abortions performed in New York simply replaced more local abortions—whether legal or illegal—then there should be little association with resident birth rates.

1.4.2.2 Association between Abortion and Birth Rates

We present the direct association of birth rates and abortion rates in Table 3. Panel A shows estimates for the continental 49 states including the District of Columbia and Panel B is limited to the New York and the 12 states considered in its catchment area. Among all women in the 49 states, each abortion is associated with a decline in 1.4 births ($p<.01$). For whites and nonwhites, births fall by 0.92 and 0.72 per abortion, respectively (Panel A, columns 2 and 3). The estimate for all women is clearly implausible and likely reflects unmeasured abortions. As shown in Table 1, the further women lived from New York generally the smaller the proportion of resident abortions performed in New York. Thus, in Panel B we present estimates of equation (2) for the 13-state sample which we consider a better approximation of the New York catchment area. The goal is to lessen the bias associated with abortions not performed in New York. As can be seen in Panel B of Table 3, all the estimates are smaller in absolute value than their counterparts in the 49-state panel, but only in the model for all women is the coefficient on the abortion rate statistically significant (Panel B, column 4). These estimates are more plausible and likely reflect the more accurate count of abortions to residents of states in the New York catchment area. Nevertheless, they remain too large in absolute magnitude as estimated by demographers, an issue we take up below (Tietze 1973).

1.4.2.3 Regression of Birth Rates on Distance

We have focused on data from New York because of the detailed abortion data and the importance of New York in the pre-*Roe* years. In this section we provide estimates of the association between distance to nearest legal abortion provider in any state and age- and race-specific birth rates. To appreciate the discrete changes in the availability of abortion services, Figure 7 shows distance to nearest legal abortion provider by four-state groupings. As is apparent the decrease in distance between

1972 and 1973 is huge, from roughly 620 miles on average to 22 miles in the post-*Roe* period. Importantly, this sharp decrease in distance is not commensurate with a sharp drop in birth rates. As shown in Figure 2, the most significant declines in birth rates in all states occurred between 1970 and 1972. There is a clear leveling off in trend from 1973 to 1975.

The association between age- and race-specific birth rates and distance to nearest abortion provider is shown in Table 4. The top panel includes all 49 states and the lower panel is limited to the 13 states we have characterized as the New York catchment area (Figure 3). We allow the coefficient on the log of distance to vary between the pre- and post-*Roe* years since the average distance to a provider is so disparate. The association between distance and birth rates is mostly positive but only for teens is the association statistically significant in non-repeal and non-reform states. To compare the magnitudes of the changes associated with distance between the two periods we compare marginal effects evaluated at the mean distance in each period. Thus, teen birth rates would be expected to be 7.5 per thousand teens in 1970-1972. This represents a decline of 12 percent of based on the mean teen birth rate of 62.3.[20] The change from 1973-75 is much smaller when evaluated at the mean distance to an abortion provider in that period. Thus we would expect the teen birth rates to fall by 0.36 births per 1000 given a mean distance of 23 miles. The decline in birth rates is consistently positive in the repeal states in 1970-72 but there is no association in post-*Roe* period.

---

[20] Distance, in hundreds of miles, is specified in logs. Thus $\delta y/\delta \ln d = (\delta y/\delta d)*d$. We use the mean of distance in each period to evaluate these marginal effects. Thus, the estimated change in teen birth rates is 7.5 per 1000 women 15-19 in 1970-72 given a mean distance of 5.21 in hundreds of miles [1.44*5.21]. The expected change in 1973-75 is 0.36 [1.61*0.23] .

The results for the 13 states in the New York catchment area are more robust. Except for non-whites, the birth rates of each group are positively related to distance to New York in the years before *Roe*. There is no association with distance in the years after *Roe.* If we evaluate the association at the mean of distance then, the abortion rates of all women would be expected to fall by 1.6 and those of teens to fall by 1.3 per 1000 women. These are relative declines of 2.2 and 2.7 percent, respectively.

An advantage of the results from the New York catchment area is that we can use the ratio of the reduced-form estimates of distance from the birth and abortion regressions to provide a rough estimate of the direct association between birth and abortion rates instrumented by distance. For instance, we obtain a ratio of -0.30 if we divide the coefficient on distance in Table 4, Panel B (0.70) by its counterpart in Table 2 (-2.35). The ratio, -0.30, suggests that every abortion led to a decrease of 0.30 births between 1970 and 1972. This is less than one-third the size of the coefficient for all women based on the direct association between birth and abortion rates in Table 3 (column 4). If we use the reduced-form coefficients for white women, we estimate that each abortion is also associated 0.29 fewer births (0.62/-2.17), approximately half the size of the coefficient in Table 3 (column 5). These approximate "IV" estimates should be interpreted cautiously, but they suggest that the OLS estimates in Table 3 overestimate the direct association between birth and abortion rates. [21]

1.4.3 Implications

[21] Tietze (1973) using only New York City (NYC) data estimated that the 65,000 abortion to NYC residents in the first year of the law replaced approximately 18,000 births six months later, a ratio of 0.278, which is very close to the ratio of our two reduced forms.

The story that emerges from these data is that the availability of legalized abortion services has a significant effect on fertility, but marginal changes in the distance to a legal provider has less of an effect. In other words, *Roe v. Wade* was arguably less important for unintended childbearing than was access to services in California, the District of Columbia and especially New York in the years before *Roe*. This is consistent with the pattern of birth rates in Figure 2. As a further illustration, consider the data in Table 5. Column (1) shows total births to women 15 to 44 years of age from 1969 to 1975; column (2) shows the yearly change in births; and column (3) displays total abortions. We can use counts instead of rates since the denominator is the same for both births and abortions. Recall also that we measure annual births from July through June whereas abortions pertain to the calendar year. Below each column are the sums of each column separately for the period before and after *Roe*. The sum of yearly changes in births from 1970-72 is -458,053 while the number of abortions over the same span is 1,247,527. If we use estimates from the New York catchment area and assume that each abortion is associated with -0.97 births (Table 3, column 4), then we would expect a decline of 1.2 million births (-0.97*1,247,527), clearly an overestimate. However, if we use the crude IV estimates and assume that each birth resulted in -0.30 births, then we obtain a predicted decline of 374,000 births, which is much more consistent with the actual decline. A similar exercise is not possible in the post-*Roe* area but the basic summary statistics suggest the impact of *Roe* on fertility was negligible. The sum of yearly changes in births is only -67,001 between 1973-75 despite almost 2.7 million abortions.

The conclusion that *Roe* had a relatively modest impact on birth rates is somewhat at odds with previous work (Gruber, Levine and Staiger 1999; Levine et al. 1999; Levine 2004). These authors argued that national legalization in 1973 led to an equally large decrease in birth rates in the non-repeal states as had occurred in the repeal states in the previous three years.[22] Although these authors were well aware that women travelled to repeal states, we have been able to flesh out this response in more detail. What is clear is that access to legalized abortion in the years before *Roe* induced thousands of women in non-repeal states to travel to repeal states for an abortion which resulted in a substantial decrease in fertility.

1.5 Conclusions

The likelihood that *Roe* is overturned in the near future is remote. Nevertheless, states have imposed new requirements of abortion providers that, if enforced, will increase the distance women have to travel to access services. Indeed, the only abortion clinics in Mississippi and North Dakota are likely to close if courts uphold the requirement that physicians performing abortions at the clinics must have admitting privileges at local hospitals. Such policies could have significant impact on the availability of services in other states in which physicians performing abortions lack admitting privileges at nearby hospitals. Based on data from the pre-*Roe* era, our results suggest that the impact of increased travel distance to a provider on abortion and birth rates will be small at the population level. We expect that the vast majority of women will travel to states in which abortion services remain available. Those most affected by the increase in distance to an abortion provider will likely be the young and

[22] Gruber, Levine and Staiger (1999) refer to this as the "bounceback" effect of *Roe*.

poor. We found a robust association between teen birth and abortion rates and distance to the nearest abortion provider. Whether the relationship between access and use of abortion services observed in the early 1970s is relevant today is open to debate. There have been significant changes in contraceptive technology, access to information via the internet, and travel costs since the early 1970s. To the extent that these advances have lowered the costs of fertility control, then estimates as to relationship between the availability and use of abortion services in 1970s likely provide an upper bound estimate as to the effect of changes in policies today.

Table 1-1: Total Resident Abortions Reported by the CDC and New York State, 1971-72

	1971			1972		
	CDC	NY	% NY±	CDC	NY	% NY
Alabama	1501	957	0.64	2100	828	0.39
Alaska¥	1191	45	0.04	1166	10	0.01
Arizona	416	59	0.14	2865	58	0.02
Arkansas	1061	370	0.35	1555	699	0.45
California¥	103929	236	0.00	106307	152	0.00
Colorado*	4589	662	0.14	5428	238	0.04
Connecticut	7808	6779	0.87	8333	6376	0.77
Delaware*	1667	540	0.32	2193	546	0.25
DC¥	11618	364	0.03	7352	143	0.02
Florida	9235	8847	0.96	11624	8085	0.70
Georgia*	4989	3276	0.66	7070	4149	0.59
Hawaii¥	4127	6	0.00	4534	12	0.00
Idaho	29	24	0.83	20	20	1.00
Illinois	15982	13440	0.84	14091	14353	1.02
Indiana	4989	4766	0.96	5481	5842	1.07
Iowa	2834	1821	0.64	2356	1607	0.68
Kansas*	4017	288	0.07	4843	286	0.06
Kentucky	2268	2184	0.96	3132	2839	0.91
Louisiana	1135	1049	0.92	1210	1269	1.05
Maine	1345	1300	0.97	1690	1848	1.09
Maryland*	10001	1136	0.11	14929	483	0.03
Massachusetts	13230	10757	0.81	17581	14035	0.80
Michigan	14361	13705	0.95	14626	15522	1.06
Minnesota	3351	2510	0.75	2227	2106	0.95
Mississippi	344	617	1.79	870	796	0.91
Missouri	4582	2113	0.46	6953	3699	0.53
Montana	420	402	0.96	172	178	1.03
Nebraska	1093	907	0.83	1797	791	0.44
Nevada	40	37	0.93	1630	36	0.02
New Hampshire	1243	1179	0.95	1483	1595	1.08
New Jersey	21207	20465	0.97	22832	25733	1.13
New Mexico*	4936	41	0.01	1962	75	0.04
New York¥	105642	112778	1.07	100615	116555	1.16
North Carolina*	6147	1703	0.28	11810	1257	0.11
North Dakota	252	229	0.91	148	145	0.98
Ohio	14209	13636	0.96	16666	17067	1.02
Oklahoma	1506	601	0.40	2843	453	0.16
Oregon*	6998	14	0.00	7178	18	0.00
Pennsylvania	20430	13466	0.66	22772	14255	0.63
Rhode Island	1697	1612	0.95	1869	2085	1.12
South Carolina*	2045	1283	0.63	3056	1820	0.60
South Dakota	170	128	0.75	116	91	0.78
Tennessee	2782	2681	0.96	4288	3247	0.76
Texas	2558	2358	0.92	16022	1131	0.07
Utah	51	31	0.61	730	33	0.05
Vermont	766	728	0.95	1052	889	0.85
Viriginia*	6995	2729	0.39	11187	1255	0.11
Washington¥	14425	72	0.00	17809	27	0.00
West Virginia	896	844	0.94	1491	719	0.48
Wisconsin	5310	2010	0.38	3090	2400	0.78
Wyoming	190	72	0.38	269	49	0.18
Total	452607	257857	0.57	503423	277905	0.55

Source: Centers for Diseases Control (1972,1974); authors tablulations of abortions by state of residence performed in New York as collected by the New York State Department of Health. ± Proportion of all CDC abortions obtained in New York. ¥ State repealed its abortion law. * State reformed its abortion law. Shaded states included in the 13-state sample.

Table 1-2: Regressions of Resident Abortion Rates on the Natural Log of Distance to New York, 1971-1975

	(1) All	(2) 15-19	(3) 20-24	(4) 25+	(5) White	(6) NonWhite
row						
1. Ln Distance*1971-72	-2.35***	-2.91***	-3.94***	-1.46***	-2.17***	-4.83***
	(0.32)	(0.53)	(0.50)	(0.17)	(0.37)	(1.01)
2. Δ mile 283 - 183	-1.02	-1.27	-1.72	-0.64	-0.95	-2.11
3. Δ mile 878 - 778	-0.28	-0.35	-0.48	-0.18	-0.26	-0.58
*4. Δ using linear distance**	-0.38***	-0.50***	-0.63***	-0.22***	-0.41***	-0.85***
5. Ln Distance*1973-74	-0.75*	-1.08*	-1.19*	-0.46*	-0.69*	-2.25***
	(0.31)	(0.45)	(0.52)	(0.18)	(0.30)	(0.58)
row 1- row 5	-1.60***	-1.84**	-2.75***	-1.00***	-1.48***	-2.58**
6. Ln Distance*1971-72*Reform[€]	-1.02*	-1.99**	-1.90**	-0.28	-1.11	-2.40
	(0.43)	(0.71)	(0.70)	(0.22)	(0.58)	(1.56)
7. Ln Distance*1973-74*Reform[ƒ]	-0.15	-0.35	-0.23	-0.02	-0.25	-0.88
	(0.19)	(0.31)	(0.30)	(0.10)	(0.26)	(0.64)
row 6 - row 7	-0.87**	-1.64***	-1.67***	-0.26*	-0.85*	-1.52
8. Ln Distance*1971-72*Repeal[¥]	0.50	1.28	1.00	-0.03	0.09	2.57
	(0.65)	(1.05)	(1.05)	(0.37)	(0.76)	(1.82)
9. Ln Distance*1973-74*Repeal[¶]	0.27	0.63	0.56	0.01	0.34	0.99
	(0.28)	(0.45)	(0.45)	(0.16)	(0.33)	(0.69)
row 8- row 9	0.23	0.65	0.44	-0.04	-0.24	1.58
Mean abortion rate 12 states[±]	8.37	11.88	13.96	4.60	8.16	13.94
Mean abortion rate 34 states≠	4.16	6.05	7.05	2.16	4.53	6.67
R2	0.98	0.95	0.98	0.99	0.96	0.97
N	245	245	245	245	220	220

* Except for the estimates in row 4, the figures in each column are from a separate estimate of equation (1) in the the text. The dependent variable is the resident abortion rate for abortions performed in New York from 1971-1975 in 48 states and the District of Columbia. Alaska and Hawaii are not included. Distance is in hundreds of miles. The marginal effect of distance in rows 2 and 3 shows the change in abortion rates associated with an increase of 100 miles between the designated distances. The marginal effect in row 4 is from a separate regression with distance entered linearly instead of in logs. Standard errors adjusted for clustering at the state level are in parentheses.

±Mean abortion rate in 1971-72 in 12 states of the New York "catchment area" (see Figure 2)

≠Mean abortion rate in 1971-72 in all states less New York, reform and repeal states.

¥ ($\alpha1 + \alpha2$); € ($\alpha1 + \alpha3$); ƒ ($\alpha4 + \alpha5$); ¶ ($\alpha4 + \alpha6$) from equation (1) in text.

* p<.05, ** p<.01, *** p<.001

Table 1-3: OLS Regressions of Age-Specific Birth Rates on Abortion Rates, 1971-72

	Panel A: 49 States			Panel B: 13 States		
	All Women	Whites	Nonwhites	All Women	Whites	Nonwhites
	(1)	(2)	(3)	(4)	(5)	(6)
Abortion Rate	-1.41***	-0.92**	-0.72	-0.97*	-0.54	-0.67
	(0.30)	(0.30)	(0.38)	(0.39)	(0.39)	(0.55)
	294	264	264	78	78	78
N	0.93	0.96	0.90	0.96	0.98	0.83

All models include state fixed effects, a year dummy, and state covariates (see equation 2 in text). Birth rates are measured from July 1-June 30 for each year. Abortion rates pertain to the calendar year. The 13 states include: CT, IL, IN, ME, MA, MI, NH, NJ, NY, OH, PA, RI, and VT. Race-specific abortion rates are missing for 5 states: ID, NV, NM, OR and UT. Robust standard errors correct for a general form of heteroskedasticity.

Table 1-4: Regressions of Age-Specific Birth Rates on the Natural Log of Distance to the Nearest Abortion Provider, 1968-1975

	(1) All	(2) 15-19	(3) 20-24	(4) 25+	(5) White	(6) NonWhite
Panel A: 49 States in Continental US						
Ln Distance*1970-72	0.22	1.44**	-1.72	-0.18	0.14	-0.67
	(0.46)	(0.46)	(1.33)	(0.39)	(0.50)	(2.20)
Ln Distance*1973-75	1.43**	1.61*	0.46	1.00*	1.39**	-1.06
	(0.43)	(0.75)	(1.02)	(0.38)	(0.43)	(2.59)
Ln Distance*1970-72*Reform[€]	0.50	0.73	0.78	0.18	0.48	0.33
	(0.55)	(0.68)	(1.22)	(0.35)	(0.49)	(1.47)
Ln Distance*1973-75*Reform[ƒ]	0.63	1.49	0.83	0.17	0.48	2.31
	(0.80)	(0.96)	(1.86)	(0.50)	(0.73)	(2.30)
Ln Distance*1970-72*Repeal[¥]	1.76***	2.41**	2.12	1.24**	1.80*	4.28
	(0.48)	(0.84)	(1.23)	(0.41)	(0.80)	(4.15)
Ln Distance*1973-75*Repeal[¶]	1.08	2.68	0.62	0.73	0.87	-0.41
	(0.90)	(1.92)	(0.50)	(0.69)	(0.83)	(2.90)
Mean Birth Rate	76.90	62.28	142.36	59.61	73.00	105.75
R2	0.97	0.97	0.98	0.97	0.97	0.75
N	392	392	392	392	392	392
Panel B: NY "Catchment Area"- 13 states						
Ln Distance*1970-72	0.70***	0.57**	0.76*	0.59***	0.62**	-2.36
	(0.14)	(0.16)	(0.27)	(0.12)	(0.15)	(2.82)
Ln Distance*1973-75	0.55	-0.35	0.27	0.78*	0.54	-6.19
	(0.37)	(0.47)	(0.98)	(0.34)	(0.33)	(5.26)
Mean Birth Rate	72.15	49.64	131.12	60.33	70.10	104.44

R^2	0.99	0.98	0.99	0.98	0.99	0.70
N	104	104	104	104	104	104

Notes: Panel A include all 49 continental states (Hawaii and Alaska excluded). The average distance to the nearest abortion provider is 521 miles from 1970 to 1972, and 23 miles between 1973 to 1975. Panel B include 13 states that are considered as New York "catchment area". See notes to Table 3 for the list of states . The average distance to the nearest abortion provider in the 13 states is 215 miles from 1970-72, and 9.7 miles from 1973-1975. *Standard error clustered at state level. For panel B, t-statistics based on 11 degrees of freedom.

¥ ($\alpha1 + \alpha2$); € ($\alpha1 + \alpha3$); ƒ ($\alpha4 + \alpha5$); ¶ ($\alpha4 + \alpha6$) from equation (3) in text.

* p<.05, ** p<.01, *** p<.001

Table 1-5: Births and Abortions: 1969-1975

Year	Births	Δ births	Abortions
	(1)	(2)	(3)
1969	3,638,374	--	--
1970	3,697,884	59,510	175,508
1971	3,378,524	-319,360	485,259
1972	3,180,321	-198,203	586,760
1973	3,096,769	-83,552	744,610
1974	3,166,785	70,016	898,570
1975	3,113,320	-53,465	1,034,170
Total 70-72	10,256,729	-458,053	1,247,527
Total 73-75	9,376,874	-67,001	2,677,350

Sources: Births based on authors compilations from national natality files. Annual births measured from July to June; Aborrtions 1970-72 (Centers for Disease Control Abortion Surveillance 1971, 1972, 1974); Abortons 1973-73 (Henshaw and Van Vort, 1992)

Figure 1-1: Average Resident Abortion Rates for Abortions Performed in NY, 1971-72

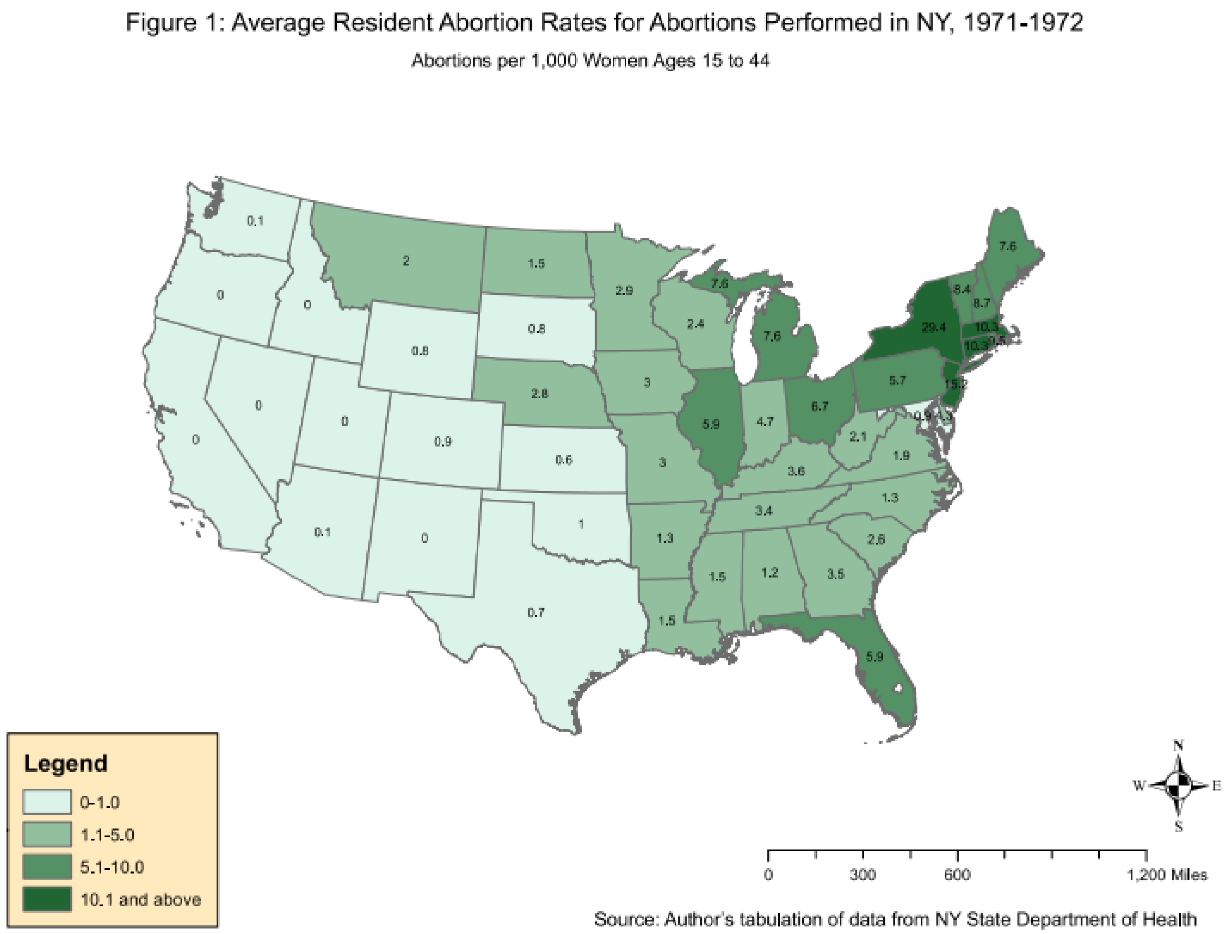


Figure 1-2: Birth Rates of Women 15-44 by Timing of Abortion Legalization

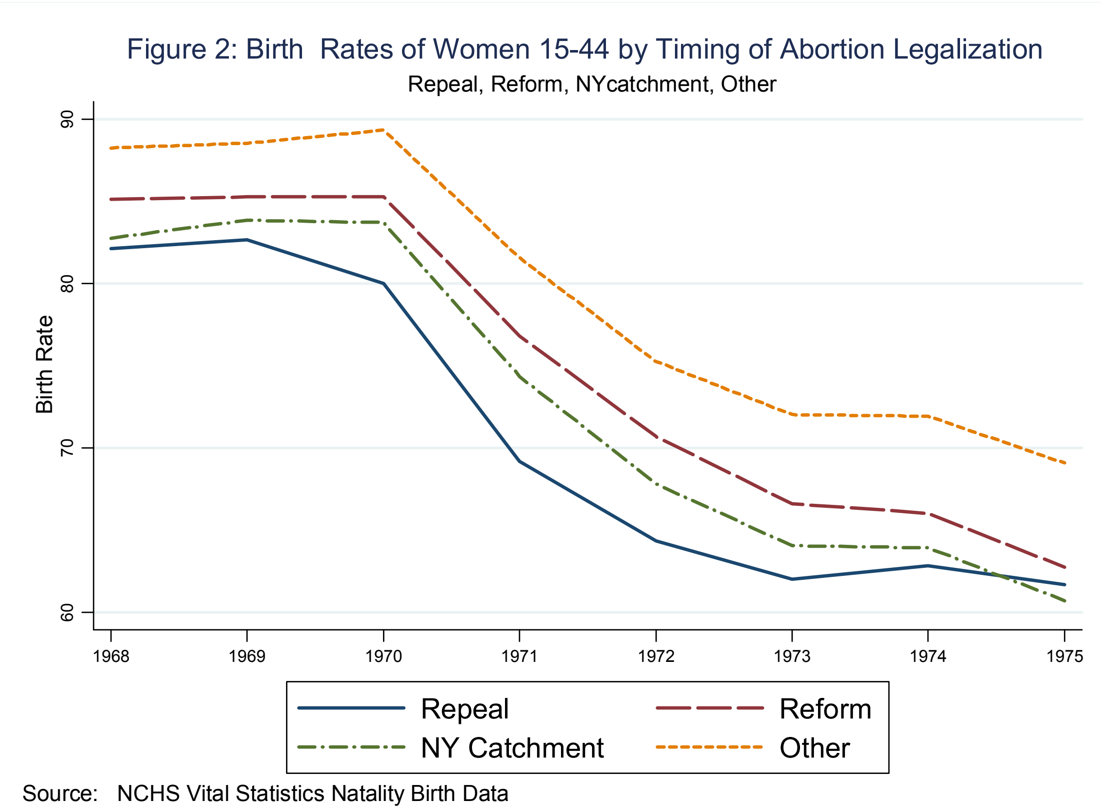
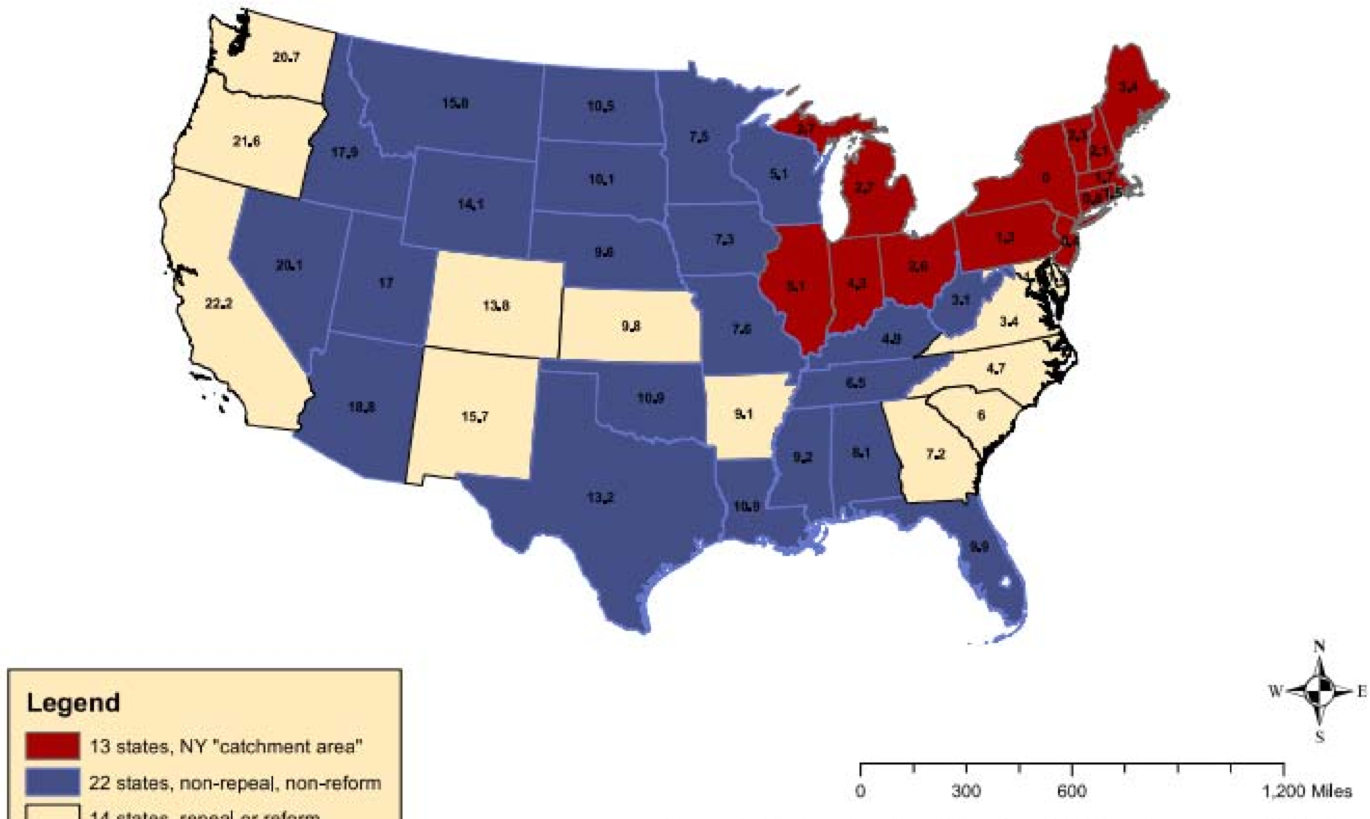
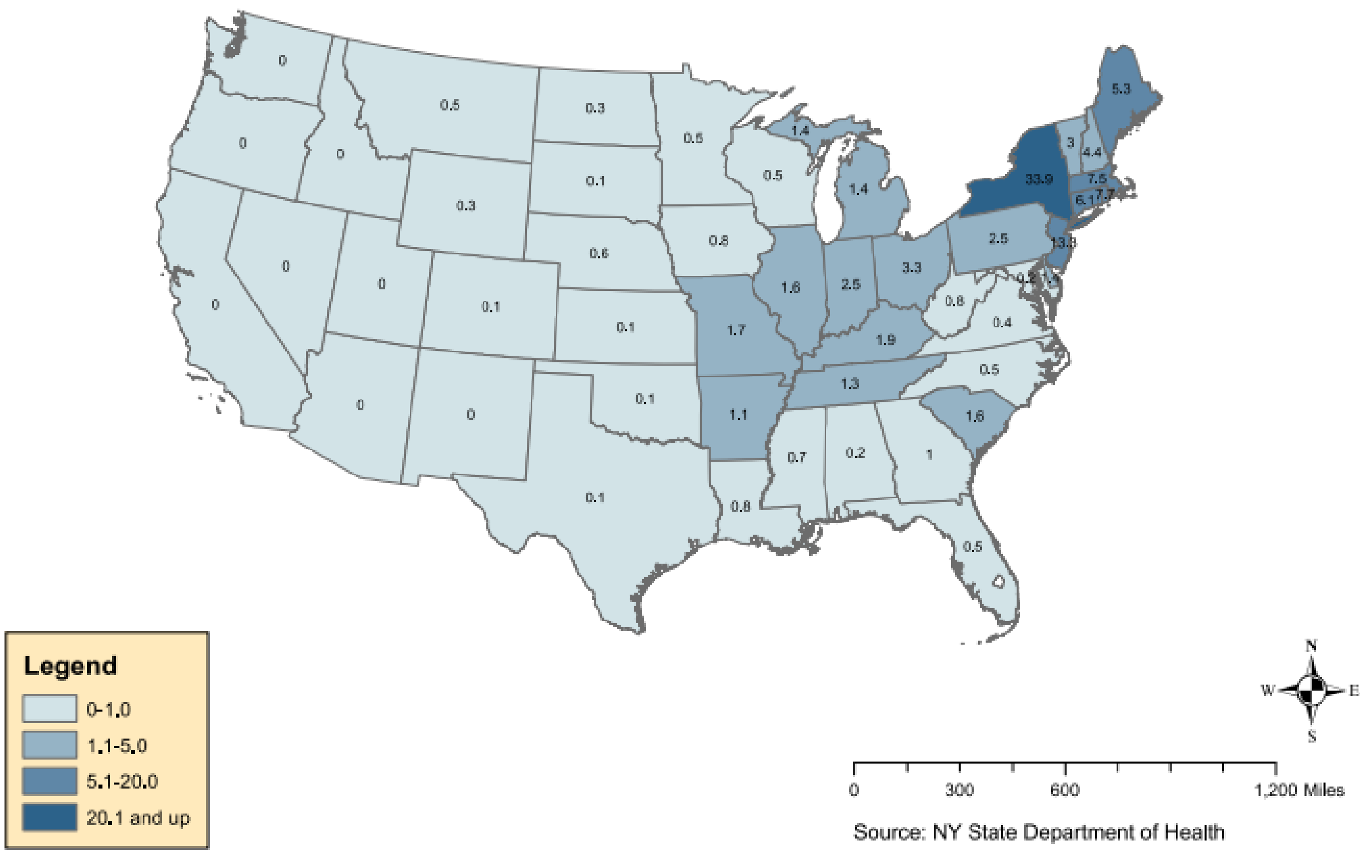


Figure 1-3: Average Distance in 100 Miles to New York by State, 1971-72



Source: Author's tabulation of data from NY State Department of Health

Figure 1-4: Resident Abortion Rates for Abortions Performed in NY, 1973



Source: NY State Department of Health

Figure 1-5: Resident Abortion Rates for Abortions performed in NY, 1975

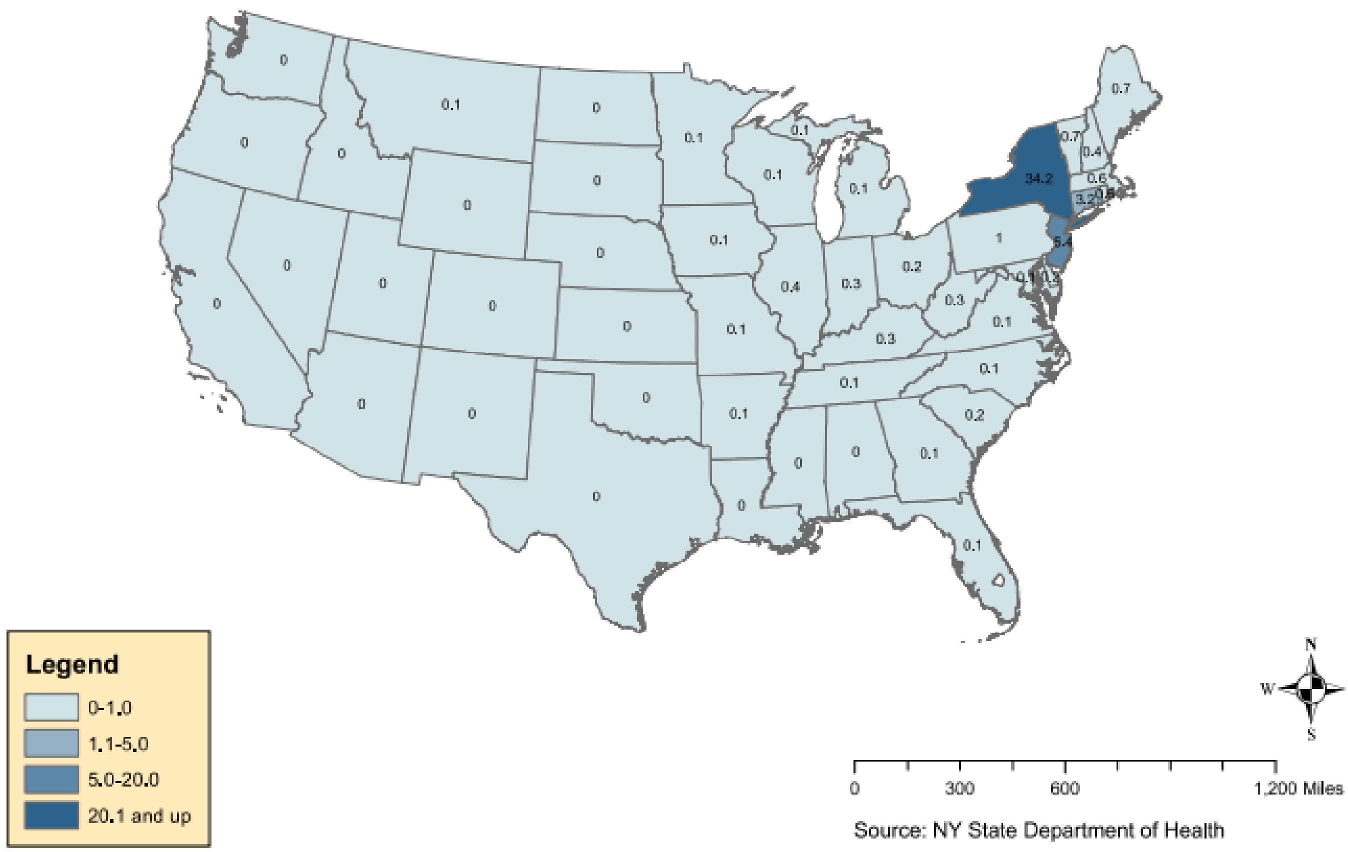
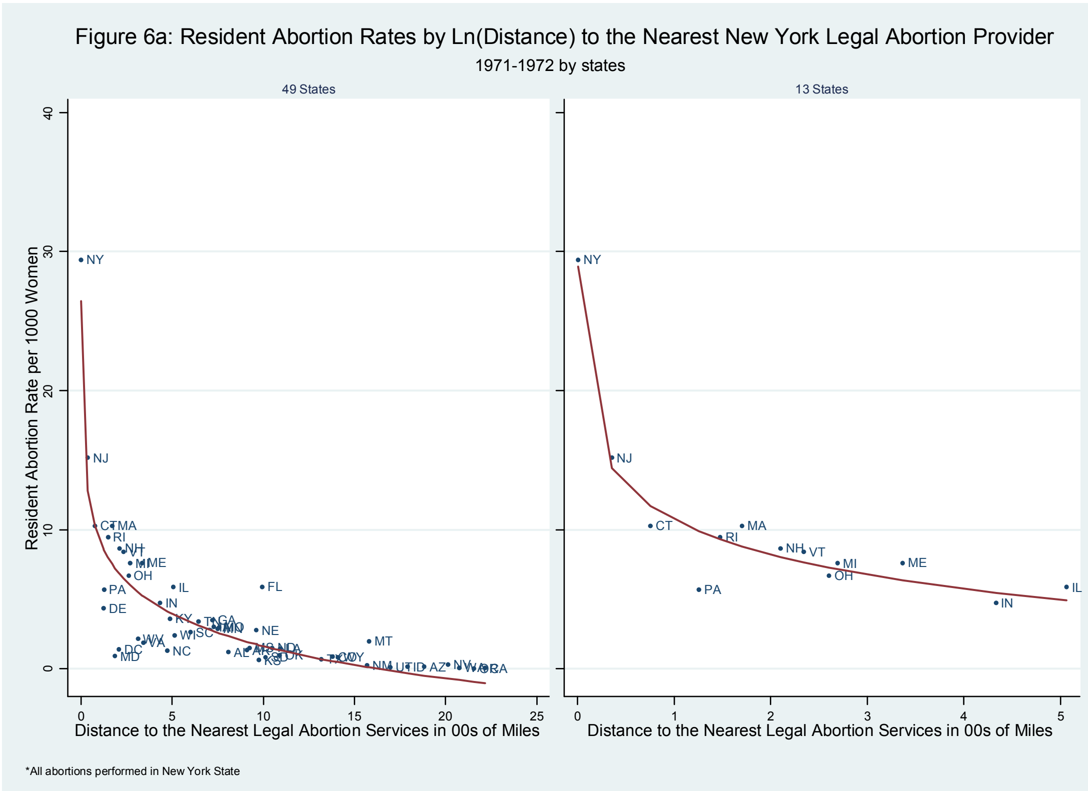


Figure 1-6: Resident abortion Rates by Distance to the Nearest New York Legal Abortion Provider



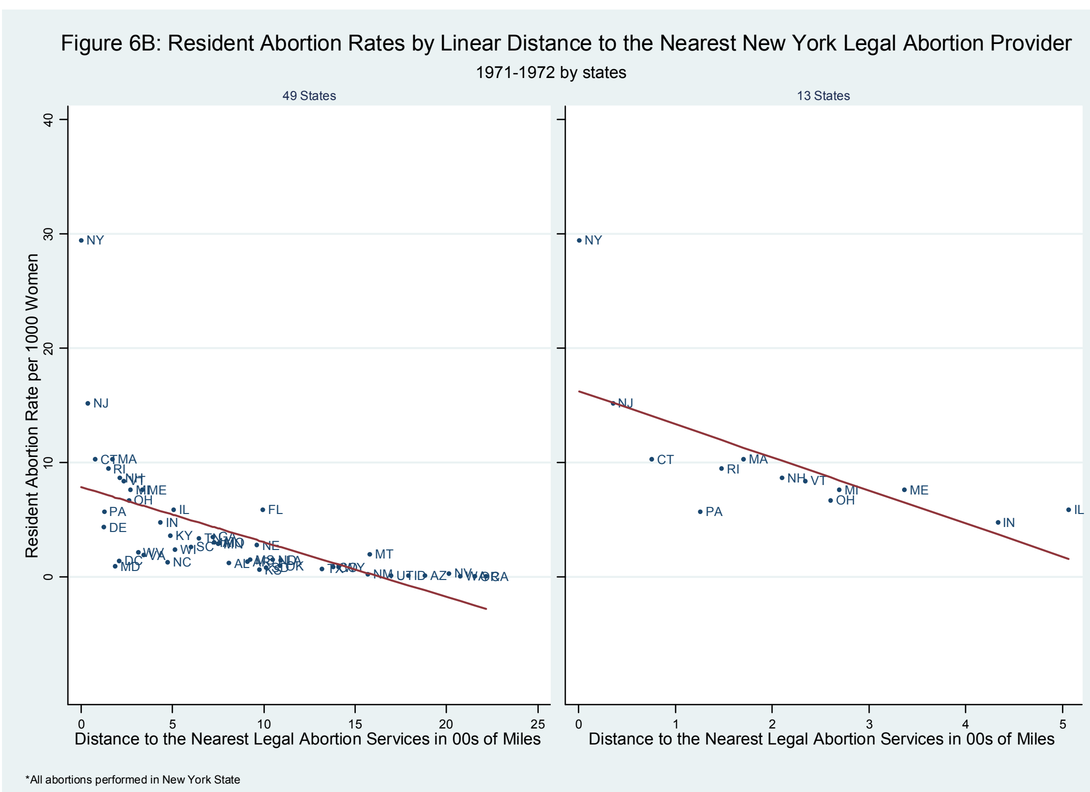
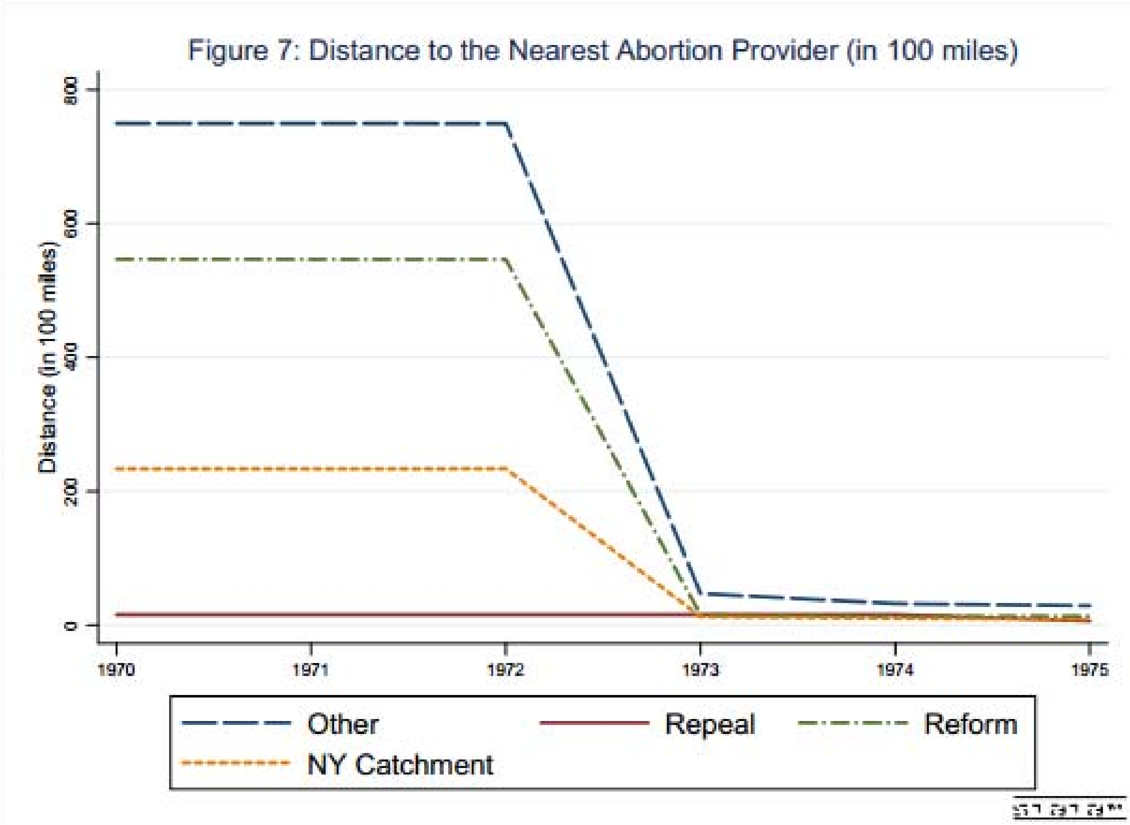


Figure 1-7: Distance to the Nearest Abortion Provider



Chapter 2 The Impact of Abortion Legalization on Young Women's Marriage Decisions

2.1 Introduction

The late 1960s and early 1970s ushered in a period of seismic change in access to abortion brought about by state legislative reform and the 1973 Supreme Court decision in *Roe v. Wade*. The impact of legalized abortion on abortion utilization and on fertility has been well documented (Sklar and Berkov 1974; Levine et al. 1999; Angrist and Evans 1999; Levine 2004). What has received less attention, however, is the impact of legalized abortion on marriage decisions, especially those of young men and women whose decisions to marry were often driven by unintended pregnancies.

Marriage that occurred after pregnancy but before birth, often called a "shotgun" wedding, was prevalent in the U.S. until the late 1960s. In 1965, nearly half of the women married before age 20 and 86 percent of women married before age 25 (Carter et al. 2006; Vital Statistics 1965). For adolescent women under age 20, 49 percent were pregnant when they took their wedding vows (Bachu 1999). Social norms bolstered by parental pressure and the stigma of an out-of-wedlock birth forced many young women to enter into early marriage.

The prevalence of shotgun marriage began to fall precipitously around 1970. In a seminal theoretical work, Akerlof, Yellen and Katz (1996; hereinafter referred to as AYK) argues that the legalization of abortion played a major role in the decline of shotgun marriages. They contend that the abrupt increase in abortion availability affected women's (and men's) decisions to marry. In the AYK model, abortion legalization and the availability of contraception is analogous to a technology shock that not only shifted out the frontier of women's choice set, but also redefined the bargain power between men and women. Abortion lessened the responsibility of men by making marriage a rational choice instead of a responsibility in the event of an unintended pregnancy. In order to attract men, women who were willing to abort if they became pregnant could engage in premarital sex without an implicit promise of marriage. On the other hand, as marriage no longer served as an enforceable contract, women who were unwilling or unable to abort were pressured to forgo the promise of marriage in sexual relations. As a result, legalization of abortion reduced the incidence of shotgun weddings while simultaneously increasing the rate of out-of-wedlock childbearing.

The AYK model provides useful insights regarding the mechanisms by which the legalization of abortion may have altered marriage markets. Not only may legalized abortion have reduced the prevalence of shotgun marriage, it may also have accelerated the decline in marriage rates in the early 1970s and contributed to significant delays in the timing of first marriage among young women.

During the past few decades, enormous public and policy attention has been placed on early teen marriage. The decisions to enter into marriage among teen women are of great policy concern for two reasons. First, the timing of first marriage for teenagers is most likely to be the defining moment of other life-course events such as schooling, labor force participation, and childbearing. Voluminous studies have documented relationships between early teen marriage and subsequent negative outcomes, including lower high school and college graduation rates (Ribar 1994; Klepinger et al. 1995), lower wages and higher unemployment rates (Katz and Autor 1999), larger families (Kalmuss and Namerow 1994), as well as poverty in adulthood (Dahl 2010).

Second, early marriages appear to be less stable. The long-term stability of teen marriage that occurred at least 40 years ago is especially vulnerable, partly because most of these early marriages were likely to be forced, shotgun marriages arranged by the parents. Teen marriages have substantially higher rates of disruption than adult marriages (Alesina and Giuliano 2007). Early teen marriages are two-thirds more likely to end in divorce within 15 years compared to marriages among twentysomethings (Dahl 2010).

Given the detrimental life-cycle effects of early marriage, it is important to understand why young women's marriage decisions underwent great changes in the early 1970s − a decade that marks a tumult of social, demographic and legislative changes. Legalization of abortion was not the only fertility control policy that could alter young women's marriage decisions. During the late 1960s and the early 1970s, a burst of state legislative changes granted young, unmarried women confidential access to the oral contraceptive pill (commonly known as the Pill). Goldin and Katz (2002) find that access to the Pill lowers the probability of early marriage among college-educated women. Around the same time, a majority of states lowered the minimum age of marriage and extended legal rights of marriage to women under age 21. Recent studies show that the changing minimum age of marriage laws increased the incidence of marriages at a young age (Blank et al. 2009; Dahl 2010; Edlund and Mochado 2011). Moreover, researchers also find that an easier "exit option" from marriage brought about by the adoption of unilateral divorce laws has contributed to the decline in marriage rates (Rasul 2003; Alesina and Giuliano 2007). Figure 1 depicts the changing legal regime that affected young women during the 1970s. The dramatic evolution of various legal rights extending to young women highlights the importance of disentangling the marriage effect of abortion from a myriad of concurrent legislative changes that jointly affected family formation behavior.

This paper, empirically examines whether legalization of abortion contributed to the divergence in early marriage while accounting for the confounding effects from contemporaneous policies. The sudden and unambiguous shift in abortion access forty years ago provided plausibly exogenous variation with which to identify the effects of abortion legalization on marriage separate from other legal changes. The two-stage quasi-experimental research design, provided by the variation in the timing of legalization between early repeal states and the rest of the US, has been widely used to evaluate the impact of abortion legalization on the well-being of women and children in much of the previous literature[23]. However, the difference-in-differences (DD) estimator underestimates the full effect of abortion legalization because the DD only captures relative changes in outcomes between states that legalized abortion early relative to those that legalized abortion later after *Roe v. Wade*. The reason is that many women in states where abortion remained illegal terminated their pregnancies in states where abortion was legal, primarily New York and to a lesser extent California and Washington, DC[24]. To illustrate the remarkable degree of interstate travel, Figure 2 shows the utilization of abortion services in NY by state of residence. The figure in each state is the average rate of abortions that were performed in New York for residents of

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23 Levine et al. (1999), Gruber et al. (1999), Angrist and Evans (1999), Charles and Stephens (2006), Ananat et. al (2007), Cunningham and Conwell (2012).

24 Published data from the Center for Disease Control (CDC) indicates that there were 396,403 out of 921,092 legal abortions obtained by women who travelled outside of their state of residence from 1971-72. Seventy-nine percent or 314,929 of abortions to non-residents were performed in New York, 33,272 (8.4%) were performed in California and 27,500 (6.9%) in Washington, D.C. (CDC 1972, 1974).

the state within two years prior to *Roe v. Wade*. The early legalization of abortion in New York effectively enabled women in state as far as Montana to abort an unwanted pregnancy in New York. This interstate travel for abortion prior to *Roe* attenuates the effect of legalized abortion as obtained by the DD estimator (Levine et al. 1996 1999). Similarly, studies that identify abortion effects based on variation in abortion rates are also biased due to the lack of data on abortion prior to 1973 (Donohue and Levitt 2001; Goldin and Katz 2002)[25]. Lastly, as I demonstrate below, there is great variation in abortion rates by age in addition to variation by state and year which to date has not be exploited due to a lack of data. Thus, a major contribution of this study is the construction of abortion rates in the years prior to *Roe*. I combine re-discovered data on abortions performed in New York in the years prior to *Roe* with data from the Centers for Disease Control (CDC) and Guttmatcher Institute (GI) to assemble a measure of abortion access among women 15 to 19 years of age by state of residence and by year from 1970 to 1980. The estimated teen resident abortion rates account for cross-state travel prior to *Roe v. Wade* and gauge the age variation in abortion access. In addition, I construct a distance index to capture the variation in geographic proximity to the nearest legal abortion provider before and after *Roe*. The changes in distance to the nearest abortion provider during the 1970s provide plausibly exogenous source of variation with which to identify the effect of increasing abortion access.

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25 Both Goldin and Katz (2002) and Donohue and Levitt (2001) include the actual abortion rate as proxy for access to abortion. However, they use only data from the Guttmacher Institute which begins in 1973. Following Donohue and Levitt (2001), Goldin and Katz (2002) assume that the abortion rate was zero prior to 1973 in 45 states and the District of Columbia and they impute the rates in 1970-1972 by linear backcasting the abortion rate in 1973 in the 5 early legalizing states. Both assumptions are questionable as it creates non-random measurement error and systematically bias the results (see also Lott and Whitley 2007; Joyce 2004 2006 2009).

Teen women's marriage behavior is analyzed in two ways. First, I create marriage rates using *Vital Statistics* marriage certificate data. This allows me to associate changes in marriage rates with abortion access in period before and after *Roe*. Second, I draw upon individual data from June supplements to the Current Population Survey (CPS) in order to analyze the timing of marriage and shotgun marriage. The main empirical results are as follows. I find evidence that legalization of abortion significantly contributed to the decline in marriage rates among women 15 to 19 years of age. Moreover, women with greater access to abortion services were more likely to postpone their entry into marriage before age 20. Lastly, I show that fewer women married in response to a premarital pregnancy, making shotgun marriages increasing rare. The latter provides new support for the AYK model of the impact of a technological shock to fertility control on the well-being of women.

The paper proceeds as follows. Section 2 presents a descriptive analysis of marriage markets in the past decades. Section 3 summarizes the legislative history of abortion legalization and reviews the past relevant research. Section 4 describes the data and the key variables, while Section 5 sets out the empirical methodology. Section 6 presents the empirical results. Finally, Section 7 concludes.

2.2 Trends in Marriage

Shotgun Marriage

Figure 3 shows that the proportion of premaritally pregnant women who married before the first birth, the so called "shotgun marriage rate"[26], remains at a steady level of

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26 The definition of "shotgun marriage rate" follows O'Connell and Moore (1980), O'Connell and Rogers (1984) and Akerlof et al. (1996) is the fraction of births conceived out-of-wedlock with marriages between conception and birth given the definition

around fifty percent from the 1930s to 1960s. However, the major decline in this statistic occurred during the 1970s. By the end of 1970s, the likelihood that pregnant women married before giving birth had fallen by more than two-thirds for teen women and forty percent for women in their twenties.

Marriage Rates

Figure 4 presents the time-series trends in marriage rates over the past half century. As the generation of baby boomer entered the marriage market, there was a steady increase in marriage rates starting from the early 1960s, reaching the peak by the end of the 1960s. A marked decline in marriage rates occurred during the 1970s. By the end of the 1970s, marriage rates had fallen by 6 percent compared with the late 1960s. Figure 4 also highlights the more profound changes in marriage rates among teen women. Between 1969 and 1979, teen marriage rates dropped 36 percent. With the remarkable declines in marriage rates following the landmark ruling of *Roe v. Wade*, more young women chose to remain single for a longer period of time.

Timing of the First Marriage

Figure 5 presents the distribution of age at first marriage among ever-married women by birth cohorts. Each point represents the fraction of newlywed women at a certain age group. For example, among women born before 1950 who turned 20 before the advent of early legalization, more than 30 percent of ever married women reported a first marriage before age 20. The number dramatically decreased for younger cohorts who gained access to legalized abortion before the age of 20. For women who were born between 1955 and 1959, the fraction that married as teenagers was 20 percent lower than the generation of women born approximately 10 years earlier. While there exists secular period trends toward marriage delay and evolving socio-economic status of women over time, the sharp difference between cohorts with access to abortion during their teenage years (born after 1955) and without access (born before 1950) suggests that cross-cohort changes in the age at first marriage correspond closely to abortion legalization.

2.3 Background

2.3.1 Legislative History and Past Research

As detailed in Garrow (1998), induced abortions had been illegal and strictly prohibited in every state throughout the country since the beginning of 20th century. It was not until 1967 that a handful of states started modestly to reform their policies toward allowing abortion under certain circumstances.[27] The first wave of legislation legalizing abortion occurred between 1970 and 1972, when anti-abortion laws were repealed and abortion was *de jure* or *de facto* legalized in five "early-repeal" states (New York, California, Washington, Alaska and Hawaii) in 1970 and District of Columbia in 1971[28]. The Center for Disease and Control (CDC) launched an abortion surveillance system in 1969 to document the number of legal induced abortion performed. According to the CDC, the reported number of abortions increased more than four-fold from 180,000 in 1970 to 780,000 in 1972 (CDC 1975). Among the early repeal states, New York was the most

27 Twenty-eight states restrict legal abortions to situations where it is necessary to preserve pregnant women's life, while four states allow abortion on the decision of a physician. Eighteen other states and D.C. allow abortions under a variety of legally specified or restricted conditions.

28 The California Supreme Court case in People v. Belous (September , 1969) resulted in de facto legalization in California. This decision was followed 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).

frequent destination for women wishing to terminate a pregnancy in the pre-*Roe* years (see note 3). In January 22, 1973, legalized abortion services suddenly became available nationwide following the Supreme Court's decision in *Roe v. Wade*, a landmark ruling in American jurisprudence history that established the constitutional right for women to terminate unwanted pregnancies. Abortion utilization expanded dramatically after legalization. By 1979, the total number of abortions had risen to nearly 1.6 million, an order of magnitude more than in 1970.

Among a wide array of outcomes that have been associated with legalization of abortion in prior empirical studies, women's childbearing behavior is the most extensively examined outcome. The finding that legalization of abortion led to a significant decline in fertility is robust across studies. Moreover, there is also evidence that the decline in fertility was greater among teens, unmarried and nonwhite women (Sklar and Berkov 1974; Joyce and Mocan 1990; Levine et al. 1999; Angrist and Evans 1999; Joyce et. al. 2012). Recent work further shows that lower birth rates associated with abortion legalization also represent a permanent reduction in life-cycle fertility (Ananat et al. 2007).

Aside from the contemporaneous fertility effects among women, a number of studies have linked the legalization of abortion to the well-being of the second generation. The characteristics of children and the outcomes of their life trajectory − such as birth outcomes, living circumstances of children, college graduation, criminal activity, teen motherhood, sexual transmitted disease − have been associated with the dramatic increase in the access to abortion during the early 1970s (Grossman and Jacobowitz 1981; Grossman and Joyce 1990; Ananat et al. 2007; Charles and Stephens 2006;

Donohue and Levitt 2001; Gruber et al. 1999; Lott and Whitley 2007; Donohue et al. 2009; Bitler and Zavodny 2002)[29].

2.3.2 Does Legalization of Abortion Affect Marriage?

Recent empirical research provides limited and contradictory evidence as to the effect of abortion on marriage. Goldin and Katz(2002), in an influential paper, find that the expansion of minors' access to the oral contraceptive pill in the early 1970s enabled young, single women to delay marriage. The concurrent legalization of abortion enters into their empirical framework as an additional control. Their estimates indicate a substantial effect of access to the pill on the likelihood that a college-educated woman marries by age 23, but they report no robust association between the timing of first marriage and legalized abortion after controlling for state linear time trends. After re-examining the legal source to the state policies related to accesses to the pill and abortion, Myers (2012) uncovers the sensitivity of Goldin and Katz(2002)'s results to alternative sample and revised legal coding. Myers (2012) finds no evidence that access to the pill let to the delayed marriage for college graduates. On the contrary, the author shows it was the changing legal environment related to access to the abortion that induced women to delay entry into early marriage and motherhood. The latter findings echo one of the findings from an earlier work by Angrist and Evans (1999), in which the authors show that state abortion reform in the early 1970s prior to legalization reduced the probability of marrying before age twenty among white women.

There are a few studies which empirically examine the association between abortion legalization on aggregate marriage rates. However, the results are far from conclusive.

29 Some authors have questioned the finding on the second generation in terms of the data, identification strategy and magnitude of the effect (Joyce 2004, 2009; Foote and Goetz 2005)

Choo and Siow (2006) find a negative effect of partial legalization of abortion on marriage rates for both young men and women. Rasul (2003) and Alesina and Giuliano (2007) examine the impact of adoption of unilateral divorce laws in the U.S. on marriage rates, but also control for abortion laws. Rasul (2003) reports a statistically significant negative relationship between abortion legalization and marriage rates. Alesina and Guiliano (2007) also report a negative relationship, although their findings are not statistically significant.

2.4 Data and Explanatory Variables

2.4.1 Access to Abortion Services in the 1970s

The first goal of this paper is to estimate abortion access during the 1970s. To proxy access to legal abortion services after 1970, I take two approaches. Abortion access is first measured by distance to the nearest abortion provider. As an alternative approach, I estimate resident abortion rates for women 15 to 19 years of age using abortion data collected from various sources.

Proxy measure 1: Distance to the nearest abortion provider

A number of previous studies have recognized the importance of the distance that a woman must travel to find an abortion provider (Levine et al.1996, 1999; Kane and Staiger 1996; Ananat et al. 2007). Travel distance may provide a good indicator of abortion access since the cost of having an abortion rises with travel distance, and is plausibly exogenous. Figure 6 (a)-(c) demonstrate how the dramatic change in the geographic distribution of abortion providers one year before and after *Roe. v. Wade* affected abortion utilization. To illustrate the remarkable degree of interstate travel prior to *Roe,* take New York for example. The legalization of abortion in New York in 1970 made the state the only state with the least restrictive abortion laws which permits abortions for non-resident women during the early state reform period (Joyce et al. 2012). Figure 6(a) shows utilization of abortion services in NY by state of residence. The number in each state is the average rate of abortions that were *performed in New York* to residents of the states from 1971-72. The figure highlights the fact that many women travelled thousands of miles from states as far as Montana to obtain a legal abortion in New York in the two years prior to *Roe*[30]. Following *Roe*, the average distance to the nearest provider greatly declined as abortion providers spread quickly throughout the whole country. By 1974, the first full year in which abortion had been legal nationally, the number of abortions in New York to non-residents had fallen by 84 percent, most of which were to women who lived in New Jersey and Connecticut (Figure 6b). In contrast, during same time period the number of abortions performed in other states grew rapidly (Figure 6c).

I calculate distance (in 100 miles) to the nearest abortion provider from 1970 to 1979 based on the population centroid. For the Pre-Roe years (1970-1972), I treat counties in New York (Buffalo and New York City) and California (Los Angeles and San Francisco) as major destinations for women living in non-repeal states31. The abortion clinics in Washington D.C. served as a local market for women living in Delaware, Maryland and

30 In 1972 alone, 210,345 abortions were obtained outside of a woman's state of residence, 156,255 or 74.2 percent were performed in New York and 110,633 or 52.5 percent in New York City alone (CDC 1972 1973 1974).

31 I exclude Washington State from the destination states in the pre-Roe years despite the legalization of abortion in Washington in December of 1970. This is because the state had a 90 day residency requirement for an abortion (Garrow 1998). Moreover, relatively few non-residents obtained an abortion in WA prior to Roe and there is an absence of decline in abortions between pre-Roe years and 1973 (17,767 to 17,319). Whereas substantial decline in the number of abortions performed in NY and CA can be found between 1972 and 1973.

Virginia; therefore I calculate distance from DE, MA and VA to Washington D.C. as well. For the years after 1973, I compute distance from the population centroid of each county to the nearest county with an abortion provider regardless of whether the provider was in the state of residence or in a neighboring state. I assume distance is zero if the county had an abortion provider. The abortion provider data is from Guttmacher Institute (GI). The survey conducted by GI is widely considered the most authoritative census of abortion providers by county and year in the United States. Finally, I take the average of the distance by state weighted by female population 15 to 44 years of age in each county to arrive at a summary measure at the state level. Average distance drop from 502 miles in 1972 to 29 miles in 1973 and continue to decline to 18.6 miles by 1975.

Proxy measure 2: Estimated teen abortion rates by state of residence

Following Goldin and Katz (2002) and Donohue and Levitt (2002 2004 2008), I use abortion rates as an alternative measure of abortion availability in the 1970s to strengthen the results. I extend the use of abortion rate as a proxy measure for abortion access in previous literature along two dimensions. First, I correct for the way abortion data were obtained for the years prior to 1973 (see note 4). For the period 1970 through 1972, I hand-entered the abortion data by state of residence published in the annual CDC Abortion Surveillance Reports (CDC 1971 1972 1974)[32]. I then augmented the data using the estimated total resident abortion rates reported by GI beginning in

32 Number of abortions were taken from CDC Abortion Surveillance 1971 Table 4:6-7, 1972 Table 5, 1974 Table 5:18-19,. I then calculate the total abortion rates per 1,000 women 15 to 44 years of age using population data from Surveillance Epidemiology and End Results (SEER).

1973[33]. A second extension of the proxy measure concerns the substantial variation in abortion utilization across age groups. As teen pregnancy rate rose steadily in the early 1970s, adolescents younger than age 20 had the highest rate of abortion among all age-groups throughout the same period. Using the overall rate as proxy for abortion access would mask the large differences and varying patterns across time and state for age subgroups. However, the most recent data on resident teen abortion rates by state is not available until the 1980s. Based on the method implemented by researchers at Guttmatcher Institute for estimating the characteristics of abortion patients (Henshaw and Van Vort 1988; Henshaw et al. 1991), I adopt a similar algorithm to estimate teen resident abortion rates. My calculations are based on two data sources. To estimate the proportion of teen women who had abortions prior to Roe, I make use of a unique data on abortions performed in New York. The data provides rich information on abortions perform in the state by age groups and residence of abortion patients. (Joyce et al. 2012). Similarly, I extract information on the age distribution of women having abortions after *Roe* from the CDC Abortion Surveillance data, which is available by age group, state of occurrence and year. Appendix A and B provide detailed explanation of the sources of each abortion data and the estimation process. The average teen abortion rates by year, as estimated in the paper, tracks the trends and matches the national-level figures reported by GI (See Figure B1 in Appendix B).

33 The GI survey of abortion providers provides the most complete count of abortions by state of occurrence. Researchers at GI use these data in combination with data from the CDC and state health department to estimate abortions by state of residence (Joyce 2006) The GI abortion data has been the most commonly used post-Roe abortion data in past studies (Donohue and Levitt 2001 2004 2006, Goldin and Katz 2002; Ananat et al. 2009)

2.4.2 Marriage Data

I use three data sources to examine the changing marriage behavior of young women during the 1970s from both cohort and period perspectives. The initial evidence of the effect of abortion on marriage rates relies on 12 years of marriage certificate records aggregated from the National Center for Health Statistics (NCHS) *Vital Statistics* from 1968 to 1979. The NCHS marriage certificate data is the official administrative record of marriages occurring each year in states that are part of a marriage reporting area[34]. The micro-level marriage certificate data is not available until 1968. For the years prior to 1968, Vital Statistics only reports marriage information by year[35]. Information such as state of residency, previous marital status and the ages of the bride and groom are included in the data. I identify the number of *first* marriages according to bride's age and her previous marital status. I then create teen first-marriage rates for women 15 to 19 years of age at state-year level from 1968 to 1979. Population estimates are from the Surveillance Epidemiological and End Results (SEER) from the National Cancer Institute[36].

Next, I turn to June fertility supplements to the Current Population Survey (June CPS) to investigate the effect of abortion on individual's age at first marriage or first shotgun marriage. The June fertility supplements to the CPS consist of repeated cross-sections surveys. I pool individual information of female respondents from the 12 waves of June

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34 The NCHS marriage certificate data represent a sample of marriages performed in the Marriage Registration Area (MRA). In 1950, MRA included less than 20 states. During the 1960s and 1970s, Vital Statistics saw a large number of increases in states report the marriage data. By 1980, virtually all states are included in MRA (Blank et. al, 2008). During the period under study, the marriage certificate data cover roughly 44 states depending on the specific year.

35 For descriptive purpose (Figure 4), I complement the dataset with hand-entered data from the annual report of the Vital Statistics for 1955-1967.

36 The SEER population estimation series starts from 1969. I use the 1969 population estimates for the year 1968.

CPS survey from 1979 to 1995[37]. The data provide retrospective marital history of all married women age 18 and above. Specifically, women were asked about their number of marriages, followed by the month and year of the beginning of each marriage (if ever married) and the ending of each marriage (if ever divorced). To further identify shotgun marriages I also make use of the detailed childbearing history information asked in the surveys. Mothers were questioned about their fertility history including the month and year in which their first child was born and the number of babies ever born to them. A key feature of the June CPS is that I am able to construct age at first marriage and age at first birth in months. Therefore, for woman ever married and who ever had children, I can pin down the order of the woman's first marriage and first birth and calculate the exact duration between them in months. Following previous literature (O'Connell et al. 1990; Akerlof et al.1996; Bachu 1999; Myers 2012), I define a shotgun wedding as a first marriage that occurred seven months or less prior to the first birth, where the bride was single at the time of the pregnancy.

The advantage of the June CPS lies in their detailed collection of birth and marriage histories. However, CPS provides the information based only on women's current state of residence at the time of the surveys. Ideally I would like to measure access to abortion for a women based on her actual location of residence during the teenage years. Discrepancies between a woman's state of residence during adolescence as compared to adulthood may create measurement error in exposure to state policies that will attenuate the estimates. Hence, I also use the 1 percent sample of the 1980

37 The survey years not included are 1984 and 1991. Female respondents in these two years were not asked about their marriage history.

Census of Population from the Integrated Public Use Microdata Series (IPUMS)[38] to examine the effect of abortion on age at first marriage. The census provides information on state of birth and age at first marriage. While neither state of birth nor current state of residence provides perfect information on where woman resided before the age 20, state of birth may be a better proxy for the exposure to abortion access during the teenage years since the vast majority of teens resided in their state of birth.

2.5 Empirical Methodology

To identify the impact of abortion legalization on marriage, I propose a general empirical framework which encompass both period and cohort analysis. I first test the period effect of increasing access to abortion on teen marriage rates in the 1970s. The period analysis provide the most direct test of abortion and marriage because it compares teen women with different access to abortion at the time of the law changes and measuring their contemporaneous marriage rates. Next, I conduct a cohort analysis by exploiting the state-by-birth cohort variation in the exposure to abortion availability. The cohort analysis examines individual's probability of entering into first marriage and having a shotgun wedding before age 20.

2.5.1 Period Analysis Using Vital Statistics

I begin by replicating the quasi-experimental research design (Difference-in-Difference) widely implemented in the previous literature. The identifying variation comes from different timing of abortion legalization across states in the early 1970s. Five states (AK, CA, NY, WA, HI) and D.C. made abortion available on demand prior to

38 I cannot extend the analysis to shotgun marriage using IPUMS data. First, census only asks about the birth history of children living within the household. Second, I can measure a shotgun marriage based on the age of first marriage and birth in years in the census data.

Roe. I refer to them as the repeal states. The other states legalized abortion with *Roe* and I refer to them as non-repeal states. The Diff-in-Diff (DD) estimation follows the following form:

$$(1) M_{st} = \beta_0 + \beta_1 * \mathrm{REPEAL_s} * \mathrm{D7072} + \beta_2 * \mathrm{REPEAL_s} * \mathrm{D7375} + \beta_3 * \mathrm{REPEAL_s} * \mathrm{D7679} + \alpha_1 * \mathrm{MAR\_Law_{st}} + \alpha_2 * \mathrm{DIV\_LAW_{st}} + \alpha_3 * \mathrm{PILL_{st}} + \alpha_4 * \mathrm{X_{st}} + \delta_s + \tau_t + \delta_s * t + \varepsilon_{st}$$

The dependent variable is teen first marriage rates by state and year from 1968 to 1979. The variable REPEAL is one if the state is a repeal state and zero otherwise. D7072, D7375, and D7679 are year dummies for the periods 1970-1972, 1973-1975, and 1976-1979, respectively. The omitted category is the period of 1968-1969. Xst is a matrix of state variables that include the insured unemployment rate, per capita income, and the percent of population that was nonwhite[39]. To adequately account for confounding contemporaneous policies that correlates with marriage behavior, I also control for minor's access to the Pill ($\mathrm{PILL_{st}}$), minimum age of marriage without parental consent ($\mathrm{MAR\_LAW_{st}}$) and unilateral divorce law ($\mathrm{DIV\_LAW_{st}}$)[40]. All specifications include state(δ_s) and year (τ_t) fixed effects. To capture omitted variables that evolved over time within state, I also include state-specific linear time trends in some specifications.

The coefficient β1 measures the impact of abortion legalization in the repeal states relative to the non-repeal states before *Roe*. The difference between coefficient β1 and

39 I thank Philip Levine for graciously sharing the data.

40 Each of the policy variables is dichotomous. Data source and legal coding are from previous literature. Following Goldin and Katz(2002), access to the Pill (PILL_LAW) equals one if state s in year t had a nonrestrictive law allowing minors(16 years and older) to obtain the Pill without parental consent (Table 2, p757). The variable MARRIAGE_LAW equals one if the minimum age of marriage in state s in year t was lower than age 21 (Blank et al. 2009). The coding of unilateral divorce law is from Gruber (2004). UNI_DIVORCE equals one if state s adopted unilateral divorce law in year t.

β2, as well as β1 and β3 capture the effect of nation-wide legalization of abortion in non-repeal states, relative to the repeal states in the eras following legalization.

Equation (1) serves to examine the *relative* impact of abortion legalization on marriage rates between repeal and non-repeal states before and after *Roe*. However, the DD estimator fails to capture the *full* effect of abortion legalization due to interstate travel prior to *Roe*. In the next set of regression I turn to a preferred specification using the continuous proxy measures for access to abortion as described above. The model is as follows:

$$(2)\ M_{st} = \beta_0 + \beta_1 * \mathrm{ABORTION\ ACCESS_{st}} + \alpha_1 * \mathrm{MAR\_LAW_{st}} + \alpha_2 * \mathrm{DIV\_LAW_{st}} + \alpha_3 * \mathrm{PILL_{st}} + \alpha_4 * \mathrm{X_{st}} + \delta_s + \tau_t + \delta_s * t + \varepsilon_{st}$$

$\mathrm{ABORTION\ ACCESS_{st}}$ is one of two measures: the estimated resident teen abortion rate or the straight-line distance to the nearest abortion providers for each state and year. Since there is no abortion data available for years prior to 1970, I impose an abortion rate of 0 for the year 1968 and 1969. Distance, on the other hand, is undefined prior to 1970. After *Roe* in 1973, distance to abortion provider fell off dramatically. Thus, I further interact distance with period dummies (1970-1972 and 1973-1978) to reflect the dramatic decline in access (see Figure 6(a)-(c)).

I hypothesize that teen abortion rates and teen marriage rates are negatively associated. Thus, the coefficient on abortion rate as proxy for abortion access is expected to be negative. Similarly, the further a teen resides from an abortion provider, the greater the marriage rate. Thus the association between distance and teen marriage rates is expect to be positive.

2.5.2 Cohort Analysis Using the CPS and IPUMS

Specification

Next, I explore whether the dramatic increase in abortion availability brought by legalization also induced marriage delay among young women. To empirically examine this connection, I model the relationship between abortion and timing of marriage based on a series of linear probability models. Specifically, I estimate models of the following general form:

$$(3)\quad Prob(M_{ics}^{k}) = \alpha + \beta_1^k * \mathrm{ABORTION\_ACCESS}(k)_{cs} + \beta_2^k * \mathrm{MAR\_LAW}_{cs} + \beta_3^k * \mathrm{DIV\_LAW}_{cs} + \beta_4^k * \mathrm{PILL}_{cs} + X_{ics} + \tau_s + \delta_c + \tau_s * c + \epsilon_{ics}$$

Where *i* indexes individuals; s indexes state; and y indexes year of birth. The dependent variables are indicators that equal one if a marriage event occurred before age *k* (*k*=16, 17, …, 20) for woman *i.* The key explanatory variable of interest is the *exposure* to abortion access before age k ($\mathrm{ABORTION\_ACCESS}(k)$). The other three right-hand-side variables of interest are access to the pill (PILL_{cs}), minimum age of marriage ($\mathrm{MAR\_LAW}_{ics}$) and non-fault divorce law ($\mathrm{DIV\_LAW}_{ics}$). Following Goldin and Katz (2002 p.757) I code the variable PILL_{ics} as one if the woman had access to the pill when she was 18 years old[41]. $\mathrm{MAR\_LAW}_{ics}$ indicates whether the woman gained the legal capability to marry early without parental consent by age 18. $\mathrm{DIV\_LAW}_{ics}$ equals one if the woman had an easier exit option from marriage under the unilateral divorce law regime before age *k*. In all specifications, I include state and cohort fixed effects.

41 PILL_{ics} is coded in the same way as the variable "nonrestrictive birth control law at age 18" in Table 4 of Goldin and Katz(2002). I thank Lawrence Katz and Claudia Goldin for graciously supplying me with their data and program files.

State linear time trends ($\tau_s * c$) are also included to capture unobserved state characteristics that evolved across cohorts. I estimate models including all women as well as separate models by race. For the regression of all women, a vector of dummies for race and ethnicity (X_{ics}) is also included.

I examine the timing of two kinds of early marriage outcomes: first marriage and shotgun marriage. In the case of timing of first marriage, for example, I focus on teen first marriage and evaluate the effect of exposure to abortion on the cumulative probabilities of marrying prior to ages 16 throughout 20. Examining the results across the age distribution is important. First, the series of models enable me to trace the treatment effects across ages and explain the shift in the distribution of age at first marriage. Second, comparing the age-specific point estimates allows me to examine whether the effect of the availability of abortion on the decisions to marry among minors younger than age 19 (marrying before age 16, 17 and 18) is different from the effect of abortion on older teen women's marriage decisions(marrying before age 19 and 20).

Identification in state-by-birth cohort analysis

The *exposure* to abortion access is the key variable of interest in cohort analysis. To illustrate this idea, I follow Angrist and Evans (1999) and construct an age-period-cohort diagram (Table 1). The diagram presents a stylized representation for birth cohorts who experienced different legal regimes toward access to abortion as they age over the same period. The vertical axis represents years of birth for women born from 1950 to 1960 and the horizontal axis shows calendar years. The entries show age in years.

To give a concrete example, consider the exposure to legalized abortion before age 20 for two women, A and B, born in 1956. Women A was born in New York while women B was born in Mississippi. If we assume they both became sexually active at age 15, then by age 20 women A would have spent five years (15 to 19) in an environment with local abortion services less than 5 miles away. In contrast, women B, a Mississippi resident, would have to travel approximately 1,000 miles if she wanted to terminate her pregnancy at age 15 and 16 (in years 1971 and 1972), and 40 miles from age 17 to 19 after *Roe*.

To formalize the idea of state-by-birth cohort variation in exposure to abortion availability before age *k*, I construct two indices that are designed to reflect the effect of all previous abortion access (from age 15 to age "k-1") for the cohorts born between 1950 and 1960[42] in each state. Distance to the nearest abortion provider, the first proxy measure of access to abortion, has been found to be directly associated with birth rates and thus implicitly, inversely related to abortion rates (Levine 1996 1999). Hence, for each age-specific outcome, I define a distance index as the average of the inverse distance to the nearest abortion provider between the years when the cohort of women was age 15 to age *k-1* (k=16, 17,…, 20) in state *s*. Each distance index variable is normalized to range from 0 to 1. A distance index equals 1 for the cohort of women who had lived closest to legal abortion providers since age 15, and 0 for older cohorts (born before 1950) with no access to legal abortion services (so that access to abortion increase as distance index rises). Similarly, I define an abortion index as the moving averages of state-year specific resident teen abortion rates between the years when different cohorts of women in state *s* aged from 15 to *k*-1. Appendix C provides detailed numerical examples for each of the measures.

42 Women born prior to 1950 had no access to abortion access before age 20. See age-period-cohort diagram in Table 1.

2.6 Results

2.6.1 Abortion and Marriage Rates

Estimates based on the quasi-experimental design

I begin by presenting period analysis results based on the quasi-experimental design that explores the relative impact of abortion legalization on marriage rates between repeal and non-repeal states. As an initial graphical analysis, Figure 7 compares the time trends of marriage rates of the group of repeal states (AK, CA, NY, WA, HI and D.C.) with the rest of the US (non-repeal states). A key observation of Figure 8 is that the trend of teen marriage rates corresponds to the staggered timing of the introduction of legalized abortion. Teen marriage rates in repeal states started to decline from 1970. In contrast, non-repeal states experienced a similar decline after 1973 following a small increase between 1970 and 1972. However, there is no full "rebound" effect as depicted.

The first two columns in Table 2 present the population weighted results for the reduced-form equation (1), with weights equal to the number of teen women living in the state. For each regression I report the coefficients for the three interaction terms (β_1 through β_3 in equation (1)). I find a significant effect of abortion on teen first marriage rates. Consistent with the pattern shown in Figure 7, regression results show a statistically significant effect of early legalization of abortion prior to *Roe* on teen first marriage rates. For the period before 1973, teen marriage rates in repeal states fell by 9 percent relative to states with no law changes during the period (β_1=-5.128)[43]. Adding state-linear trends leaves the coefficients largely unchanged. The difference between

43 Percentage effect measured relative to the mean.

repeal and non-repeal states diminished in the years following *Roe*, however the estimated coefficients are not statistically significant.

Estimates using proxy measures for abortion access

The findings of equation (1) provide evidence regarding the *relative* impact of abortion legalization on marriage rates. However, the DD estimator fails to capture the *full* effect of abortion legalization due to interstate travel prior to *Roe*. Next, I turn to a preferred specification which substitutes the continuous proxy measures of the abortion access for the qualitative measure of abortion legality. Regression results based on equation (2) are reported in columns 3 to 6. Each column represents a separate regression. Columns 5 and 6 show the effect of abortion rates on teen marriage rate. In columns 3 and 4 abortion access is represented by is distance to the nearest abortion provider in 100 miles interacted with period dummies in columns 3 and 4. The coefficient on Dist * 1970-72, for example, shows the change in teen marriage rates for every 100 mile increments in distance a women must travel to terminate a pregnancy.

In all the cases, the increasing abortion access (proxied by *increasing* abortion rate and *decreasing* distance to the nearest abortion providers) significantly reduces teen first marriage rates. To roughly compare the results with Diff-in-Diff estimates in columns 1 to 2, I do some back-of-the-envelope calculations. For instance, the average difference in teen abortion rates between “early-repeal” states and “non-repeal” states during the pre-*Roe* years (from 1970 to 1972) is 30 abortions per 1000 teen women. An increase of this magnitude is associated with with a decline in the e marriage rates by 6.4 per 1000 teen women, or 10.3 percent relative to the mean. The result is one percentage point higher than the Diff-in-Diff estimates. For the years after *Roe*, an increase of one S.D. in the teen abortion rate (17.28 per 1000 teen women) is associated with a 6 percent decline in the marriage rates. For the measure of abortion access by distance to the nearest abortion provider, a decrease of 500 miles during the pre-*Roe* period[44] is associated with a 5 percent reduction in marriage rates.

In addition to the results regarding teen first marriage rates, I also examine the effect of abortion on young adults' first marriage rates among women ages 20 to 24 based on equation (1) and (2). Neither the graphical analysis, nor the regression results provide any evidence of significant association between rates of abortion and first marriages among women in the older age group (results available upon request)[45].

Robustness Checks

I explore the robustness of these results in two ways. First, as noted in Angrist and Pischke (2009), one potential caveat associated with aggregate level population weighted analyses is that weighting may bias estimates if the variance is not inversely proportional to the state population. As an additional robustness check, I also present the unweighted DD estimates of equation (1) and (2) in Appendix Table 1. Results shown in columns (1) to (3) indicate that estimated coefficients are roughly unaffected by weighting.

44 For women living in states outside of early repeal states before 1973, the average distance to the nearest abortion provider is 517 miles. The same statistic for women living in one of the early repeal state is only 18 miles.

45 I use the estimated young women's abortion rate (abortions per 1,000 women ages 20 to 24) following the same process as described in Appendix B. Regression results based on abortion rates as proxy for access is based on the estimated resident young adult women abortion rate.

Moreover, distance to the nearest abortion provider could be endogenous in the years after Roe. For example, the location of abortion providers after legalization may reflect the interplay of abortion supply and demand in each state. Hence, to lessen the endogenous bias associated with distance to nearest abortion provider, I limit the estimation of equation (2) to the years 1968-1975. The results based on the restricted sample are shown in columns (4) and (5) of Appendix Table 1. The findings are largely the same[46].

2.6.2 Abortion and the Timing of Marriage

June CPS results

The basic trend depicted in Figure 5 presents descriptive evidence of a coincident turning point in access to abortion and distribution of age at first marriage across group of cohorts. Table 3 presents the results by formally examining this connection in a regression setting. Each column represents a separate regression based on equation (3). Since the dependent variables are the cumulative probability of marrying prior to age 16 through age 20, I restrict my attention to all women older than age 21 at the time of the survey. Given the retrospective nature of the data, I further restrict the sample to women under age 65 to lessen the recall bias. To minimize measurement error, I drop observations with allocated values on the key variables (Bailey 2006). The final sample includes 230,095 women who were born between 1935 and 1960. The cohort restriction means I am looking at women who turned 20 between 1955 and 1980, a period during which the average abortion rate went from 0 to 29.1 per 1000 women.

46 The estimated coefficients of access to abortion across all regressions are larger in magnitude. However, given the fact that the mean of teen marriage rates is larger in the restricted sample, the relative percentage decline in teen marriage rates remains approximately the same.

There are two sets of regression results reported in Table 3, corresponding to the two indices of exposure to abortion access. Panel A reports estimates of equation (3) based on the moving average of teen resident abortion rates when the individual was ages 15 to k-1[47]. The coefficients of exposure to abortion access are large, negative and statistically significant across all outcomes, meaning that higher abortion rates are associated with lower probabilities of marrying before ages 16 to 20. Consider the probability of marrying before age 20. The mean moving average of abortion rate increased from 0 through the 1950 cohort to 30.5 (per 1,000) for the 1958 cohort. Thus the coefficient of $\mathrm{ABORTION\ ACCESS}(20)$ (-0.12) implies that the change in access to abortion (from zero to 30.5 per 1000) led to a 10 percent decrease, relative to the mean, in the fraction of women who married before age 20.[48] This roughly accounts for 50 percent of the cross-generational decline in the proportion of marrying before age 20. Panel B of Table 3, a parallel analysis using the constructed distance index as an alternative measure of ABORTION ACCESS(k), yields qualitatively similar findings with different magnitude. As expected, the negative relationship between distance indices and the probabilities of marrying prior to ages 16 through 20 are significant across all outcomes. Taken together, the results shown in Table 3 suggest sizable effects of marriage delay due to the increasing exposure to abortion access, controlling for women's access to the pill along with the minimum age of marriage and unilateral divorce law.

Race-specific estimates

47 I rescale the abortion access variable from per 1,000 women to per 10 women to retain the decimal places

48 The percentage change is obtained by [(0.12)*(30.5/100)]/0.37=9.8%

The separate estimates for whites and blacks in Table 4 suggest that the exposure to abortion access had different effects for white (Columns 2 and 5) and black teen women (Columns 3 and 6). Columns 1 and 3 reproduce the corresponding estimates on the probability of marrying prior to age 18 and age 20 for the full sample. The point estimates for white women are similar to the results for all women in sign and significance level. Moreover, the larger estimated coefficients suggest that the exposure to abortion access have led to a bigger decline in the probability of early marriage for white women. In contrast, the estimated effects for blacks are smaller in magnitude and statistically insignificant. The results suggest that the early marriage decision among black teen women were not affected by the increasing exposure to abortion accessibility.

Sensitivity Analysis: Census Results

Since I am concerned with the possible measurement error associated with measuring access to abortion based on a women's current state of residence instead of her actual state of residence as a teenager, Table 5 presents estimation results obtained from the 1980 IPUMS 1 percent sample. Using specification comparable to equation (3), I generate exposure to abortion access indices based on individual's state of birth. The estimated effects of increasing abortion access in the birth state are consistent of the estimates obtained from CPS. Following Table 4, I show the estimates on the probability of first marriage occurring prior to age 18 and 20 respectively based on the full sample and race-specific subsamples. The results suggest that the potential measurement error associated with migration does not systematically bias the result.

The estimated coefficients are slightly smaller in magnitude and significant across all outcomes for all women and whites. Again, I find no significant association between exposure to abortion access and black teen marriage decisions. The overall finding suggest that although I am unable to observe actual state of residence before age 20, using current state of residence may lead only to slight attenuation of the estimates.

2.6.3 Abortion and the Shotgun Marriage

Graphical Analysis

The above estimation results indicate that the dramatic increase in the access to abortion during the early 1970s significantly reduced teen marriage rates and induced women to postpone first marriages before age 20. These implications motivate the following empirical analysis to further examine whether the significant delay in marriage before age 20 was mainly due to the demise of shotgun marriage among teen women. Figure 9 shows trends in the changing likelihood of a woman's first marriage being a shotgun marriage, across cohorts by different age groups. One important observation from the graph is that teen women 15 to 19 years of age have the highest probability of having shotgun marriages across all cohorts. The likelihood of a shotgun marriage reached its peak for women born around 1953, who turned 19 in 1972. The following monotonic decline for cohorts born afterwards coincides with the advent of legalized abortion. For women in older age groups, the cross-generational decline in the probability of having a shotgun first marriage started earlier among women who were born between 1949 and 1952. These are the women who turned 20- to 24 years of old when abortion was legalized.

Regression results

The trends in shotgun marriages lead to the following analysis in the context of regressions. I test the AYK's hypothesis by examining the impact of increasing abortion access on the probability of having a shotgun first marriage. The specification follows equation (3) in the previous section. The sample restriction criteria remain the same. The data set draws on the 12 waves of the June CPS surveys with both marriage and fertility information available[49]. I include in the analysis sample all women regardless of their marital and fertility status so that the estimates measure the effect of abortion exposure on the *unconditional* probability of having shotgun marriage.

I ran regressions for all women and the race-specific sub-samples separately. The dependent variable is a single indicator variable which equals 1 if woman *i* living in state *s* ever had a shotgun first marriage before age 20[50]. The outcome variable equals 0 if at the time of the survey 1) the woman had never been married 2) the woman had been married without children 3)the woman had had a first birth at least 7 (shouldn't this be 8 months?) months *after* the first marriage. The key explanatory variable is ABORTION ACCESS(20)[51]. Table 6 presents the results. The exposure to abortion access during the teenage years significantly reduced the probability of having a shotgun marriage before age 20 among all women (columns 1 and 2). Comparing women who were born before 1950 and women born 10 years later, the increase in access to abortion (36 per

49 I further exclude 1991 and 1993 because women in these two years were not asked about their marriage history.

50 I also estimate effect of abortion on the probability of having shotgun marriage between ages 20 to 24 based on three versions of definition (results available upon request). None of the coefficients appears significant.

51 The variable ABORTION ACCESS(20) equals to the average of the estimated resident teen abortion rates in state s during the years when women i was between the ages from 15 to 19.

1,000 women) led to a 2.9 percentage-point decline in the probability of teen shotgun marriage, or 54.3 percent decline relative to the mean.

One concern regarding the above definition of a shotgun marriage is that the narrow time frame between the first marriage and the first birth may not capture all marriages that were actually associated with the decision of legitimizing a premarital pregnancy. Although the duration of seven months has been the most commonly used cutoff in the literature to define shotgun marriage, it is possible that men may marry women for the same reason shortly *after* the birth of their first out-of-wedlock child. To minimize this potential measurement error, I expand the definition of shotgun marriage by including first marriages that occurred 3 months, or 6 months *after* the first birth. Panel B and C of Table 6 show the results for 3 and 6 months, respectively. The estimated coefficients are very similar to those of panel A, suggesting that the results are not sensitive to alternative definitions of shotgun marriage.

After stratifying the sample by racial groups, the estimation results reveal important heterogeneity between whites and blacks. Access to abortion had a significant negative effect on the probability of a teen shotgun marriage for white woman, but not for blacks. One possible explanation is that there were fewer stigmas associated with nonmarital pregnancy among blacks, whereas for most of the white women the premarital pregnancies that used to be resolved by shotgun marriages before the 1970s were more likely to be terminated by abortion afterwards. Figure 9 shows the trends in the shotgun marriage rate (following the same definition as in Figure 3) for whites and blacks separately. The trend in the black shotgun marriage rate started to fall around the early 1960s and exhibited no significant change during the 1970s. This suggests that there might be other explanations of the decline in the shotgun marriage rate among blacks. For example, the declining black male employment levels may have contributed to the declining shotgun marriages since the 1960s (Wilson 1987). Moreover, the change in the social welfare system, namely the increasing generosity of Aid to Families with Dependent children (AFDC) program, has also been considered as one of the key catalysts for the prevalence of non-marital childbearing among the blacks (Murray 1994; Rosenzweig 1999).

Falsification Test

If the dramatic increase in abortion access during the 1970s mainly affected teen women's marriage behavior by reducing the need or pressure to marry in the event of premarital pregnancy, one possible hypothesis is that abortion had less to do with the kind of marriages that were *less* likely to be associated with pregnancy and childbearing. I therefore pursue an additional falsification test, in which the dependent variables equal 1 if a woman had ever had a teen first marriage that occurred at least one year, or three years before the first birth[52].

Table 7 shows the regression results of the falsification test. Results support the hypothesis. The exposure to abortion access has no significant effect on the probability of being teen wife but not teen mother for at least 1 to 3 years.

2.7 Conclusion

In this paper, I analyze how the sudden increase in abortion availability brought about by legalization of abortion in the early 1970s changed the decision to marry among

52 I also assign 1 to childless women who married before age 20.

young women. I fill in the gap in the abortion literature by establishing the link between marriage decisions and abortion policies. I also extend the literature on the effect of abortion legalization in several dimensions. First, I provide new estimates of abortion rates based on newly discovered data that reveal the substantial number of abortions prior to *Roe* performed to women traveling from states in which abortion was illegal. Second, I disentangle the effect of abortion on marriage from a variety of concurrent legislative changes that jointly affected family formation.

Three major findings stand out from this study. First, legalization of abortion reduced the state-level teen marriage rate by 10 percent. Second, young women postponed the entry into first marriage because of greater abortion access brought about by legalization. Last but not least, my empirical results are consistent with Akerlof et al. (1996)'s hypothesis that the increasing access to abortion is associated with fewer shotgun marriages among teen women. Overall, this paper provides evidence that legalization of abortion effected women's marriage decisions and that its impact is most significant among teen women and white women.

The legalization of abortion "met a large number of needs and radically changed our society" (Caldwell 2008). This study has demonstrated its profound impact on family formation. And yet, there are reasons to believe that the estimated impact is e conservative. The ultimate difficulty in drawing causal inferences from any of these estimates lies in constructing a convincing counterfactual: would couples marry earlier and would the rate of shotgun marriages increase if *Roe v. Wade* is overturned? After all, the changes in social attitudes toward sexual behavior, the growing acceptance of premarital pregnancy, along with the substantial demographic changes in the past century are non-reversible.

Appendix A: Sources of Abortion Data

In this paper, I assemble abortion data from different sources to construct key explanatory variable that measure the dramatic increase in the abortion availability during the 1970s.

Abortions prior to Roe

For the information on induced abortion performed prior to *Roe*, I use two data sources. The numbers of abortion performed in each state were obtained from historical record of abortion surveillance surveys, conducted by the Centers for Disease Control and Prevention (CDC) from 1971 to 1973. These reports are the early precursors to the CDC's current Abortion Surveillance summaries. The data are not available electronically and must be loaded manually (CDC 1971 1972 1974)[53]. The surveillance summaries also include total abortions and age-specific abortions by state of occurrence. However, there is no age-specific resident abortion data available.

The second source of pre-Roe abortion data is obtained from New York State Department of Health (NYSDOH). The NYSDOH archived data on all abortions performed in the state from 1971-1975 by women's state of residence. I have obtained an aggregated extract of the number of abortions performed in New York by year (1971-1972), state of residence and age groups (15, 15-17, 18-19, 20, 21-24, 25+, and unknown) [54]. Since New York was the most frequented destination for women living out-

53 The CDC data on abortions by state of residence come from Table 5 in the 1971 and 1972 abortion surveillance summaries for those years (CDC 1972, 1973) (see notes 6 on page 12). However, there is no equivalent table for 1970. The CDC summary for 1970 includes a bar chart with resident abortion ratios (abortions per 1000 live births) by state of residence in 1970. I converted the bar graphs to actual numbers using the Pixel Ruler 3.10 10 (http://www.fileheap.com/download-pixel-ruler-18593.html). To recover the number of abortions from abortion ratios, I multiplied the abortion ratio by births/1000 in 1970. Data on the number of live births were obtained from the CDC NCHS Vital Statistics Natality Report (See http://www.cdc.gov/nchs/products/vsus.htm).

54 The NYSDOH would not release individual level data.

of-state seeking an abortion before 1973, the richness of New York data provide a unique opportunity for researchers to uncover interstate travel for abortions and age variation in abortion utilization prior to *Roe* (Joyce et al. 2012).

Abortions after Roe

Abortion rates by year and state of residence come from the Guttmacher Institute (GI) beginning in 1973. The GI researchers survey abortion providers periodically as to the number of abortions performed at the facility after 1973. The GI publishes reports on total number of abortions by state of occurrence in each state in selected years. Researchers at GI then employ an algorithm to convert data by state of occurrence to one in which it is arranged by mother's state of residence. The *estimated* resident abortions reported by GI become the most commonly used series in previous studies (Donohue and Levitt 2001 2004 2006, Goldin and Katz 2002; Ananat et al. 2009).

Despite the completeness of the survey, the GI data on abortions are not available by state and any other characteristics such as age. Thus, many researchers also use data reported by the CDC abortion surveillance program as an additional data source. The CDC collect the numbers of abortions annually from participating state health departments from 1970-2006. Data stratified by age groups are available by state of occurrence. In this paper, I use data from 1973 to 1985.

Appendix B   Estimation of Teen Resident Abortion Rates

My first goal is to construct a complete series of total abortion rates (number of abortions per 1,000 women 15 to 44 years of age) by state of residence and year from 1970. My approach is to combine CDC resident abortions for 1970-72 with the GI *estimated* resident abortions from 1973 to 1985[55]. I then create a fifteen-year series of total resident abortion rates by state and year from 1970 to 1985. Population is from the Surveillance Epidemiological and End Results (SEER) from the National Cancer Institute.

Next, I implement an algorithm to estimate the proportion of abortion performed to women 15 to 19 years of age for residents in each state. The proportion is approximated by the ratio of the abortion rate for teen women to the overall abortion rate within each state. I then multiply the estimated teen-to-total abortion ratio by the total resident abortion rate to arrive at an estimate for resident teen abortion rate.

To estimate the ratio of teen-to-total abortion rate prior to *Roe*, I make use of the aforementioned unique data on abortions in 1971 and 1972 performed in NY by women's state of residence. I assume that women from eastern states are more likely to travel to New York to terminate their pregnancies. Therefore for states east of the Mississippi, I use the calculated ratio based on the NYS data. For states west of the Mississippi I use the average ratio for all the states east of the Mississippi (1.46 in 1970 and 1971, 1.56 in 1972). For the four early legalizing state (Alaska, California, Hawaii

55 Note that there is no equivalent data in 1983 GI data. I took the average of 1982 and 1984 for data in 1983.

and Washington), I use the actual ratio of teen to total abortions rates occurred in those states reported by the CDC (Table 5 CDC 1972, Table 6 CDC 1973).

For the period from 1973 to 1985, I use the CDC abortion surveillance data by age, state of occurrence and year. I computed the teen abortion rate (Abortions 15-19/(Female population15-19), the rate for all women (all abortions/ female population 15-44) and the ratio of the two. I make an assumption that the ratio of teen to total abortion rate by state of occurrence after *Roe* is approximately equal to the ratio of resident teen to total abortion rate.

I imputed the missing data in certain state-year cell based on the following criteria. If there were missing years within a state, then we used the average of the known ratios for the missing years. If there was no ratio in any year then we used the ratio from a neighboring state (except for TX in which I used CA given the large Hispanic populations) as follows:

AL use MS	ND use SD
FL use GA	OK use KS
ME use NH	TX use CA
MI use IN	KY use TN
	IA use NE

The final step is to multiply the estimated ratio of teen-to-total abortion rate by total resident abortion rate assembled from the CDC and GI for each state and year. Two final adjustments are made. First, I substitute the actual resident teen abortion rate in NY as reported by NYSDOH for the estimated rate. In addition, for New Mexico in 1971, resident abortion rate equals to (the estimated rate)*(1-68.9%). The reason is that in 1971, numbers of abortions reported by NM include both in-state and out-of-state abortions performed in NM. In 1972 CDC report Table 4, it is said 68.9% abortions were performed on out-of-state residents. So I use this percentage to estimate the 1971 rate for residents. Appendix Figure 1 compares the time-series trend of estimated resident teen abortion rate with the available figures reported by GI (Henshaw and Kost 2008). The difference between the two series after 1973 is less than 2 percent.

Appendix C: Numerical Examples of the Exposure to Abortion Access in Cohort Analysis

Distance and abortion indices provide gauges of the potential differences in exposure to abortion access across state and birth cohorts. To explain the construction of the key explanatory variable ABORTION ACCESS(k) in equation (2) in greater detail, this appendix provide some concrete numerical examples for each of index.

Recall that distance index is defined as the average of inverse distance to the nearest abortion provider during the years when a women was age 15 to age k-1 (k=16, 17,…, 19) lived in state s. Specifically, for each marriage event before age k, the distance index is calculated as:

$(A-1)\text{: } Distance\ Index\ (k)$

$$= \frac{\sum_{a=15}^{a=k-1} \mathrm{I}(\mathrm{c}+\mathrm{a} \geq 1970) * \left(\frac{1}{\mathrm{Distance}}_{(\mathrm{c+a})\mathrm{s}}\right)}{k-15}, \text{normalize to } 0-1 \text{ scale}$$

To give a concrete example, consider the exposure to abortion access before age 20. Women born in 1951 are the first generation who gained one-year access to early legalized abortion services. The mean distance index for this cohort is 0.03 (S.D. 0.07). Specifically, for residents who lived in New York, where abortion was legalized within the state when the 1951 cohort turned 19 in 1970, the value of the distance index is .03, whereas the distance index is .00007 for women born in the same year who lived in Ohio. To compare, the mean exposure for the 1957 cohort is 0.17 (S.D 0.3). For women who lived in New York the distance index is 0.51 whereas the index is .02 for women from Ohio.

For the use of teen resident abortion rates as proxy for abortion access, I define the cohort-state measure of average exposure to abortion availability as:

$$(A-2)\ Average\ Abortion\ Rate\ (k) = \frac{\sum_{a=15}^{a=k-1} Teen\ Abortion\ Rate_{(c+a)s}}{k-15}$$

Again, consider the exposure to abortion access before age 20 between women who lived in New York and Ohio. Women who were born in 1951 would turn age 19 in 1970. The average teen abortion rate for the cohort is New York resident 5.3 per 1,000 teen women (S.D. 4.3). However, New York residents had the highest exposure to abortion access as on average every 15 per 1000 teen women obtained legal abortions in that year. The same static for teenage women lived in Ohio in 1970 was only 3 per 1,000. In contrast, for women born in 1958 who turned age 15 to 19 after Roe between 1973-77, the mean exposure to abortion access is 31.1 per 1,000 teen women (S.D. 13). Again, for women who lived in New York the average teen abortion rate is as high as 49.4 per 1,000 teen women, whereas the average exposure is 26.2 per 1,000, or 47 percent lower for Ohio residents.

Table 2-1: Age-Period-Cohort Diagram

	Calendar Years															
						Pre-Roe era					Post-Roe era					
Birth Cohort	**65**	**66**	**67**	**68**	**69**	**70**	**71**	**72**	**73**	**74**	**75**	**76**	**77**	**78**	**79**	**80**
1950	15	16	17	18	19	**20**										
1951		15	16	17	18	19	**20**									
1952			15	16	17	18	19	**20**								
1953				15	16	17	18	19	**20**							
1954					15	16	17	18	19	**20**						
1955						15	16	17	18	19	**20**					
1956							15	16	17	18	19	**20**				
1957								15	16	17	18	19	**20**			
1958									15	16	17	18	19	**20**		
1959										15	16	17	18	19	**20**	
1960											15	16	17	18	19	**20**

*Age in each cell

Table 2-2: Reduced Form Estimates of the Effect of Legalization of Abortion on First Marriage Rates among Teen Women 15- to 19-year olds, 1968-1979

	Dep.Var= First marriage rates for teen women 15 to 19 years of age					
	Diff-in-Diff		*WLS: Proxy Measures of Abortion Access*			
	(1)	(2)	(3)	(4)	(5)	(6)
Repeal State*1970-1972	-5.281*	**-5.128*** **				
	[2.917]	[1.836]				
Repeal State*1973-1975	-0.9	-2.113				
	[4.949]	[1.988]				
Repeal State*1976-1979	1.794	0.987				
	[6.878]	[3.057]				
Resident Teen Abortion Rate			-0.267**	**-0.224*** **		
			[0.101]	[0.079]		
Distance*1970-1972					0.777**	**0.553** **
					[0.327]	[0.233]
Distance*1973-1979					-0.144	0.017
					[0.864]	[0.314]
Unilateral Divorce Law	-2.17	-0.406	-1.325	-1.381	-1.914	-1.435
	[2.254]	[1.156]	[2.344]	[1.459]	[2.494]	[1.430]
Early Marriage Law	7.810***	3.531**	7.704**	4.439**	7.448**	4.743***
	[2.720]	[1.593]	[3.084]	[2.047]	[2.872]	[1.665]
Early Access to the Pill	2.829	2.284	2.96	1.789	3.709	2.804*
	[2.683]	[2.103]	[2.846]	[2.007]	[2.521]	[1.509]
State, year FE	y	y	y	y	y	y
State*year linear trend	n	y	n	y	n	y
N	496	496	496	496	417	417
Mean of Dep. Var		*56.997*			*55.31*	

Source: NCHS Vital Statistics 1969-1979. Number of states included in the MRA (marriage report area) varies by year.

Notes: First marriage rate is defined as the number of first marriages divided by number of female population ages 15 to 19. All regressions are weighted by the state female population in the corresponding age group. Column (1)-(2) pertain to Diff-in-Diff estimates specified as equation(1) in the text. Column (3)-(6) pertain to WLS estimations specified in equation (2). Distance measures are undefined before 1970 and are treated as missing variables. All standard erros are clustered at state level. * p<0.10, ** p<0.05, *** p<0.01

Table 2-3: Estimated Effects of Exposure to Abortion Access on Age at First Marriage

Dependent Variable: 1= Married before Age k

	16	**17**	**18**	**19**	**20**
Mean Dependent Variable	*0.0188*	*0.0546*	*0.1195*	*0.238*	*0.3707*
	Panel A: Proxy Meaure 1 - Average Resident Teen Abortion Rates				
Abortion Access(*k*)*	-0.0375***	-0.0747***	-0.1252***	-0.2023***	-0.1200**
	[0.0107]	[0.0172]	[0.0220]	[0.0373]	[0.0553]
Pill Access for minors#	-0.0003	0.001	0.0023	0.0054	0.0078
	[0.0019]	[0.0034]	[0.0040]	[0.0053]	[0.0062]
Nonconsent legal marriage+	-0.004	-0.0062	-0.0073	-0.0071	-0.0098
	[0.0028]	[0.0064]	[0.0068]	[0.0100]	[0.0133]
Unilateral Divorce§	0.004	0.0003	0.001	-0.0019	-0.0038
	[0.0026]	[0.0032]	[0.0044]	[0.0053]	[0.0068]
	Panel B: Proxy Meaure 2 - Distance Index				
Abortion Access(*k*)*	-0.0154***	-0.0316***	-0.0390***	-0.0606***	-0.0357***
	[0.0038]	[0.0062]	[0.0084]	[0.0090]	[0.0129]
Pill Access for minors#	-0.0005	0.0006	0.0026	0.0062	0.0086
	[0.0019]	[0.0033]	[0.0044]	[0.0063]	[0.0067]
Nonconsent legal marriage+	-0.0050*	-0.0081	-0.0086	-0.0074	-0.0094
	[0.0029]	[0.0062]	[0.0072]	[0.0118]	[0.0145]
Unilateral Divorce§	0.0056**	0.0027	0.0031	0.0013	-0.0017
	[0.0028]	[0.0033]	[0.0055]	[0.0066]	[0.0075]

Data: June Fertility Supplements to the Current Population Survey, 1979-1995. Sample: women who were born between 1935 and 1960, whose ages were between 21 and 65 at the time of the survey (n=230,095).
Note: Each column within each panel is a separate estimate of equation (3) in which the dependent variable indicates whether a women had married prior to ages 16 to age 20. All models include control for race and ethnicity, state fixed effects, cohort fixed effects and state*cohort linear time trends. Standard errors are clustered at state level. *p<0.10 **p<0.05 ***p<0.01
*Abortion Access(k) in panel A is defined as the moving average of resident teen abortion rates in the individual's state of residence when the individual was 15 to k-1 years old (see equation(A-1)). Abortion Access(k) in panel B is the distance index defined as the average of inverse distance to the nearest abortion provider between the years when a women was age 15 to age k-1 who resided in state s. See equation (A-2). # The coding and definition of the pill access for minors follows Goldin and Katz(2002). The dummy variable equals one if minors (16 years and older) had access to birth control pill without parental consent in the individual's state of residence by the time the individual was 18 years old. +The source of mininum age of marriage law is from Blank et. al (2002) and Edlund and Mochado(2011). Nonconsent legal marriage is coded as a dummy variable equals one if an individual women could legally marry without parental consent before age 21. §Unilateral Divorce equals one if the women had an easier exit option from marriage under the unilateral divorce law regime before age k (Gruber 2004)

Table 2-4: Race-specific Estimated Effects of Exposure to Abortion Access on Age at First Marriage

	Dep. Var. : 1=Married before Age 18			Dep. Var. : 1=Married before Age 20		
	All	Whites	Blacks	All	Whites	Blacks
	(1)	(2)	(3)	(4)	(5)	(6)
Mean Dependent Variable	*0.1195*	*0.1184*	*0.1273*	*0.3707*	*0.3767*	*0.3272*
	Panel A: Proxy Meaure 1 - Average Resident Teen Abortion Rates					
Abortion Access(k)*	-0.1252***	-0.1382***	-0.0301	-0.1200**	-0.1262**	-0.0958
	[0.0220]	[0.0268]	[0.0422]	[0.0553]	[0.0567]	[0.1014]
Pill Access for minors#	0.0023	0.0044	-0.0121	0.0078	0.0093	-0.0042
	[0.0040]	[0.0050]	[0.0114]	[0.0062]	[0.0067]	[0.0143]
Legal Teen Marriage+	-0.0073	-0.0039	-0.0223	-0.0098	-0.0092	-0.0054
	[0.0068]	[0.0080]	[0.0153]	[0.0133]	[0.0130]	[0.0285]
Unilateral Divorce	0.001	0.0017	-0.0044	-0.0038	-0.0039	-0.0037
	[0.0044]	[0.0049]	[0.0102]	[0.0068]	[0.0071]	[0.0191]
	Panel B: Proxy Meaure 2 - Distance Index					
Abortion Access(k)*	-0.0390***	-0.0415***	-0.0085	-0.0357***	-0.0368**	-0.0044
	[0.0084]	[0.0115]	[0.0195]	[0.0129]	[0.0156]	[0.0323]
Pill Access for minors#	0.0026	0.005	-0.0123	0.0086	0.0102	-0.0043
	[0.0044]	[0.0054]	[0.0112]	[0.0067]	[0.0072]	[0.0139]
Legal Teen Marriage+	-0.0086	-0.0054	-0.0223	-0.0094	-0.009	-0.003
	[0.0072]	[0.0081]	[0.0155]	[0.0145]	[0.0140]	[0.0303]
Unilateral Divorce	0.0031	0.0037	-0.0038	-0.0017	-0.0018	-0.0034
	[0.0055]	[0.0061]	[0.0111]	[0.0075]	[0.0076]	[0.0196]
N	230095	202452	27643	230095	202452	27643

Data: June Fertility Supplements to the CPS. Sample: women who were born between 1935 and 1960, whose ages were between 21 and 65 at the time of the survey.

Note: Each column within each panel is a separate estimate of equation (3) for whites(non-hispanic) and blacks. Columns (1) and (4) reproduce the estimates on the probabiliy of marrying before age 18 and 20 for the full sample. See notes to Table 3 for definition of abortion access. All models include controls for the access to the pill, minum age of marriage and unilateral divorce law. State fixed effects, cohort fixed effects and state X cohort linear time trends are also included in each regression. Standard errors are clustered at state level. *p<0.10 **p<0.05 ***p<0.01

Table 2-5: Sensitivity Analysis of Exposure to Abortion Access and Age at First Marriage, 1980 Census Results

	Dep. Var. : 1=Married before Age 18			Dep. Var. : 1=Married before Age 20		
	All	Whites	Blacks	All	Whites	Blacks
	(1)	(2)	(3)	(4)	(5)	(6)
Mean Dependent Variable	*0.138*	*0.1367*	*0.1456*	*0.3684*	*0.3781*	*0.3099*
	Panel A: Proxy Meaure 1 - Average Resident Teen Abortion Rates					
Abortion Access(k)	-0.0552***	-0.0615***	-0.0492*	-0.0611*	-0.0667**	-0.0566
	[0.0188]	[0.0211]	[0.0287]	[0.0319]	[0.0299]	[0.0380]
Pill Access for minors#	0.0013	0.0028	-0.0077	0.0072	0.0076	0.0003
	[0.0034]	[0.0038]	[0.0053]	[0.0061]	[0.0060]	[0.0070]
Legal Teen Marriage+	0.0058	0.0081*	-0.0024	0.0058	0.0046	0.0097
	[0.0037]	[0.0045]	[0.0105]	[0.0061]	[0.0053]	[0.0140]
Unilateral Divorce	0.0004	0.0014	-0.0128*	0.0081	0.0025	-0.0052
	[0.0042]	[0.0045]	[0.0071]	[0.0055]	[0.0059]	[0.0122]
	Panel B: Proxy Meaure 2 - Distance Index					
Abortion Access(k)*	-0.0195*	-0.0233**	0.0026	-0.0260**	-0.0294***	-0.0295
	[0.0107]	[0.0113]	[0.0127]	[0.0097]	[0.0085]	[0.0326]
Pill Access for minors#	0.0015	0.003	-0.0075	0.0075	0.0081	0.009
	[0.0036]	[0.0041]	[0.0054]	[0.0058]	[0.0057]	[0.0088]
Legal Teen Marriage+	0.0055	0.0077	-0.0017	0.0057	0.0045	0.0132
	[0.0038]	[0.0048]	[0.0098]	[0.0061]	[0.0055]	[0.0126]
Unilateral Divorce	0.0015	0.0027	-0.0130*	0.0094*	0.004	0.0382***
	[0.0044]	[0.0048]	[0.0070]	[0.0052]	[0.0057]	[0.0134]
N	399747	343146	56601	399747	343146	56601

Data: 1980 Census of Population, IPUMS, 1 percent sample (Ruggles and Sobek 1997) Sample: women who were born between 1935 and 1960, whose ages were between 21 and 65 in 1980.

See note to Table 2 and 3. Each column within each panel is a separate estimate of equation (3) for whites(non-hispanic) and blacks. Abortion access before age k (proxied by average moving average of abortion rates in panel A and distance index in panel B) are all determined according to individual women's state of birth. *p<0.10 **p<0.05 ***p<0.01

Table 2-6: The Effect of Exposure to Abortion Access on the Probability of Having a Shotgun First Marriage before Age 20

Dependent Variable: Probability(1st marriage as shotgun marriage before Age 20)						
Panel A: Shotgun Marriage=1 if First Marriage Occurred within 7 months before the First Birth						
	All Women		**Non-White**		**White**	
	1	2	3	4	5	6
Mean of Dep. Variable	*0.053*		*0.055*		*0.053*	
Abortion Access(20)	-0.063***	-0.079***	-0.005	-0.003	-0.071***	-0.090***
	[0.022]	[0.025]	[0.036]	[0.057]	[0.025]	[0.029]
Minor's Access to the Pill	-0.006	-0.003	-0.033**	-0.007	-0.003	-0.002
	[0.005]	[0.008]	[0.014]	[0.014]	[0.006]	[0.009]
Minimum Marriage Age Law	0	0.006	0.012	0.02	0	0.004
	[0.004]	[0.005]	[0.013]	[0.012]	[0.004]	[0.005]
Unilateral divorce Law	-0.005	-0.004	0.005	0.017	-0.008*	-0.006
	[0.005]	[0.005]	[0.014]	[0.020]	[0.005]	[0.005]
Panel B: Shotgun Marriage=1 if First Marriage Occurred within 7 months before or 3 months after the First Birth						
Mean of Dep. Variable	*0.056*		*0.064*		*0.055*	
Abortion Access(20)	-0.058***	-0.076***	-0.01	-0.029	-0.066***	-0.084***
	[0.021]	[0.023]	[0.034]	[0.090]	[0.024]	[0.025]
Minor's Access to the Pill	-0.004	-0.001	-0.029*	-0.001	-0.002	0
	[0.006]	[0.009]	[0.015]	[0.013]	[0.006]	[0.009]
Minimum Marriage Age Law	0	0.007	0.015	0.022	0	0.005
	[0.004]	[0.005]	[0.014]	[0.015]	[0.004]	[0.005]
Unilateral divorce Law	-0.005	-0.005	-0.002	0.003	-0.008	-0.005
	[0.005]	[0.005]	[0.017]	[0.024]	[0.005]	[0.005]

Continued on next page

Table 6(continued)

Panel C: Shotgun Marriage=1 if First Marriage Occurred within 7 months before or 6 months after the First Birth						
	All Women		**Non-White**		**White**	
	1	2	3	4	5	6
Mean of Dep. Variable	*0.059*		*0.071*		*0.057*	
Abortion Access(20)	-0.069***	-0.079***	-0.041	-0.106	-0.074***	-0.077**
	[0.023]	[0.025]	[0.045]	[0.107]	[0.025]	[0.030]
Minor's Access to the Pill	-0.007	-0.004	-0.037**	-0.012	-0.003	-0.002
	[0.006]	[0.008]	[0.018]	[0.018]	[0.006]	[0.009]
Minimum Marriage Age Law	0.002	0.008*	0.01	0.014	0.002	0.007
	[0.004]	[0.005]	[0.016]	[0.015]	[0.004]	[0.005]
Unilateral divorce Law	-0.005	-0.004	-0.004	-0.001	-0.007	-0.004
	[0.005]	[0.006]	[0.019]	[0.025]	[0.005]	[0.005]
State, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State Linear Trends	No	Yes	No	Yes	No	Yes
N	230095	230095	27531	27531	202452	202452

Data: CPS June Fertility Supplements 1979-1995(excluding 1991 and 1993 with no information on marriage history). Sample: All women who were born between 1935 and 1960, ages 25 and 65.

Note: shotgun marriage is defined based on the duration between age at first marriage and age at first birth(in months). Shotgun marriage in Panel A is defined as a first marriage that occurred seven months prior to the first birth, where the bride was single at the time of the pregnancy. Panel B and C expand the definition by including first marriages that occurred 3 months, or 6 months after the first birth. Abortion Access (20) is defined as the moving average of resident teen abortion rates for individual in state s from ages 15 to 19. All models include controls for the access to the pill, minimum age of marriage and unilateral divorce law. See Table (3) for the detailed coding and data sources. All estimations are weighted by CPS sampling weight. Standard errors are clustered at state level. *p<0.10 **p<0.05 ***p<0.01

Table 2-7: Falsification Tests -The Effect of Exposure to Abortion Access on Early Teen Marriages Less Likely to be Associated with Childbearing Decision

Dep. Var.=1 if women had been a teen wife but not teen mother for at least 1 or 3 years						
		k=1			***k=3***	
	All	Whites	Non-whites	All	Whites	Non-whites
	(1)	(2)	(3)	(4)	(5)	(6)
Mean of Dep. Variable	*0.132*	*0.064*	*0.141*	*0.069*	*0.037*	*0.074*
Abortion Access(20)	0.018	0.035	0.002	-0.01	-0.03	-0.015
	[0.028]	[0.037]	[0.036]	[0.016]	[0.035]	[0.021]
Minimum Marriage Age Law	-0.003	-0.004	-0.003	0.001	0	0.002
	[0.005]	[0.010]	[0.006]	[0.004]	[0.007]	[0.004]
Minor's Access to the Pill	0.013	0.013	0.012	0.010*	-0.013*	0.013*
	[0.010]	[0.012]	[0.012]	[0.005]	[0.008]	[0.007]
Unilateral divorce Law	0.001	0.002	0	0.005*	0.01	0.005
	[0.004]	[0.008]	[0.004]	[0.003]	[0.008]	[0.003]
State, Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes
N	230095	230095	27531	230095	230095	27531

See notes to Table (6). Each column present result from a separate regression. Dependent variable equals one if women married before age 20 but had ever had a first-time childbearing experience at least one year (columns 1 to 3), or three years (columns 4 to 6) after the first marriage (or had remained childless by the time of the survey). All models include controls for the access to the pill, minimum age of marriage and unilateral divorce law. See Table (3) for the detailed coding and data sources. Models for all women include control for race and ethnicity. All estimations are weighted by CPS sampling weight. Standard errors are clustered at state level. *p<0.10 **p<0.05 ***p<0.01

Table 2-8: Robustness Checks of Reduced Form Estimates of the Effect of Legalization of Abortion onTeen First Marriage Rates

	Dep.Var.= first marriage rates for teen 2omen 15 to 19 years of age				
	Unweighted Estimates			Sub-sample: 1968-1975	
	(1)	(2)	(3)	(5)	(6)
Repeal State*1970-1972	-7.053**				
	[2.819]				
Repeal State*1973-1975	-5.689				
	[3.409]				
Repeal State*1976-1979	-0.565				
	[4.337]				
Resident Teen Abortion Rate		-0.189***		-0.263**	
		[0.051]		[0.117]	
Distance*1970-1972			1.007***		0.865**
			[0.290]		[0.345]
Distance*1973-1979			0.314		-0.056
			[0.458]		[0.558]
Unilateral Divorce Law	-0.149	-0.572	-1.26	0.346	-0.253
	[1.128]	[1.121]	[1.398]	[0.952]	[1.588]
Early Marriage Law	4.775***	4.642**	6.075***	5.964**	5.200**
	[1.716]	[1.870]	[1.906]	[2.575]	[2.371]
Early Access to the Pill	1.029	1.408	2.785**	2.067	2.151
	[1.972]	[2.015]	[1.355]	[2.542]	[1.522]
State, year FE	y	y	y	y	y
State*year linear trend	y	y	y	y	y
N	498	498	417	333	252
Mean of Dep. Var	63.671		63.978	62.524	62.494

See Notes to Table 2. Columns (1)-(3) are estimates of equation (1) without population weighting. Column (4) to (6) show WLS estimates of equation (2) on the sample restricted from 1968 to 1975. Each model include state, year fixed effects and state-year linear trends. * p<0.10, ** p<0.05, *** p<0.01

Figure 2-1: The Changing Legal Environment for Young Women: 1965-1985

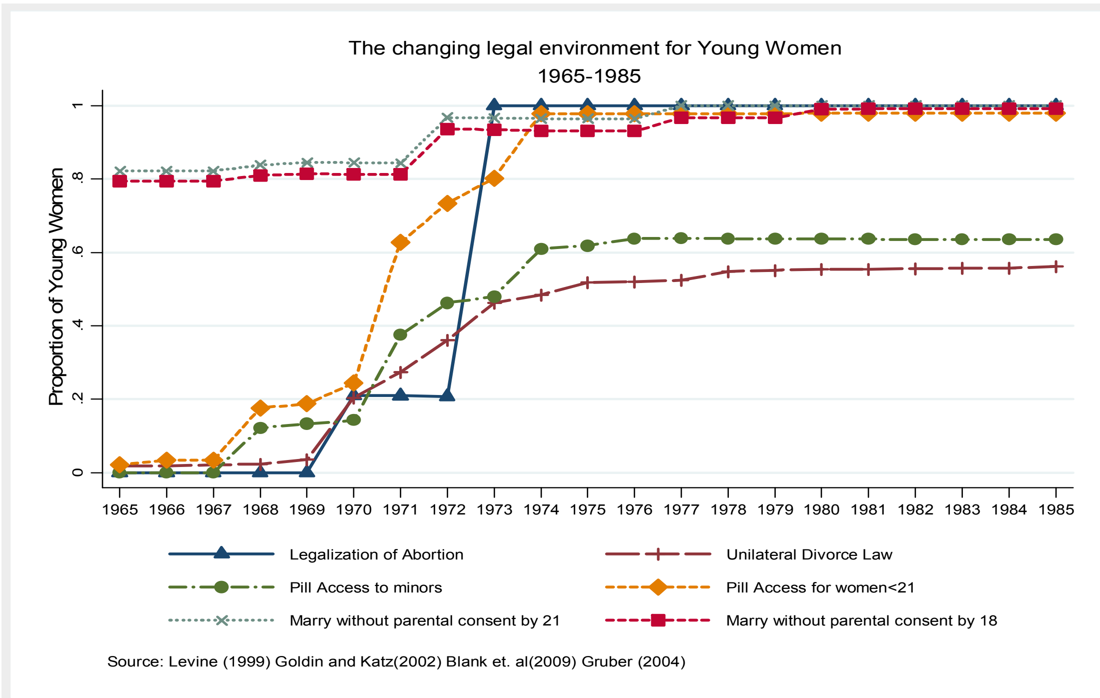


Figure 2-2: Average Resident Abortion Rates for Abortions Performed in NY, 1971-72

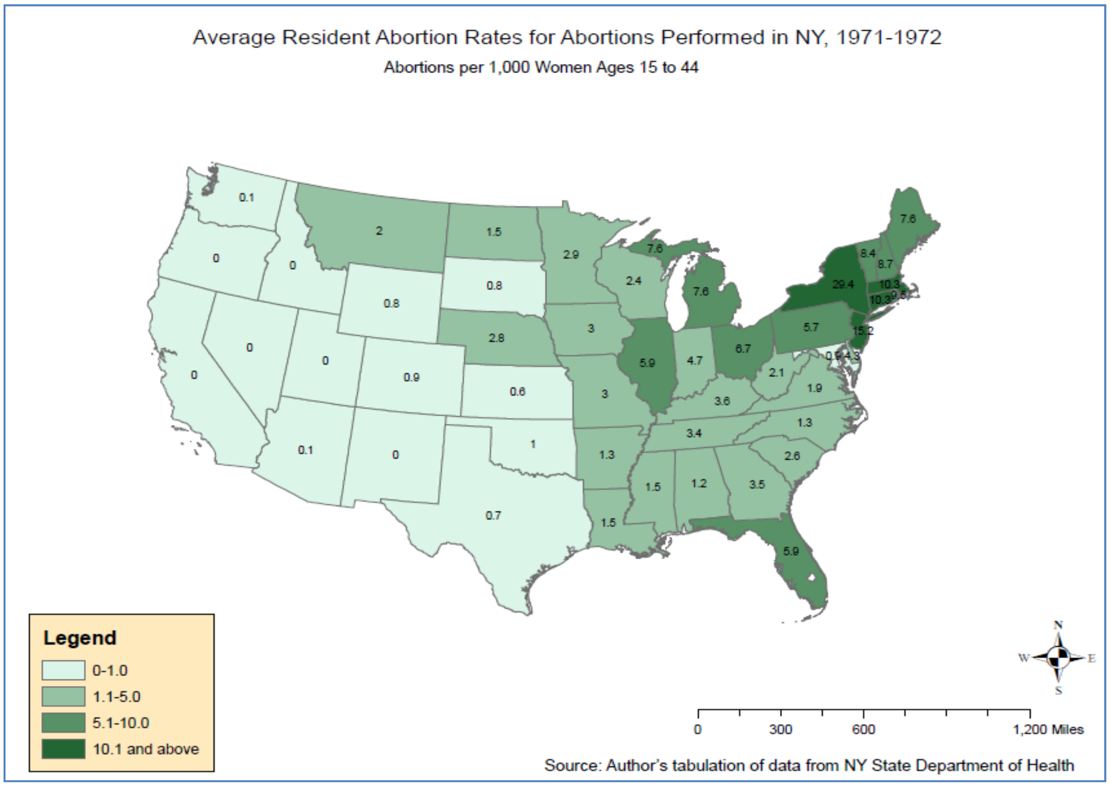


Figure 2-3: The Percentage of Premarital Pregnancies Resolved by Shotgun Marriage

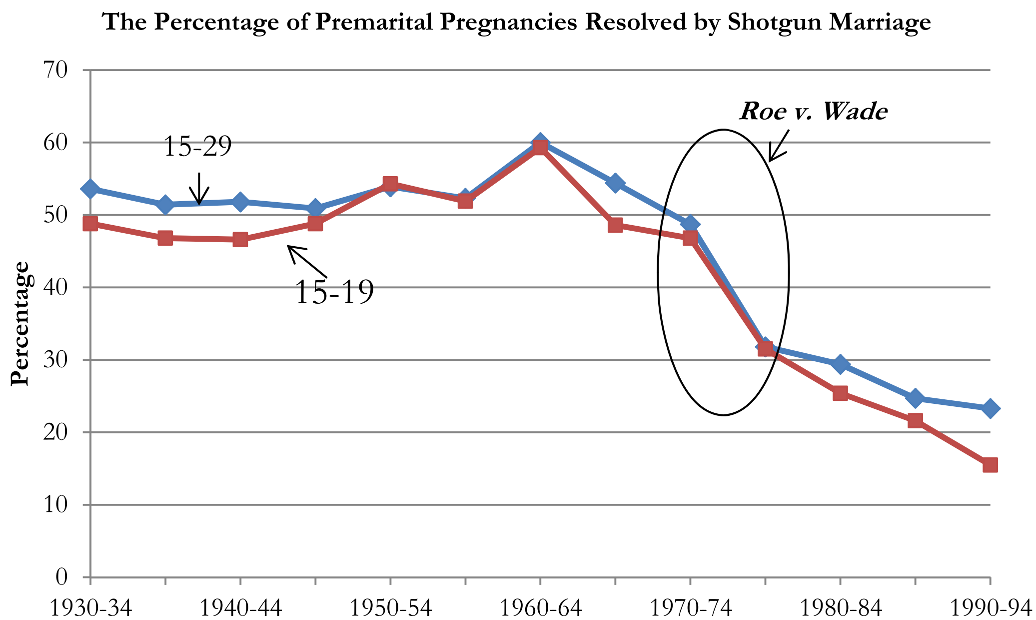


Figure 2-4: Marriage Rates: 1955-2000

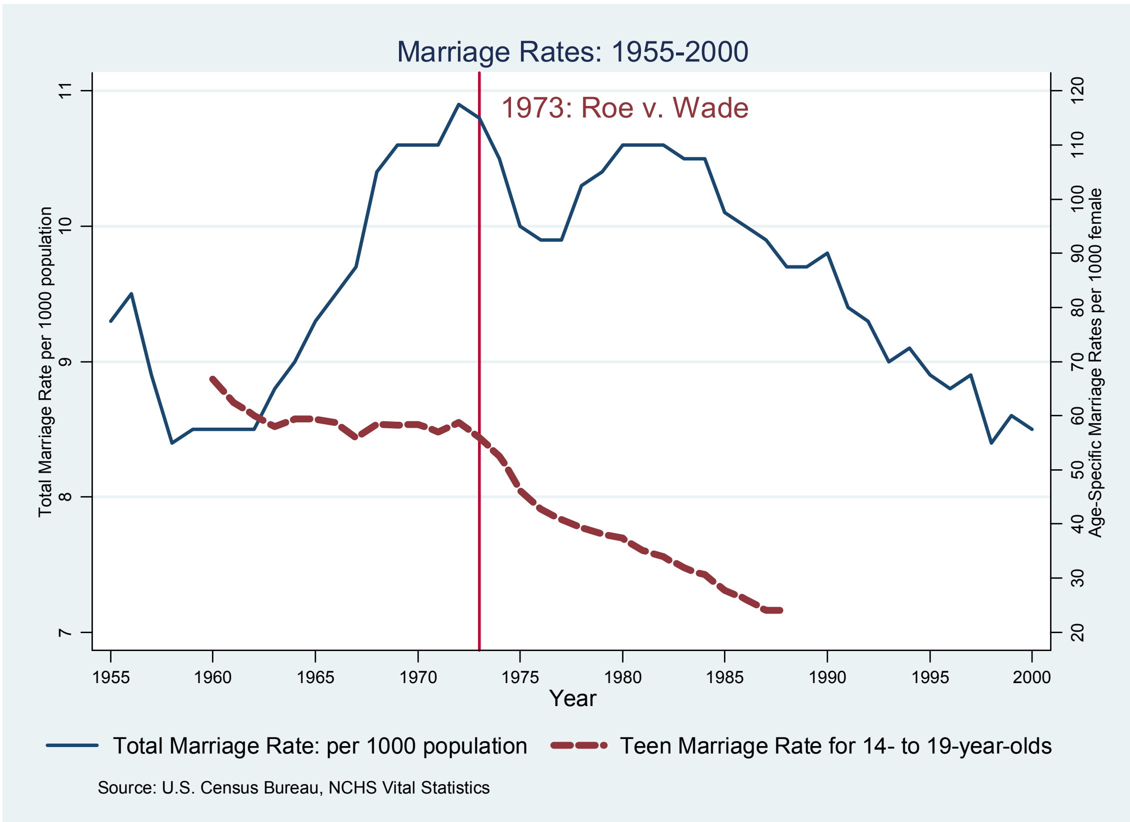


Figure 2-5: The Distribution of Age at First Marriage

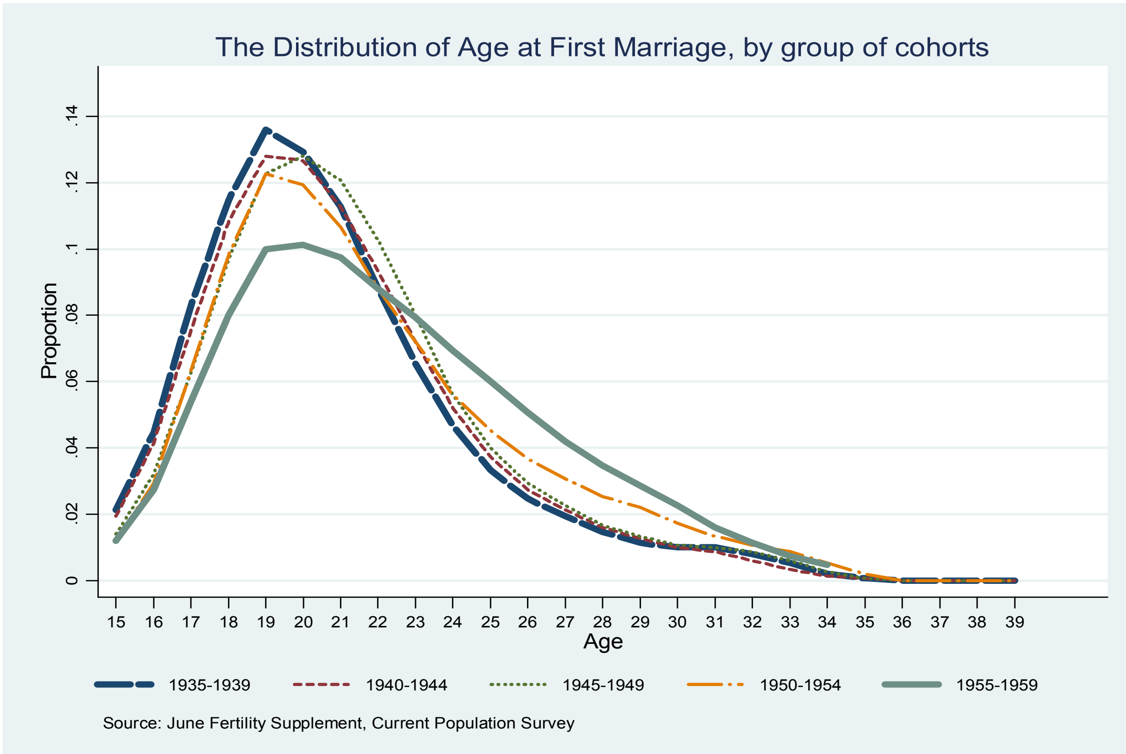
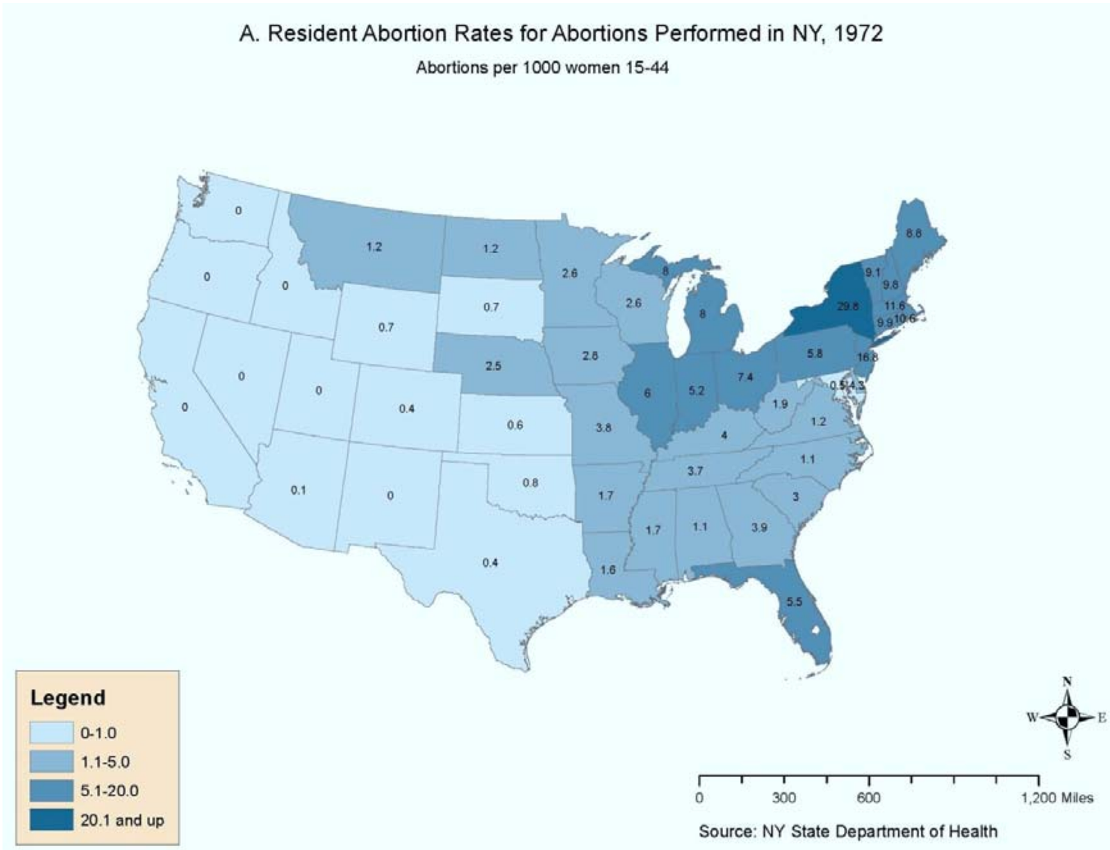
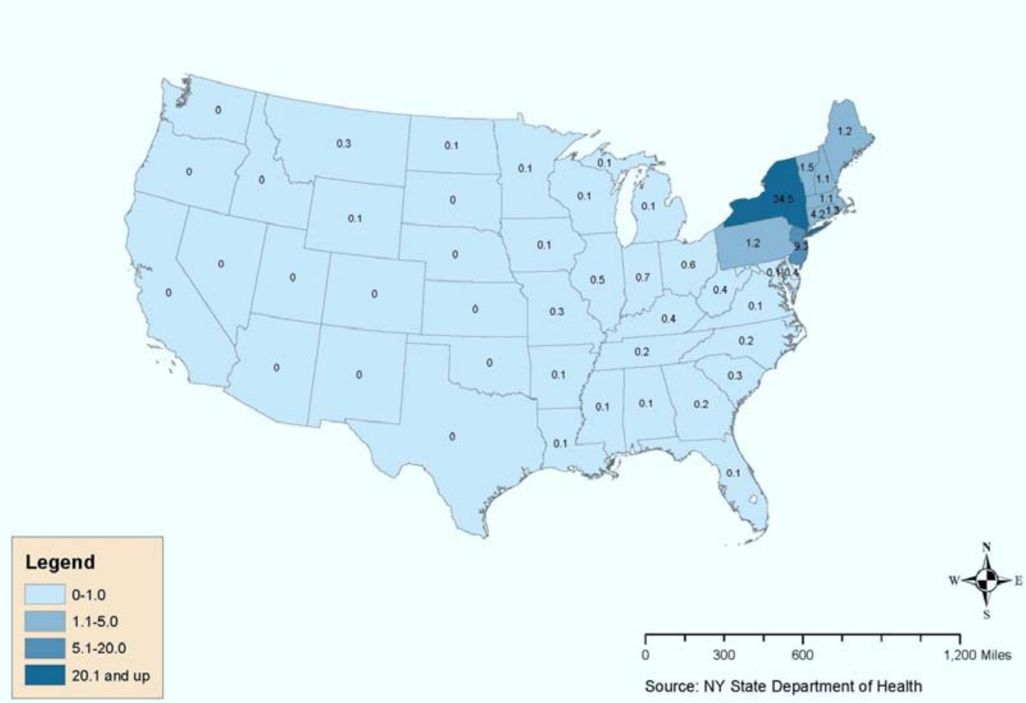


Figure 2-6: Resident Abortion Rates: 1972 and 1974



B. Resident Abortion Rates for Abortions Performed in NY, 1974

Abortions per 1000 women 15-44



C. Total Resident Abortion Rates, 1974

Abortions per 1000 women 15-44

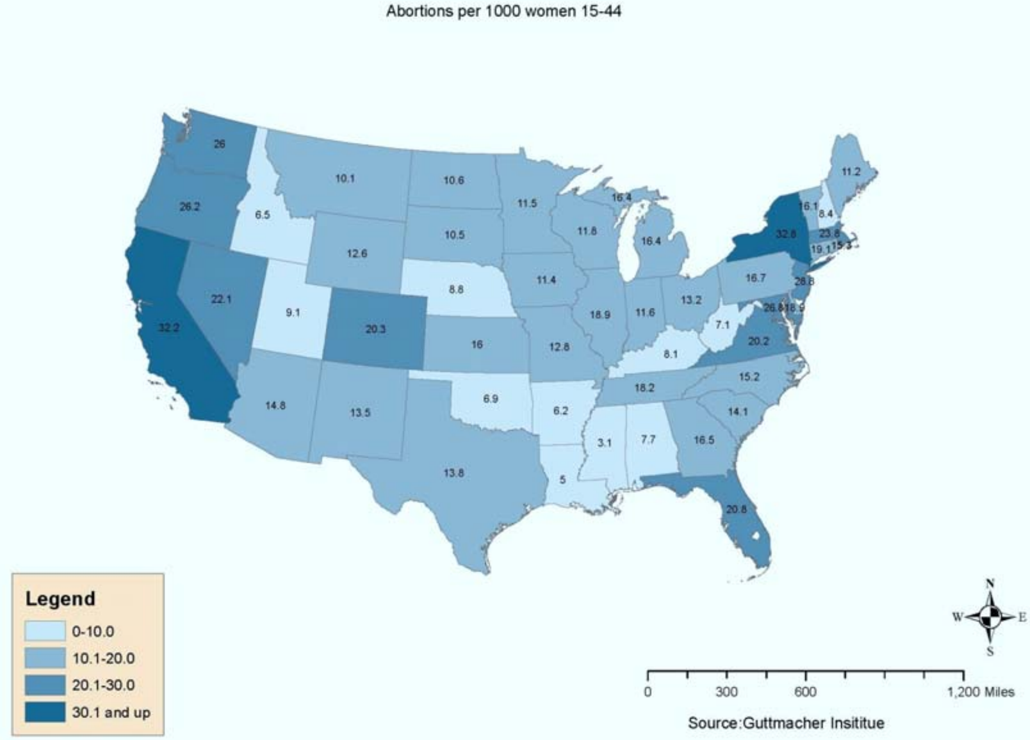


Figure 2-7: First Marriage Rates for Teen women 15- to 19-year-olds

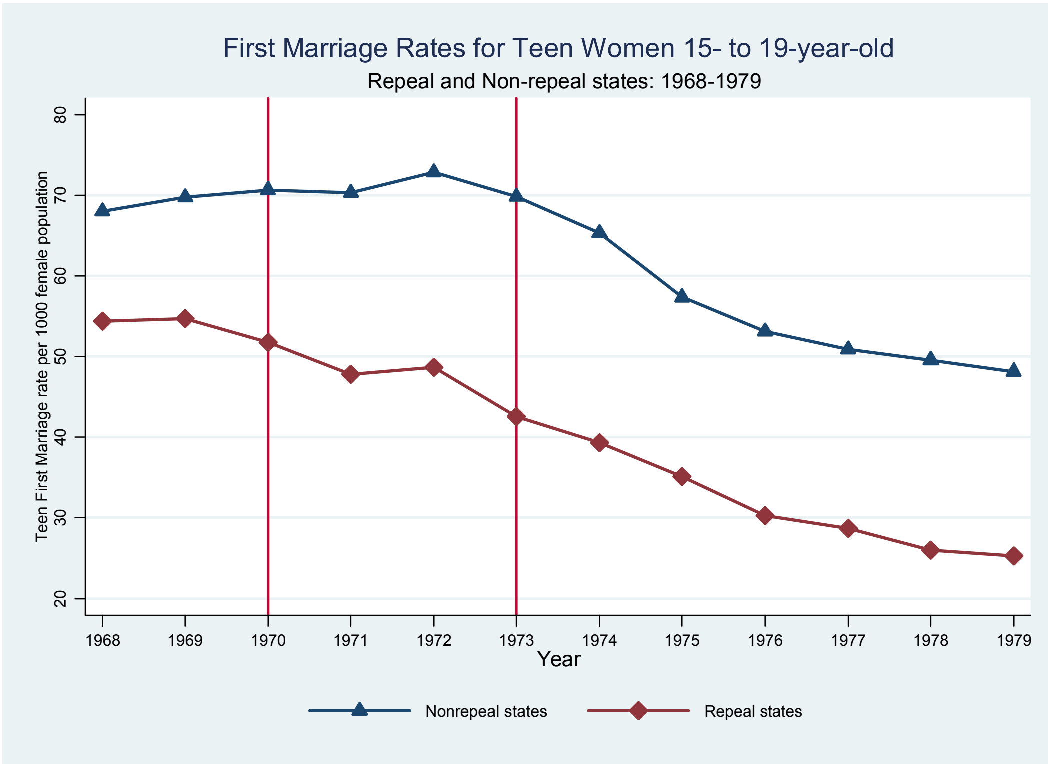


Figure 2-8: Probability of Having First marriage as Shotgun Marriage

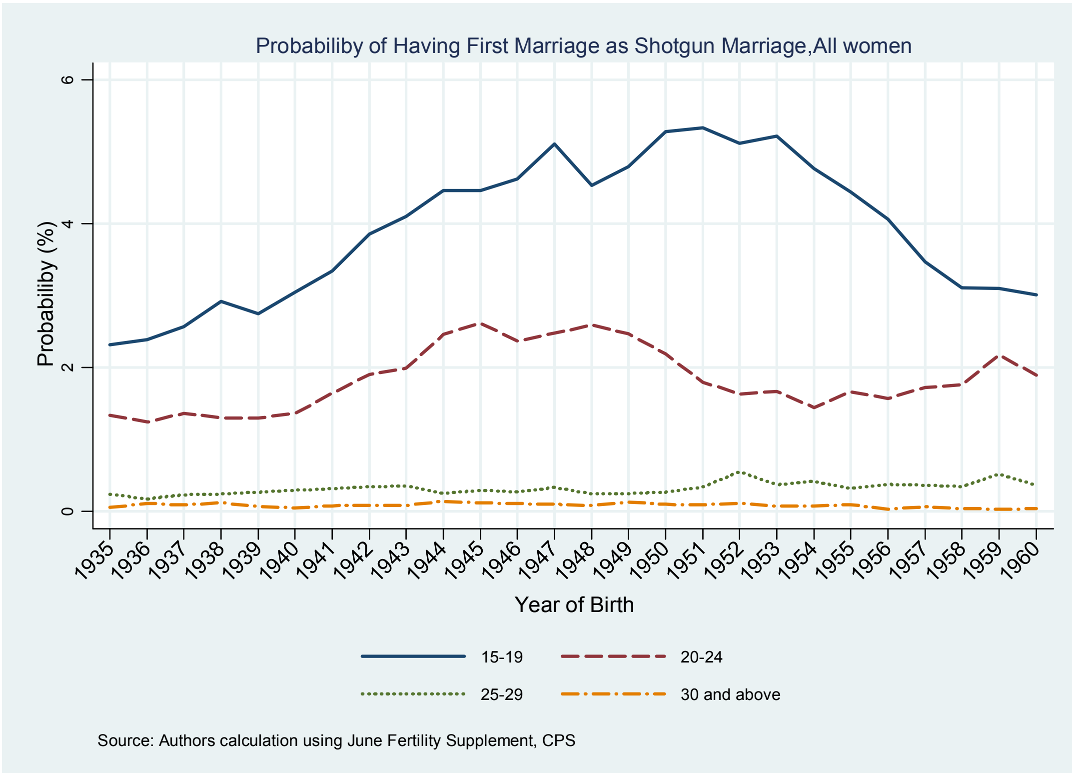
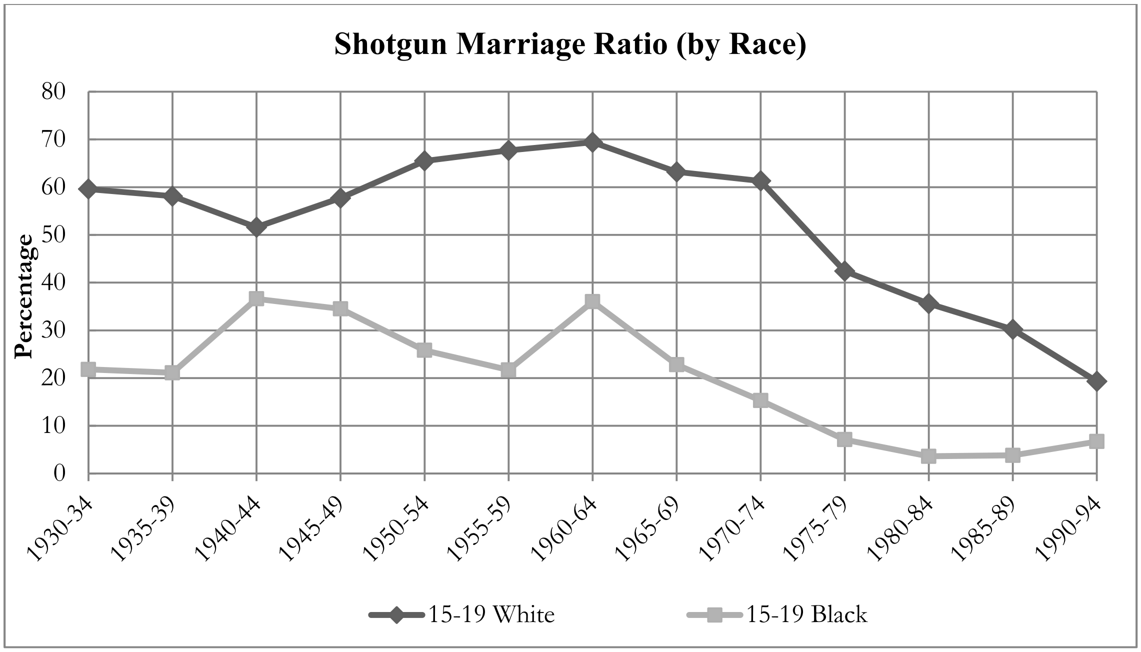


Figure 2-9: Shotgun Marriage Ratio



Chapter 3 Easier Access to Emergency Contraception and Youth Risky Sexual Behavior: Evidence from Washington State

3.1 Introduction

In 2006, the Food and Drug Administration (FDA) approved over the counter (OTC) purchases of emergency contraception (EC) for women 18 years of age or older. The decision ended a decade long battle over making EC available for women without prescription requirement. Proponents of EC contend that the easier access to EC will prevent unintended pregnancy and abortion. Opponents, on the other hand, view the prescription requirement as a necessary restriction that curtails risky sexual activity. They note that any new contraceptive method could increase exposure to sexually transmitted disease (STD) by reducing condom use. As evidence supporting their concern, opponents, and even some more neutral observers, often point to a rise in STDs among adolescents in countries such as UK where EC had been made available without a prescription.

In the U.S., various concerns have also been raised regarding the alarming trend of STD prevalence, high rate of unintended pregnancy and abortion. The attention from social, public health and political perspectives is directed at primarily young unmarried women whose rates of STDs and unintended pregnancy are the highest. The STD prevalence among sexually active women 15- to 24-year old is close to 40 percent (Hampton 2008), making women in these age groups particularly vulnerable to reproductive health problem and unintended pregnancy. More than one unwanted pregnancy occurred for every 10 women 15-24 years of age (Finer and Henshaw 2006). Consequently, the burden of abortion falls heavily on these women. Over fifty percent of women obtaining abortions are teenagers and young adults 15 to 24 years old (Jones et. al 2010). The introduction of emergency contraception pill (EC), commonly known as morning-after-pill, or Plan B, can help to reduce the risk of unwanted pregnancy and the need for abortions. As its name implied, EC has a narrow time frame of effectiveness. It must be used within 72 hours of unprotected sex and is most effective if taken within 24 hours. The OTC availability of EC is especially important for outreach to teens and minors who may have particular difficulty accessing physician services and obtaining a prescription within one to three days.

Prior to the FDA approval, there have been various policy efforts to promote wider and easier availability and access to EC during the past decade. Figure 1 displays a map of U.S. continental states with different color indicating the timeline of state legislative events allowing pharmacy access to EC. Before 1998, all states required a prescription for the use of EC regardless of a women's age. In 1998, the state of Washington became the first state to allow nonprescription sale of EC by launching the Emergency Contraception Collaborative Agreement Pilot Program. The program was the first policy initiative enabling pharmacists to directly dispense EC without a prescription to women of all ages. During the two-year implementation period, pharmacies from eighteen counties were involved in the program. [56] These counties, taken together, consist of more than 90% of the WA state population. Over 93% of the abortions were performed to women residing in these counties [57]. Following

[56] According to Durrance (2013), these counties are Benton, Clallam, Clark, Cowlitz, Island, King, Kitsap, Pierce, Skagit, Skamania, Snohomish, Spokane, Thurston, Wahkiakum, Walla Walla, Whatcom, Whitman, and Yakima.
[57] Authors' calculation based on Washington State Pregnancy and Induced Abortion Statistics, 1999 (*www.doh.wa.gov/ehsphl/chs/chs-data/public/Abort_report_1999.pdf)*

Washington's lead, eight other states[58] took similar legislative effort to ensure the provision of EC's OTC access between 1998 and 2006 (Guttmacher Institute 2009). Like Washington, some of these eight states allowed pharmacies to dispense EC without a prescription provided they had a collaborative practice agreement with local physicians. Other states agreed to follow a state protocol. . In August of 2006, the FDA approved the nation-wide nonprescription sale of EC to women age 18 or older. The access to EC for adolescents age 17 or younger, however, was still kept behind-the-counter. In April of 2009, the FDA approved the OTC availability of EC for 17-year-olds.

In this paper, we analyze whether pharmacy access to EC is associated with increased rates of gonorrhea and decreased rates of abortion among teens (ages 15-19) and young women (ages 20-24). The question can be linked to the more general question of whether legal or technological changes that reduced the risk of unintended birth lead to riskier sexual behavior. For example, there was concern that early legal access to the contraceptive Pill without parental consent would promote promiscuity among unmarried teens in the early 1970s; the legalization of abortion raised similar concerns (Klick and Straumen 2003; Akerlof et. al 1996; Bailey 2006; Gruber et. al). More recently, proponents of parental involvement laws for minors seeking an abortion have argued that such statutes reduce rates of unintended pregnancy. Several studies are consistent with these claims (Kane and Staiger 1996; Levine 2003). The initial studies of EC using randomized control trials reported no association with riskier sexual behavior. But these studies have been criticized for a lack of statistical power. Authors of two recent studies with much larger samples reported substantial increases in STDs

[58] These states are: Alaska, California, Hawaii, Maine, New Hampshire, New Mexico and Vermont.

and decreases in abortion rates associated with OTC access to EC in states that approved such availability prior to national FDA approval (Durrance 2010; Oza 2011). However, the large samples in these studies come at the expense of weaker designs.

From a research perspective, the lack of consensus on whether the effect of OTC access of EC is detrimental or beneficial is primarily driven by the problem related to research design and data availability. A major challenge is that only nine states allowed access to EC without a prescription prior to the national approval. The small sample of early adopters has hampered identification strategies and limited statistical power. Moreover, many of these state laws were passed quite recently, leaving the researchers with limited length of sample period.

To address the aforementioned methodological limitations, we apply the synthetic control method recently introduced by Abadie et. al (2010) to study the impact of OTC access to EC on young unmarried women in Washington State. The synthetic control method is an appealing data-driven procedure for case studies with only one or very few treated units. The main idea is to create counterfactual for the treated unit by constructing a weighted combination of control units based on pre-intervention characteristics. The weights are chosen so as to minimize the mean squared prediction error between the outcome for the treated unit and the synthetic controls throughout the pre-intervention period. With the weights researchers are able to project the time path of the treated unit had there been no policy change or intervention. The method provides a systematic way to generate a credible counterfactual. It also provides as a straightforward means of statistical inference based on a type of permutation test.

The initiation of Washington Emergency Contraception pilot program provides a natural application of the synthetic control methodology. First, it was the first state to allow the OTC distribution of EC. Data on STDs and induced abortions are available back to 1981 from the Center for Disease Control (CDC) for a large number of states, allowing us to utilize the most post-implementation years and evaluating the long-term effect of OTC access to EC. In addition, Washington State law does not prohibit the provision of EC to minors. In fact, while the majority of teenagers in the U.S. are still required to obtain physician's prescription for the use of drug nowadays under FDA regulation, the innovative pilot project has enabled thousands of teens in Washington to purchase EC directly from their pharmacists. Note that OTC access to EC may have a greater impact on teens relative to young women since the latter are better able to obtain prescriptions from private physicians for EC prior to or immediately after unprotected sex. The inclusion of teenage groups for OTC access in WA allows us to obtain separate estimate for teenagers and young adults respectively.

Following the statistical inference procedure in synthetic control analysis, we conduct a series of placebo studies to evaluate the causal inference of our estimates. We find no evidence suggesting that nonprescription sale of EC increases the rates STDs among teenage and young women. We also find that easier access to EC does not affect teen abortion rates, but accounts for 6 percent of the decline in abortion among women 20-24 years of age. Our results are in direct contrast to recent manuscripts by Durrance (2010) and Oza (2010) who report large and significant increases in rates of STDs and declines in rates of abortion between 10 and 20 percent associated with OTC access to EC.

3.2 Conceptual Issues

In an economic framework, the behavioral response of adolescents and young adults follows the prediction of rational decision model. Like the introduction of the contraceptive Pill or legalized abortion, fast and ready access to EC lower the monetary, time, search and psychological cost associated with obtaining the prescription for the pill. The policy change allows couples to engage in sex while lowering the risk of an unintended pregnancy, which in effect reduces the cost of unprotected intercourse and in turn increases the utility of sexual activity relative to abstinence. In other words, the easier pharmacy access raises EC's insurance value against unintended pregnancy.

The rational behavior model provides two testable hypotheses. First, as an example of moral hazard, individuals respond to the decreasing cost and increasing utility by taking fewer precautious to avert pregnancy and engaging in more risky sex behavior. As a well-insured and accessible backup plan, OTC access to EC may induce sexually active teenagers and young adults to substitute EC for condom use. Even assuming no change in contraceptive use, an increase in sexual activity would itself lead to an increased risk of STDs due to contraceptive failure or improper use. A competing theory of teen reproductive behavior argues that teens are spontaneous deciding to have sex. They give litter consideration to the costs of unwanted pregnancy (Paton 2006). The alternative assumption does not support the moral hazard hypothesis and it would predict no change in the rate of STDs.

The second testable implication is that improved access to EC leads to a substitution away from abortion. After the act of unprotected sex, abortion and EC are the two options available for preventing an unwanted birth. The higher costs and controversy associated with abortion as a surgical procedure limit its wide accessibility to general populations. If EC and abortion are substitutes, we would expect to observe a decline in abortions at population level when EC is made available over-the-counter. However, the substitution hypothesis is built on the assumption of no change in sexual activity. If OTC access to EC causes more individuals to engage in risky sexual activity and leads to more unwanted pregnancy on the margin, the empirical implication based on aggregate level abortions rates would be unclear.

3.3 Review of Existing Evidence

Evidence of an association between increased access to EC and increased sexual activity remains inconclusive among Epidemiological studies, some of which use randomized designs report no association between access to EC and changes in sexual behavior. Three experimental evaluations conducted in the U.S. analyzed the of the effect of advanced provision or pharmacy access to EC on the frequency of unprotected sex, condom use, use of other hormonal contraceptives, and number of sex partners. (Gold et al. 2004; Raine et al. 2005; Raymond et al. 2006). Two of the evaluations also analyzed changes in STDs (Raine et al. 2005; Raymond et al. 2006). These authors found that increased access to EC increased its use but there was no consistent link between increased EC access and a change in either pregnancy or unprotected sex. The lack of a change in pregnancies given increased use of EC is surprising but likely due to the studies being under-powered to detect changes in low frequency outcomes such as abortion or pregnancy. Sample sizes were relatively small and there was control group crossover and sample attrition. In addition, the use of convenience samples makes it impossible to generalize the findings to the larger population of women. Gold et al. (2004), for example, evaluated the difference in the uptake of EC among a sample of adolescents attending an urban hospital-based clinic in Pittsburgh, Pennsylvania. While the authors also collected information on self-reported STDs, the available sample provided insufficient power to detect only relatively large (and most likely unrealistic) changes in the incidence of STDs resulting from an uptake of EC. Moreover, the high rate of attrition at the six-month follow-up (over 30 percent) raised the likelihood of bias in the estimates. Raine et al. (2005) recruited young women (ages 16-24) from four clinics in San Francisco and Daly City, California, and randomized them into two treatment groups (advanced provision and pharmacy access) and a control group (clinic access to EC). In addition to evaluating differences in self-reported sexual risk taking, the authors evaluated differences in the incidence of STDs and pregnancies as measured by the use of biomarkers. While the study was designed originally to have suitable power to examine differences in pregnancy, shortly after the start of sample enrollment California implemented legislation allowing all women pharmacy access to EC. As a result, enrollment in the control group was ended before it reached the desired sample size and the control group sample was contaminated by subsequent access to EC services. In turn, like the Gold et al. study, this study had poor statistical power for examining the link between EC access and sexual risk taking and related outcomes.

Two recent studies have used state and federal changes in access to EC to evaluate its association with STDs and abortion (Durrance 2010, Oza 2010). Oza (2010), for instance, using a large-scale, national claims database found that OTC access to EC accounts for 37.2 percent of the decline in abortion and 17.8 percent of the increase in STD during the three years after FDA approval. The large estimated effects of easier access to EC, however, lack external validity due to several biases. First, the assumption that the nine early adopters serving as appropriate comparison groups for the rest of the U.S. may not be necessarily true. Unobserved factors associated with pharmacy access legislations that vary over time would easily confound the difference-in-difference model estimation. Another potential source of bias lies in the use of claim-based data. Women who seek reproductive health services at clinic or pay out-of-pocket are not covered by the data, a significant portion of these women are teenagers younger than 18 who are systematically different from women observed in the claim data. Moreover, it is hard to draw conclusions in terms of the dynamics of the long-term effect because the data is limited to three-year post-intervention period. Durrance (2010), another study using quasi-experimental design, focuses on Washington State only. Based on the fact that pharmacies across counties joined the pilot program at different time, Durrance (2007) identifies counties with at least one participating pharmacy as "treated" counties. The author finds large and significant effect using county-level data of STD and abortion rates. The pharmacy access to EC is associated with a 23 percent increase in Chlamydia rates and a decrease in abortion rates of 12 percent and 14 percent among teens and young women. The difference-in-difference approach applied in the study, however, could suffer from several potential biases. First, the estimated effect could be contaminated by the border crossing—for example, a women living in a later participating county could easily drive to a neighboring county to purchase EC. The scenario of border crossing would bias the estimates toward finding no effect. Second, if the participation decisions made by pharmacists individually was driven by the local demand of EC, then the significant negative estimates reported in the study would simply implies negative correlation instead of a causal relationship.

3.4 Data and Sample

Gonorrhea

The Centers for Disease Control and Prevention (CDC) maintains a surveillance system on sexually transmitted disease (STD) in all 50 states and D.C. As one of the most commonly reported STD in the US, gonorrhea rate is a good proxy for risky sex behavior. The disease has a very short incubation period, is easily cured, and, other than mother-to-infant infections, is only transmitted by sexual intercourse (Sen 2003). Moreover, gonorrhea is the only STD for which the CDC has consistent data by age-group and by race. The data collection process of gonorrhea is part of national public health notifiable disease reporting system. Each year, CDC receives the records of the incidences of gonorrhea collected and aggregated by state health departments across the country. In this paper, we use the annually released CDC data report about gonorrhea cases by state, 5-year age group and race from 1985 to 2006. The Washington pilot program was launched in 1998. Thus, we have 13 years of pre-intervention data and more than 8 years in post-intervention period. Our sample period ends in 2006 because the OTC access to EC was nationally approved by FDA in 2006. The age-specific rates for teenage females 15-19 years of age and young female adult 20-24 years of age are calculated per 100,000 of the relevant population.

Recall that the potential candidates for synthetic control unit of Washington are states that meant to be able to reproduce the gonorrhea and abortion rates that would have been observed for Washington in the absence of non-prescription access to EC, we therefore exclude states that adopted some similar programs granting easier access to EC before the FDA approval during our sample period. There are 8 states (Alaska, California, Hawaii, Maine, Massachusetts, New Hampshire, New Mexico, Vermont) introduced similar programs during 1999-2006 post intervention period that are not included in the pool of control units (Trussel 2011). We also exclude D.C. because the gonorrhea rate in D.C. is 120 percent higher than the average gonorrhea rates of the other states. Finally, the group of control units consists of 41 remaining states.

Abortion

We use the CDC's annual abortion surveillance by age, state and year to estimate the effect of easier access to EC on abortion rates. Unlike the Guttmacher Institute's survey of abortion providers, the CDC surveillance does not include all 50 states and national estimates of abortion are approximately 15 percent less than totals reported by Guttmacher. The big advantage of the CDC data is that they are available annual and by age whereas the Guttmacher survey is conducted every 4 years after 1988 and is not stratified by age. We use abortion rates to teens ages 15-19 and young adults ages 20-24. The abortion rate is defined as abortions per 1000 age-specific women.

To obtain a balanced state-level panel data, we only include states with no missing data throughout the sample period. We also exclude state that report zero abortions in some years since true zeros are extremely unlikely. Moreover, we discard states whose trend of reported number show unusual spikes or troughs. For example, the number of reported abortions in Nevada increased by 77% from 2000 to 2001. The unusual increase was most likely caused by data reporting problem. Finally, the remaining 25 states with complete and consistent data consist of the pool of control units for the following synthetic control analysis.

Population

The denominator of gonorrhea and abortion rates is the total number of female population in relevant age groups. We use population data by state, year, age, gender and race from the Surveillance Epidemiological and End Results (SEER) of the National Cancer Institute (NCI). The SEER population data are available from 1969-2008.

3.5 Empirical Methodology: Synthetic Control Method for Comparative Case Studies

A major challenge in estimating the effect of state policy or intervention in a single state is choosing an appropriate comparison group. Researchers often use neighboring states or all states with no equivalent policy (Card and Kruger 1993; Evans and Lien 2004; Colman and Joyce forthcoming). The choice of neighboring states can be subjective. If using all possible comparison states, the pre-policy level and trend of the outcome under study may differ substantially between the two groups. The latter can seriously bias the estimated impact of a policy impact when using a difference-in-difference (DD) estimator. In addition, the DD estimator only controls for time-invariant sources of confounding, which may also be violated with a long pre-intervention period.

Finally, estimation of the appropriate standard errors with only one or a few treatment states is an additional challenge as standard approaches to account for correlations within units are based on large-sample asymptotic theory (Donald and Lang 2007; Angrist and Pishcke 2009).

The synthetic control methodology addresses each of these issues. The comparison unit is a weighted combination of units from a “donor pool” of possible controls. The weights are obtained by minimizing the distance between pre-intervention determinants of the outcomes in the treated and control units. The weights are then applied to outcomes in control units in post-intervention period to generate the synthetic outcome for the treated unit. The difference in each period between the actual and synthetic outcome in the treated unit is the estimated effect of the intervention. Another virtue of the synthetic control method is its transparency as plots of the pre-intervention difference between the actual and synthetic outcomes is a natural way to present the data. Lastly, the synthetic control method uses repeated placebo tests to estimate the distribution of effects for all control units. This distribution is used to assess whether the effect for the treated unit is large relative to that of the controls, a form of permutation test based on the exact distribution of the controls. This circumvents the problem of using large-sample tests for inference in the case-study context.

3.6 Results

We present the results of synthetic control analysis for two outcome variables over two age groups: gonorrhea and abortion rates for teens 15-19 years of age and , young adult 20-24 years old. Women 15 to 24 account for XX percent of all cases on gonorrhea in 1990 and XX percent of all abortions.

We use the following variables to predict the pre-intervention rates of gonorrhea and abortion: the percentage of population that is non-white, currently married living in rural area, poverty and college educated;. In addition, we include female labor force participation rate, the state unemployment rate, and real state income per capita. Finally we include alcohol control policies as well a state policies towards abortion. These are the total beer tax (state and federal), and indicator for whether the legal drinking age is above 20, whether the state has a Parental involvement law for abortions to minors, and restricted Medicaid payments for abortion. These variables are averaged over the ten-year pre-intervention period from 1987 to 1998 and augmented by adding three years of lagged value of outcome of interest (1997, 1990, 1985). Appendix A lists the predictors as well as their data sources. In all estimations, we minimize the mean squared prediction error of the pretreatment outcome variables of the synthetic Washington by using nested optimization procedure that searches among all sets of weights for the best fitting convex combination of the control units.

3.6.1 Gonorrhea Rates

Figure 2(a) plots the trends in gonorrhea rates of teenage women 15-19 years of age in Washing and the average rates of the 41 control states. It is obvious that the 41 control states altogether may not serve as appropriate comparison groups for Washington to study the effect of easier access to EC on teen gonorrhea rates. During the pre-intervention period, the gonorrhea rates in Washington decreases smoothly while the level of gonorrhea rates in the 41 control states is substantially greater during the pre-intervention period. As discussed above, we construct synthetic outcomes for Washington based on a combination of control states With respect to gonorrhea rates the synthetic Washington is a weighted average of 6 control states (weights in parentheses): Oregon (61%), Nevada (24.4%), Rhode Island (4.8%), West Virginia (4.3%), Michigan (3.8%) and Florida (1.7%). The rest of the states are assigned zero weight. Figure 2(b) displays the affinity between the teen gonorrhea rates for Washington and its synthetic counterpart prior to the 1998 program.   Panel (a) of Table 2 in Appendix displays the mean of pre-intervention control variables of actual Washington, synthetic Washington, as well as the population weighted average of all the 41 states.   The table highlights the fact that the characteristics of synthetic Washington is substantially closer to actual Washington relative to the average of 41 control states.   The synthetic unit closely tracks the trajectory of teen gonorrhea rate and its predictor variables occurred in Washington prior to the pilot program. The root mean square prediction error (RMSPE)[59] is 35.74, which corresponds to only 5% of the average teen gonorrhea rate in Washington prior to the program.

The estimated effect of easier access on teen risky sex behavior is the difference between teen gonorrhea rates in Washington and its synthetic version starting from 1998. Although there is noticeable divergence between Washington and its synthetic counterpart just after the initiation of the program (see Figure 3 (a)), the negative effect of easier access to EC on STD prevalence is counterintuitive and the significance of our estimates need to be evaluated. Based on the idea of statistical inference in synthetic

[59] RMSPE is the square root of the mean squared discrepancies of the outcome variable between WA and its synthetic counterpart during the pre-intervention years.

control analysis, we perform a series of placebo tests by iteratively reassign the treatment to one of the 41 control states while shifting Washington to the pool of control states. Figure 3 (b) displays the distribution of estimated gaps from the 41 times iterative placebo runs. As the figure graphically shows, the magnitude of estimated negative effect of non-prescription sales of EC is not large relative to the placebo studies. Therefore, the counterintuitive negative effect is not statistically meaningful. There is no significant impact of easier access to on the prevalence of STD among teenager women 15-19 years of age.

For the gonorrhea rates among 20-24 year old women, the counterfactual that resembles the most of Washington with respect to the pre-intervention predictors is a weighted combination of another group of 6 states: Nevada (39.2%), Utah (30.4%), West Virginia (13.5%), Oregon (9.4%), New Jersey (2.7%) and Michigan (4.6%). Again, panel (b) of Table 2 in the Appendix presents the pre-intervention characteristics of actual Washington, synthetic Washington and the population weighted mean of all 41 control states. Panel (a) and (b) of Figure 4 plot the trend of young adult gonorrhea rates of WA versus 41 control states, and WA versus its synthetic counterpart respectively. An eyeballing of the figures reveals much more affinity between Washington and its new synthetic control unit during pre-intervention years. There is discernable divergence between the two lines after 1998. Nonetheless, the significance of seemingly large negative effect of easier access on young female gonorrhea rates (Figure 5(a)) was ruled out by the following statistical examination of 41 iterative placebo tests. As Figure 5(b) makes clear, under a random assignment of the treatment among all 41 control states, the magnitude of estimated effect is not significantly different from the results we obtained through placebo tests. Again, our results suggest that the quantity of estimated effect of easier access on young adults' gonorrhea rate is not large enough to be more than random. Taken together, the lack of any statistically significant relationship between the OTC access to EC and STD prevalence among adolescents and young adults provide little evidence of increasing risky sexual behavior among young women when they gain easier access to EC. The conclusion is in line with the finding of no evidence of sexual behavioral changes with EC from the previous medical literature (Raymond, et al, 2003; Gold, et al, 2004; Jackson, et al, 2003).

3.6.2 Abortion rates

Next, we turn to the examination of the substitutability between EC and abortion services. As discussed earlier, the number of candidate states that serve to reproduce the teen abortion rate in WA before 1998 is small due to data limitation. The control group consists of 25 states with consistent and complete reported abortion rates throughout the sample period.

We first focus on estimating the impact of nonprescription sale of EC on the use of abortion services among teenage women 15-19 years of age. The optimal weights are attributed to Rhode Island (65.8%), Montata (25.7%), Michigan (7.3%), New York (1.2%). Panel (a) and (b) of Figure 6 depicts the teen abortion rate for the actual WA, synthetic WA and the population weighted average of 25 control states. Figure 6 and 7 depicts the trend, the estimated effect and the results from placebo tests. Several points can be made from the graphs presented: First, There are big differences in terms of the trend and magnitude between WA and the 25 control states, suggesting that the 25 state may not provide an appropriate comparison group for WA. Second, the synthetic WA generally tracks the trajectory of teen abortion rate in actual WA with small RMSPE of 1.99 throughout the pre-intervention period. However, the two line start to diverge even before the implementation of the program. Recall that a synthetic unit only serve as meaningful counterfactual if the pre-intervention gaps on the outcome variables is small or close to zero. Finally, since the statistical examination rule out the possibility of any potential effect of OTC access to EC on teen abortion rates, we would not read too much into these findings.

Our finding for the effect of OTC access to EC on young adult abortion rates for women 20-24 years of age, however, suggest a partially significant impact of the WA program. The young adult abortion ratio in WA can be best reproduced by a weighted combination of 5 states: RI (78.3%), PA(8.3%), NE (8.2%), GA(4.6), NJ (0.6%). The application of synthetic control analysis (Figure 8b) dramatically shrinks the wide difference between WA and its comparison groups (Figure 8a) before 1998. Despite the reasonable good fit with RMSPE as low as 1.67 during pre-intervention period, the construction of synthetic unit produces a non-negligible post-intervention gap. Moreover, the estimated effect get more pronounced during the following 4 years and it starts to die out from 2002 (Figure 9a). The average estimated effect can be read as a 6% reduction of the mean pre-intervention average. As usual, we conduct a serious of placebo studies and plot all the estimated effects in Figure 9b. The gap estimated for WA stands out (the dark line) because it is large relative to most of the gaps for the control states except Colorado and Minnesota.

Apart from the graphical examination, we follow the idea of Abadie et. al (2010) to use the ratio of post/pre-intervention RMSPE as a tool to evaluate the significancy of our results. Recall that the post/pre-intervention RMSPE ratio is a measure of relative affinity between treated unit and its synthetic counterpart after and before the intervention. If the treatment effect estimated is not randomly created due to the lack of fit, we would expect to find relatively large post/pre-intervention ratios of RMSPE for WA as opposed to the other placebo runs. Figure 10 show that WA ranks the 2nd among the 25 states. If one were to assign the pilot program at random to all states in the data, the probability of obtaining a post/pre RMSPE as large as our results is 0.08. The probability is less than the level of 10% in conventional statistical tests, suggesting our estimated effect is partially significant.

3.7 Conclusion

The public health consequence and economic impact of the over-the-counter sell of EC have been the focus of a long-running health policy debate. The arguments between advocates and opponents are directed at primarily teenage and young unmarried women whose rates of STDs, abortion and unintended pregnancy are the highest. An unsolved question is whether the improved access to EC, by significantly reducing the risk of unintended pregnancy, would induce teenager and young unmarried women to change their sexual risk-taking behavior in a way that leads to an increase in STD and a substitution away from abortions. The lack of conclusive empirical finding in the literature is primarily due to difficulty for identification. Applying synthetic control analysis on the case study of Washington, the first state granted pharmacy access to EC, we find little evidence to support the hypothesis that OTC provision of EC would increase STD rate among young unmarried women. We do no observe any change of abortion rates among teenage women 15-19 years of age due to the nonprescription sell of EC. For women 20-24 years old, our finding suggests a partial significant substitution effect of easier access to EC on the utilization of abortion services.

Figure 3-1: Timeline of OTC Access to EC

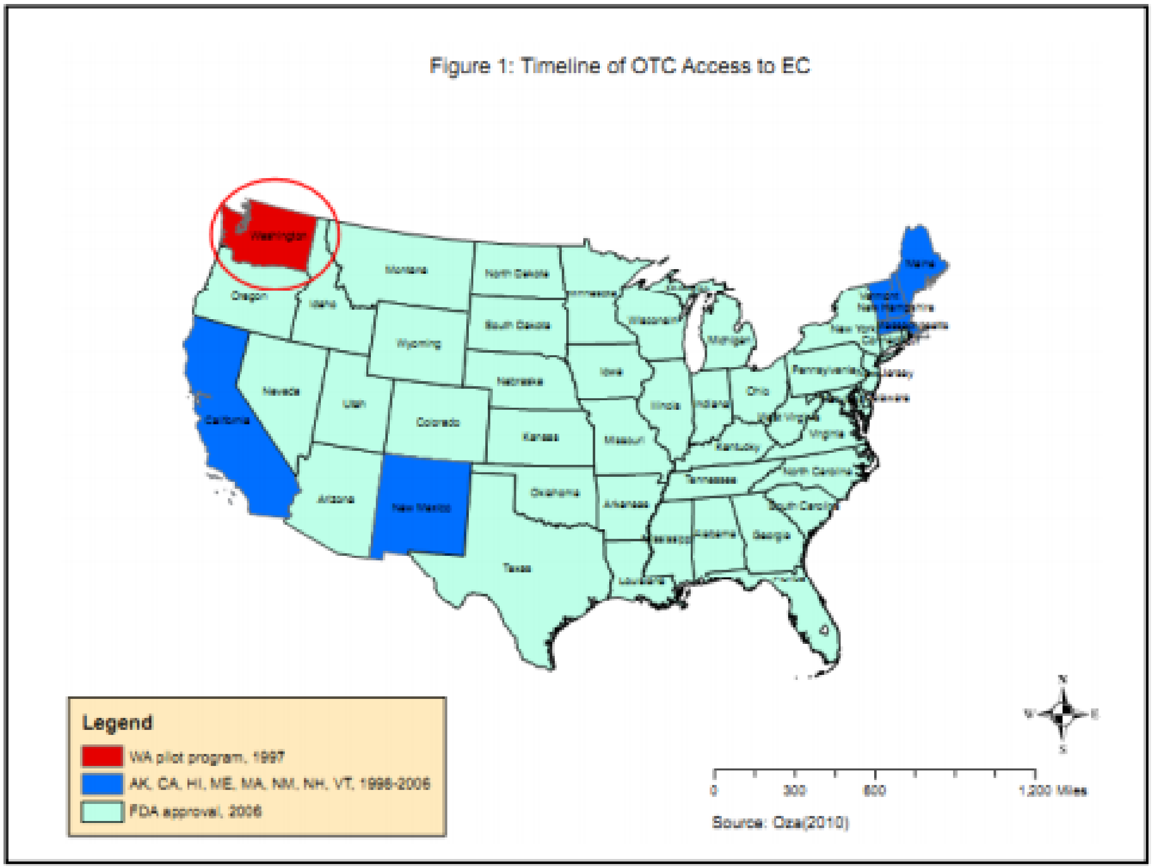


Figure 3-2: Trends in Gonorrhea Rates of Teen women 15-19 Years of Age

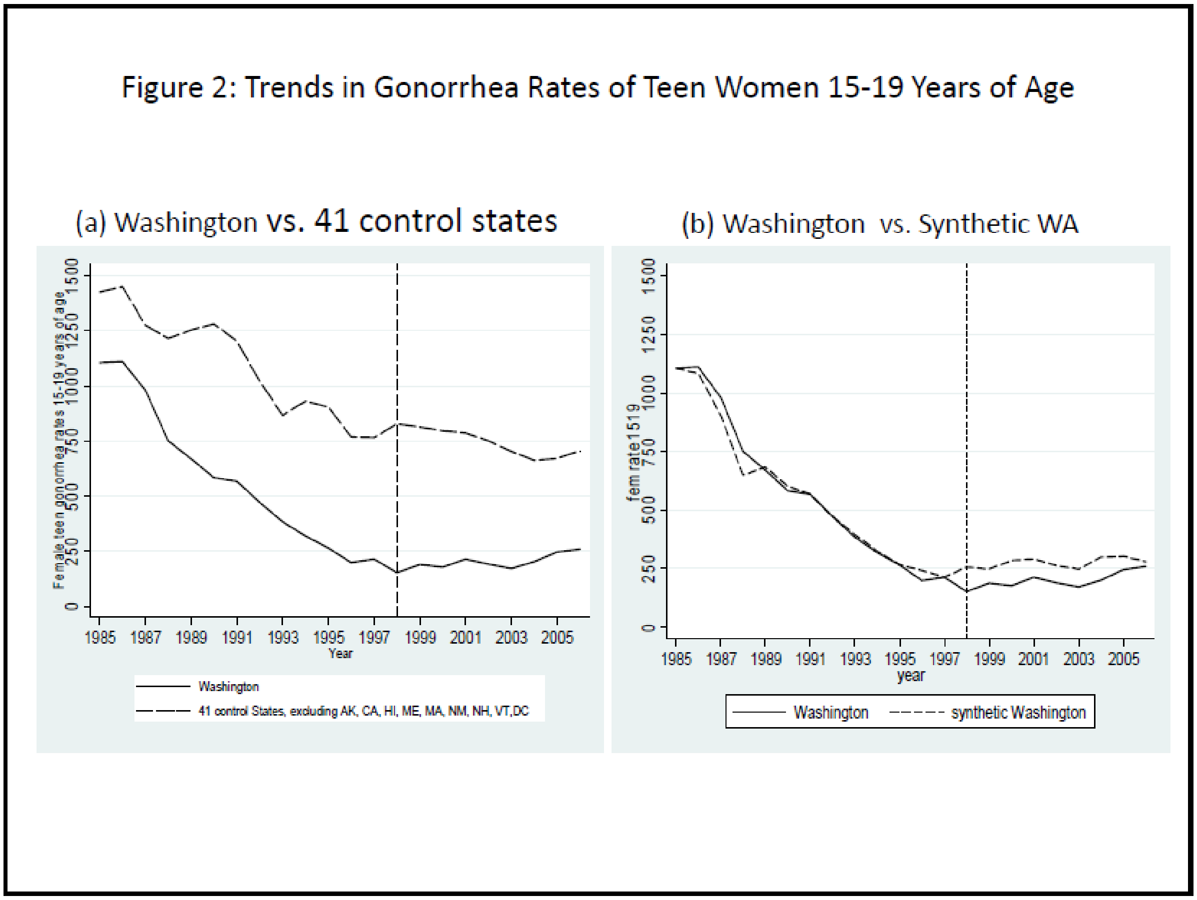


Figure 3-3: Estimation and Inference of Washington Pilot Program: Gonorrhea Rates for Teen women

Figure 3: Estimation and Inference of Washington Pilot Program on Gonorrhea Rates of Teen Women 15-19 Years of Age

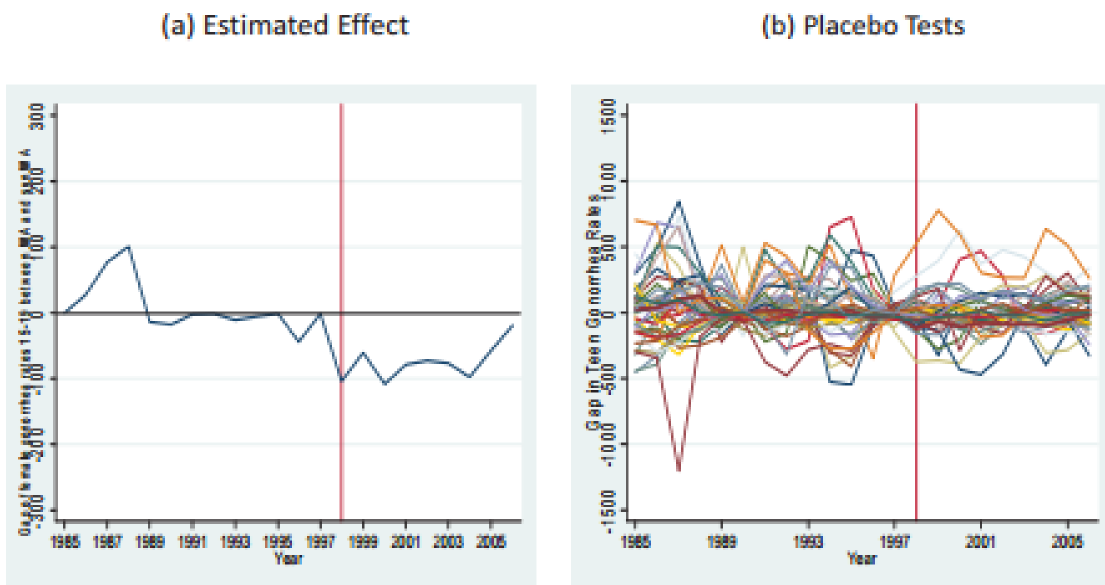


Figure 3-4: Trends in Gonorrhea Rates of Young Adult Women 20-24 Years of Age

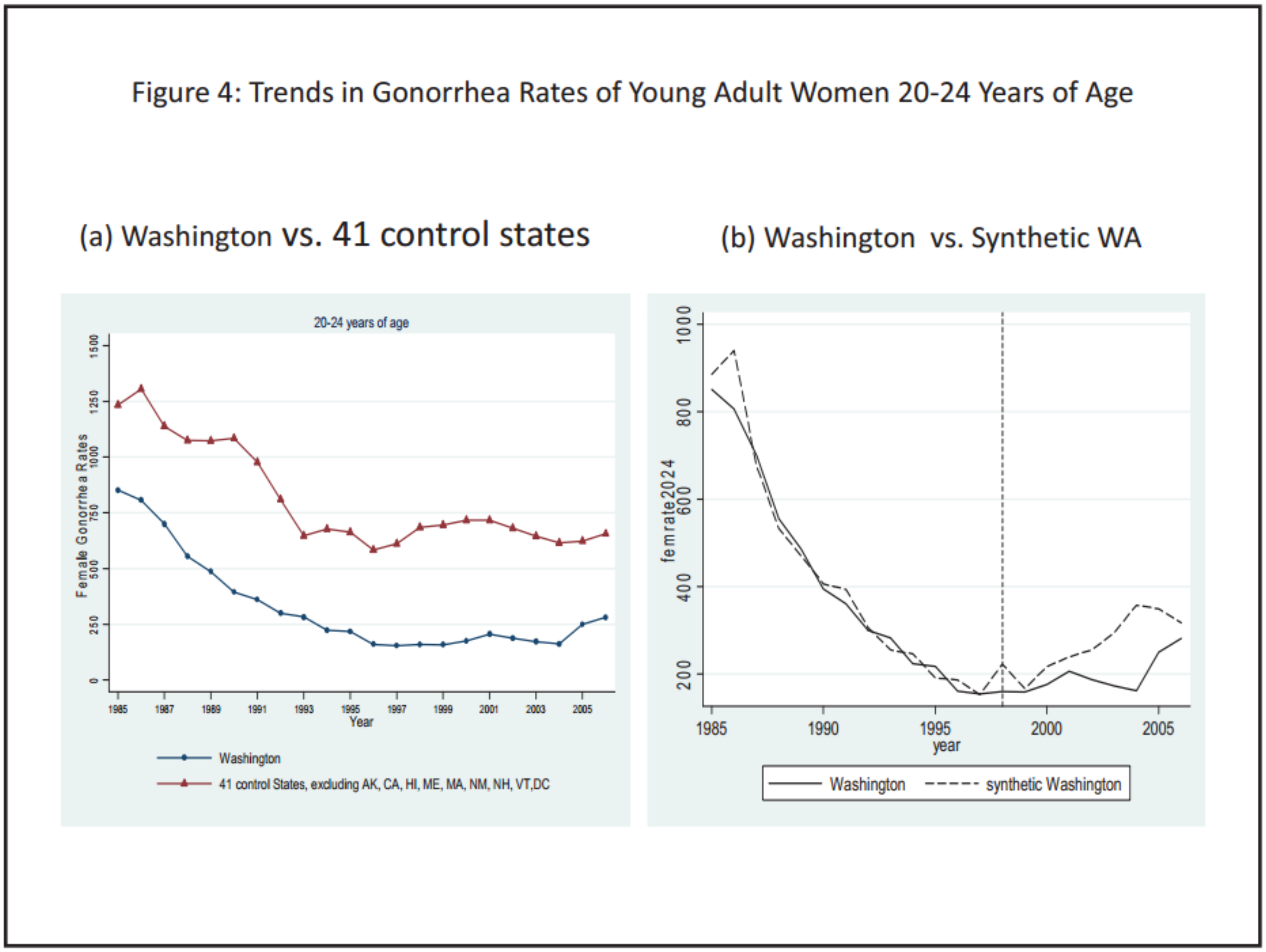


Figure 3-5: Estimation and Inference of Washington Pilot Program: Gonorrhea Rates for Young Adult women

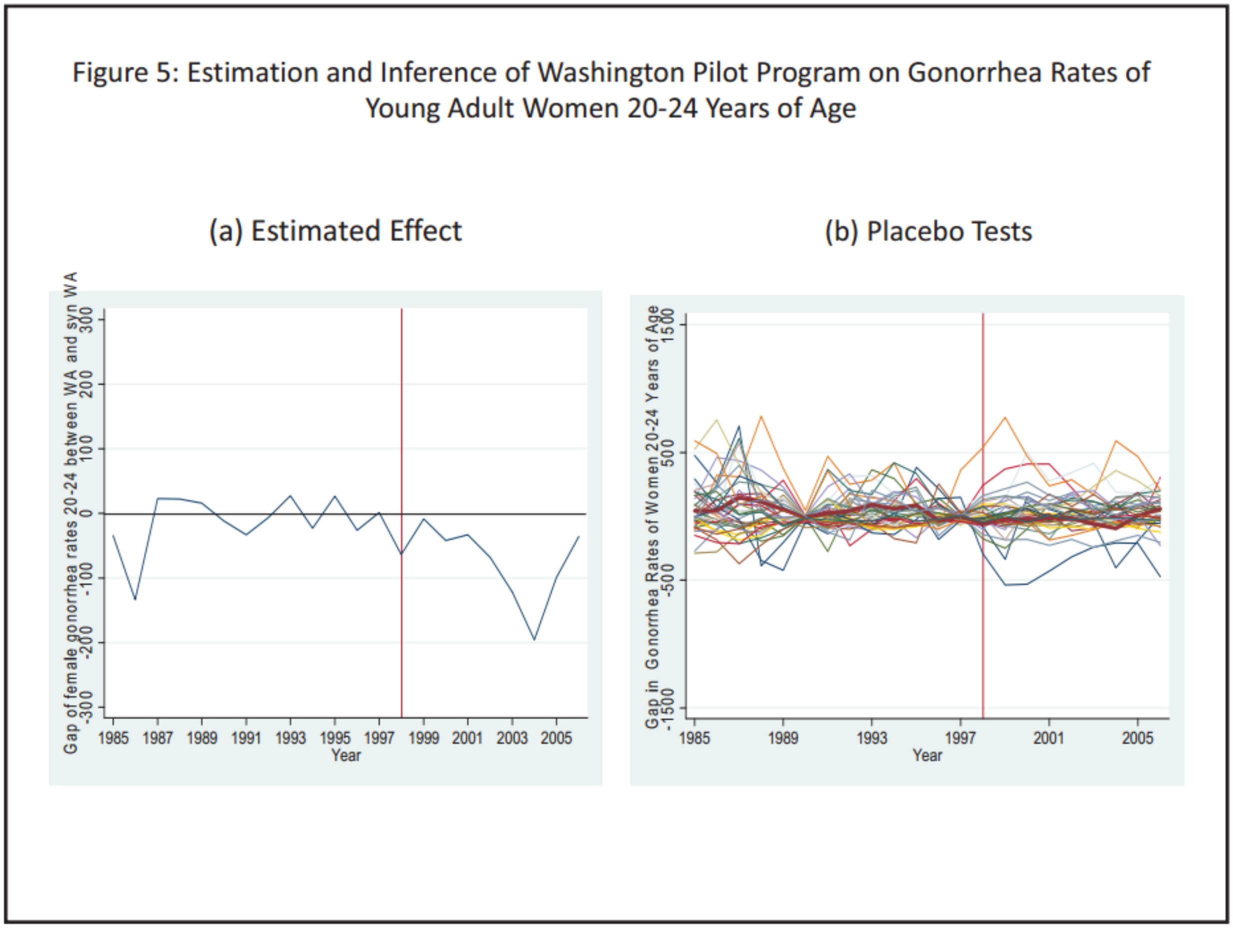


Figure 3-6: Trends in Abortion Rates of Teen women 15-19 Years of Age

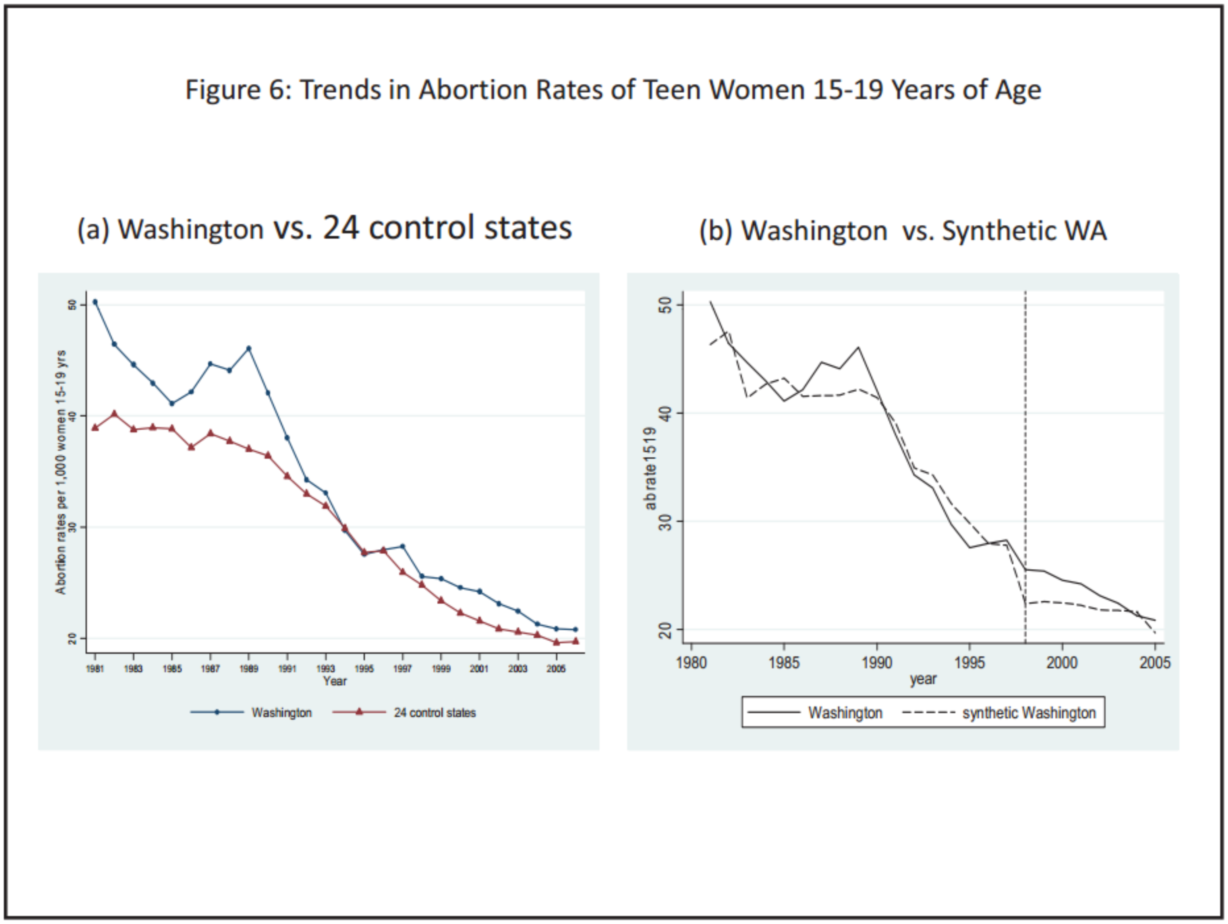


Figure 3-7: Estimation and Inference of Washington Pilot Program: Abortion Rates for Teen women

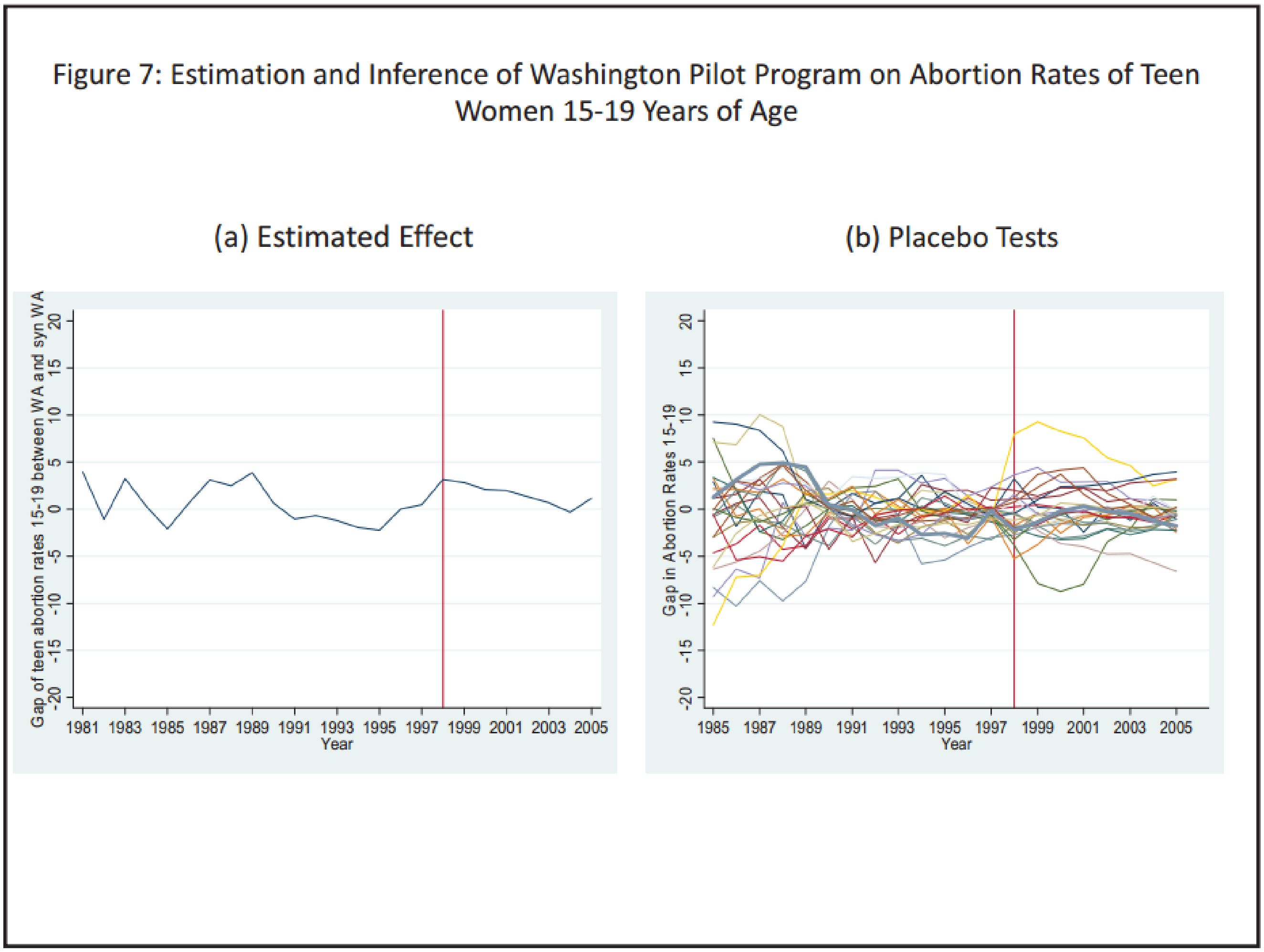


Figure 3-8: Trends in Abortion Rates of Young Adult Women 20-24 Years of Age

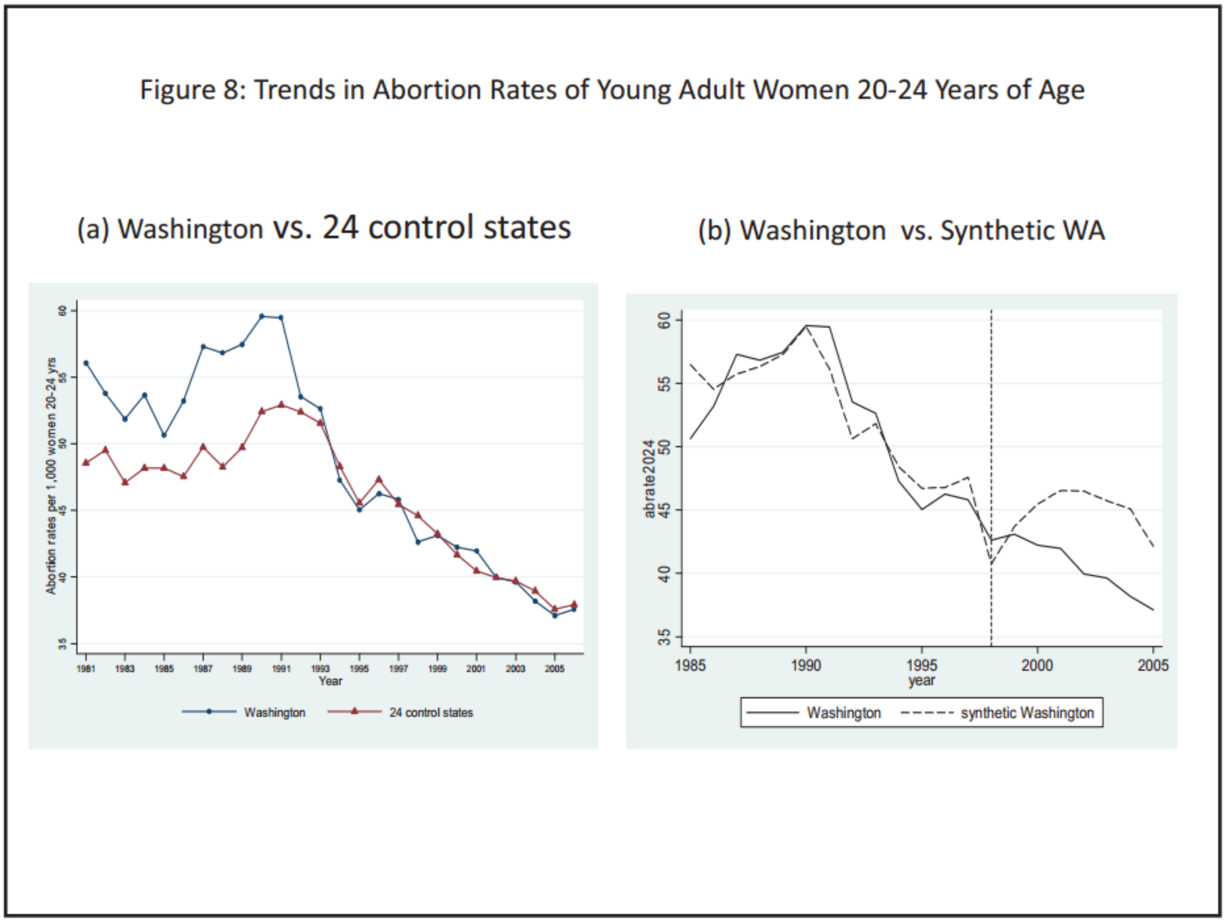


Figure 3-9: Estimation and Inference of Washington Pilot Program: Abortion Rates for Young Adult women

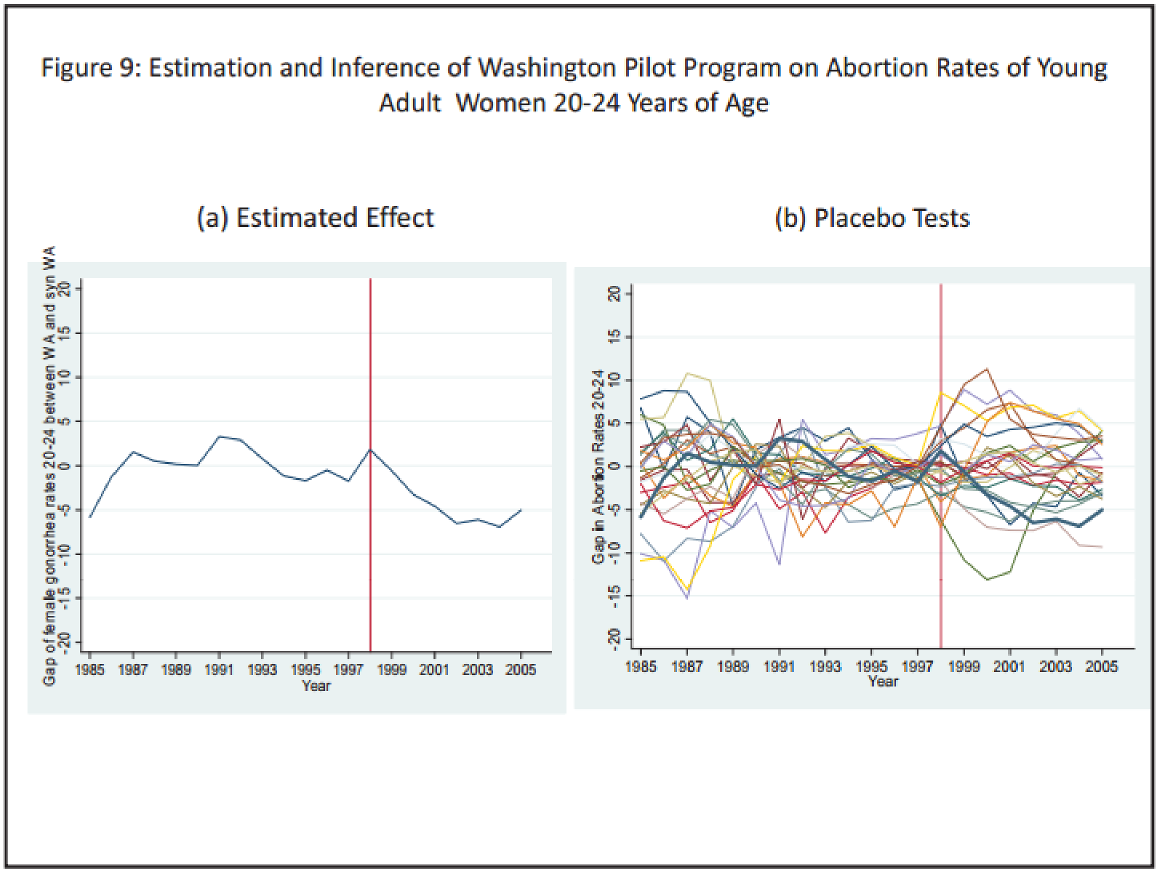


Figure 3-10: Post/Pre-RMSPE Ratio

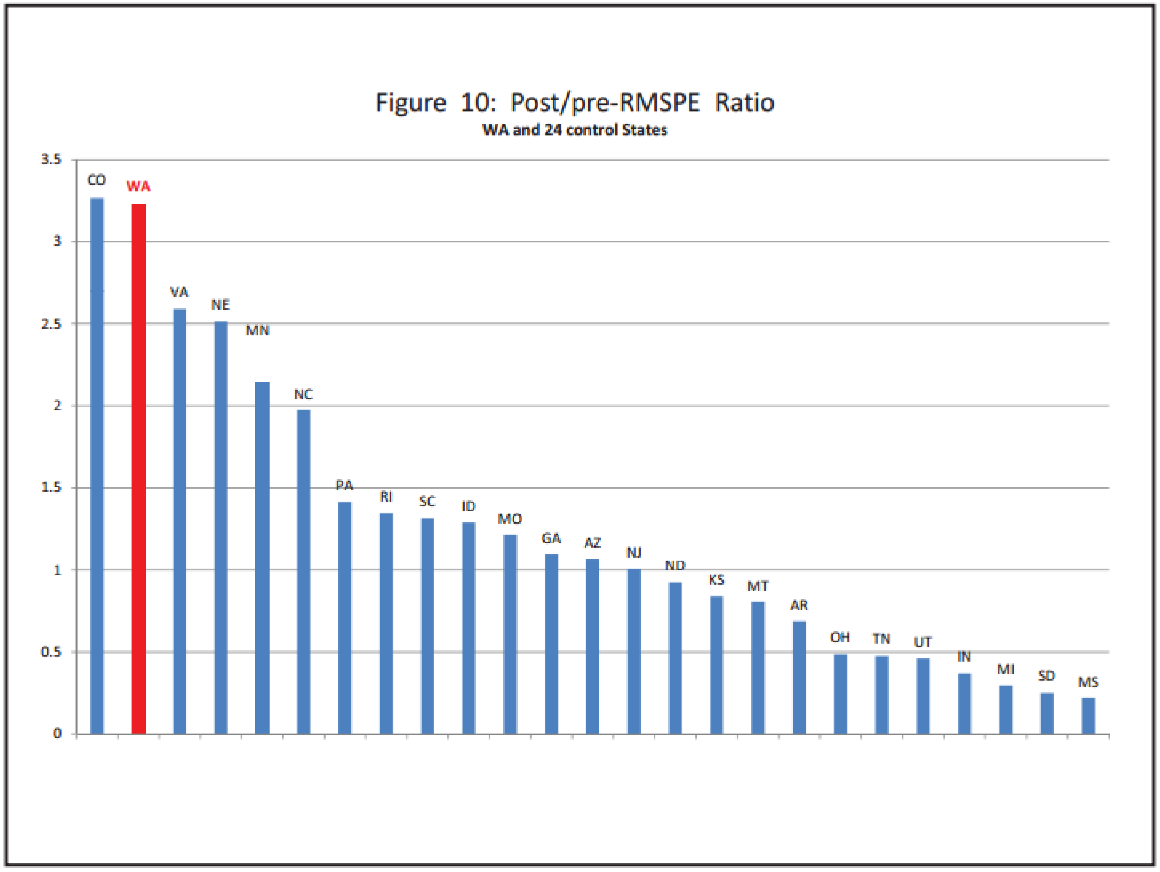


Table 3-1: Mean Predictors of Outcome Variable

Panel (a) Gonorrhea Rates						
	Ages 15-19			Ages 20-24		
	Washington		Non-OTC States	Washington		Non-OTC States
Control Variables	Actual	Synthetic	weighted mean	Actual	Synthetic	weighted mean
% of population in poverty	1.34	1.44	1.71	1.34	1.34	1.71
real per capita income	14.65	13.88	13.90	14.65	14.44	13.90
federal+state beer tax	0.55	0.50	0.65	0.55	0.54	0.65
legal drinking age 20 and older	1.00	0.99	0.79	1.00	0.91	0.79
female labor force participation	58.11	59.05	56.17	58.11	60.34	56.17
unemployment rate	7.54	7.13	6.67	7.54	5.95	6.67
% of rural population	23.36	23.71	27.27	23.36	23.09	27.27
% of nonwhite population	0.09	0.07	0.16	0.09	0.09	0.16
education attainment	23.25	19.76	19.17	23.25	19.07	19.17
% currently married	0.53	0.53	0.51	0.53	0.55	0.51
lagged outcome 1997	212.33	230.77	765.52	154.03	161.56	606.66
lagged outcome 1990	582.85	598.12	1266.26	394.62	409.49	1065.65
lagged outcome 1985	1105.39	1089.30	1420.36	851.68	852.08	1290.17
Panel (b) Abortion Rates						
	Ages 15-19			Ages 20-24		
	Washington		Non-OTC States	Washington		Non-OTC States
	Actual	Synthetic	weighted mean	Actual	Synthetic	weighted mean
% of population in poverty	1.34	1.34	1.57	1.34	1.48	1.57
real per capita income	14.65	14.44	13.56	14.65	14.23	13.56
federal+state beer tax	0.55	0.54	0.66	0.55	0.55	0.66
legal drinking age 20 and older	1.00	0.91	0.81	1.00	0.96	0.81
female labor force participation	58.11	60.34	56.83	58.11	58.17	56.83
unemployment rate	7.54	5.95	6.53	7.54	6.14	6.53
% of rural population	23.36	23.09	31.04	23.36	17.02	31.04
% of nonwhite population	0.09	0.09	0.15	0.09	0.08	0.15
education attainment	23.25	19.07	18.68	23.25	20.27	18.68
% currently married	0.53	0.55	0.52	0.53	0.50	0.52
lagged outcome 1997	28.26	27.79	17.17	45.83	47.57	30.42
lagged outcome 1990	42.06	41.41	26.99	59.59	59.53	36.77
lagged outcome 1985	41.10	43.21	29.03	50.64	56.50	33.78

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