Evolution of Women's Lifetime Earnings in Response to Early Fertility Shocks*

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Abstract

This paper evaluates the effect of unplanned fertility shocks on women's careers. I exploit the early repeal of abortion bans in five US states. This leads to variation in access to abortion across states and birth cohorts, which allows the estimation of the effect of accessing abortion at a certain age on women's fertility. The evidence suggests that accessing abortion before the age of 21 delayed the age at which women gave birth to their first child by half a year on average. I also document an increase in completed fertility among black women who received access to abortion early in their fertility cycle. The resulting variation in fertility realizations is then used to estimate the effect of fertility on women's careers. I find that labor earnings increase by 13% as a result of the delay of an unplanned start of motherhood. Results from the effect of age of start of motherhood on labor supply and occupation status suggest that most of the earnings gains are due to better occupations rather than increase in labor hours worked.

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1 Introduction

Two important features marked the lives of women in the United States in the past 60 years. The average number of children to which an American woman gave birth dropped by half from 3.65 births per woman in 1960 to 1.80 in 2017. During the same period, American women's participation in the labor force increased from 38% to 57%. The relationship between fertility and women's career choices has drawn the attention of economists and demographers, leading to the emergence of a large body of empirical work investigating the effect of family size and birth events on women's labor market outcomes. However, studies that quantify the effect of a fertility shock on women's careers remain scarce.

There are several reasons why fertility shocks and their consequences for women merit consideration. First, unplanned pregnancies account for a large share of pregnancies. For instance, in 2006 half the pregnancies in the U.S. were unintended[1], with the rate of unintended pregnancies varying extensively by age group (Mosher *et al.*, 2012). Second, the consequences of unplanned births might differ from the effects of planned births. A planned pregnancy is an endogenous choice that results from a cost benefit analysis and intra-household bargaining (Becker, 1993). Women who self-select into pregnancy are therefore likely to have a lower birth cost[2]. Birth cost depends on the opportunity cost of the time dedicated to parenting which is most likely larger for high earning women (Wilde *et al.*, 2010). Fertility shocks nonetheless can occur at different rates among women with different potential earning and birth penalties. Moreover, the birth cost might vary during a woman's lifetime: if pregnancy timing was completely deterministic, then it would occur in periods where the cost of birth is the lowest. Ward and Butz (1980) find that couples time births to avoid periods when female wage rates are expected to be high. However, a fertility shock can occur in phases that might be crucial in determining the shape of the earnings profile. For example, an unplanned birth during teenage years can disrupt schooling, decreasing future earnings as a result of lower human capital. Another crucial phase is the early labor market experience (Blundell *et al.*, 2016), where an unplanned birth of a child can drive a woman outside the labor market at a young age making it harder for her to rejoin the labor market later.

The main challenge in identifying the effect of fertility shocks on women's earnings arise from omitted variables. Unobserved factors, such as perception of gender role and

[1] The U.S. has one of the highest rates of unintended pregnancies among rich countries. The rate of unintended pregnancy among women of fertile age in 1987 was 54 pregnancies per 1,000 women. It decreased slowly to reach 45 per 1,000 in 1994 (Henshaw, 1998). The trend in unintended pregnancy got inverted in the following decade to regain it's previous level of 54 unintended pregnancy per 1,000 women in 2008, to drop again to 45 per 1,000 women in 2011 (Finer and Zolna, 2016).

[2] Kuziemko *et al.* (2018) finds that women, especially those with higher education, underestimate the consequences of pregnancy on their labor supply. However, they do not refute that women anticipate a birth penalty when making their fertility decisions

fertility and career preferences, are likely determinant of women's career trajectories and may be correlated with the probability of unplanned pregnancies. Moreover, a woman's career potential can affect her optimal contraceptive effort choice, leading to self selection into different levels of unplanned pregnancy risks. Given the impracticality of an experimental design that would randomly assign fertility shocks, researchers have to rely on quasi-experimental variation to identify the desired effect. There are two kinds of approaches that have been developed in the literature. The first employs an instrumental variable approach exploiting variations resulting from random biological shocks such as birth of a twin (Rosenzweig and Wolpin, 1980; Bronars and Grogger, 1994; Angrist and Evans, 1998) or miscarriages (Hotz *et al.*, 2005; Miller, 2011). The twins instrument constitutes a shock on the intensive margin of fertility. If the wage penalty is suffered by the birth event exclusively then a twin births will not capture the marginal penalty due to the shock if the pregnancy was planned. The miscarriage instrument is most likely correlated with baseline health of pregnant women, which is an important determinant of future earnings. Moreover, both instruments rely on events that have a very low frequency of occurrence. The second approach, that I pursue in this paper, exploits policy changes that could lead to variation in fertility shocks realizations. One such policy is abortion legalization (Angrist and Evans, 2000).

In this paper, I estimate the effect of an early fertility shock –unplanned start of motherhood– on women's earnings. I exploit between state variation in timing of abortion legalization to identify the effect of abortion accessibility on various fertility outcomes. The cross state and birth cohort variations in exposure allow me to identify the effect of access starting at a given age. The estimates show a significant delay of half year in start of motherhood for women who obtained access to abortion before the age of 21. These effects are large given that the in sample age at which motherhood starts is 23 on average. Estimates of completed fertility show a precisely estimated zero effect of abortion accessibility at all ages on completed fertility of white women. In contrast, I find a substantial increase in completed fertility for black women that obtained early access to abortion. While the latter result seems counter intuitive at first glance, investigation of marriage outcomes show that, while the probability of marriage is not affected by access to abortion, husbands of black women who had early access to abortion had significantly higher earnings and were more likely to hold college degrees. The combined findings on fertility outcomes suggest that the effect of abortion accessibility is restricted to the early years of the fertility cycle, where it causes a substantial delay in the start of motherhood by avoiding an unplanned start of motherhood.

I then proceed to estimating the effect of age at the start of motherhood on earnings. An instrumental variable approach is adopted to estimate the causal effect, using state abortion exposure as an instrument. I find that postponing motherhood for one year increases yearly labor earnings by an average of $\$2,194$, which corresponds to a 13%

increase from the mean. Moreover, the findings show that improvement in occupation status account for most of the observed gains in earnings. This is attested to by the significant increase in the reported occupation index caused by delayed start of motherhood, whereas the effects found on labor supply choices are mostly insignificant.

This study contributes broadly to the literature on the effect of motherhood on women's careers. This includes a large body of literature that evaluates the effect of motherhood on labor supply and earnings (Rosenzweig and Schultz, 1985; Hotz and Miller, 1988; Lundberg and Rose, 2000; Sigle-Rushton and Waldfogel, 2007; Lundborg *et al.*, 2017; Chung *et al.*, 2017; Aaronson *et al.*, 2018; Kleven *et al.*, Forthcoming). Specifically, my paper contributes to the literature evaluating the consequences of fertility shocks on women's careers. Previous papers in this literature find mixed effects of fertility shocks on women's earnings and labor supply. For instance, using miscarriage as an instrument for avoided teenage pregnancies Hotz *et al.* (2005)[3] find an increase in labor earnings and annual hours worked at older age for women who became mothers while they were teenagers. In contrast Klepinger *et al.* (1999) find that adolescent motherhood leads to a significant decrease in schooling and early labor market experience resulting in lower labor earnings. Similarly, using exposure to abortion, Angrist and Evans (2000) document an increase in high school completion and labor supply of black women who avoided teenage pregnancy. The focus of these studies is restricted to fertility shocks among teenage women, investigating schooling as a main mechanism. However, I investigate potential fertility shocks throughout the whole fertility cycle. My results show that access to abortion has an effect beyond teenage years on delaying unplanned start of motherhood. The importance of investigating the effect of first birth beyond teenage years is highlighted in Herr (2016), who finds that a first birth disrupting a woman's early career might be more consequential than a first birth before entering the labor market.

This paper is closest to Miller (2011) who studies the effect of motherhood timing on career path. My findings on the effect of delaying start of motherhood on yearly earnings concur with her findings. In Miller's study, however, the increase in earnings is mostly attributed to an increase in labor supply, while increase in wage rates account for one third of the increase in total earnings. Whereas I find that most of the effect streams from the increase in wage rate.

This paper also contributes to a second line of literature evaluating the consequences of abortion legalization. Previous studies of abortion policy changes in the U.S. and elsewhere assess the effect of abortion legalization on children selection (Gruber *et al.*, 1999; Donohue and Levitt, 2001; Ananat *et al.*, 2009), children outcomes (Pop-Eleches, 2006), mother's health (Clarke and Muhlrad, 2018) and fertility (Kane and Staiger, 1996; Levine *et al.*, 1999; Angrist and Evans, 2000; Levine and Staiger, 2004). Two particular features distinguish my paper. First, I study the effect of abortion access throughout the

[3] see also Hotz *et al.* (1997)

fertility cycle in contrast to previous literature focusing on women's fertility during their teenage years. Second, I investigate the effect of the access on both fertility quantity and timing. Most previous papers investigate yearly rate at state or county levels. This could be misleading as a drop in yearly fertility rate can be due to abortion delaying timing of births rather than decreasing completed fertility.

Studying the consequence of abortion accessibility on women fertility outcomes is of particular policy relevance. This is especially true in light of the recent regulatory trends put in place by multiple states to limit access to abortion. Since the abortion rate attained its historic peak of 29.3 abortions per 1,000 women aged 15-44 in 1981, usage has been in steady decline for the past three decades. It is widely believed that the decline in abortion use can be fully or partially attributed to state abortion restrictions. The first type of regulations target abortion providers by limiting public funds to facilities providing abortion procedures as well as other regulations that increase the cost of operation. Another set of regulations target abortion patients directly, including husband consent, parental consent for minors, mandatory counseling and waiting period between counseling and abortion date. The number and restrictiveness of state regulations have been associated with a sharp decrease in abortion providers (Jones and Kooistra, 2011; Jones and Jerman, 2014, 2017), along with an increase in average distance to nearest provider (Bearak *et al.*, 2017) and the average out-of-pocket cost of abortion procedures (Jones *et al.*, 2018) as well as an increase in antiabortion harassment (Jones and Kooistra, 2011). The literature evaluating the effect of state regulations remains scarce. One particular experience, Texas House Bill 2 of 2013, has been studied in a series of recent papers (Quast *et al.*, 2017; Grossman *et al.*, 2017; Cunningham *et al.*, 2018; Fischer *et al.*, 2018). Their findings suggest that the increase in distance to the nearest abortion provider, caused by the bill, decreased the abortion rate in the state, while the evidence on the effect of the law on fertility is mixed. While this paper does not speak directly to the effect of state regulations, its findings can shed light on their potential consequences.

The remainder of this paper is organized as follows. A brief history of abortion related legislative changes in the United States is provided in section 2. Section 3 discusses the details of the strategy employed to identify the effect of access to abortion on fertility. Section 4 presents the data used in estimation. Estimation results of the effect of abortion access on fertility are presented in section 5. In section 6, I present the estimation approach and results for the effect of early fertility shocks on earnings. The last section concludes.

2 Abortion Legalization at State and Federal Levels

In 1973 the United States Supreme Court in the case of Roe v Wade ruled that State and Federal restrictions on abortions in the United States violate the 14th amendment and henceforth granted women legal access to abortion in all States. Prior to that landmark decision abortion accessibility largely depended on a woman state of residency. Between 1967 and 1970 a partial liberalization of abortion laws were adopted in 15 states. Most of these changes permitted abortion in cases of rape, incest and pregnancy complications that could be life threatening to the mother. Sklar and Berkov (1974) conclude that legal changes in what they call "reform states" led to a significant reduction in overall fertility, especially among unmarried women. Angrist and Evans (2000) use fertility variation resulting from state reforms to evaluate the effect of teenage fertility on schooling and labor outcomes. They document a significant positive effect of abortion accessibility on schooling and employment of black women.

The most important legal changes prior to national legalization took place in 1970, when five States (Alaska, California, Hawaii, New York and Washington), repealed their anti abortion laws [4]. This paper uses legislative changes in the legal status of abortion in the early 70s to measure the effect of abortion accessibility on fertility and labor market outcomes of women.

There are two main reasons for considering Repeal states rather than the Reform states as the treatment group for our analysis. First, as reported in Sklar and Berkov (1974) Repeal states had the highest rates of legal abortions per thousand women in 1971 with New York (27.1), Hawaii (23.6), California (23.5), Washington (19.7) and Alaska (17.4). Among the Reform states only Oregon (15.7), Delaware (13.7) and Maryland (11.4) had rates that were close to those of Repeal states while the rest of them had a rate of abortion lower than 10 per thousand women [5]. Second, in Repeal states abortion became legal on request while in Reform states abortion accessibility was conditional on specific circumstances. Given the interest in understanding the effect of abortion accessibility on labor outcomes, optional rather than necessary abortion is more relevant for our purpose.

3 Identification Strategy

A typical woman's reproductive cycle extends for around thirty years. Ovulation starts at around the age of 14 and continues through her forties, with the likelihood of conceiving a child dropping drastically after the age of 42. Fertility decisions are made by women throughout this phase of their life. These decisions could be summed up as choosing the total number of children they wish to have and the timing at which they choose to have them. Women often make these choices simultaneously with their career choices, as both are interrelated. Career aspirations might affect their fertility choices, but

[4] In California it was the opinion of the State Supreme Court in People v Belous that state laws banning abortion are unconstitutional.

[5] See Sklar and Berkov (1974), table 3

also fertility realizations might affect their career paths. However, fertility realizations are not fully deterministic, as there is always the chance of unplanned pregnancies. Several biological and behavioral factors cause the probability of unwanted pregnancy to vary during the reproductive cycle. Moreover, the consequences of an unplanned pregnancy on a woman's career, if any, might depend on the phase of her life at which it occurs. Hence, the effect of legal access to abortion on realized fertility, and consequently on labor market choices, can vary widely by the age at which women obtain access to it. The remainder of this section describes how the effect of access to abortion at every year of age can be identified.

3.1 Sources of Variation

To identify the effect of legal access to abortion at every year of age, exogenous variations to abortion accessibility throughout women's fertile life are needed. This is exactly the type of variation that women, who were fertile in the 1970s and the 1980s, have experienced as a result of the legal changes described above. The three year lag in nationwide repeal generated variation across states in the age at which women of similar birth cohorts could obtain access to abortion. Furthermore, differences in age at legalization within state led to variation in access across birth cohorts. Consider for instance a woman born in 1955 and living in one of the repeal states. She was 15 years old when she obtained access to legal abortion for the first time. A woman born one year earlier and residing in the same state, obtained access to legal abortion in the same year as her state peer, however that happened when she was 16 years old. Meanwhile their birth cohort peers living in a non-repeal state did not have access to abortion until national legalization three years later, which also happened with one year of age difference. Comparing outcomes of these women across states and birth cohorts allows identification of the effect of access to abortion at age 15. This approach of using variation in intensity of exposure to legal abortion across birth cohorts and states is similar to the approach employed in Duflo (2001).

Women residing in different states might have different preferences for fertility and career paths. We could also expect such heterogeneity to exist across birth cohorts. The validity of the difference-in-differences strategy rely on the assumption that in the absence of treatment, there are no differences between repeal and non-repeal states in the rate of change in taste for fertility and labor market choices between subsequent cohorts. This assumption would not hold if the treatment assignment (states choosing to repeal abortion ban) was conditional on differences in the trend of between cohorts change in preferences. For instance, if states' repeal in 1970 was due to stronger shifts in preferences towards smaller family size among younger women in these states compared to women of similar age in the other states, then the estimates of the effect of abortion access would be biased, showing larger (in absolute value) effects for younger women compared to older women. It should also be noted, that fertility outcomes are age censored and labor market outcomes correlate with age. Hence, when comparing across birth cohorts, it is crucial that fertility realizations are achieved and labor market outcomes are compared at similar ages. Failure to do so will confound the treatment effect with the age at observation.

An illustration of the identification strategy is presented in Table 2. The averages of two fertility outcomes are compared across three birth cohort groups in repeal and non-repeal states. The youngest group are those born between 1950 and 1955, who were between the ages of 15 and 20 when repeals at the state level took place. The second group of women are those born between 1940 and 1945, who were between the ages of 25 and 30 when state repeals took place. The eldest group consists of women born between 1930 and 1935, who were between the age of 35 and 40 when state repeals were put in place. The data are from the 1970, 1980 and 1990 census (5 percent Public Use Microdata Samples). I use the 1970 observations for the eldest group, the 1980 for the middle group and the 1990 observation for the youngest group. This guarantees that the comparison happens for observations at similar ages, when the women in our sample are between the ages of 35 and 40. Two fertility outcomes are considered; a woman's age at the time she gave birth to her first child and the total number of children she gave birth to. The youngest group obtained access to legal abortion early in their fertility cycle, the second group obtained access in the middle of their fertility cycle, while for the eldest group, abortion was legalized when they were in the final phases of their fertility cycle. If the impact of access to abortion diminishes with age at exposure, fertility of the younger group should be significantly altered compared to the older cohorts, and there should be no significant difference for the two eldest groups.

For each outcome, the first and second rows in Table 2 present the averages for both groups of women in repeal and non-repeal states respectively. The third row presents the differences between repeal and non-repeal state for each group. Women in repeal states in all age groups delay the age at which they give birth to their first child compared to women in non-repeal states. The delay for the youngest group is 13 months on average, and 7 months for the other two groups. The difference-in-differences estimates in Module A show that there is a significant difference of 6 months between women that obtained access at the beginning of their fertility cycle and those that obtained access in the middle of their fertility cycle, while the difference between the latter and the women that did not obtain access to abortion is insignificant. Similarly in Module B of the table, we observe smaller family size for women in repeal states. Total fertility is around 10 percent lower in repeal states compared to non-repeal states across all age groups. The difference-in-differences estimates between the two eldest groups is insignificant, whereas the difference between the youngest and the second group is statistically significant but the magnitude of the effect is small. This simple exercise provides evidence that earlier access to legal abortion has a significant effect on fertility, most notably on increasing the age at which mothers give birth to their first child. In what follows a more formal discussion of the identification strategy is presented.

3.2 Estimation of the Effect of Access to Abortion on Fertility Outcomes

Let $y^A_{i(sb)}$ be a fertility outcome observed at age A of women i, born in year b in state s. To identify the direct effect of access to abortion at different years of age on fertility, I estimate the following equation:

$$y^A_{i(sb)} = \sum_{I=\underline{a}}^{\bar{a}} \alpha_I Repeal_{i(s)} * I^{70}_{i(b)} + \sum_{J=\underline{a}}^{\bar{a}} \beta_J J^{73}_{i(b)} + \delta_{i(s)} + \epsilon_{i(sb)} \qquad (1)$$

where $Repeal_{i(s)}$ is a dummy variable indicating residency in a state s that repealed it's abortion ban in 1970. $\underline{a}$ and $\bar{a}$ are the ages at which fertility starts and ends respectively. $I^{70}_{(b)}$ are a series of indicator variables that take value one if the individual of birth cohort b is of age I in 1970 (date at which state repeal is assumed to be in effect). Similarly $J^{73}_{(b)}$ are indicator variables that take value one if an individual of birth cohort b is of age J in 1973 (date at which abortion is legalized at the federal level). δ_s are states fixed effects.

In this regression framework, differencing outcomes of women across repeal and non repeal states and across birth cohorts identifies the effect of access to abortion at earlier age. The chart below illustrates the identification strategy using two birth cohort groups of women born k years apart, with the youngest being of age $i \in (\underline{a}, \bar{a})$ in 1970. The difference $\alpha_i - \alpha_{i+k}$[6] is the effect of obtaining access to abortion at age i compared to obtaining access at age $i + k$.

	Repeal	Non Repeal	**Difference**
younger cohort	$\alpha_i + \beta_{i+3}$	β_{i+3}	α_i
older cohort	$\alpha_{i+k} + \beta_{i+k+3}$	β_{i+k+3}	α_{i+k}
Difference			$\alpha_i - \alpha_{i+k}$

The difference-in-differences estimates compare the effect of access to abortion at two different ages during the fertility cycle. Given that women are fertile for almost 30 years during their lifetime, this provides a large number of estimates for every year of age. One particular estimate of interest is $\gamma_i = \alpha_i - \alpha_{\bar{a}}$, which identifies the effect of access to

[6]This "differences-in-differences" estimation strategy is valid under the following identifying assumption $E[y^A_{s=R,b+k} - y^A_{s=R,b}|T = 0] = E[y^A_{s=\bar{R},b+k} - y^A_{s=\bar{R},b}|T = 0]$, where T is an indicator of treatment which in this framework is early exposure to abortion due to legislative changes at the state level. This assumption would not hold if legislative changes at State levels were endogenously enacted due to change in preferences for fertility and labor market choices between subsequent birth cohorts.

abortion when a woman is of age i compared to obtaining access to abortion when she is at the end of her fertility cycle. We could as well investigate the age trend in the α coefficients; a decline in the magnitude of coefficients α with i indicates that early access to abortion has a stronger effect on the outcomes of interest. Alternatively, an increasing (in absolute value) α trend implies that access to abortion has a stronger effect at a later age.

For the remainder of this paper, it is assumed that women are fertile between the ages of 15 and 40. This assumption is not restrictive even if it is known for a fact that women can still conceive children beyond this period. Nevertheless, most pregnancies are conceived within this age interval, and more importantly, this assumption is more about restricting our interest to the effect of exposure to abortion during this age group rather than actually imposing a fertility restriction.

For the main analysis I use an alternative specification of equation (1), where instead of looking at the effect of exposure by age year I investigate the effect of exposure at coarser age group. Age years between 15 and 40 are put in groups of 3 years of age each. Similar to the individual age year exposure above, variation in exposure by age group and across states allow identification of the effect of exposure at a certain age group. The following equation is estimated:

$$y_{sg} = \sum_{g=1}^{9} \alpha_g Repeal_S * AG_g^{70} + \sum_{g=1}^{9} \beta_g AG_g^{73} + \theta_1 age + \theta_2 age^2 + \delta_s + \epsilon_{sg} \tag{2}$$

where AG_g ($AG_1 = [15-17]; AG_2 = [18-20], \ldots AG_9 = [39-40]$) is a indicator for age group. Using age group instead of individual years of age allow us to control for age, I do so by including age and age^2. Similar to the per age year case above the effects we are interested in identifying are the effect of exposure to abortion starting age phase g captured by $\gamma_g = \alpha_g - \alpha_9$. The standard errors ϵ_{sg} are clustered at the state level.

3.3 Threats to Identification and Validity of Research Design

As stated earlier, the difference-in-differences strategy described above provides consistent estimates of the effect of abortion accessibility on fertility outcomes, under the assumption that in the absence of state repeal the trends in fertility preferences are similar across states.

This assumption would not hold if the repeal of the abortion ban at the state level was conditional on some differential trend in fertility outcomes. For example, if the repeal of abortion bans in the different states was in response to a more rapid increase in teenage pregnancy among younger birth cohorts in repeal states compared to non-repeal states, then the estimate would suffer from a serious selection bias problem that understates the effect of abortion access. If the repeal was due to stronger preference for smaller family size among the younger birth cohorts in repeal states, then the estimate confound the effect of abortion access and fertility preferences which would most likely cause an upward bias in the estimation of the effect.

To verify the validity of the research design, I investigate the trends in fertility outcomes for cohorts that had different levels of access to abortion. Averages of the two main fertility outcomes of interest - completed fertility and age at start of motherhood - are computed separately for repeal and non-repeal states for different birth cohorts. The trends in completed fertility are shown in Figure 1a, and Figure 1b shows the trends in age of start of motherhood. The two vertical lines represent the cutoffs for the birth cohorts with different levels of exposure across repeal and non-repeal states. The birth cohorts to the left of the first vertical line have no access to abortion during their fertility cycle in both groups of states, while the birth cohorts to the right of the second vertical line have similar access to abortion in both groups of states throughout their fertility cycle, as they are both exposed to the federal accessibility post Roe v. Wade. The birth cohorts in between are the ones with the varying levels of exposure to abortion. The plots clearly show that for both fertility outcomes, the trends in fertility are similar across both state groups for the birth cohorts that had no access or late access to abortion. As for the birth cohorts that had early access to abortion (1950-1959), we can see that the trend in age at start of motherhood become steeper in the repeal states compared to non-repeal states, suggesting that abortion access led to a delay in the start of motherhood. As for completed fertility, Figure 1a shows convergence in completed fertility for the birth cohort that had difference in abortion access early in their life, suggesting that abortion access early in fertility cycle led to increase in completed fertility.

4 Data

To estimate equation (1) I would ideally like to have access to a panel dataset of women who were at various phases of their fertility cycle during the abortion reforms of the early 70's. In addition to permitting proper estimation of the equation of interest, such data would allow me to study the dynamic effect of abortion access. However, no such data set is available. Instead, I rely on cross sectional samples to estimate the effect in question. The following section discusses the main data set used in this analysis, and how the estimation is adapted for the cross sectional nature of the data.

The analysis is conducted using data from four observation years (1970, 1980, 1990 and 2000) of the census 5 percent Public Use Microdata Samples (PUMS Bureau of the Census). Given the dates of state and federal policy changes and the assumption that women's fertility cycles span between the ages of 15 and 40, I restrict the focus of the study to women born in the United States between the years 1930 and 1955. This insures that at the time of the state level repeal, the youngest birth cohort is 15 years of age and the oldest cohort is 40. Three years later, when the federal court repeal takes place the age span is 18 to 43.

There are two main advantages of using the census data. First, they provide a large sample size that allows better estimation precision. This is important since the objective is to estimate the effect of exposure to abortion at each year during the fertility cycle, even if abortion has a considerable effect over the lifetime of a woman, the effect might be modest for some years of age. Second, census data reports a wide range of information on fertility and labor market choices for women of various birth cohorts.

Despite these advantages, the use of the census for the purpose of this study poses two main challenges . First, the census reports birth state and the state of residency at observation, whereas I would like to observe the state of residency during the fertility phase, since it's the laws of this state that determines if a woman has access to legal abortion of not. For the main analysis I assume that the state of residency during the fertile period of life is the same as the state of birth. This would potentially bias the estimates if inter state migration decisions were based on a systematic relationship between a woman's fertility preference and availability of legal abortion in the migration destination. The state of residency at observation is then used to construct an indicator for potential non-movers[7] and do the estimation using this subsample as a robustness check. The second challenge arise out of the cross-sectional nature of the data used. As noted in the equation specification, identification of the causal effect requires observation of fertility measures at an age where the fertility outcome has been realized. Birth cohorts used in the estimation span over 25 years, meaning that their fertility outcomes were realized at different points in time. Hence, observation of women of different birth cohorts at different sample years is required in order to obtain a proper measure of fertility outcomes.

I consider three fertility outcomes. The first is completed fertility which is defined as the total number of children a woman gives birth to during her lifetime. The other two fertility outcomes are the age of start of motherhood and spacing between the first two children. Variables construction and further sample restrictions for estimating the effect of abortion access on fertility are discussed in detail in appendix A.1. Summary statistics of fertility outcomes are reported in Module A of Table 1.

On average women in repeal states gave birth to a smaller number of children compared to women in non-repeal states. The average number of children per woman are 2.46 in non-repeal states and 2.21 in repeal states. Age of start of motherhood shows as well a significant difference between repeal and non-repeal states. Women in repeal states are on average one year older than women in non-repeal states when they give birth to their first child. Looking at these outcomes by race reveals heterogeneity between black and white women. On average, black women gave birth to more children and started having

[7] 62% of women in the sample reside at observation in the same state as their birth state.

children at a younger age than white women, a difference that is persistent across repeal and non-repeal states. Between state differences show that in terms of children born the largest difference is among black women, for whom non-repeal states average of children born was larger than the repeal state average by 0.68. The corresponding difference between states in the number of children for white women is much smaller (0.2 children). The average age at the birth of the first child shows a significant delay of 0.85 years (10 months) for white women in repeal states compared to white women in non-repeal states, while for black women the difference is smaller and is around 7 months. The averages of birth spacing show that for women who have two children the average spacing between children is around 0.24 years (3 months) larger in non-repeal states compared to repeal states. The average spacing for white women is around 3.5 years whereas for black women it is around 4.6 years.

5 Effect of Abortion Access on Fertility

This section presents estimation results of the effect of access to abortion on fertility outcomes. Equation (2) is estimated for the full sample and separately for black and white women for all fertility outcomes of interest. Estimates for completed fertility are reported in Table 3, while estimates for the birth timing outcomes are reported in Table 5. The difference-in-differences estimates for the effect of abortion access on completed fertility and timing of birth are reported in tables 4 and 6 respectively. All estimation results are reported graphically in Figures 2 to 7. Estimates of equation (1) supplement the main analysis and the results are reported in Appendix B.1. Impact of abortion access on completed fertility is discussed first, followed by the effect on birth timing outcomes.

5.1 Completed Fertility

Estimated coefficients of equation (2), reported in Table 3 and Figure 2a, show that women in repeal states had lower completed fertility rates compared to their birth cohort peers in non-repeal states. However, the difference between repeal and non-repeal states is smaller for the younger birth cohorts. As a matter of fact, the difference in completed fertility between states for the youngest birth cohort is only significant at the 10% level. Given that the younger cohorts had earlier access to abortion, the decreasing trend in these differences suggests that cohorts that had earlier access to abortion had an increase in completed fertility. More formally, the difference-in-differences estimates of the effect of abortion access on completed fertility (Table 4 and Figure 2b) show that early abortion access increases completed fertility. Women who obtained access to abortion between the age of 15 and 23 had on average 0.06 more children than women who did not have access to abortion during their fertility cycle. There is no significant effect of abortion on completed fertility for women who obtained access at a later age. The small, but statistically significant effect, masks important heterogeneity. When looking at the effect by race, it can be clearly seen that the result for the full sample is driven by the effect that abortion access had on completed fertility of black women. White women had an average completed fertility of 2.35 per woman, with no significant difference between repeal and non-repeal states (Figure 5). The difference in completed fertility of black women shows that among earlier birth cohorts, black women born in repeal states had significantly lower number of children during their lifetime, whereas for the later birth cohorts convergence in completed fertility in repeal and non-repeal states is documented (Table 3 and Figure 5). The results show that access to abortion before the age of 20 increases fertility of black women by an average of half a child (Figure 5c). The effect diminishes as access is delayed, but a significantly positive effect of abortion access on completed fertility persists until the age of 28 (see Table 12). The null effect of abortion on completed fertility of white women is not surprising. It has been hypothesized that, even if abortion were unavailable, women would still have been able to control the total number of children they give birth to during their lifetime. In response to an unplanned pregnancy that leads to an unplanned birth, a woman can always readjust her later fertility choices and still achieve her intended family size. However, the effect that abortion access has on completed fertility of black women is puzzling. Potential explanations for these results are explored later in the paper.

5.2 Birth Timing

Age at Birth of First Child

As pointed out in the discussion above, women might be able to control completed fertility in the absence of abortion. Nevertheless, since abortion is the only voluntary method to avoid an untimely birth once an untimely pregnancy has taken place, abortion accessibility can have a significant effect on birth timing. The first birth timing outcome studied is the age at start of motherhood. The effect of abortion access on this fertility realization has drawn particular attention in the literature, the focus being on the probability of teenage motherhood. Estimation results for age at start of motherhood are reported in Table 5 and Figure 3a. The average woman in the sample gave birth to her first child at age 24. Women in non-repeal states attain motherhood at a younger age than their birth cohort peers in repeal states. For birth cohorts that were older than 21 at the time of state legal changes, the difference in age at start of motherhood is half a year. However, for women younger than that the difference across states increases to almost a full year. Difference-in-differences estimates (see Figure 3b) show that obtaining access to legal abortion before the age of 21 delays the start of motherhood by 6 months, and the effect fades out when access is delayed beyond that age. The effect on white women is consistent with the one found using the full sample (Figure 6c).

Estimation results for black women (Figure 6b) show that the delay in age at first child is restricted to those who obtained access to abortion by the age of 18. The results for black women also show a statistically significant negative estimates for age groups 27 to 29 and 30 to 32. This result is surprising, as it suggests that access to abortion between the ages of 27 and 32 led to an earlier start of motherhood among black women, whereas I would have anticipated an insignificant effect. I believe this result is due to a measurement error in the outcome variable. As explained in Appendix A.1 age at start of motherhood is constructed by taking the difference between the age of the mother at observation and the age of the eldest child in the household. Sample restrictions were set to minimize the possibility of measurement errors in variable construction. If women in the sample gave birth early in their life, then their eldest child is more likely to have left the household by the time of observation. Given that black women were on average younger when they gave birth to their first child (Table 1), they are more susceptible to this type of measurement error. In other words, the measurement of age at first child for the oldest age group (33-35), which is used as the reference group, may be overstated. This measurement error led to downward bias in the estimated effect of abortion access on age of start of motherhood. Therefore, the reported estimates should be thought of as lower bound of the effect of abortion access on age of start of motherhood for black women.

Angrist and Evans (2000) document a 7 percentage point decrease in probability of motherhood before the age of 20 for black women who obtain access to abortion while they are teenagers. The results in this paper reconcile with their findings for black women conditional on teen abortion users delaying the birth of the first child by 7 years. This means that black women who used abortion to prevent an early start of motherhood during their teenage years later started motherhood at around the age of 24 (average age at start of motherhood in the sample). While I find a positive effect of abortion access on delaying motherhood for white women, they find a small and insignificant effect on the probability of having children before the age of 20.

In my opinion there are two reasons why the effect found in this paper is larger. First, the difference in treatment group definition. This paper restricts the treatment group to states that repealed abortion bans and made abortion available to women at request while their treatment group includes all states that had reforms allowing women partial access to abortion. As mentioned in section 2, abortion use differed considerably between these two groups of states, with higher usage rates in states that had a complete repeal of abortion bans. A second reason is that the average effect found in this paper might be capturing some spillover effects that are not relevant for the outcome considered in their study. Access to abortion might affect the age of start of motherhood of some women even if they did not particularly use abortion to end an unwanted pregnancy. Peer effects streaming from abortion users can affect the preference of other women about fertility choices (Kohler *et al.*, 2001). While this kind of peer effect would delay the average age at which women in the population give birth to their first child, they would not necessarily affect the rate of teenage pregnancies.

Birth Spacing

The second birth timing outcome investigated is the birth spacing for women who had two children. The age in the sample used for this estimation ranges between 29 and 40, with the majority of the observation being in their mid 30s. Before analyzing the estimation results, it should be noted that the findings from this particular subsample do not provide a full understanding of birth spacing choices and how they are affected by abortion access. On one hand, women in this sample could potentially still have more children and hence the variable considered does not constitute a comprehensive measure of birth spacing. On the other hand, these are women who had only two children by the mid-point of their fertility cycle. While this is the norm in current days, for the birth cohorts in question this sample might be highly selective in terms of family size and career preferences. The purpose of estimating this outcome is to complement the findings on start of motherhood and to see whether delaying start of motherhood had a subsequent effect on birth of the next child. I mostly find that abortion access starting at any age has no effect on spacing between the first two children (Table 6 and Figure 4). Consequently, regardless of the age at which a woman gives birth to her first child, this would not affect the number of years a women will wait to give birth to her second child. This result is not surprising given that the literature on the effect of fertility on women's earnings finds that the wage penalty is incurred as soon as the first child is born (Lundberg and Rose (2000); Kleven *et al.* (Forthcoming)). This implies that once the first child is born and the wage penalty is suffered, the opportunity cost of a second child is now smaller, which could mean that once motherhood is realized there is no longer any benefit to delaying subsequent births. It should be noted that for black women the results show a jump in spacing for two age groups (Figure 7b); black women who received access to abortion between the ages of 21 and 23 and between the ages of 27 and 29 had a statistically significant increase in spacing of 5 months between the first and second child. One possible explanation for this observation is that while these birth cohorts did not have access to abortion in order to prevent an unplanned start of motherhood, they still benefited from it to delay the birth of the second child in an effort to mitigate the adverse consequences of the first unplanned birth.

5.3 Black Women Marriage Outcomes

As shown in the previous section, early access to abortion prevent an unplanned start of motherhood at a young age for both black and white women. Many teenage or young mothers end up being single mothers, which could potentially affect many subsequent outcomes, for instance the probability and the quality of marriage. An unplanned teenage pregnancy leading to a birth can potentially decrease the chance of remarriage of the mother. It can as well lead to marriage with a partner of lower schooling and income. Hence, escaping teenage motherhood for women can potentially increase the probability of forming stable families in the future as well as higher household income, which could potentially increase the demand for children as a result of the positive income effect.

In this section, I provide suggestive evidence supporting marriage as a potential channel leading to the observed increase in completed fertility of black women. I do so by investigating the effect of abortion access on marriage outcomes of black women. Using the sample of black women used in the birth timing estimation, I estimate equation (2) for a variety of marriage outcomes.

The difference-in-differences estimates reported in Table 8 show no significant effect of abortion access on probability of marriage. Two earnings outcomes are investigated, husband yearly earnings and occupation status. While abortion access does not seem to have a significant effect on occupation status, husbands of black women who received early access to abortion had significantly larger labor earnings. I also investigate the effect of abortion access on husbands schooling. College completion rates for husbands of black women who obtained early access to abortion are significantly higher. Black women who received access to abortion before the age of 26 are married to men who are 8 percent more likely to have a college degree. Given the sample mean of college degree completion in the sample this a very large effect. Results of black women's completed fertility and marriage outcomes show that the cohorts that experienced an increase in completed fertility are the cohorts that have husbands with higher college completion rates and earnings. These findings give support to the proposition stated earlier about the increase in completed fertility being the result of an increase in household income brought about by a higher earning husband.

5.4 Robustness Checks

In all the estimations reported above, a woman's state of birth is used to determine her exposure to abortion. The possibility of between state migration implies that the treatment variable (interaction of repeal with age at repeal) is measured with error. This measurement error could bias the estimates of the effect of abortion access on fertility outcomes. What could be concerning in particular is the possibility that migration decisions are correlated with the state legislative changes. Such correlation could arise as a result of selective migration of women due to state decisions to repeal or uphold abortion bans. It might as well be due to migration decisions due to other factors that could correlate with the legal status of abortion in the state. For instance if repeal state colleges were more appealing for females, there would a systematic migration from repeal to non-repeal states for college age women. Information on state of residency at various points in time available in the IPUMS is used to check the sensitivity of the results found above. The details of these robustness checks are provided in appendix C and discussed briefly in what follows. However, it should first be noted that a systematic measurement error due to selective migration from non-repeal to repeal states would most likely cause a downward bias. Given the magnitude of the estimates found, I believe it is highly unlikely that such measurement error exist.

As discussed in the data section the IPUMS reports both the state of birth and the state of residency at observation. While these observation do not provide full information on the complete migration history of observed individuals, they inform us about which women did indeed migrate at some point during their life. Then equation (2) is estimated using the subsample of potential non-movers, which exclude all women that are known to have migrated. Estimation results reported in appendix C.1 are consistent with the results found in the main estimation. To further check that there was no systematic migration among women of various age between repeal and non-repeal changes at the time of the legal changes, I take advantage of state of residency information available in the 1970 sample of the census, which report both the state of residency at observation and 5 years earlier (1965). Appendix C.2 investigates women's migration decisions using men as a comparison group. Migration decisions of men were not potentially affected by the state abortion legal status or any other factor that could be appealing to women. I find that there is no difference between men and women of all birth cohorts in the propensity to emigrate from repeal to non-repeal states. As for the migration from non-repeal to repeal, men who were between the ages of 19 and 24 at the time of legal changes were significantly more likely to migrate compared to women, while no significant difference in the propensity to migrate for the other birth cohorts.

This section establishes a significant effect of early access to abortion on delaying age of start of motherhood. The results suggest that abortion access had no significant effect on other fertility outcomes, with the exception of completed fertility of black women. Improvements in marriage quality, husbands with higher earnings and education as a result of early exposure to abortion seems to be the most likely explanation for the increased completed fertility of black women. This phenomenon is most likely the byproduct of avoided unplanned start of motherhood. In other words, the findings in this section establish that the direct effect of abortion access on women's fertility is through the avoidance of early fertility shock. In the next section, I use the variation in abortion exposure as an instrument to estimate the effect of age of start of motherhood on women's earnings.

One might worry that the observed effect on age of start of motherhood is due to a fertility trend change among younger birth cohorts that coincided with the legal changes. This possibility can be ruled out by observing the post-trend for age of start of motherhood in Figure 1b. The 1960-1969 birth cohorts had similar access to legal abortion across states, since women in these birth cohorts were too young at the time of state legalization and were therefore all exposed to federal abortion legalization. While the trends are not fully parallel, the sharp difference in the trend of age of motherhood observed for the early exposure group (1950-1959) is no longer present for this group. Moreover, I extend the specification of equation (2) to include women born between 1956 and 1958. The added cohorts were between the ages of 12 and 14 at the time of the state legal changes and hence are unlikely to be affected by the treatment. These birth cohorts experienced equal access to abortion across states, as they all obtained access to abortion at the start of their fertility cycle through federal legalization. Estimation results are reported in Appendix B.2 for the full sample and by race (Figure 12). The results show that access to abortion between the ages of 12 and 14 had no effect on age of start of motherhood. This result provides further evidence that the effect found above is indeed due to abortion access and not due to a trend change in fertility preferences.

6 Consequences of Unplanned Start of Motherhood on Women's Career

The baseline reduced form relationship between fertility realization $y^F_{i(s)}$ and a labor market outcome $y^L_{i(s)}$ of a woman i living in state s can be written as follows

$$y^L_{i(s)} = \delta_0 + \delta_1 y^F_{i(s)} + \theta X + \delta_s + \epsilon_{i(s)} \tag{3}$$

Where X is a vector of observable and δ_s is a state of residency fixed effect. Omitted variable bias as well as simultaneity of fertility and labor supply choices imply that OLS estimates are unlikely to recover a consistent estimates of δ_1. Since random assignment of fertility events is unfeasible, the best course of action to identify the effect of fertility realizations on labor market outcome is to exploit a quasi natural experiment that leads to random variation in fertility realizations. The discussion above shows that the early repeal of abortion is a plausible natural experiment to estimate the desired effect. Angrist and Evans (2000) use state variation in access to abortion to estimate the effect of teenage pregnancy on schooling and labor market outcomes. Similarly, I used variation in access to legal abortion as an instrument to measure the effect of age of start of motherhood on a variety of labor market outcomes. Estimation of equation 2 for age of motherhood is used as a first stage of the 2SLS estimation strategy. The fitted values for the fertility outcome are then used to estimate the second stage equation. Validity of the instrumental variable approach is conditional on the instrument satisfying the exclusion restriction, meaning that the effect of abortion access on labor market outcomes should be exclusively through the effect of access on the realizations of the early fertility shocks.

This approach provides a consistent estimate of the average effect of fertility realization y^F on labor market outcomes y^L. More importantly for the purpose of this paper, as noted by Angrist *et al.* (1996), these average effects are for the subsample of women whose fertility realization was altered by abortion legalization (LATE). In other words, the estimate 2SLS estimate $\hat{\delta}_1$ for age of start of motherhood can be interpreted as the average effect of delaying an unplanned start of motherhood for women who chose to use abortion.

6.1 Data on Labor Market Outcomes

The main variable of interest is yearly labor earnings which is readily available in every sample of the IPUMS. The cross sectional nature of the data has been one of the main challenges in this paper. Proper estimation of the effect in question relies on appropriate choice of sample. The sample used in this estimation is the same as the one used in estimating the effect of abortion access on birth timing outcomes. Several reasons justify this sample selection. First, the focus is on estimating the effect of age at start of motherhood on labor earnings using exposure to abortion as an instrumental variable, and as argued earlier this is the proper sample to estimate the first stage. Second, wages vary with age of individuals, and to properly identify the effect of an earlier fertility shock on a woman's earnings, we should compare earnings outcomes for women of similar age. The age range of women in the selected sample varies between 29 and 41, with most of them concentrated around the age of 35. The earning data reported are nominal earnings measured in three samples that are 10 years apart, I hence deflate the earnings and express them in 2012 dollars to make them comparable.

The main limitation of estimating the effect of fertility shocks on earnings using cross sectional data is the inability to capture the dynamics. What is observed in the sample is a one time snapshot of a woman's labor earnings when she is around the age of 35. If fertility shocks have any effect on earnings, we would want to understand the mechanisms that lead to this effect. One particular mechanism this paper aims to explore is the labor market experience. While the data set at hand does not allow investigation of this mechanism directly, I provide suggestive evidence on potential mechanisms using an occupation index measure reported in the IPUMS and observations of labor force participation and weekly hours worked.

Details on sample definition and labor outcome variables construction are reported in appendix A.2. Summary statistics of labor market outcomes are reported in Module

B of Table 1. Black women born in non-repeal states have higher labor participation than black women born in repeal states. White women's labor force participation is the same in both groups of states and is lower than the labor force participation of black women. Averages for hours worked are computed for the full sample and for the subsample of working women. In the full sample, women born in repeal states work 22.51 hours a week while women born in non-repeal states work 23.17. Black women have the largest between state difference, with black women in non-repeal states working on average 1.35 more hours per week than black women in repeal states. In the working women subsample, women born in non-repeal states work around one more hour per week compared to women in repeal states. There is no difference between black women who work on average 38.2 hours a week. White women in repeal states work the least number of hours per week at an average of 35.57, compared to an average of 36.38 for white women born in non-repeal states. Put together, these statistics show that there is no large difference in labor supply decisions between repeal and non-repeal states difference, with the exception of the higher labor force participation of black women in non-repeal states.

Labor earnings are significantly higher in repeal states. On average, a woman born in a repeal state earns $\$18,502$ compared to $\$16,010$ average yearly earnings for a woman born in a non-repeal state. Looking at the difference per earnings by race, I note that the difference in earnings is larger among black women. The difference in earnings among repeal and non-repeal states is $\$3,270$ for black women and $\$2,480$ for white women. Hourly wage rates are similarly higher in repeal states, not surprisingly given that there are no significant differences in labor hours worked of working women and they are constructed using yearly earnings and average weekly hours worked of working women. The occupation index variable is a two digit variable taking values between 0 and 96, with higher values reflecting occupations with higher wages and better education. The reported averages show that women in repeal states have significantly better occupations than women in non-repeal states. The difference in occupation is largest among black women.

6.2 Estimation Results

In this section, I present and discuss estimation results of equation (3). OLS and 2SLS estimates of δ_1 for labor earnings and labor supply choices are reported in Tables 9 and 10.

Yearly Labor Earnings

Unsurprisingly, estimation results show a positive significant association between age of start of motherhood and yearly labor earnings. Postponing the birth of the first child by one year is associated with \$282 more in yearly earnings for a woman in her mid 30s. For the subsample of black women, the association between earnings and age of motherhood is twice the size found for white women. The OLS results are not very informative for the purpose of answering the question posed in this paper. In many cases, women choose the age at which they enter motherhood based on the potential effect of this event on their career. It is likely that women with higher potential earning ability self select into later start of motherhood. It is also likely that both fertility choices and labor market earnings are affected by un-observable characteristics, such as views on gender roles. More importantly, in this paper I am interested in determining the effect of fertility shock due to the unplanned start of motherhood on earnings rather than the effect of the age of start of motherhood itself. The estimates from the instrumental variable approach are therefore more relevant for this purpose, as these estimates will identify the effect of an averted unplanned start of motherhood on a woman's earnings. The results show substantial and statistically significant gains in earnings from delaying an unplanned start of motherhood. The 2SLS estimate for the full sample shows that a one year delay in unplanned start of motherhood increases yearly earnings by \$2,194, which is a 13 percent increase from the mean. The effect is significant and of the same order of magnitude for both black and white women.

The larger magnitude of the 2SLS estimates in comparison to the OLS estimates is not surprising, given that the variation in age of start of motherhood in the IV estimation is arising from women who had access to abortion and chose to use it. Among women with equal access, abortion usage is not random. Women who chose to abort an unplanned first child are most likely women who potentially would have suffered a high wage penalty from such an early start of motherhood.

The gains in earnings could be the result of higher labor supply or higher paying occupation or a combination of both. As a first step in investigating the source of these gains, I look at the effect of age of start of motherhood on hourly wage rates. The first row in table 10 reports regression estimates for the effect of age at start of motherhood on hourly wage rates. A one year delay in the start of motherhood increase the wage rate by average of \$2. There is a slight heterogeneity in the effect by race, with the gains for black women being larger than those for white women. White women's wage rates increase by 13% from the mean, while for black women the increase in wage rates is 16%. These numbers show that the increase in wage rate accounts for all the gains in labor earnings for white women. For black women the percentage increase in wage rate is higher than the percentage increase in yearly labor earning, which might be due to income effect on labor supply decision.

The wage rate variable is constructed using yearly labor earnings variable and reported weekly labor hours. Potential measurement errors in reported labor hours could mean that the reported estimates suffer from division bias. Therefore, I directly investigate the effect of age at start of motherhood on labor supply and occupation status.

Occupation versus Labor Supply

Estimation results reported in the second row of table 10 show a significant increase in occupation status as a result of delaying the unplanned start of motherhood. Similar to the results found on earnings, the IV estimates are larger than the OLS estimates, which as stated earlier is most likely due to women choosing to delay birth by means of abortion are those with better career prospects. The effects are particularly large for black women, for whom delaying the age of start of motherhood by one year put them at equal occupation level as the average working white woman.

Equation (3) is estimated for three labor supply outcomes. Estimation results of the effect of age at start of motherhood on labor force participation and weekly labor hours are reported in Table 9. While the estimate of the effect on weekly labor hours conditional on employment are reported in the third row of Table 10. The results suggest that the effect of early fertility shocks on later lifetime labor supply are minimal if any. The most significant effect is on labor force participation of white women, where I find a significant 2.8% increase as a result of delaying start of motherhood by 1 year. Delaying fertility has an opposite effect on black women labor force participation, where the point estimate show a 3.1% decrease in labor force participation as a result of a one year delay in motherhood, however the estimate is only significant at 10 percent significance level. Similarly, the effect on labor hours worked seems to be small. For the sample of working women delaying age of start of motherhood increase hours supplied of employed black women by slightly more than half an hour per week, while white women decrease weekly hours worked by slightly less than an hour per week. For the full sample of women, including those not working, delaying age of start of motherhood has no significant effect on weekly hours worked.

The mild effect found on labor supply at both extensive and intensive margins, in addition to the positive and significant causal effect of age of start of motherhood on occupation status, lead me to believe that the documented earning gains are mostly due to improvement in occupation status. While the cross sectional nature of the IPUMS does not allow observation of the lifetime earning profile, the findings above suggest strong dynamic effects of an early fertility shock on lifetime earnings. By preventing an unplanned start of motherhood, women attain significantly higher earnings when they reach their mid thirties. The fact that these gains are due to obtaining better occupations that pay higher wages indicate that if they weren't avoided fertility shocks would have lead to permanent decline in lifetime earnings.

7 Conclusion

In this paper I asses the effect of fertility shock on earning by exploiting a difference in timing of policy change at the state and federal level. The early repeal of abortion ban in 5 states led to variation in abortion access at different years of age. This variation resulted in difference in fertility shocks realizations. Specifically, women who received early access to abortion were less likely to start their motherhood early. I then exploit this random variation in fertility shocks to identify the effect of an unplanned start of motherhood on earnings. I find statistically and economically significant positive effect of delaying motherhood on women earnings. The evidence in this paper suggest that most of the increase in earnings is due to higher wage rate resulting from a better occupation.

The findings in this paper provide a better understanding of the potential consequences of abortion legalization in the United States on fertility and careers of women. While effect of abortion access on total fertility seems to be limited if any, abortion has a considerable effect on fertility timing, more specifically on the age at which women enter motherhood. These effect are most significantly early in the fertility cycle of women. Although this paper does not study the effect of state restrictions targeting abortion users and providers, it can speak to some of their consequences on women fertility. While today no ban is imposed on access to abortion, many policies are enacted to limit it's accessibility. Moreover, these policies can penalize women unevenly by making the cost of access higher for some of them compared to other. Young women, who as shown in this paper are the largest beneficiaries in terms of controlling their fertility are also the most vulnerable to state restrictions as they tend to have less resources to afford more costly abortions. As is also shown in this paper, abortion access is not just about control over fertility cycle. Variations in fertility realization due to abortion utilization has significant effects on women career. There are sustainable gains in earnings documented as a result of delaying motherhood, especially if entrance to motherhood was due to an unplanned pregnancy.

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Tables

Table 1: Summary Statistics of Fertility and Labor Market Outcomes

This table presents descriptive statistics for the main outcome variables studied. Data are from the 5% Public Use Microdata Samples (IPUMS), construction of variables and sample restrictions are described in details in appendix A. The averages are reported separately for repeal and non-repeal states, for the full sample and by race.

		Full Sample	White Women	Black Women
Module A: Fertility Outcomes				
Completed Fertility				
	Repeal	2.21	2.18	2.23
	Non Repeal	2.46	2.38	2.91
Age at Birth of First Child				
	Repeal	24.80	24.90	22.83
	Non Repeal	23.83	24.05	22.21
Birth Spacing				
	Repeal	3.51	3.45	4.53
	Non Repeal	3.75	3.66	4.65
Module B: Labor Market Outcomes				
Labor Force Participation				
	Repeal	0.676	0.671	0.699
	Non Repeal	0.680	0.671	0.739
Weekly Labor Hours				
	Repeal	22.51	22.29	23.66
	Non Repeal	23.17	22.89	25.01
Weekly Labor Hours if Working				
	Repeal	35.82	35.57	38.29
	Non Repeal	36.65	36.38	38.26
Yearly Wage Earnings				
	Repeal	18,502	18,215	20,575
	Non Repeal	16,010	15,835	17,305
Hourly Wage Rate				
	Repeal	15.45	15.34	16.54
	Non Repeal	13.12	13.10	13.35
Occupation Index				
	Repeal	43.04	43.23	40.67
	Non Repeal	39.74	40.47	35.27

Table 2: Illustration of the Identification

This table presents a comparison of fertility outcomes for women with different levels of exposure to abortion. Data is extracted from the IPUMS (1970, 1980 and 1990). The no exposure group consists of women born between 1930 and 1935, the mid fertility cycle exposure group consists of women born between 1940 and 1945 and the early exposure group consists of women born between 1950 and 1955.

		Birth cohorts 1950-1955 observed in 1990		Birth cohorts 1940-1945 observed in 1980		Birth cohorts 1930-1935 observed in 1970
Module A: Age at First Child						
Repeal State	(1)	25.72		24.41		24.46
		(4.85)		(3.94)		(3.75)
Non-Repeal State	(2)	24.60		23.75		23.69
		(4.69)		(3.85)		(3.66)
Difference	(1)-(2)	1.12		0.66		0.77
		[0.00]		[0.00]		[0.00]
Difference-in-Differences			0.46		-0.11	
			[0.00]		[0.38]	
Module B: Total Fertility						
Repeal State	(3)	1.75		2.18		2.81
		(1.36)		(1.50)		(1.90)
Non-Repeal State	(4)	1.91		2.41		3.08
		(1.36)		(1.61)		(2.08)
Difference	(3)-(4)	-0.16		-0.23		-0.26
		[0.00]		[0.00]		[0.00]
Difference-in-Differences			0.07		0.03	
			[0.00]		[0.78]	

Note: Standard deviations are reported in parenthesis under the sample means and p-value for the test of significance of the differences are reported in brackets under.

Table 3: Cross States Differences in Completed Fertility (Age Group)

This table reports estimation results of equation (2) for the completed fertility outcome variable, for the full sample and by race. Completed fertility is defined as the total number of children a woman give birth to during her lifetime. It is measured as the total number of children women reported to have given births to in the 1990 sample.

	Mean	α_1 [15-17]	α_2 [18-20]	α_3 [21-23]	α_4 [24-26]	α_5 [27-29]	α_6 [30-32]	α_7 [33-35]	α_8 [36-38]	α_9 [39-40]
Full Sample										
Completed Fertility	2.42	-0.05*	-0.05**	-0.06***	-0.07***	-0.07***	-0.09***	-0.08***	-0.12***	-0.11***
		(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
						N=1,761,209				
Black Women										
Completed Fertility	2.86	-0.10***	-0.05	-0.17***	-0.18***	-0.31***	-0.43***	-0.65***	-0.64***	-0.57***
		(0.03)	(0.03)	(0.06)	(0.02)	(0.10)	(0.09)	(0.04)	(0.12)	(0.07)
						N=180,627				
White Women										
Completed Fertility	2.35	0.02	0.01	0.01	-0.01	0.01	0.01	0.03**	-0.02	-0.03
		(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
						N=1,535,533				

Note: All regressions have been weighted by population weights.
Clustered robust standard errors are reported in parenthesis.
* – $p < 0.1$; ** – $p < 0.05$; *** – $p < 0.01$.

Table 4: Difference-in-Differences Estimates of the Effect of Abortion Access on Completed Fertility

This table reports the difference-in-differences estimates for the effect of abortion access on completed fertility. The columns report the effect of abortion access starting a certain age phase compared to receiving access to abortion at the end of the fertility cycle. These coefficients are computed using estimates from Table 3.

	Mean	γ_1 [15-17]	γ_2 [18-20]	γ_3 [21-23]	γ_4 [24-26]	γ_5 [27-29]	γ_6 [30-32]	γ_7 [33-35]	γ_8 [36-38]
Full Sample									
Completed Fertility	2.42	0.06**	0.06*	0.05*	0.05	0.04	0.03	0.03	-0.001
		(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Black Women									
Completed Fertility	2.86	0.47***	0.52***	0.40***	0.39***	0.26***	0.14*	-0.08	-0.06
		(0.09)	(0.09)	(0.11)	(0.07)	(0.08)	(0.08)	(0.06)	(0.09)
White Women									
Completed Fertility	2.35	0.04	0.04	0.03	0.01	0.03	0.03	0.06***	0.01
		(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.01)	(0.01)

Note:* $- p < 0.1$; ** $- p < 0.05$; *** $- p < 0.01$.

Table 5: Cross States Differences in Births Timing (Age Group)

This table reports estimation results of equation (2) for age at birth of first child and births spacing, for the full sample and by race. For information on variables construction and sample definition refer to Appendix A.1.

	Mean	α_1 [15-17]	α_2 [18-20]	α_3 [21-23]	α_4 [24-26]	α_5 [27-29]	α_6 [30-32]	α_7 [33-35]
Full Sample								
Age at First Child	24.00	0.99***	0.92***	0.60***	0.57***	0.43***	0.37***	0.46***
		(0.08)	(0.13)	(0.08)	(0.04)	(0.02)	(0.06)	(0.04)
					N=800,730			
Birth Spacing	3.71	-0.59***	-0.61***	-0.56***	-0.57***	-0.56***	-0.54***	-0.59***
		(0.03)	(0.05)	(0.02)	(0.03)	(0.03)	(0.02)	(0.06)
					N=354,611			
Black Women								
Age at First Child	22.26	0.83***	0.27***	0.01	0.06	0.08	-0.08	0.28**
		(0.07)	(0.06)	(0.14)	(0.11)	(0.10)	(0.16)	(0.12)
					N=80,000			
Birth Spacing	4.64	-0.60***	-0.72***	-0.23**	-0.72***	-0.28**	-0.64***	-0.63***
		(0.16)	(0.10)	(0.08)	(0.09)	(0.12)	(0.09)	(0.13)
					N=27,156			
White Women								
Age at First Child	24.20	1.11***	1.06***	0.74***	0.74***	0.60***	0.54***	0.60
		(0.07)	(0.14)	(0.06)	(0.03)	(0.02)	(0.06)	(0.05)
					N=706,113			
Birth Spacing	3.62	-0.62***	-0.63***	-0.62**	-0.61***	-0.62**	-0.58***	-0.64***
		(0.02)	(0.04)	(0.02)	(0.03)	(0.03)	(0.02)	(0.06)
					N=321,787			

Note: All regressions have been weighted by population weights.
Clustered robust standard errors are reported in parenthesis.
* – $p < 0.1$; ** – $p < 0.05$; *** – $p < 0.01$.

Table 6: Difference-in-Differences Estimates of the Effect of Abortion Access on Births Timing

This table reports the difference-in-differences estimates for the effect of abortion access on birth timing outcomes. The columns report the effect of abortion access starting a certain age phase compared to receiving access to abortion at the end of the fertility cycle. These coefficients are computed using estimates from Table 5.

	Mean	γ_1 [15-17]	γ_2 [18-20]	γ_3 [21-23]	γ_4 [24-26]	γ_5 [27-29]	γ_6 [30-32]
Full Sample							
Age at First Child	24.00	0.53***	0.46***	0.14	0.11	-0.03	-0.09**
		(0.07)	(0.16)	(0.11)	(0.06)	(0.04)	(0.04)
Spacing	3.71	-0.00	-0.02	0.03	0.02	0.03	0.05
		(0.08)	(0.07)	(0.07)	(0.08)	(0.08)	(0.06)
Black Women							
Age at First Child	22.26	0.54***	-0.01	-0.27	-0.23**	-0.20***	-0.37***
		(0.15)	(0.16)	(0.25)	(0.09)	(0.07)	(0.10)
Spacing	4.46	0.03	-0.09	0.41***	-0.08	0.35**	-0.01
		(0.26)	(0.16)	(0.15)	(0.15)	(0.16)	(0.17)
White Women							
Age at First Child	24.20	0.51***	0.46**	0.14	0.13	-0.00	-0.06
		(0.06)	(0.18)	(0.11)	(0.07)	(0.06)	(0.04)
Spacing	3.62	0.02	0.01	0.02	0.03	0.02	0.05
		(0.07)	(0.07)	(0.07)	(0.09)	(0.09)	(0.06)

Note:* $- p < 0.1$; ** $- p < 0.05$; *** $- p < 0.01$.

Table 7: Cross States Difference in Black Women's Marriage Outcomes

This table reports estimation results of equation (2) for marriage outcome of black women. Marriage is a dummy variable that takes value 1 if a woman is married at the time of observation and 0 otherwise. The three other outcome variables are defined for married black women only. Husband College Completion is a dummy variable that takes value 1 if the husband has a college degree and 0 otherwise. Husbands Yearly Earnings is a continuous measure of yearly labor earnings deflated and expressed in 2012 dollars. Husband Occupation Index is a measure of occupation status that takes value between 0 and 100, with larger values indicating occupations with higher median wage rates.

	Mean	α_1 [15-17]	α_2 [18-20]	α_3 [21-23]	α_4 [24-26]	α_5 [27-29]	α_6 [30-32]	α_7 [33-35]
Married	0.48	-0.09***	-0.09***	-0.09***	-0.11***	-0.08***	-0.13***	-0.06***
		(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.02)
					N=146,494			
Husbands College Completion	0.07	0.03*	0.04***	0.04***	0.04***	-0.03***	-0.02***	-0.04**
		(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
					N=69,682			
Husbands Yearly Earnings	16,891	4,672***	4,891***	2,427***	189	-688	240	-1,495***
		(465)	(651)	(476)	(535)	(534)	(1,112)	(513)
					N=69,679			
Husbands Occupation Index	65.97	-6.68***	-1.79	-4.12***	-2.60***	-2.65***	-5.49**	-6.89**
		(1.32)	(1.20)	(0.53)	(0.45)	(1.27)	(2.74)	(2.73)
					N=50,962			

Note: All regressions have been weighted by population weights.
Clustered robust standard errors are reported in parenthesis.
* – $p < 0.1$; ** – $p < 0.05$; *** – $p < 0.01$.

Table 8: Difference-in-Differences Estimates of the Effect of Abortion Access on Black Women's Marriage

This table reports the difference-in-differences estimates for the effect of abortion access on black women marriage outcomes. The columns report the effect of abortion access starting a certain age phase compared to receiving access to abortion at the end of the fertility cycle. These coefficients are computed using estimates from Table 7.

	Mean	γ_1 [15-17]	γ_2 [18-20]	γ_3 [21-23]	γ_4 [24-26]	γ_5 [27-29]	γ_6 [30-32]
Marriage	0.48	0.02	-0.02	-0.03*	-0.05***	-0.02	-0.06***
		(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.02)
Husband College Completion	0.07	0.06**	0.07***	0.08***	0.08***	0.00	0.01
		(0.03)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Husband Yearly Earnings	16,891	6,167***	6,386***	3,922***	1,684**	807	1,736
		(562)	(1,030)	(906)	(689)	(616)	(1,484)
Husband Occupation Index	65.97	0.21	5.10	2.77	4.29	0.24	1.40**
		(3.81)	(3.76)	(3.13)	(2.56)	(1.64)	(0.60)

Note: * – $p < 0.1$; ** – $p < 0.05$; *** – $p < 0.01$.

Table 9: Effect of Start of Motherhood on Earnings and Labor Supply

This table reports OLS and 2SLS estimation results of equation (3) for the effect of age of start of motherhood on yearly labor earnings, labor force participation and weekly hours worked. The sample includes all women both working and not working, with labor hours and earnings assigned the value of 0 if a woman is not working. The estimation results for the full sample are reported in columns (1) and (2), for the subsample of black women are reported in columns (3) and (4), and for the subsample of white women are reported in columns (5) and (6). Details of variables construction and sample restrictions are provided in Appendix A.2.

	Full Sample		Black Women		White Women	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Yearly Labor Earnings	282***	2,194***	516***	1,989***	252***	1,980***
	(10)	(263)	(20)	(514)	(12)	(243)
Average Yearly Earnings	16,433		17,559		16,253	
Labor Force Participation	-0.008***	0.027***	0.002***	-0.031*	-0.009***	0.028***
	(0.000)	(0.010)	(0.000)	(0.016)	(0.000)	(0.010)
Average Labor Force Participation	0.679		0.736		0.671	
Weekly Hours Worked	-0.34***	0.50	0.13***	-0.64	-0.41***	0.43
	(0.02)	(0.43)	(0.02)	(0.66)	(0.02)	(0.42)
Average Weekly Hours Worked	23.06		24.90		22.79	
Number of Observations	800,730		80,000		706,113	

Note: All regressions have been weighted by population weights.
Clustered robust standard errors are reported in parenthesis.
* $-$ $p < 0.1$; ** $-$ $p < 0.05$; *** $-$ $p < 0.01$.

Table 10: Effect of Start of Motherhood on Labor Outcomes of Employed Women

This table reports OLS and 2SLS estimation results of equation (3) for the effect of age of start of motherhood on hourly wage rate, Occupation Index and Weekly hours worked. The sample is restricted to working women only. The estimation results for the full sample are reported in columns (1) and (2), for the subsample of black women are reported in columns (3) and (4), and for the subsample of white women are reported in columns (5) and (6). Details about outcome variables construction and sample restrictions are provided in Appendix A.2.

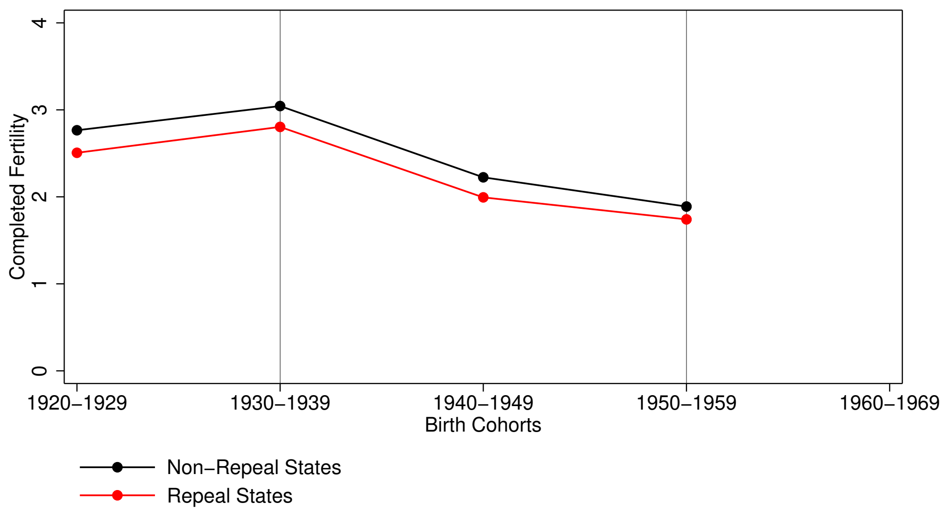
	Full Sample		Black Women		White Women	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Hourly Wage Rate	0.47***	1.96***	0.36***	2.18***	0.49***	1.79***
	(0.02)	(0.11)	(0.01)	(0.48)	(0.03)	(0.10)
Average Hourly Wage Rate	13.5		13.6		13.5	
Occupation Index	1.15***	3.03***	0.96***	3.42***	1.19***	2.79***
	(0.02)	(0.13)	(0.03)	(0.52)	(0.03)	(0.12)
Occupation Index Average	40.30		35.67		40.95	
Weekly Hours Worked if Employed	-0.21***	-0.64***	0.00	0.68***	-0.26***	-0.78***
	(0.02)	(0.22)	(0.02)	(0.22)	(0.02)	(0.21)
Average Weekly Hours Worked if Employed	37.33		38.19		37.23	
Number of Observations	474,609		53,434		411,933	

Note: All regressions have been weighted by population weights.
Clustered robust standard errors are reported in parenthesis.
* $-$ $p < 0.1$; ** $-$ $p < 0.05$; *** $-$ $p < 0.01$.

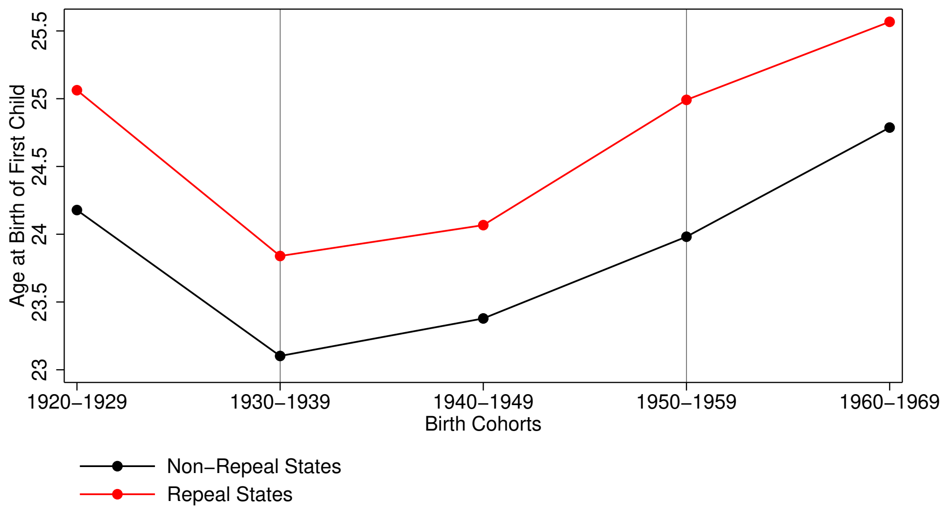
Figures

Figure 1: Fertility Outcome Trends

This figure shows fertility outcomes trends for birth cohorts that received various levels of access to legal abortion. The two vertical lines demarcate the birth cohorts that had varying level of exposure to legal abortion across states. Observations of total number of children born, age of mother and age of eldest child in the household from the 1960, 1970, 1980, 1990 and 2000 IPUMS samples are used to constructed the measures of completed fertility and age at birth of first child for the 1920-1929, 1930-1939, 1940-1949, 1950-1959 and 1960-1969 birth cohorts respectively.



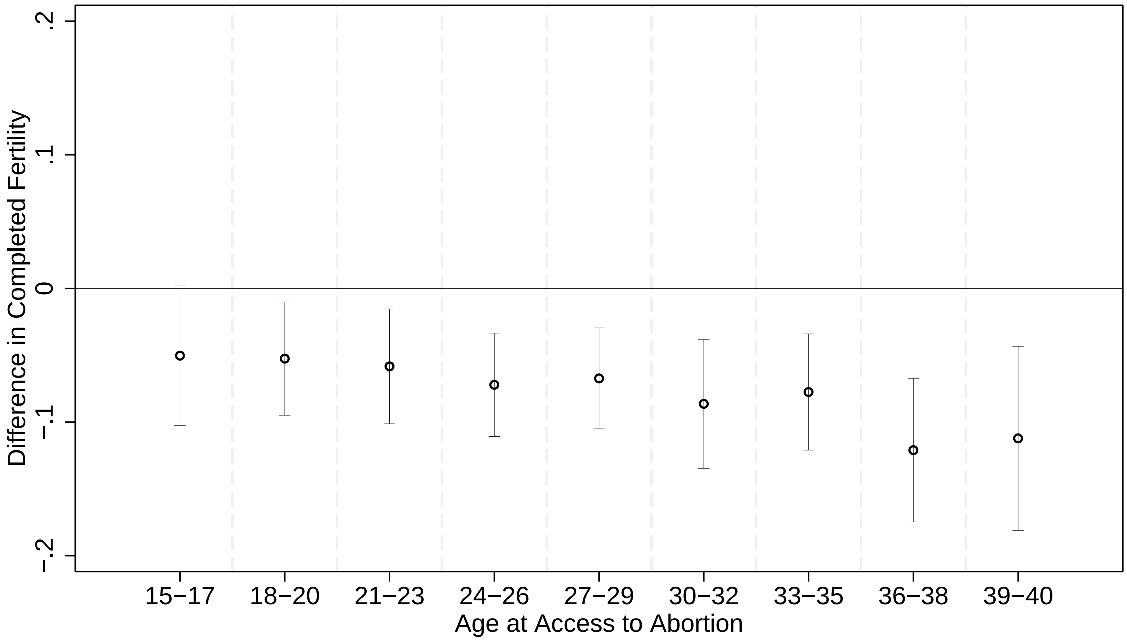
(a) Completed Fertility



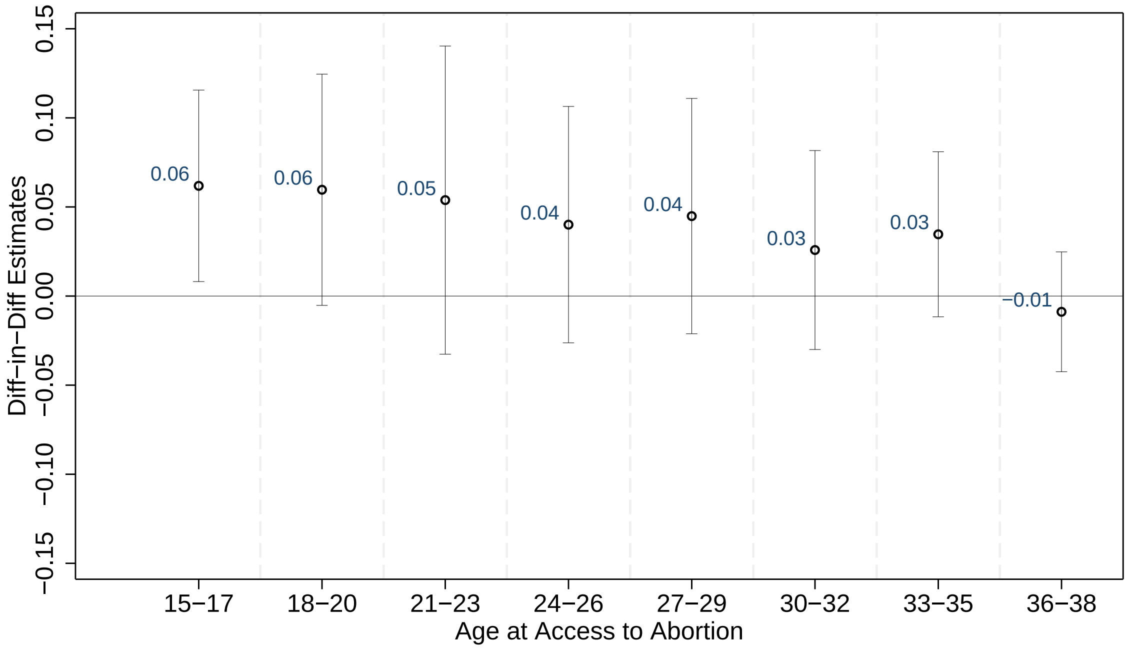
(b) Age at Birth of First Child

Note: Completed fertility for the 1960-1969 birth cohorts is not reported due to the variable not being available in the 2000 IPUMS sample.

Figure 2: Effect of Access to Abortion on Completed Fertility

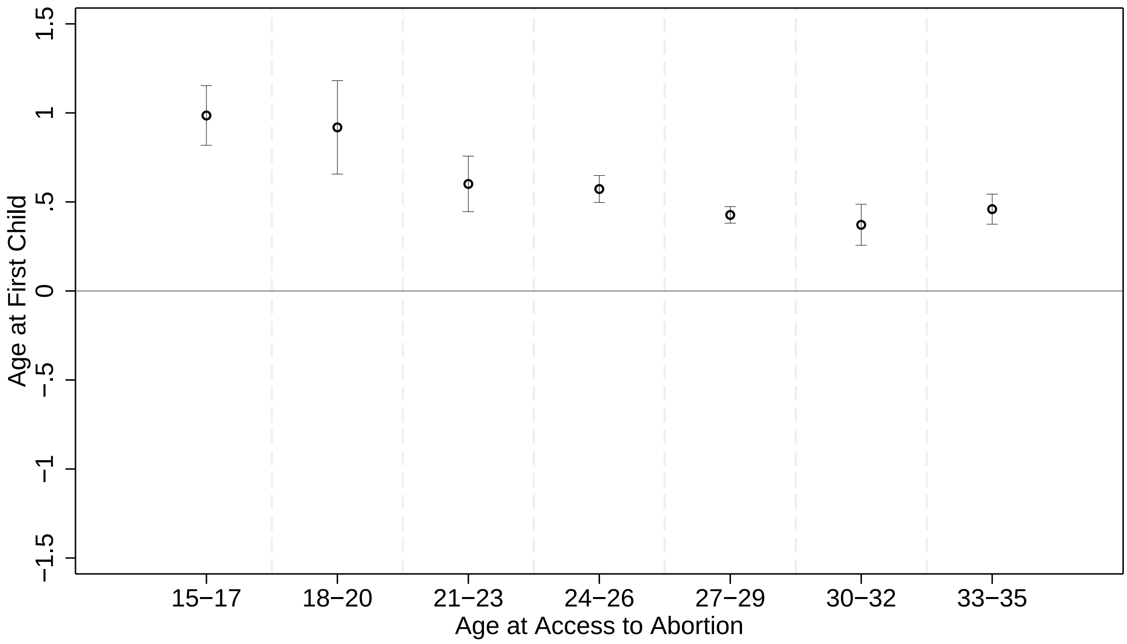


(a) Completed Fertility Trend by Age Group

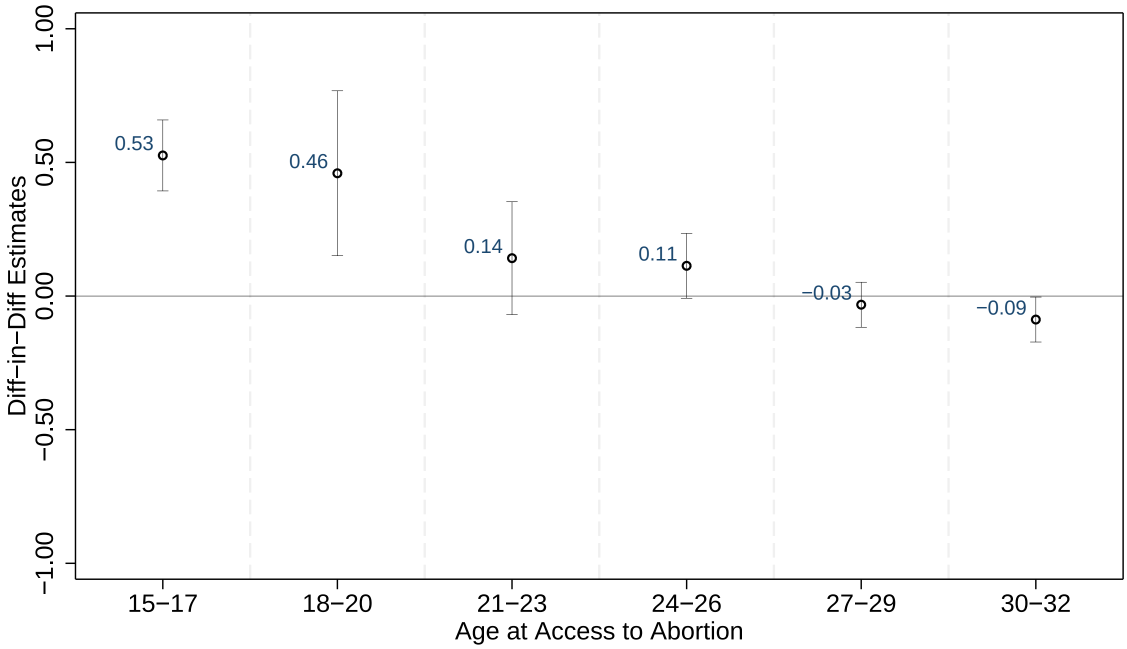


(b) Estimates of the effect of abortion access on Completed Fertility - Age Group

Figure 3: Effect of Access to Abortion on Age at Start of Motherhood

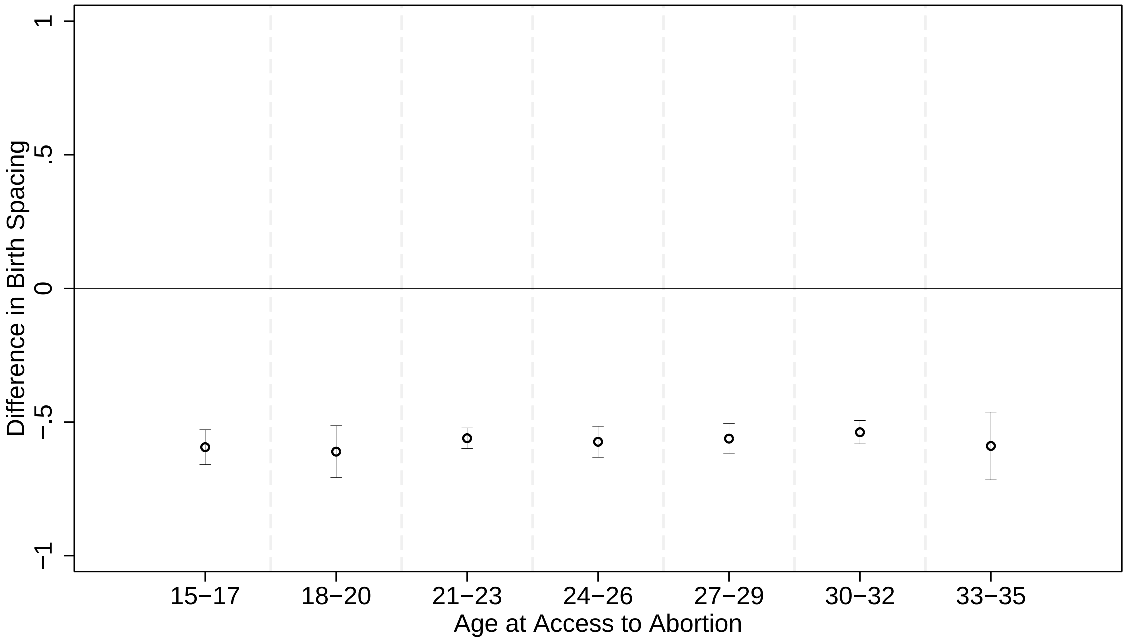


(a) Age at Birth of First Child Trend by Age Group

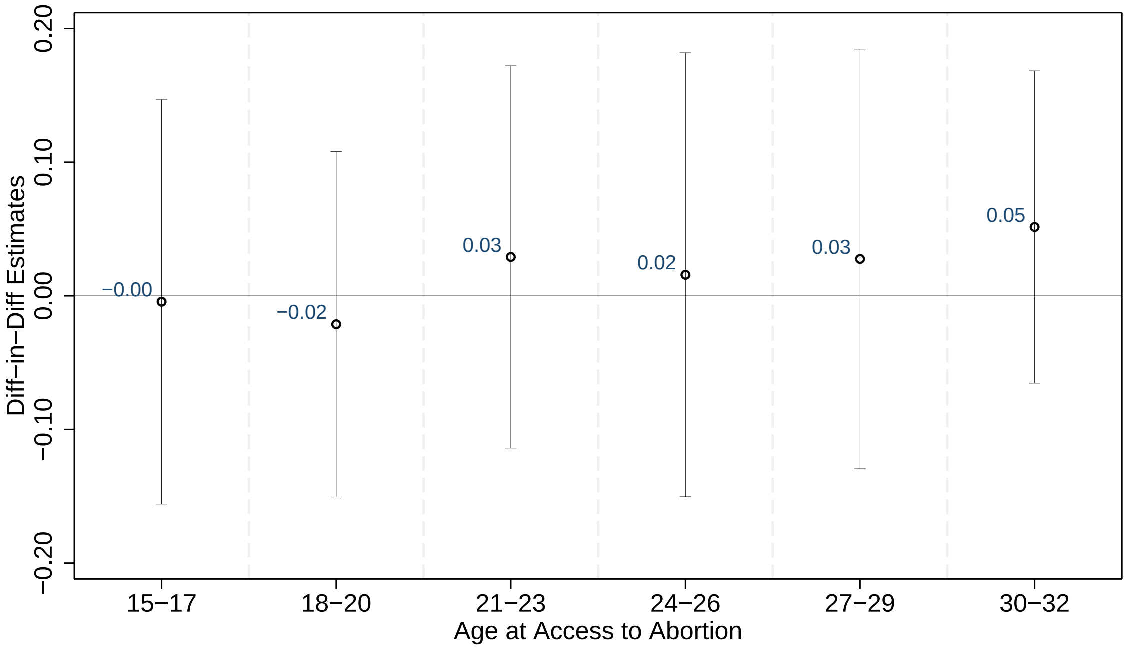


(b) Estimates of the effect of abortion access on Age at Birth of First Child - Age Group

Figure 4: Effect of Access to Abortion on Birth Spacing

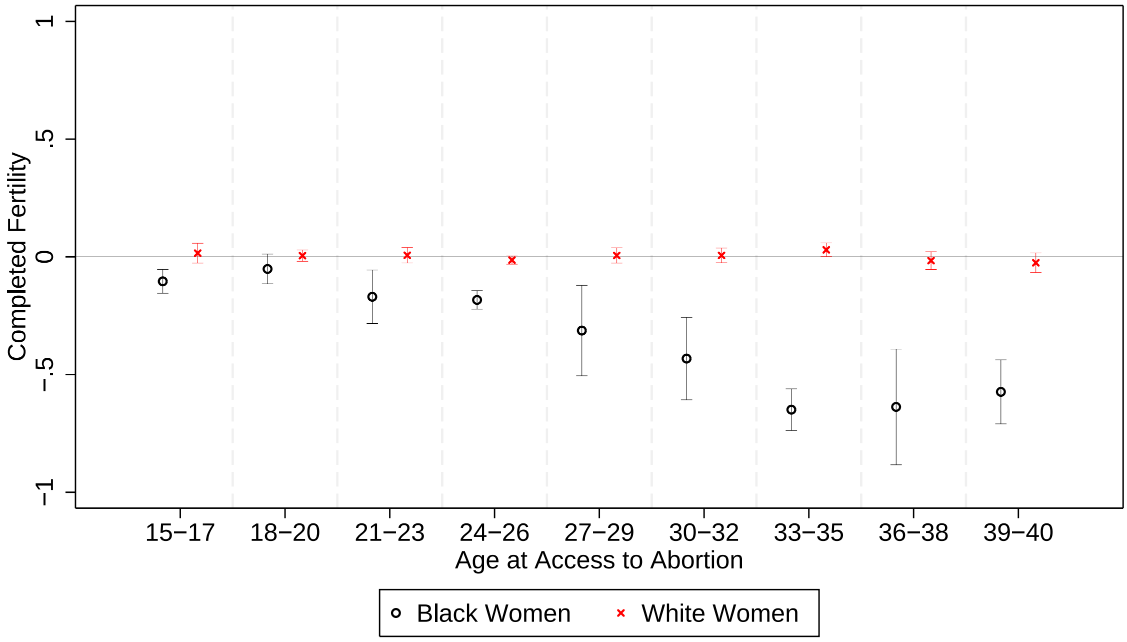


(a) Birth Spacing Trend by Age Group

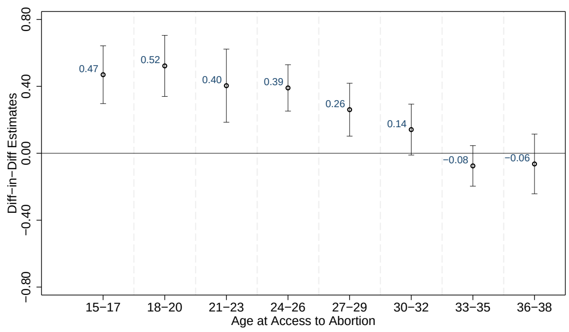


(b) Estimates of the effect of abortion access on Births Spacing - Age Group

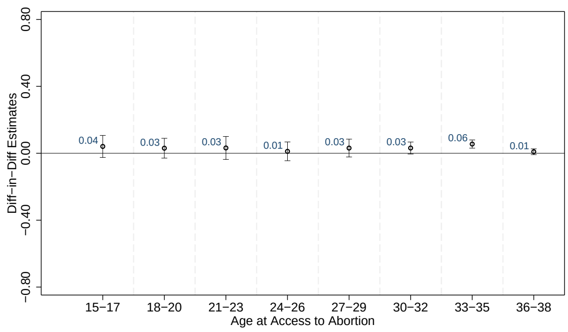
Figure 5: Effect of Access to Abortion on Completed Fertility by Race



(a) Completed Fertility Trends for Black and White Women

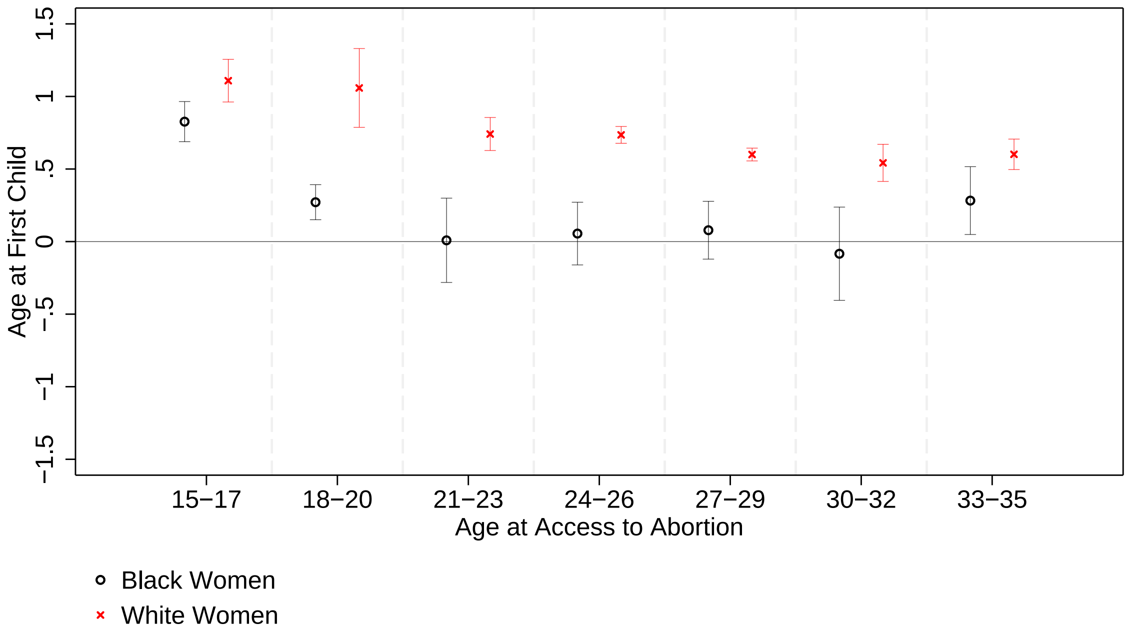


(b) Difference-in-Differences Estimates for Black Women

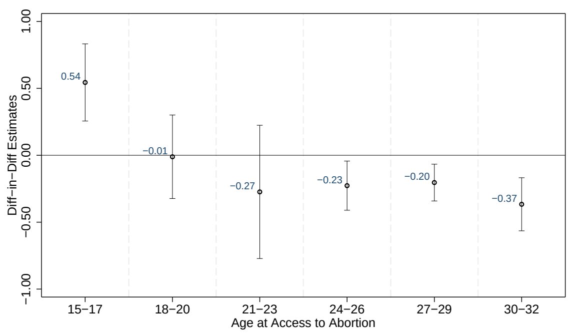


(c) Difference-in-Differences Estimates for White Women

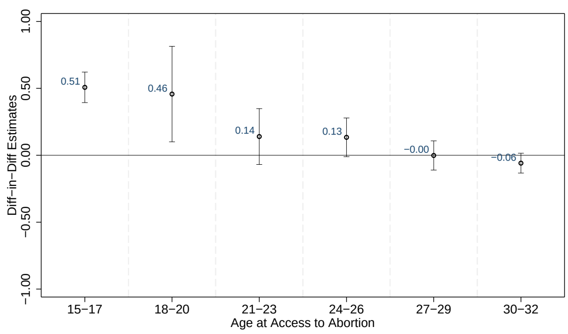
Figure 6: Effect of Access to Abortion on Age at Start of Motherhood by Race



(a) Age at Birth of First Child Trends for Black and White Women

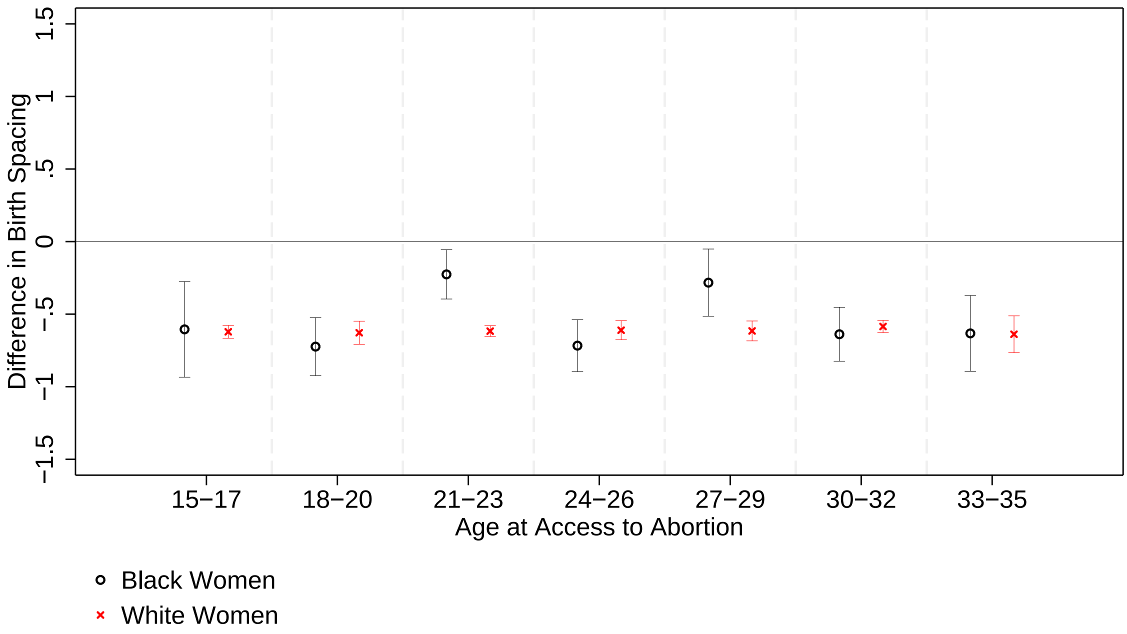


(b) Difference-in-Differences Estimates for Black Women

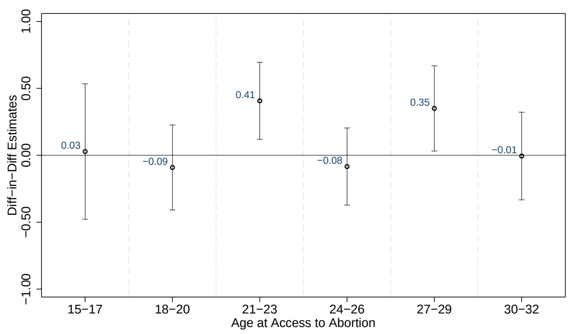


(c) Difference-in-Differences Estimates for White Women

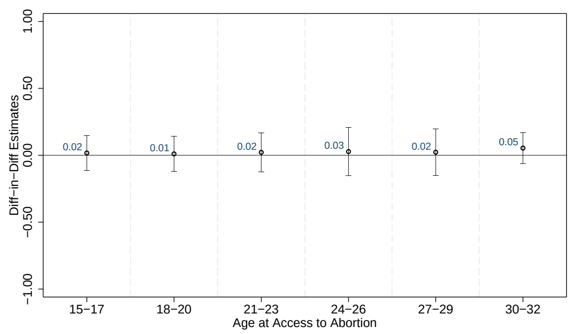
Figure 7: Effect of Access to Abortion on Birth Spacing by Race



(a) Birth Spacing Trends for Black and White Women

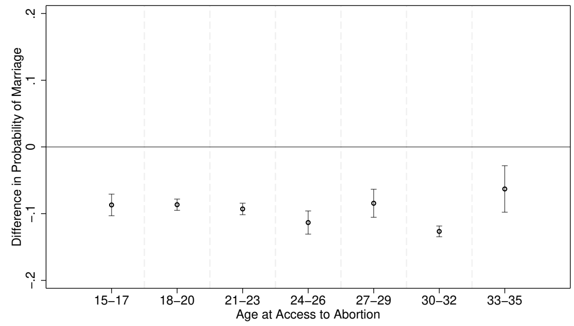


(b) Difference-in-Differences Estimates for Black Women

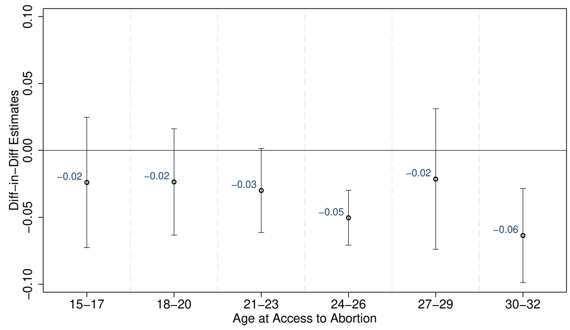


(c) Difference-in-Differences Estimates for White Women

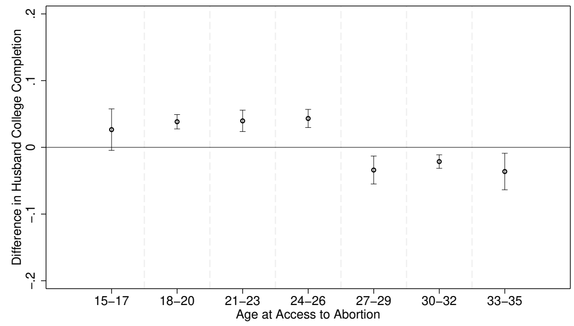
Figure 8: Effect of Access to Abortion on Marriage Outcomes for Black Women



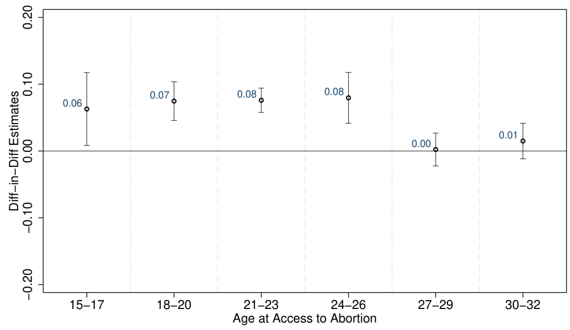
(a) Difference in Rate of Marriage for Black Women



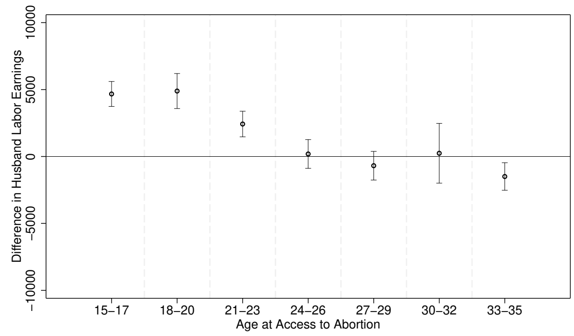
(b) Estimates of the effect of abortion access on Rate of Marriage for Black Women



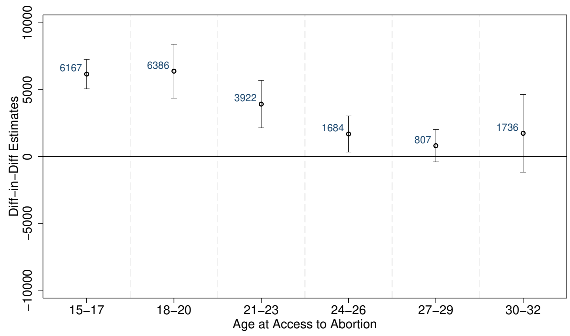
(c) Difference in Husbands College Completion for Black Women



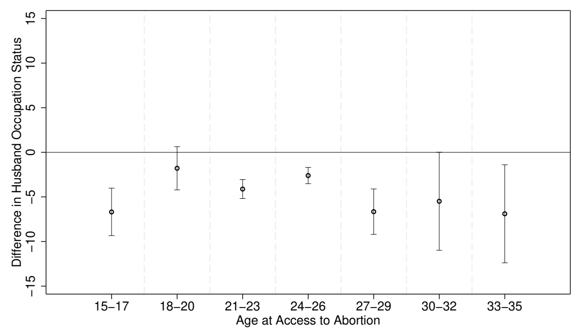
(d) Estimates of the effect of abortion access on Husband College Completion for Black Women



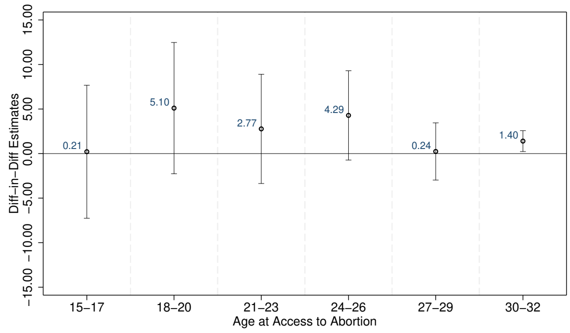
(e) Difference in Husbands Earnings for Black Women



(f) Estimates of the effect of abortion access on Husband Earnings for Black Women



(g) Difference in Husbands Occupation for Black Women



(h) Estimates of the effect of abortion access on Husband Occupation for Black Women

Appendix

A Data

The data available for the purpose of this study are the 1960, 1970, 1980, 1990 and 2000 samples of 5% Public Use Microdata Samples (IPUMS Bureau of the Census). In this section I describe in detail the construction of the main outcome variables as well as the different sample restrictions imposed to estimate the effect of access to abortion throughout the fertility cycle. I first restrict the focus of study to women born in the US between 1930 and 1955. This restriction guarantees that at the time of early state level legalization of abortion in 1970 the age of women observed extend between 15 and 40, which is the assumed range of fertility cycle in this study. Further restrictions are imposed to estimate the effect on various fertility and labor market outcomes the details of which are provided in what follows.

A.1 Fertility Outcomes

Identification of the effect of abortion access on fertility outcomes require observation of these outcomes once they have been achieved. Observing outcomes earlier than achievement can lead to estimation bias. This is particularly true in the case of cross sectional data such as the one used in this paper. For instance, if in a sample year we observe the total number of children born to women prior to achieving their fertility cycle, estimation of the effect of abortion will likely be overstated for the younger birth cohorts, since at the time of observation total number of children born to older women is a better measurement of completed fertility as compared to younger women.

The first outcome of interest, completed fertility, is defined as the total number of children a woman gave birth to during her fertility cycle. Hence an accurate measurement of completed fertility requires the observation of total number of children born to a woman after she has concluded her fertility phase, which is usually around the age of 45. The census reports total number of children women gave birth to by the time they were interviewed. Given that birth cohorts are restricted to women born between 1930 and 1955, the ideal measurement of completed fertility is the observed total number of children in the 2000 sample. Unfortunately this variable is not reported in that sample year, as such completed fertility is set to equal the total number of children a woman gave birth to observed in the 1990 sample. This potentially create a measurement error for the completed fertility observation of the younger birth cohorts. Knowing that most women give birth to their children by the age of 42, this measurement error is most likely restricted to women in the sample born after 1948.

The second fertility outcome considered is the age at which women become mother. This variable is constructed by taking the difference of the age of the eldest child and the age of the mother. There are two potential challenges constructing this variable. First, it requires observation of women after they had given birth to at least one child. Second, the survey reports the age of the eldest child still living in the household, consequently the observation should be at a time where the first born child still live in the household. To satisfy these two restriction women in the sample should be at least 30 years old and not older than 40. The sample therefore includes women born between 1935 and 1941 and observed in 1970, women born between 1942 and 1948 and observed in 1980 and women born between 1949 and 1955 and observed in 1990. I additionally restrict the sample to households where total number of children living in the household is equal to total number of children the mother gave birth to. This was due to some women being older than 40 at observation and hence their eldest child might have left the household. This restriction potentially exclude women who had children very early in their life. It should also be noted that the eldest birth cohorts in this restriction (1935-1941) are observed prior to legal change. I am assuming that for these birth cohort the birth of first child occurred prior to abortion legalization and hence the legal changes had no effect on this particular fertility realization. This assumption is reasonable given that the average age at birth of a child in the sample is 24[1]. Finally a third fertility outcome is considered. Restricting the sample to women who had two children only, I construct birth spacing variable by taking the difference of the age of the eldest and youngest child.

A.2 Labor Market Outcomes

The sample used in estimation of labor market outcomes is the same as the one used for estimating the effect of abortion access on birth timing. There are two sets of labor outcomes I study in this paper; Labor supply and labor earning Outcomes. I use multiple labor supply variables in order to study the effect on labor supply at intensive and extensive margins. The first of these variables is labor force participation status which is readily available in the data. Weekly hours worked are reported as a continuous variable in the data for the 1980 and 1990 sample. However, in the 1970 sample the hours worked are reported in interval. Hence in constructing the continuous measure of hours worked I used the reported values in the 1980 and 1990 sample and take the midpoint of the reported interval in the 1970 sample. Women who are not working are assigned a value of zero hours worked. Analyzes of labor market outcomes is conducted using both the full sample and the subsample of working women.

The survey reports nominal values of yearly labor earnings. Since I'm stacking three years of survey that span over 20 years, nominal wages in the later samples will be automatically larger due to inflation. To make wage earnings comparable across survey years, I deflate the earnings and express them in 2012 dollars. I construct a wage rate variable for working women by taking the ratio of yearly labor earnings and hours worked. This variable is likely to suffer from division bias as a result of measurement error in the hours worked variable, so analyzes of results on this variable should be studied with care. The last labor market outcome variable I study is an occupation index variable reported in the IPUMS. The occupation index takes values between 0 and 100, with larger values indicating occupation with higher median earned income.

[1] see also Mathews and Hamilton (2002)

B Tables and Figures

B.1 Estimation Results by Year of Age

Table 11: Cross States Differences in Completed Fertility (Age Years)

	Completed Fertility		
	Full Sample	Black Women	White Women
α_{15}	-0.06**	-0.15***	0.01
	(0.03)	(0.05)	(0.02)
α_{16}	-0.05*	-0.03	0.01
	(0.02)	(0.07)	(0.02)
α_{17}	-0.04	-0.13***	0.02
	(0.03)	(0.03)	(0.02)
α_{18}	-0.04*	-0.09***	0.02
	(0.02)	(0.03)	(0.01)
α_{19}	-0.06***	0.03	-0.01
	(0.02)	(0.03)	(0.01)
α_{20}	-0.05**	-0.10**	0.01
	(0.02)	(0.05)	(0.02)
α_{21}	-0.06**	-0.09	0.00
	(0.02)	(0.08)	(0.02)
α_{22}	-0.07**	-0.13***	-0.00
	(0.02)	(0.03)	(0.02)
α_{23}	-0.05**	-0.29***	0.02
	(0.02)	(0.06)	(0.02)
α_{24}	-0.05**	0.01	0.01
	(0.02)	(0.08)	(0.02)
α_{25}	-0.09***	-0.36***	-0.04***
	(0.03)	(0.04)	(0.01)
α_{26}	-0.07***	-0.22**	-0.01
	(0.02)	(0.10)	(0.01)
α_{27}	-0.05***	-0.30***	0.02*
	(0.02)	(0.08)	(0.01)
α_{28}	-0.06**	-0.21***	0.01
	(0.03)	(0.05)	(0.03)
α_{29}	-0.08***	-0.46**	-0.02
	(0.02)	(0.20)	(0.02)
α_{30}	-0.10***	-0.32*	-0.01
	(0.03)	(0.19)	(0.02)
α_{31}	-0.08**	-0.57***	0.01
	(0.03)	(0.08)	(0.03)
α_{32}	-0.08**	-0.44***	0.02
	(0.03)	(0.11)	(0.02)
α_{33}	-0.06**	-0.57***	0.03
	(0.03)	(0.10)	(0.02)
α_{34}	-0.06**	-0.56***	0.04***
	(0.02)	(0.08)	(0.02)
α_{35}	-0.11***	-0.83***	0.01
	(0.03)	(0.10)	(0.02)
α_{36}	-0.13***	-0.63***	-0.02
	(0.04)	(0.07)	(0.03)
α_{37}	-0.107***	-0.64***	-0.00
	(0.04)	(0.07)	(0.03)
α_{38}	-0.13***	-0.64***	-0.03
	(0.03)	(0.29)	(0.03)
α_{39}	-0.08***	-0.42**	-0.00
	(0.03)	(0.19)	(0.02)
α_{40}	-0.14***	-0.73***	-0.06
	(0.05)	(0.11)	(0.03)

Note: All regressions have been weighted by population weights.
Clustered robust standard errors are reported in parenthesis.
* – $p < 0.1$; ** – $p < 0.05$; *** – $p < 0.01$.

Table 12: Difference-in-Differences Estimates of the Effect of Abortion Access on Completed Fertility

	Completed Fertility		
	Full Sample	Black Women	White Women
γ_{15}	0.07**	0.58***	0.06
	(0.03)	(0.15)	(0.04)
γ_{16}	0.09**	0.69***	0.06*
	(0.04)	(0.09)	(0.03)
γ_{17}	0.10**	0.59***	0.07*
	(0.04)	(0.12)	(0.03)
γ_{18}	0.10**	0.64***	0.06*
	(0.04)	(0.12)	(0.04)
γ_{19}	0.08	0.76***	0.04
	(0.05)	(0.11)	(0.04)
γ_{20}	0.09**	0.63***	0.05
	(0.04)	(0.10)	(0.04)
γ_{21}	0.08	0.63***	0.05
	(0.06)	(0.07)	(0.05)
γ_{22}	0.07	0.60***	0.04
	(0.06)	(0.09)	(0.05)
γ_{23}	0.09	0.43***	0.07
	(0.05)	(0.07)	(0.05)
γ_{24}	0.09	0.74***	0.06
	(0.06)	(0.07)	(0.05)
γ_{25}	0.05	0.37***	0.01
	(0.04)	(0.13)	(0.03)
γ_{26}	0.07	0.50**	0.04
	(0.04)	(0.20)	(0.04)
γ_{27}	0.09**	0.43**	0.07*
	(0.04)	(0.18)	(0.04)
γ_{28}	0.07	0.52***	0.06
	(0.07)	(0.14)	(0.05)
γ_{29}	0.06	0.27	0.03
	(0.04)	(0.30)	(0.04)
γ_{30}	0.04	0.41	0.04
	(0.03)	(0.28)	(0.03)
γ_{31}	0.06	0.16	0.06
	(0.06)	(0.16)	(0.05)
γ_{32}	0.06	0.29	0.07
	(0.04)	(0.17)	(0.03)
γ_{33}	0.08	0.15	0.08**
	(0.06)	(0.09)	(0.04)
γ_{34}	0.08**	0.17	0.09***
	(0.04)	(0.17)	(0.03)
γ_{35}	0.03	-0.10	0.06***
	(0.03)	(0.19)	(0.02)
γ_{36}	0.01	0.09	0.03
	(0.02)	(0.16)	(0.03)
γ_{37}	0.03	0.09	0.04*
	(0.03)	(0.16)	(0.02)
γ_{38}	0.01	0.08	0.02
	(0.05)	(0.38)	(0.05)
γ_{39}	0.06	0.31	0.05
	(0.03)	(0.28)	(0.04)

Note:* $- p < 0.1$; ** $- p < 0.05$; *** $- p < 0.01$.

Table 13: Cross States Differences in Births Timing (Age Years)

	Age at First Child			*Birth Spacing*		
	Full Sample	Black Women	White Women	Full Sample	Black Women	White Women
α_{15}	0.98***	0.98***	1.09***	-0.69***	-0.82***	-0.69***
	(0.12)	(0.15)	(0.12)	(0.05)	(0.13)	(0.04)
α_{16}	0.98***	0.46***	1.15***	-0.53***	-0.27	-0.58***
	(0.10)	(0.12)	(0.09)	(0.03)	(0.47)	(0.03)
α_{17}	0.99***	1.06***	1.08***	-0.56***	-0.61***	-0.60***
	(0.06)	(0.21)	(0.04)	(0.04)	(0.10)	(0.04)
α_{18}	0.94***	0.30***	1.07***	-0.61***	-0.63***	-0.63***
	(0.08)	(0.08)	(0.05)	(0.05)	(0.18)	(0.04)
α_{19}	0.99***	0.31***	1.13***	-0.63***	-0.96***	-0.63***
	(0.14)	(0.09)	(0.14)	(0.06)	(0.24)	(0.05)
α_{20}	0.81***	0.21	0.97***	-0.59***	-0.40	-0.62***
	(0.21)	(0.14)	(0.24)	(0.04)	(0.28)	(0.04)
α_{21}	0.66***	0.10	0.81***	-0.63***	0.22	-0.67***
	(0.22)	(0.32)	(0.21)	(0.06)	(0.16)	(0.04)
α_{22}	0.51***	0.08	0.64***	-0.55***	-0.31***	-0.62***
	(0.05)	(0.07)	(0.03)	(0.02)	(0.10)	(0.03)
α_{23}	0.63***	-0.12	0.78***	-0.53***	-0.31**	-0.58***
	(0.05)	(0.29)	(0.03)	(0.04)	(0.15)	(0.05)
α_{24}	0.61***	0.12	0.75***	-0.55***	-0.92***	-0.57***
	(0.04)	(0.15)	(0.03)	(0.04)	(0.06)	(0.05)
α_{25}	0.62***	-0.14	0.79***	-0.53***	-0.26	-0.57***
	(0.04)	(0.25)	(0.03)	(0.02)	(0.19)	(0.03)
α_{26}	0.47***	0.20	0.65***	-0.66***	-0.84***	-0.70***
	(0.05)	(0.19)	(0.06)	(0.03)	(0.06)	(0.03)
α_{27}	0.54***	0.02	0.73***	-0.58***	-0.43***	-0.63***
	(0.07)	(0.19)	(0.07)	(0.03)	(0.14)	(0.02)
α_{28}	0.35***	-0.12*	0.54***	-0.69***	-0.91***	-0.73***
	(0.05)	(0.07)	(0.05)	(0.02)	(0.22)	(0.02)
α_{29}	0.39***	0.24**	0.54***	-0.48***	-0.13	-0.54***
	(0.06)	(0.11)	(0.07)	(0.06)	(0.11)	(0.08)
α_{30}	0.35***	-0.19**	0.53***	-0.48***	-0.65*	-0.52***
	(0.06)	(0.09)	(0.05)	(0.04)	(0.34)	(0.04)
α_{31}	0.42***	0.07	0.60***	-0.61***	-0.29	-0.68***
	(0.08)	(0.30)	(0.09)	(0.05)	(0.31)	(0.05)
α_{32}	0.34***	-0.10	0.50***	-0.53***	-0.81***	-0.55***
	(0.13)	(0.13)	(0.15)	(0.07)	(0.21)	(0.09)
α_{33}	0.49***	0.18	0.66***	-0.73***	-1.14***	-0.77***
	(0.08)	(0.19)	(0.08)	(0.09)	(0.22)	(0.08)
α_{34}	0.56***	0.69***	0.66***	-0.56***	-0.94***	-0.58***
	(0.07)	(0.15)	(0.05)	(0.05)	(0.14)	(0.04)
α_{35}	0.32***	-0.02	0.48***	-0.46***	0.24	-0.54***
	(0.05)	(0.15)	(0.05)	(0.11)	(0.25)	(0.10)

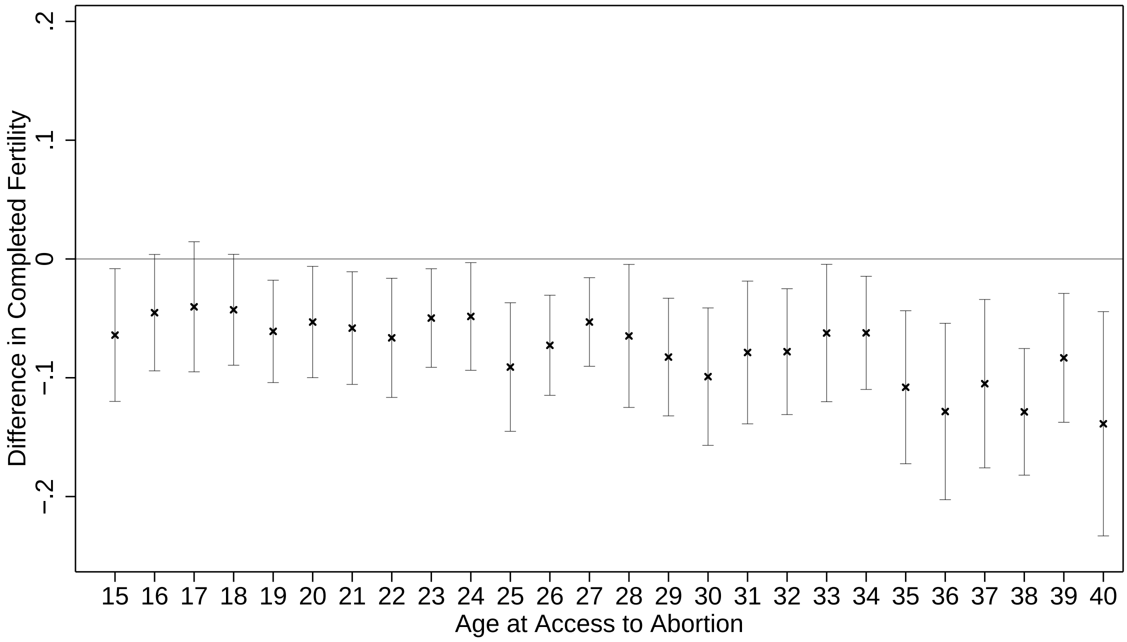
Note: All regressions have been weighted by population weights.
Clustered robust standard errors are reported in parenthesis.
* – $p < 0.1$; ** – $p < 0.05$; *** – $p < 0.01$.

Table 14: Difference-in-Differences Estimates of the Effect of Abortion Access on Births Timings

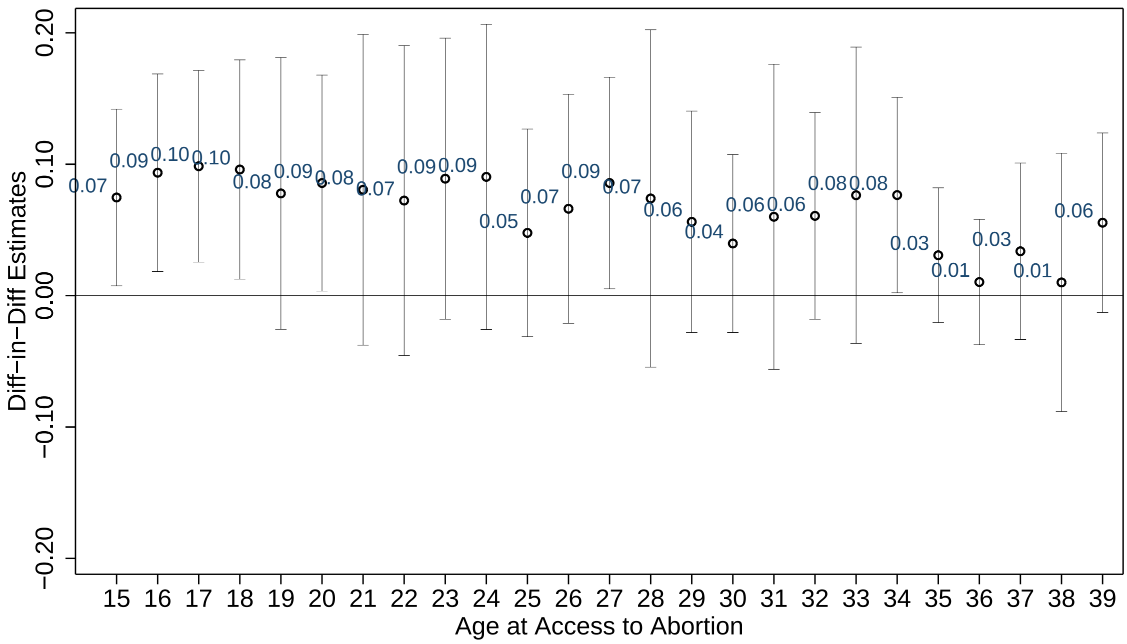
	Age at First Child			*Birth Spacing*		
	Full Sample	Black Women	White Women	Full Sample	Black Women	White Women
γ_{15}	0.65***	1.01***	0.61***	-0.23*	-1.07***	-0.15
	(0.13)	(0.20)	(0.10)	(0.14)	(0.30)	(0.12)
γ_{16}	0.66***	0.49***	0.67***	-0.06	-0.52	-0.03
	(0.10)	(0.15)	(0.08)	(0.13)	(0.55)	(0.09)
γ_{17}	0.66***	1.09***	0.60***	-0.10	-0.86***	-0.05
	(0.07)	(0.30)	(0.07)	(0.11)	(0.29)	(0.10)
γ_{18}	0.62***	0.32*	0.58***	-0.15	-0.88***	-0.09
	(0.08)	(0.18)	(0.08)	(0.11)	(0.31)	(0.11)
γ_{19}	0.67***	0.33*	0.65***	-0.17	-1.21***	-0.09
	(0.15)	(0.18)	(0.18)	(0.10)	(0.35)	(0.10)
γ_{20}	0.49**	0.23	0.49*	-0.13	-0.65	-0.08
	(0.21)	(0.20)	(0.27)	(0.11)	(0.39)	(0.09)
γ_{21}	0.34	0.12	0.33	-0.16	-0.03	-0.12
	(0.22)	(0.41)	(0.24)	(0.10)	(0.35)	(0.09)
γ_{22}	0.19**	0.10	0.16**	-0.08	-0.55**	-0.08
	(0.07)	(0.16)	(0.08)	(0.13)	(0.26)	(0.12)
γ_{23}	0.31***	-0.10	0.30***	-0.07	-0.56*	-0.04
	(0.08)	(0.35)	(0.07)	(0.13)	(0.29)	(0.12)
γ_{24}	0.29***	0.14	0.27***	-0.08	-1.17***	-0.03
	(0.07)	(0.24)	(0.07)	(0.14)	(0.26)	(0.13)
γ_{25}	0.30***	-0.12	0.31***	-0.07	-0.51	-0.03
	(0.07)	(0.27)	(0.07)	(0.12)	(0.31)	(0.12)
γ_{26}	0.14	0.22	0.17*	-0.19	-1.09***	-0.15
	(0.09)	(0.19)	(0.10)	(0.13)	(0.26)	(0.12)
γ_{27}	0.21**	0.04	0.25**	-0.12	-0.68**	-0.08
	(0.10)	(0.23)	(0.12)	(0.10)	(0.28)	(0.10)
γ_{28}	0.03	-0.10	0.07	-0.23*	-1.15***	-0.19
	(0.07)	(0.16)	(0.08)	(0.12)	(0.36)	(0.11)
γ_{29}	0.06	0.26	0.06	-0.01	-0.11	0.00
	(0.08)	0.16)	(0.06)	(0.15)	(0.28)	(0.15)
γ_{30}	0.02	-0.17	0.05	-0.01	-0.89**	0.02
	(0.09)	(0.18)	(0.09)	(0.12)	(0.37)	(0.13)
γ_{31}	0.09	0.09	0.12	-0.15	-0.54	-0.14
	(0.10)	(0.33)	(0.08)	(0.13)	(0.44)	(0.12)
γ_{32}	0.02	-0.07	0.02	-0.07	-1.06***	-0.01
	(0.14)	(0.19)	(0.13)	(0.08)	(0.34)	(0.07)
γ_{33}	0.16*	0.21	0.18***	-0.27***	-1.38***	-0.22***
	(0.09)	(0.23)	(0.07)	(0.06)	(0.27)	(0.05)
γ_{34}	0.24***	0.71***	0.18***	-0.10	-1.19***	-0.05
	(0.09)	(0.21)	(0.06)	(0.13)	(0.32)	(0.10)

Note: All regressions have been weighted by population weights.
Clustered robust standard errors are reported in parenthesis.
* – $p < 0.1$; ** – $p < 0.05$; *** – $p < 0.01$.

Figure 9: Effect of Access to Abortion on Completed Fertility

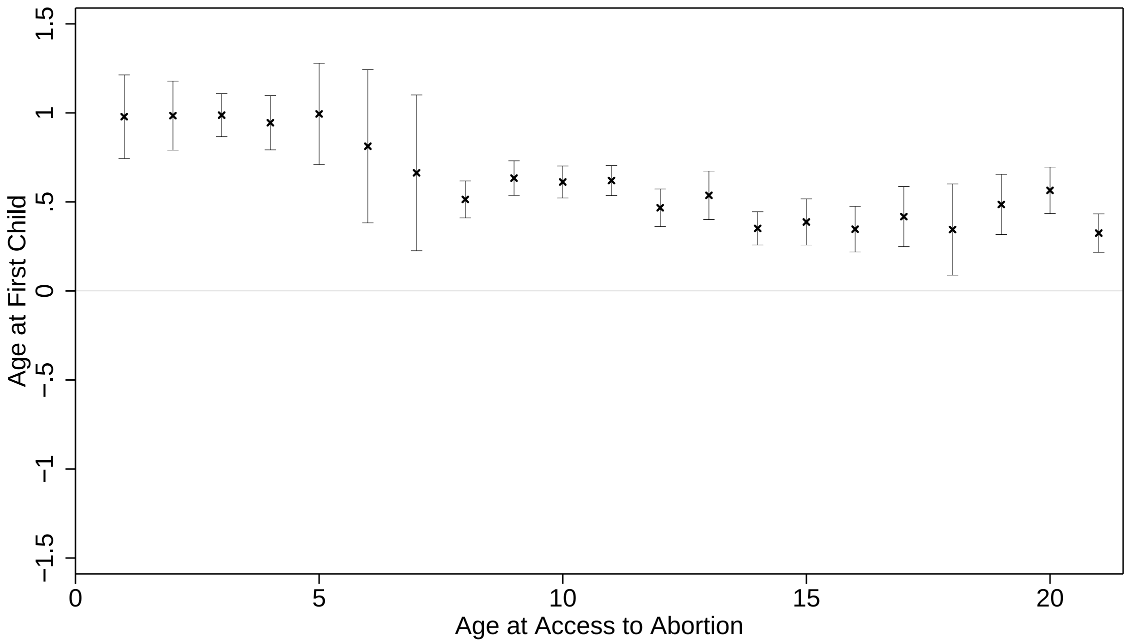


(a) Completed Fertility Trend by Year of Age

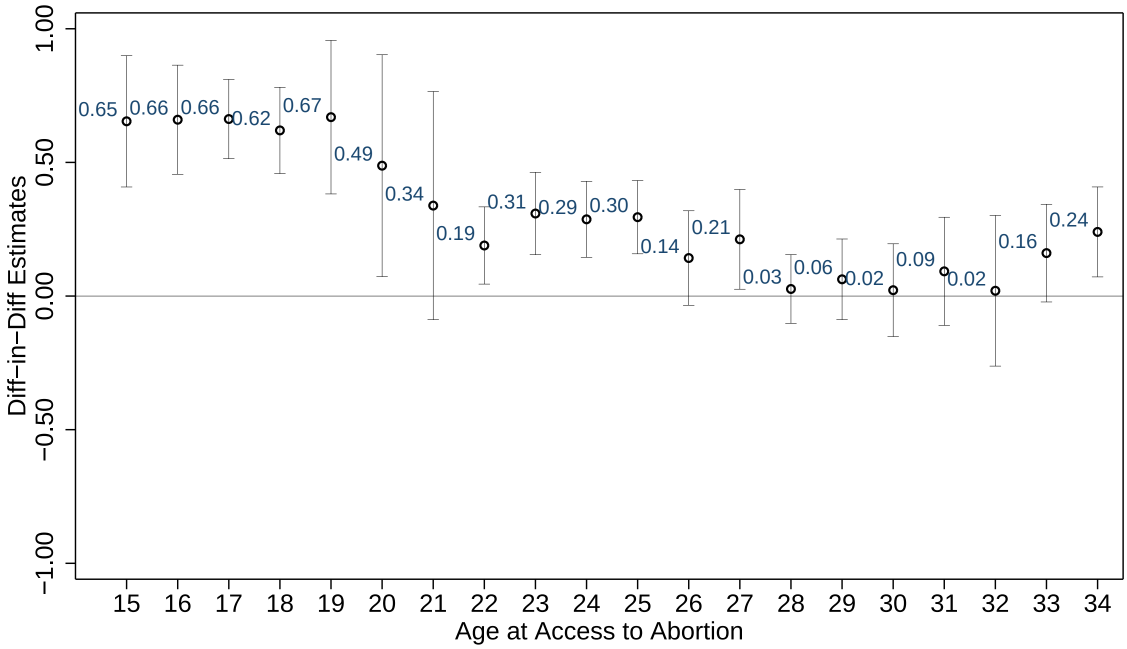


(b) Estimates of the effect of abortion access on Completed Fertility - Year of Age

Figure 10: Effect of Access to Abortion on Age at Start of Motherhood

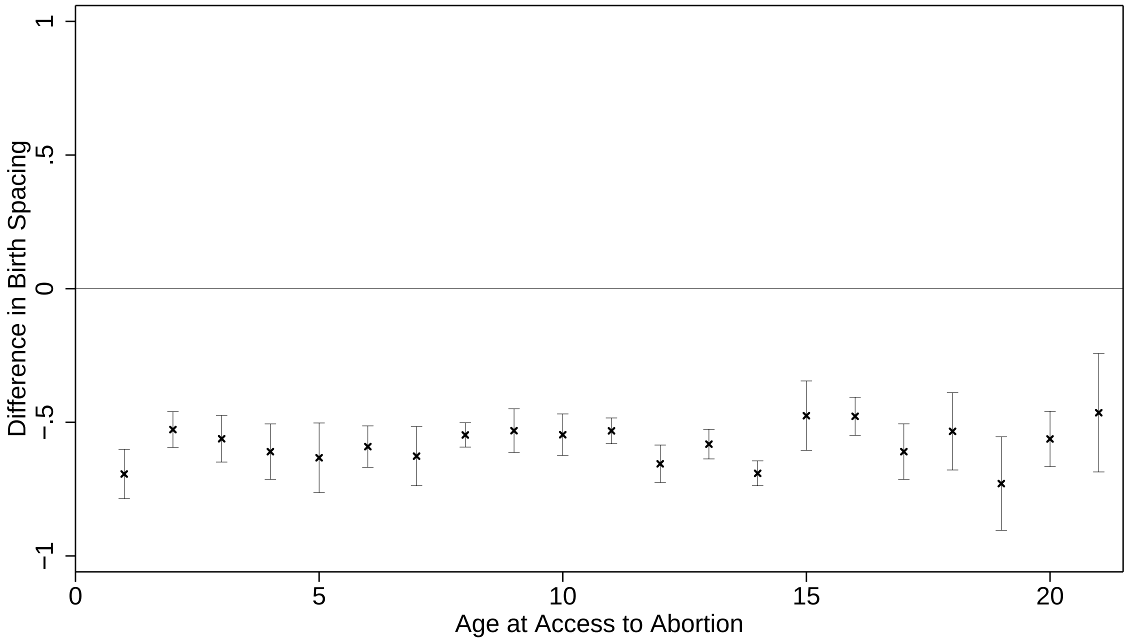


(a) Age at Birth of First Child Trend by Year of Age

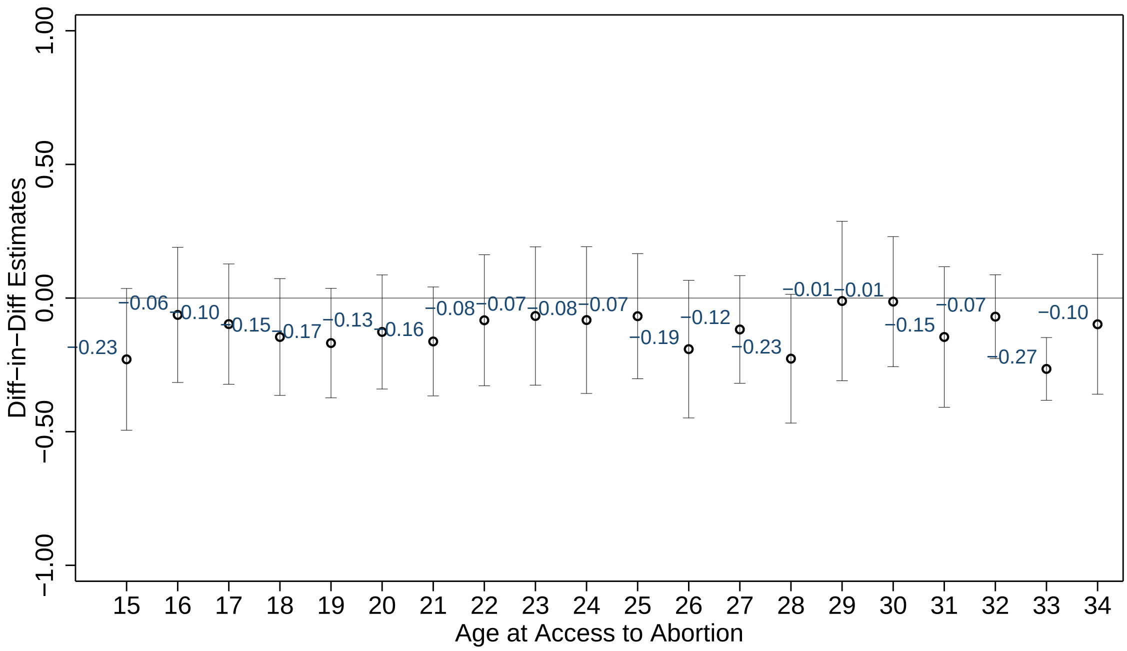


(b) Estimates of the effect of abortion access on Age at Birth of First Child - Year of Age

Figure 11: Effect of Access to Abortion on Birth Spacing



(a) Birth Spacing Trend by Year of Age

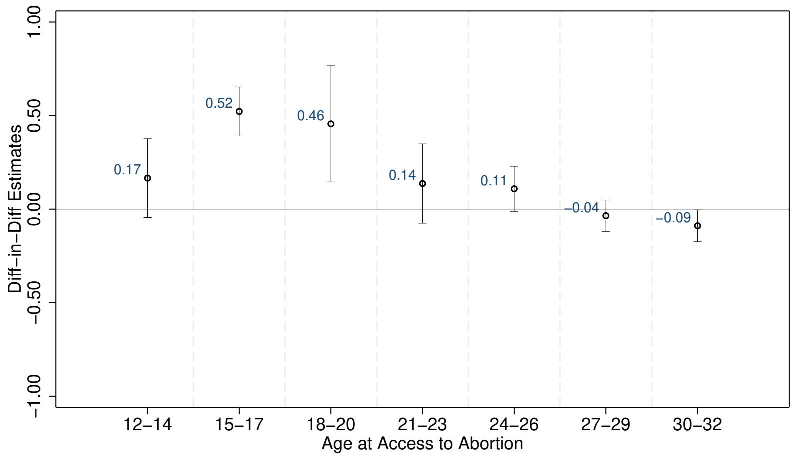


(b) Estimates of the effect of abortion access on Births Spacing - Year of Age

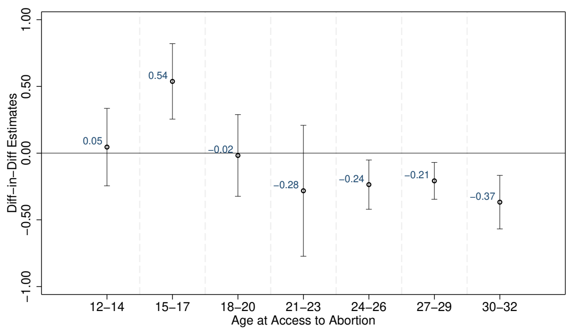
B.2 Placebo Test: Ruling Out Changes in Fertility Trends

I extend the sample to include women born between 1956 and 1958. For these birth cohorts there is no difference in exposure to abortion as they were between the ages of 12 and 14 at the time of state legalization, and they all receive equal access to abortion at the beginning of their fertility cycle which happens post Roe v. Wade. The plots below report the difference-in-differences estimates obtained by estimating equation (2) for the extended sample.

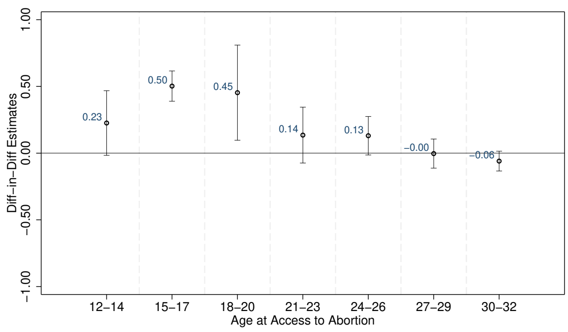
Figure 12: Effect of Abortion Access on Age of Motherhood



(a) Full Sample



(b) Black Women



(c) White Women

C Robustness to Potential Migration

C.1 Estimation Using Potential Non-Movers

In addition to state of birth, the IPUMS include the state of residency of women at the time of observation. This additional information does not provide full information about the state of residency of women during their fertility cycle. If a woman at the time of observation is observed in the same state as her birth state then she is assumed to have been living in her birth state during her fertility cycle. I then estimate equation (2) using the subsample of potential non-movers. Estimation results for the three main fertility outcomes are reported below. Comparing these results with the results found in the main estimation, I find that the direction of the effects are preserved with slight change in the magnitude and precision of the estimates.

Table 15: Cross States Differences in Completed Fertility (Potential Non-Movers)

	Mean	α_1 [15-17]	α_2 [18-20]	α_3 [21-23]	α_4 [24-26]	α_5 [27-29]	α_6 [30-32]	α_7 [33-35]	α_8 [36-38]	α_9 [39-40]
Full Sample										
Completed Fertility	2.45	-0.04*	-0.06**	-0.07***	-0.10***	-0.08***	-0.09***	-0.09***	-0.13***	-0.10*
		(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.04)	(0.06)
			N=1,082,124							
Black Women										
Completed Fertility	2.91	-0.52***	-0.47***	-0.60***	-0.71***	-0.87***	-1.01***	-1.23***	-1.29***	-1.19***
		(0.04)	(0.03)	(0.03)	(0.03)	(0.09)	(0.11)	(0.06)	(0.10)	(0.07)
			N=109,018							
White Women										
Completed Fertility	2.38	0.05***	0.03*	0.02	-0.02	0.02	0.03	0.05**	0.01	0.01
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)
			N=941,926							

Table 16: Difference-in-Differences Estimates of the Effect of Abortion Access on Completed Fertility (Potential Non-Movers)

	Mean	γ_1 [15-17]	γ_2 [18-20]	γ_3 [21-23]	γ_4 [24-26]	γ_5 [27-29]	γ_6 [30-32]	γ_7 [33-35]	γ_8 [36-38]
Full Sample									
Completed Fertility	2.45	0.05	0.04	0.02	-0.00	0.02	0.01	0.01	-0.03
		(0.05)	(0.06)	(0.06)	(0.07)	(0.05)	(0.05)	(0.04)	(0.02)
Black Women									
Completed Fertility	2.91	0.68***	0.73***	0.60***	0.49***	0.32***	0.18*	-0.04	-0.09
		(0.10)	(0.09)	(0.08)	(0.07)	(0.10)	(0.11)	(0.06)	(0.09)
White Women									
Completed Fertility	2.38	0.04	0.01	0.01	-0.03	0.01	0.02	0.04	0.00
		(0.04)	(0.05)	(0.05)	(0.06)	(0.04)	(0.04)	(0.02)	(0.01)

Table 17: Cross States Differences in Births Timing (Potential Non-Movers)

	Mean	α_1 [15-17]	α_2 [18-20]	α_3 [21-23]	α_4 [24-26]	α_5 [27-29]	α_6 [30-32]	α_7 [33-35]
Full Sample								
Age at First Child	23.72	1.20***	1.10***	0.72***	0.69***	0.53***	0.46***	0.63***
		(0.07)	(0.21)	(0.07)	(0.03)	(0.06)	(0.12)	(0.11)
					N=508,242			
Birth Spacing	3.76	-0.71***	-0.72***	-0.65***	-0.65***	-0.67***	-0.66***	-0.79***
		(0.04)	(0.05)	(0.02)	(0.03)	(0.04)	(0.03)	(0.07)
					N=223,340			
Black Women								
Age at First Child	22.08	1.38***	0.53**	0.46***	0.54***	0.47***	0.47***	0.94***
		(0.08)	(0.21)	(0.14)	(0.13)	(0.16)	(0.15)	(0.14)
					N=49,188			
Birth Spacing	4.71	-0.62***	-0.66**	0.01	-0.65***	-0.16	-0.55***	-0.61***
		(0.14)	(0.07)	(0.11)	(0.16)	(0.11)	(0.12)	(0.15)
					N=16,516			
White Women								
Age at First Child	23.90	1.26***	1.20***	0.78***	0.77***	0.61***	0.53***	0.67***
		(0.07)	(0.23)	(0.06)	(0.04)	(0.06)	(0.14)	(0.12)
					N=448,915			
Birth Spacing	3.67	-0.73***	-0.74***	-0.71***	-0.68***	-0.72***	-0.69***	-0.82***
		(0.03)	(0.04)	(0.03)	(0.04)	(0.04)	(0.02)	(0.08)
					N=202,959			

Table 18: Difference-in-Differences Estimates of the Effect of Abortion Access on Births Timing (Potential Non-Movers)

	Mean	γ_1 [15-17]	γ_2 [18-20]	γ_3 [21-23]	γ_4 [24-26]	γ_5 [27-29]	γ_6 [30-32]
Full Sample							
Age at First Child	23.72	0.57***	0.47	0.09	0.06	-0.10*	-0.17***
		(0.15)	(0.32)	(0.18)	(0.13)	(0.06)	(0.04)
Birth Spacing	3.76	0.08	0.07	0.14	0.13	0.11	0.13
		(0.08)	(0.07)	(0.09)	(0.09)	(0.10)	(0.08)
Black Women							
Age at First Child	22.08	0.44***	-0.41	-0.48*	-0.40***	-0.47***	-0.47***
		(0.13)	(0.33)	(0.26)	(0.12)	(0.10)	(0.09)
Birth Spacing	4.71	-0.01	-0.05	0.62***	-0.04	0.45**	0.06
		(0.24)	(0.18)	(0.20)	(0.20)	(0.20)	(0.20)
White Women							
Age at First Child	23.90	0.59***	0.53	0.11	0.10	-0.06	-0.14***
		(0.18)	(0.35)	(0.18)	(0.16)	(0.07)	(0.04)
Birth Spacing	3.67	0.09	0.09	0.11	0.14	0.10	0.13
		(0.08)	(0.07)	(0.10)	(0.11)	(0.12)	(0.08)

C.2 Evidence on Selective migration

Furthermore I test if there was any selective migration among women of different age group to and from repeal states. The 1970 sample of the IPUMS report the state of residency in 1970 as well as the state of residency in 1965. This allow me to construct two migration dummy variables. The first variable $M^{R}_{i(b)}$ takes value 1 in case individual i of birth cohort b migrated from a non-repeal to a repeal state and 0 otherwise. The second migration variable $M^{NR}_{i(b)}$ takes value 1 if individual i of birth cohort b migrated from a repeal to a non-repeal and 0 otherwise. If there is any selective migration conditional on the state abortion legality status, this migration should not affect men. Therefore I estimate the following equation for each of the migration outcomes

$$M_{i(b)} = \sum_{b=1930}^{1955} \alpha_b BC_{i(b)} + \sum_{b=1930}^{1955} \beta_b Female_i * BC_{i(b)}$$

where BC_i is a birth cohort indicator and $Female_i$ is a gender indicator of individual i. In case of positive selective migration among women of certain birth cohorts, β coefficients of these cohorts should be positive and significant. Estimation results are reported in the table below for both migration outcomes. No serious selective migration is detected. Except for the 1946 to 1951 birth cohorts were female were found to be significantly less likely to migrate from non-repeal to repeal states. These birth cohorts are of college attendance age in 1970, this suggest that men from non-repeal state were much more likely to attend colleges in repeal states compared to their state birth cohort peer women.

Table 19: Evidence on Selective Migration

Birth Cohort	*Migration to Repeal States*	*Migration to Non-Repeal States*
1955	-0.000	-0.001
	(0.001)	(0.001)
1954	-0.001	-0.002*
	(0.001)	(0.001)
1953	0.000	-0.002
	(0.001)	(0.001)
1952	0.000	0.001
	(0.002)	(0.001)
1951	-0.007***	-0.002
	(0.002)	(0.001)
1950	-0.018***	-0.008
	(0.004)	(0.009)
1949	-0.026***	-0.015
	(0.005)	(0.013)
1948	-0.020***	-0.015
	(0.010)	(0.010)
1947	-0.011***	-0.012
	(0.003)	(0.007)
1946	-0.005**	-0.010
	(0.003)	(0.006)
1945	-0.002	-0.011
	(0.003)	(0.009)
1944	-0.000	-0.013
	(0.003)	(0.008)
1943	-0.011***	-0.014*
	(0.003)	(0.008)
1942	-0.007**	-0.003
	(0.003)	(0.004)
1941	-0.005*	-0.008**
	(0.003)	(0.004)
1940	-0.011***	-0.004
	(0.003)	(0.005)
1939	-0.006**	-0.003**
	(0.003)	(0.001)
1938	-0.007**	-0.004
	(0.003)	(0.003)
1937	-0.003	-0.002**
	(0.002)	(0.001)
1936	-0.005**	-0.004**
	(0.002)	(0.002)
1935	-0.004*	-0.009*
	(0.002)	(0.005)
1934	-0.005**	-0.009*
	(0.002)	(0.005)
1933	-0.005**	-0.004
	(0.002)	(0.004)
1932	-0.003	-0.010*
	(0.002)	(0.005)
1931	-0.006***	-0.008*
	(0.002)	(0.004)
1930	-0.005**	-0.009*
	(0.002)	(0.004)