The Impact of Legal Abortion on the Wage Distribution: Evidence from the 1970 New York Abortion Reform

Dana Rotz[1]

Mathematica Policy Research

March 2013

Abstract

In 1970, New York became the first state to allow all women legal access to abortion. I determine how this change impacted the characteristics of mothers giving birth in New York, their newborns, and these children's future wages. Using natality data and a regression discontinuity design, I demonstrate that after abortion's legalization, children were born into families with greater resources. I then analyze the eventual wages of adults born in New York around the time of abortion's legalization using the American Community Surveys. This analysis demonstrates that the legalization of abortion increased the eventual wages of minorities and lower-wage workers.

[1]Email: drotz@mathematica-mpr.com. I would like to thank Roland Fryer, Claudia Goldin, and Larry Katz for continued guidance and support on this project, as well as Martha Bailey, Timothy Bond, Phillip Levine, Jeff Liebman, and seminar participants at Harvard University and the University of Michigan for helpful comments and discussions. I am also grateful to Larry Katz and Phillip Levine for providing data on oral contraceptive pill access and abortion rates respectively. All remaining errors are my own. This research has been supported by the NSF-IGERT program, "Multidisciplinary Program in Inequality and Social Policy" at Harvard University (Grant No. 0333403). The views expressed herein are those of the author and not necessarily those of Mathematica Policy Research.

1 Introduction

On July 1, 1970, New York became the first state in the US to allow women legal access to abortion on demand. In this paper, I examine the extent to which this major change in reproductive rights impacted the characteristics of mothers giving birth in New York, their newborns, and these children's future wages. If mothers used abortion to more optimally time their births, the group of women delivering babies after abortion became legal will be positively selected. This selection could then lead to improved welfare among cohorts in adulthood.

To demonstrate that abortion access led to a sharp change in the characteristics of pregnancies carried to term, I first analyze natality data from the National Center for Health Statistics. This dataset contains information on date of birth and gestational duration, allowing me to determine the end of a woman's first trimester of pregnancy. If a woman ended her first trimester after New York's reform, I assume she had access to legal abortion. This analysis uses a regression discontinuity design and provides estimates indicating that access to abortion led to smaller cohorts characterized by proportionately fewer low-weight and African American births, mothers with more education, and increased data on paternal characteristics (a proxy for father's involvement). These changes suggest that mother's access to abortion impacted childhood living conditions.

To examine if changes in the characteristics of mothers and births yielded changes in long-run outcomes, I use wage data from native-born New Yorkers in the 2005 to 2010 American Community Surveys (ACS). Although this data does not allow one to calculate precise dates of conception or birth, I can use age (in whole years), survey year, and quarter of birth to estimate mother's abortion access. I thus compare the wages of individuals reporting the same age in the same survey year, but with different quarters of birth, implying that their mothers were in different stages of pregnancy when abortion was legalized in New York. Assuming that quarter of birth influences wages in the same way across age-survey year pairs, this procedure estimates the effect of abortion's legalization on the eventual wages of children carried to term.[2]

[2]See Buckles and Hungerman (2010) on the relationship between birth quarter and socioeconomic characteristics.

Using the constructed likelihood of mother's abortion access, I find that the wages of native-born New Yorkers whose mothers had access to legal abortion are higher than the wages of those in utero when abortion was illegal. In particular, mother's abortion access is associated with higher wages for black and Hispanic workers. Further, mother's access implies increases in the bottom quantiles of the wage distribution. Access does not imply large or significant changes in the upper-half of the wage distribution or the wages of whites. Together with the results from the natality data, these findings imply that abortion access quickly changed the selection of women into motherhood and thus the characteristics of birth cohorts.

Although a comparison of adults born within a few months of each other implies substantial wage gains associated with abortion access, comparing more broadly spaced cohorts yields smaller, but still significant, effects of abortion. Given time, women could react to abortion's legalization by altering their contraceptive behavior. Social norms may also slowly change in response to abortion reform (c.f., Akerlof, Yellen, and Katz 1996). Neither change completely diminishes the impact of abortion on cohort outcomes, suggesting potentially important medium- and long-term effects of abortion's legality.

Beginning with Donohue and Levitt (2001), economists have debated the relationship between abortion and crime.[3] Additional work has focused on the impact of abortion on child and young-adult outcomes.[4] This work adds to the literature on abortion's effect on cohort-level characteristics in several ways. First, past work does not consider how abortion reform shaped cohorts at later ages.[5] By analyzing cohorts in adulthood, I can determine how abortion's legalization affected cohorts in the long run.

Additionally, most past analyses of the impact of abortion legalization compare children born in states that first legalized abortion to those born in states where the Supreme Court's ruling on *Roe v. Wade* later gave women access (c.f., Gruber, et al.1999). But the states that legalized abortion

[3]For details on this debate, see Charles and Stephens (2006), Donohue and Levitt (2001, 2004a, 2004b, 2008), Dills and Miron (2006), Dills, Miron, and Summers (2008), Foote and Goetz (2008), and Joyce (2004a, 2004b, 2009, 2010).

[4]See Ananat, et al. (2010), Gruber, Levine, and Staiger (1999), and Pop-Eleches (2006).

[5]Of course, cohorts exposed to legal abortion in the United States are only now of an age where wages represent a reasonable proxy for permanent income.

before *Roe v. Wade* are non-random and non-representative of America.[6] The comparison might also misrepresent the effect of abortion by assuming that women had access only if they lived in a state where abortion was legal. Indeed, many women from other states gained access to abortion by traveling to New York.[7] By using only native New Yorkers in my analysis, I can be agnostic about geographic variation in abortion access.

Finally, the literature on abortion generally focuses on the average marginal effect of abortion access; however, evidence on the illegal and legal use of abortion suggests that the effect of reform may vary with race and socioeconomic status. To more completely consider the relationship between abortion and birth cohort outcomes, I use quantile regressions, analyzing changes throughout the wage distribution. I also examine abortion's effect within racial groups.

I begin my analysis by reviewing the history of abortion in New York and explaining why this state provides an ideal natural experiment to study the effects of abortion legalization. Next, I briefly outline why one might expect a relationship between abortion access and cohort characteristics. Analysis of both birth certificate and wage data then suggests that the composition of expectant mothers, and thus the characteristics of children carried to term, changed when abortion became legal. Finally, I demonstrate that abortion reform in New York also caused increases in the wages of those born elsewhere in the United States.

2 History of Abortion in New York

Throughout history, many different laws and norms allowed a woman to limit her fertility after conception. In early America, English common law permitted abortion prior to a fetus's "quickening" (approximately the end of a woman's first trimester).[8] Most scholars indicate that midwives

[6] Alaska, California, Hawaii, New York, and Washington legalized abortion on demand in 1970; abortion was legalized in all other states in 1973.

Other analyses instead compare the outcomes of those born in states with higher or lower take-up of abortion, potentially leading to endogeneity and omitted variable biases (c.f., Donohue and Levitt 2001).

[7] See Joyce, Tan, and Zhang (2012) and Levine, et al. (1999).

[8] Even if abortion was illegal, no conclusive test for pregnancy before quickening existed until 1926, making many abortifacients *de facto* legal. For details, see Dellapenna (2006).

traditionally used many techniques to terminate unwanted pregnancies.[9] Despite these practices, New York became the first state to outlaw abortion in 1826 (David, et al. 1988). By 1900, the rest of America had followed.

Limited evidence exists on the nature or number of illegal abortions during the period of restriction. Lee (1969) provides a rare glimpse into illegal abortion in an interview study of 114 women who purposefully terminated a pregnancy in the 1950s or 1960s, before any state legalized abortion on demand. The women in the sample received abortions from a variety of different practitioners, including licensed doctors in the US and abroad, doctors whose licenses had been revoked, dentists, nurses, and a chiropractor. The study indicates that although illegal abortions were obtainable (particularly for wealthy or highly educated women), the procedures were both dangerous and costly. On average, the women in Lee's study paid $488 for an abortion. Over one-third of them traveled more than 250 miles. Moreover, almost 20 percent of the women in the study experienced a medical crisis after their procedure. Popular publications further indicate that some abortionists robbed or sexually abused their clients (c.f., Reuben 1969).

The nature of abortion in America changed on April 11, 1970, when New York became the first state to allow women legal access to abortion on demand up to the 24th week of pregnancy (see Garrow 1998).[10] Infrastructure in public and private hospitals and clinics quickly increased to meet the demand for abortion. On July 1, 1970, doctors performed the first legal procedures.

Both national and local media coverage ensured that women all over the United States knew they could come to New York to have an abortion. The reform received much attention in the *New York Times* and in nationally circulated women's magazines (e.g., *Cosmopolitan*). To inform women of their new options, organizations handed out pamphlets with titles such as "If You Want

[9] Common techniques include the administration of abortifacients and injury of the mother (Dellapenna 2006). See Garrow (1998) for details on the majority opinion for *Roe v. Wade* and its historical discussion of abortion prior to quickening. Dellapenna (2006) provides an alternate historical analysis.

[10] Formally, the law stated that "An abortional act is justifiable when committed upon a female with her consent by a duly licensed physician acting (a) under reasonable belief that such is necessary to preserve her life, or (b) within 24 weeks of the commencement of her pregnancy" (Planned Parenthood 1972, p. 1). Although the gestational limitation of abortion was extended to 24 weeks to make the bill less appealing to swing voters, the New York legislature passed the bill by a single vote when Representative Michaels declared: "I realize, Mr. Speaker, that I am terminating my political career, but I cannot in good conscience sit here and allow my vote to be the one that defeats this bill. I ask that my vote be changed from 'no' to 'yes'" (Garrow, 1998 p. 420).

an Abortion..." both in New York and out-of-state (Brody 1970, *New York Times* 1970). Advertisements for abortion referral services and packages ranged from informative ads in national magazines warning women of profit-seeking doctors to a "banner drawn by a blimp flying over a beach in Miami" (Edmiston 1971, p. 42).[11]

The total cost of an abortion declined rapidly after legalization, particularly for poorer and less-educated women. The cost of abortion during one's first trimester ranged from zero to 200 dollars in most New York hospitals and clinics, with fee reductions based on income.[12] Early legal abortions were also exceptionally safe. By the second half of 1971, the complication rate had dropped to 0.57 percent, from an initial 1.27 percent in 1970 (Planned Parenthood 1972). Maternal mortality in New York also decreased by 40 percent from 1970 to 1971 (Pakter and Nelson 1971).

Figure 1 shows the number of abortions performed in New York City over time. The trend suggests that the news coverage, easy access, and low costs of abortion culminated in a surge in the number of procedures performed.[13] By 1971, over 200,000 pregnancies were aborted in New York City alone (Pakter and Nelson 1971). Work by Levine, et al. (1999) suggests that these abortions implied small but non-trivial changes in fertility.

The characteristics of women receiving abortions in the year following New York's legalization suggest that women used abortion to better-time their pregnancies and prevent pregnancies later in life. Pregnant women over 35 and under 20 utilized abortion most heavily, respectively comprising 10 and 25 percent of the women receiving abortions (Planned Parenthood 1972). Women who

[11] An example of the former appears in *Ms. Magazine*'s November 1972 issue. It states "The main difference between a $150 abortion and a $1000 abortion is that the doctor makes an extra $850" and lists contact information for a free referral service.

[12] Medicaid covered 84% of first-trimester abortions in New York City's municipal hospitals from July 1, 1970 to March 31, 1971.

Second-trimester abortions were less common and more costly, ranging from $350 to $450 (Pakter and Nelson 1971).

[13] This figure also contains the total number of legal, illegal, and spontaneous abortions by year in New York City. Spontaneous abortions (miscarriages) make up the bulk of such procedures prior to 1970 and are included in the figure if reported to the Department of Health by a hospital. If a woman miscarried at home, the lost pregnancy would not be reflected in this figure. Although detailed records are not available, work by Lee (1969) suggests that many recorded miscarriages were actually illegal abortions. Many abortionists practiced catheterization, a procedure that began with the abortionist but was completed in a hospital and looked like a miscarriage. Additionally, many women had to be hospitalized after having an illegal abortion. If the abortion was incomplete at the time of hospitalization, but it was not completely evident that the woman had purposely induced the abortion, such an event would be counted as a spontaneous (and not illegal) abortion.

sought abortions were also disproportionately African American. Although black women gave birth to 31.2 percent of children in New York City in 1971, they had 42.8 percent of all abortions in the first year of legality (563 abortions per 1,000 live births). Hispanic women had far fewer abortions in the same period (only 250 per 1,000 live births), likely because of their Catholic roots. White women had a rate in between (Pakter, et al. 1973).[14]

During the first year of legalization, most women seeking abortions in New York City were not city residents. Figure 2 demonstrates the rate at which women in other states received abortions in New York.[15] More than 0.5 percent of women age 15 to 44 living in New Jersey, Connecticut, Vermont, Massachusetts, Rhode Island, and New Hampshire traveled to New York from July 1, 1970 to June 30, 1971 to receive a legal abortion. The trend in Figure 1 also suggests the strong out-of-state demand for abortion; the number of abortions in New York declined in 1973, when the Supreme Court's ruling on *Roe v. Wade* allowed women to obtain procedures closer to home. These facts suggest that reform in New York led to major changes in access to abortion for women across the United States.

3 Abortion, Selection, and Cohort Characteristics

A simple conceptual model demonstrates how an increase in abortion access could lead to changes in cohort characteristics. Abortion legalization acts to decrease the effective price of abortion and leads fewer pregnant women to choose to carry their pregnancies to term. Women will also respond to a drop in the cost of abortion by decreasing spending on contraception. In essence, when abortion becomes legal, more women with relatively low expected utility from becoming pregnant will conceive and fewer women with low expected utility from having a child will give birth. If the utility a woman receives from her children correlates with child traits, the average characteristics of cohorts will then change in response to changes in abortion law. Moreover, if abortion reform implies different changes in the costs of abortion for different demographic

[14] According to Jones and Kooistra (2011), similar abortion rates prevailed in New York during the 2000s.

[15] See Joyce, et al. (2012) for further details.

groups, the composition of the population will shift as fertility rates change differentially across these groups.

Although changes in selection into pregnancy and motherhood are likely the main mechanisms behind abortion's effect on eventual wages, other forces could also lead abortion legalization to impact wages. Donohue and Levitt (2001) suggest that decreases in cohort size could imply changes in outcomes. Additionally, Akerlof, et al. (1996) suggest that abortion access may have led to important changes in social norms or marriage markets, which could also differentially affect the children of mothers who had access to legal abortion.[16] The data needed to address if such changes occurred quickly enough to be relevant to this study does not exist; however, these mechanisms are probably not highly relevant to identification strategies, like mine, that compare workers born within months of each other.

4 Immediate Effects of Abortion Legalization

In my first strategy, I use the National Center for Health Statistics Vital Statistics Natality Birth Data (NCHS) to determine how mother's abortion access changed the size of cohorts and the attributes of mothers and their newborns.[17] The NCHS contains information on both date of birth and weeks of gestation, allowing one to closely estimate the week a woman became pregnant, and thus the final week of her first trimester. If the mother of a baby completed her first-trimester after July 1, 1970 (when the New York law went into affect), I consider her to have had access to legal abortion ($abort\_access$). I similarly determine mother's access to oral contraceptives at conception ($pill\_access$). I select all births from this dataset where a woman became pregnant between April 1968 and March 1972.

First, I use a differences-in-differences approach to determine the effect of mother's access to

[16]Also, see Goldin and Katz (2002) on how giving better reproductive control to a limited group of women may have important spillover effects.

[17]See Appendix A.2 for details.

abortion, estimating regressions such as

$$Y_{iym} = \alpha_y + \gamma_m + \beta_B(abort\_access_{iym}) + \phi_B(pill\_access_{iym}) + \varepsilon_{iym}, \tag{1}$$

where Y is a given outcome at birth and α and γ are year and month of conception fixed-effects. I then use even finer variation in the date of conception in a regression discontinuity (RD) design. Because this technique uses very closely spaced births, it has the added advantage of estimating a relatively pure selection effect of abortion. Changes in cohort size should not influence characteristics at birth. Moreover, social norms and contraceptive use likely did not change over the extremely short horizon used in this analysis.

My RD design compares the outcomes of eventual births to mothers in their 10th week of pregnancy when abortion became legal to those in their 14th week of pregnancy at the time of legalization.[18] Although both groups of women could have received a legal abortion, the cost to the woman only slightly later in pregnancy was far higher. Until a woman's twelfth week of pregnancy, doctors could use dilation and curettage (D and C) or suction to terminate a pregnancy. Gutcheon (1973) compares the trauma of these out-patient procedures to having one's wisdom teeth removed. If a woman was more than 12 weeks pregnant, physicians could not use these simpler methods to terminate her pregnancy and instead used more financially, physically, and mentally costly in-patient procedures.[19]

My RD estimator calculates the effect of abortion legalization using

$$\beta_{RD} = \lim_{x \downarrow 7/1/1970} E[Y_i|X = x] - \lim_{x \uparrow 7/1/1970} E[Y|X = x] \tag{2}$$

where X is the (medically-determined) final date at which a doctor could perform an early-pregnancy

[18] I omit women finishing their first trimester within 2 weeks of legalization due to potential measurement issues or errors in calculating pregnancy length. The omission also ensures women had access to first trimester abortion if and only if they conceived after abortion reform was approved by the New York legislature.

[19] Doctors would generally make a woman who was seeking an abortion after the 12th week of pregnancy wait until the 16th week of pregnancy and then induce a still-birth using an in-patient procedure known as "salting out" or saline induction. For details, see Gutcheon (1973).

abortion.[20] I estimate the two limit functions using both split-side polynomial regression and local linear regression. Because my wage analysis will compare adults by quarter of birth, I use a 12-week bandwidth. The RD results are generally robust to using multiples of this distance or the Imbens and Kalyanaraman (2009) optimal bandwidth.

Table 1 contains the estimated effects of abortion access and Figures 3 and 4 graph the split-side polynomial RD estimates. The results suggest that children whose mothers had access to first-trimester abortion were born into different average circumstances than those whose mothers did not have access. The cohort of babies conceived less than 12 weeks before New York's abortion reform is about 10 percent smaller than the cohort conceived only slightly earlier. Further, all estimates suggest that abortion access leads the proportion of a cohort that is black to decrease by about 10 percent.[21] Moreover, mothers with access are far less likely to have had two or more children before their recorded births, suggesting that their babies are born into homes with greater per capita resources. Mother's abortion access also led to marginally significant declines in the share of babies born less than one year, or more than 10 years, after the birth of their last sibling.

The RD estimates also suggest that mothers with access to abortion have significantly healthier babies. Mother's access is associated with heavier babies (who are about 6 to 10 percent less likely be classified as low-weight). Women with access to abortion also report seeking prenatal care earlier in pregnancy. Further, abortion access implies a highly significant 20 percent drop in the number of children born with congenital defects.

Additionally, the socioeconomic composition of mothers giving birth changed with abortion reform. Women who had children after abortion became legal in New York were more educated

[20] This analysis essentially assumes that second-trimester abortion had little or no effect on cohort characteristics. If the legalization of second-trimester abortion and first-trimester abortion had opposite effects on the population, estimates from eq. (2) will overstate the impact of legalization. Otherwise, this method understates abortion's effect. Estimates using earlier cutoff dates to determine the effect of second-trimester abortion suggest the latter. Thus, choosing $x = 7/1/1970$ as the cutoff of interest likely yields a lower bound for the impact of abortion on characteristics at birth.

[21] Information on Hispanic ethnicity is not available in the NCHS.

If second-trimester abortion also lowered the birth rate, the true decline attributable to abortion's legalization is larger. Moreover, if abortion access led the birth rate to continue to decline after its immediate effect (as is suggested by Figure 3), the RD estimator understates the effect of the policy change.

(significantly so in some specifications).[22] Although New York did not collect information on mother's marital status, one can use the availability of information on a child's father to proxy for single motherhood. About 10 percent of birth records indicate valid data on a mother's education but no information for father's education. This rate of non-reporting is 1.0 to 1.9 percentage points lower in the group of mothers who had access to legal abortion, suggesting an increase in the prevalence of births within two-parent families. Furthermore, if one focuses on births to women living in New York City, where articles in the *New York Times* suggest abortion access increased most rapidly, similar results hold. Despite these changes, average age at birth (excluded from the tables and graphs) does not change after the reform.[23]

Panel B of Table 1 contains the corresponding estimates within the sample of African American newborns. The number of African American births shrank by 20 percent or more after abortion's legalization. Additionally, black infants whose mothers had access to legal abortion are less likely to have low weight at birth, are heavier on average, and are born into smaller families. Although these changes are large and robust to various RD specifications, changes in other variables are less clear. The average age and education level of black mothers do not change with abortion's legalization. Nonetheless, the change in birthweight suggests important unobservable variables could shift in the black families having children.

The changes estimated here likely reflect the impact of abortion; however, if incentives to become pregnant shifted three months prior to abortion's legalization, the estimates may be confounded.[24] Indeed, throughout the 1960s, men with children were exempt from conscription. Thus, draft-age men could avoid service in Vietnam by having a child. When this deferment option ended by executive order in April 1970, men under age 26 had less of an incentive to start a family, potentially leading to changes in the characteristics of children (see Kutinova 2009 and Bitler and

[22] Limiting births to mothers over age 18, 22, or 25 implies qualitatively similar effects of abortion access on education, suggesting this result does not simply reflect a decrease in births to mothers still in school.

[23] Note that although pregnant teens had the highest proportion of abortions, abortion was also utilized heavily by women over 30. The proportion of babies born to mothers between 18 and 29 significantly increases with abortion reform. Additionally, the interquartile range of mother's age at birth significantly decreases.

[24] A careful search of the *New York Times* archives suggests that policies targeted at pregnant women were otherwise stable over this period

Schmidt 2012). This policy change occurred 10 weeks prior to abortion's legalization in New York; however, subgroup analyses suggest that the change does not lead the impact of abortion to be overstated.

The end of paternity deferments should only bias my estimates within two subgroups of children: those with fathers under 27 (men 26 and older were not subject to the draft) and first children (one child was sufficient for deferment). This allows me to disentangle the effects of the abortion and deferment policies. If the impact of mother's abortion access was larger within these subgroups, one might be concerned that the changes related to abortion actually stemmed from the other policy change. In fact, the effect of mother's access is larger when one focuses on higher order births or newborns with fathers age 27 or older. Therefore, the change in draft eligibility likely does not lead me to overstate the impact of abortion's legalization.

All together, this analysis suggests a real and perceptible change in the characteristics and number of births after abortion's legalization. The birth certificate data indicate that when women gain better and safer access to abortion, the children they carry to term are better-off upon arrival. These advantages could easily persist through adulthood.

5 Identification in the ACS

To determine the effects of legal abortion access on cohort outcomes in adulthood, I use data from the 2005 to 2010 American Community Surveys (ACS). Focusing on legalization in New York, I select a sample of black, white, and Hispanic men and women born in-state between 1964 and 1975.[25] I consider both the total earned income and hourly wages of workers in my analysis. The sample (N=95,314) is selected in a standard manner and excludes the self-employed, active-duty military, and those with allocated data.[26] I draw similar samples from the rest of the United States.

[25] Formally, $1965 \leq survey\ year - age \leq 1975$.

[26] The selection procedure closely follows Autor, Katz, and Kearney (2008). See Appendix A.3 for details.

5.1 Estimating Abortion Access in the ACS

Unlike in the NCHS, one cannot precisely determine mother's access to abortion in the ACS.[27] But one can use quarter of birth to predict the probability that a person's mother had legal access to abortion. A person of age a, interviewed in year t, was either born in year $t-a$ if they had celebrated their birthday that survey year or year $t-a-1$ if they had not done so already. Further, because ACS interviews are conducted uniformly across the year, one can conclude that those with birthdays earlier in the year are more likely to have celebrated a birthday already when interviewed.[28] Thus, an a-year-old interviewed at t and born in Quarter 1 is more likely to have been born in year $t-a$ than an a-year-old interviewed at t and born in Quarter 2.

Assuming all pregnancies last exactly nine months, a person's mother would have access to (first-trimester) abortion if he or she was born after January 1, 1971, as this implies New York legalized abortion prior to the end of the mother's first trimester. Therefore, people with $t-a=1971$ who were born in different quarters have different probabilities of being born after January 1, 1971. These individuals then have mothers with different predicted access to legal abortion.

To clarify this, I compare two people interviewed in 2005 at age 34 ($t-a=1971$) who are of the same expected age but have mothers with different expected access to legal abortion (see Table 2).[29] First, consider Amy, born in Quarter 1 (Panel A). Assuming, on average, that people like Amy were born in mid-February, if Amy is interviewed in the second half of Quarter 1 or anytime in Quarters 2 through 4, she has already had her birthday. Thus, Amy was born in 1971. If Amy was interviewed in the first half of Quarter 1 and reports being 34, she was born in 1970. Therefore, assuming that interviews and birthdays occur uniformly across and within quarters, the probability that Amy was born after January 1, 1971 is $7/8$ and $abort\_access = 7/8$.

Alternatively, consider Bob, who was born in Quarter 2 (Panel B). If Bob was interviewed

[27] Although the restricted-use version of the ACS contains date of birth, both date of birth and gestational age are necessary to determine mother's access with the precision needed for a regression discontinuity design.

[28] See Appendix A.4 for details on the uniformity of interviews across the year.

[29] Note that calculations are based on the null hypothesis that abortion did not affect the composition or number of births. If abortion decreased the birth rate (see Section 4), then more births occurred in 1970 than 1971. Thus, my measure of abortion access may be overstated, implying I understate the effect of abortion access on wages.

between mid-April and the end of December, one can infer that he was born in 1971. Otherwise, his responses suggest he was born in 1970. Therefore, the probability that Bob was born after January 1, 1971 is $5/8$ and $abort\_access = 5/8$.[30]

Even though Amy and Bob have mothers with different expected access to abortion, they are the same expected age (again, see Table 2). If Amy was interviewed in Quarter 2, 3, or 4, she would be 34 and 3, 6, or 9 months old. If she was interviewed in Quarter 1, she either just turned 34 or will be 35 very soon (on average, she is 34.5). Given that interviews occurred uniformly across quarters, Amy's expected age is the average of these values: 34.5. Likewise, Bob would be three months away from his 35th birthday if interviewed in Quarter 1, just 34 or almost 35 if interviewed in Quarter 2, 34.25 if interviewed Quarter 3, and 34.5 if interviewed in Quarter 4. Thus, he is also predicted to be 34.5. My identification strategy essentially compares the average Amy with the average Bob to determine the impact of abortion reform.

5.2 Identification Strategy

To formally compare those of the same expected age, interviewed in the same survey year, but with mothers who had different access to abortion, I use the specification

$$Y_{iatq} = \alpha_{at} + \gamma_q + \beta(abort\_access_{atq}) + \phi(pill\_access_{atq}) + \varepsilon_{iatq}, \tag{3}$$

where Y is a given outcome of interest (e.g., wages, earnings), α is a vector of age-survey year fixed-effects, and γ is a vector of quarter of birth fixed-effects. I cluster standard errors by cohort, defined as the expected year and quarter of birth. If quarter of birth does not differentially influence earnings across age-year pairs (see Buckles and Hungerman 2010), this regression will estimate the effect of abortion access on cohort characteristics.[31]

[30] See Appendix A.4 for a validation of these calculations using birth certificate data.
Similar calculations yield mother's pill access at conception.

[31] This assumes that abortion legalization was the only reform impacting pregnant women in the third quarter of 1970. If other important changes occurred in these three months, my estimator will yield the combined effect of these policies. To mitigate this concern, I control for access to birth control and search historical records of changes in New

To assess this assumption, Figures 5A-D contain the quarter of birth premium for both wages and earnings across ages and survey years in a sample of workers whose mothers' access to abortion does not depend on quarter of birth. Deviations appear random in all plots, lending credibility to my identification strategy. Similarly random trends exist for the race-specific wage premia. I thus use specification (3) to estimate both the mean change in wages associated with abortion's legalization and quantile effects. As abortion access is determined probabilistically, one can think of this paper's main specification as the reduced-form of an underlying instrumental variable approach.

6 The Effect of Abortion Legalization on New Yorkers

6.1 Overall Effects

To determine the effect of abortion reform on average earnings, I estimate eq. (3) using the entire sample of individuals born in New York State. Table 3 contains the coefficients from regressions of log earnings and hourly wages. Neither outcome significantly changes in response to abortion access in specifications excluding access to the pill. When one includes this control, mother's access to abortion is associated with average hourly wages rising by (a marginally significant) 4.3 percent. The effect on log earnings is small and positive, though noisily estimated.

Focusing on the average impact of abortion legalization obscures potentially important heterogeneity. Although one cannot link adults to their parents in Census data, biological children generally inherit their mother's race. As abortion records included information on patient's race, one can then see if differences in mothers' use of abortion imply differences in children's outcomes.

Column (3) of Table 3 therefore contains coefficients from the interaction of mother's abortion access and indicator variables for being black and Hispanic. Although there were no significant average earnings gains, African Americans whose mothers had access to abortion realized a signif-

York's policies on prenatal care, public health, and other potentially important factors in pregnancy. No other major changes occur in New York during the period of interest.

icant 17 percent increase in earnings. Considering hourly wages instead, both African Americans and Hispanics (but not whites) have higher wages if their mother had access to legal abortion. The effect for blacks may be expected, as black women utilized abortion most heavily in New York from 1970 to 1972. The result for Hispanics is more puzzling, as Hispanic mothers had far fewer abortions per birth than whites. But if Hispanic women were more judicious about when to have abortions, one could see stronger positive selection of mothers (and thus larger wage changes) in this subgroup after legalization.[32]

If one controls for standard Mincerian (1974) predictors of wages (education and potential experience, see column 4), the interaction between abortion access and Hispanic ethnicity decreases in size and becomes insignificant. The coefficient on the black interaction term also shrinks but remains statistically significant. Thus, while these key terms explain the differential effect of abortion access on Hispanics, they cannot rationalize the stronger effect that abortion legalization had on blacks.

I also consider how abortion access differentially impacted male and female babies carried to term. Sex-selective abortion was not relevant in the 1970s but mother's abortion access may differently impact male and female babies. Although boys tend to be more sensitive to childhood living conditions than girls (c.f., Bertrand and Pan 2013), work by Dahl and Moretti (2008) suggests that unwantedness may be more detrimental to female children. The specifications in column (5) of Table 3 contain the interaction of an indicator for being female and mother's abortion access. Though earnings regressions are very noisy, wage regressions suggest that women's wages significantly increase with mother's access to abortion. The average effect for males alone is small and insignificant. Adding individual-level controls does little to change these results.

Changes in parental selection likely drive the observed wage changes but other forces could also be important. If abortion access influenced social norms or the marriage market, such changes likely occurred slowly. Though one cannot rule out these forces, the very short time-horizon used in this analysis suggests only a minor potential role for these effects.

[32]The assumption that Hispanics used abortion more judiciously is consistent with differences in beliefs about the morality of abortion at this time. However, one cannot directly assess this assumption.

Differently, cohort size (shown in the previous section to fall after abortion's legalization) could affect earnings in two ways. First, if different cohorts have limited substitutability in production, the wages of smaller cohorts should exceed those of larger cohorts. If one compared people in widely-spaced cohorts, cohort size differences could result in significant wage differences; however, my analysis compares people born over the course of a short period, who are likely good substitutes, *ceteris paribus*. Thus, cohort size should not act through this particular channel.

Alternatively, resources (e.g., classroom space) could be scarcer in childhood for those born into larger cohorts (c.f., Bound and Turner 2007). Again, my analysis uses the wages of closely-spaced cohorts to infer the effect of abortion. Thus, changes in resources across schools or neighborhoods will not be reflected in my estimates. But if abortion reform led to a change in class size, one cannot rule out effects through this channel.[33] School-starting-age rules in New York indicate that children whose mothers had abortion access were generally in different grades than those whose mothers did not have access (see Barua and Lang 2009). Therefore, a drop in the birthrate will imply a rather similar change in classroom size. Using the estimates of Fredriksson, et al. (2011) and assuming a two-person decrease in average class size implies that about one-quarter of the overall estimated effect of abortion can be accounted for by class-size changes. Therefore, changes in parental selection likely drive the changes in wages estimated in Table 3.

To get a sense of how the labor force must have changed to produce these effects, I ask: if abortion simply led to a truncation of the wage distribution, at what point would this truncation have to occur to yield the estimated effects? By examining the wages of those in my sample with $abort\_access = 0$, I find that removing the bottom 2 percent of the wage distribution would imply the estimated 4.3 percent rise in overall wages. That is, if mothers' abortion decisions were strictly based on the future wages of their children, an abortion rate of only 2 percent could yield the estimated change in wages. For African Americans, a 17 percent average wage increase is equivalent to dropping the bottom 12 percent of the race-specific wage distribution. For Hispanics, the 12 percent increase in wages could be achieved by removing workers below the eighth percentile of

[33] See Chetty, et al. (2010) and Fredriksson, Ockert, and Oosterbeek (2011).

the wage distribution.[34]

Differently, one can use changes in the wage rate and birth rate associated with mother's access to abortion to back out the implied difference in the potential wages of those observed and not observed in the post-reform sample. This exercise implies that the wages of workers "missing" from the wage distribution after abortion's legalization are 40 percent lower than the wages of workers in the post-reform sample (about 60 percent lower for blacks). The estimate is similar in magnitude to the results of Gruber, et al. (1999), who found that the marginal child born after abortion reform was 40 to 60 percent more likely than average to live in poverty, with a single parent, or in a family receiving welfare.

Of course, these estimates represent extremes derived from ignoring the aforementioned class-size effects and the additional pregnancies occurring after legalization. Although the birth rate dropped by only 10 percent after legalization, over 25 percent of New York pregnancies ended in abortion during the year following New York's reform. By using the birth rate, and not the abortion rate, I overstate the degree of selection needed to rationalize my point estimates. Differently, if all women in the absolute worst circumstances have abortions but would otherwise birth children who do not participate in the labor force, one would need the "missing" workers to be drawn from further into the bottom tail of the observed wage distribution.

My regression results are robust to a wide variety of clustering procedures and the use of different techniques to account for serial correlation. Despite this, comparing the magnitude of the estimated effects with the quarter of birth premium across ages (see Figure 5) raises some questions. Although my estimates of β come from a combination of the quarter of birth premia in Figure 5, the large range of these premia suggests that I may underestimate the standard error of β.

A placebo test allows me to assess the size of β in relation to the variation in the quarter of birth premium. I consider the set of coefficients derived from placebo reforms, defined as the "effect"

[34] Alternatively, one can use the estimated change in the birth rate to set the proportion of workers "missing" and calculate the subset of the wage distribution they would have to be drawn from to produce the estimated results. Births to New Yorkers fell by about 10 percent after legalization (20 percent for blacks). If one randomly omitted 10 percent of earners from the bottom 65 percentiles of the wage distribution, wages would increase by about 4.3 percent, the estimated effect of abortion reform. If one omitted 20 percent of African Americans from the bottom four deciles of the race-specific wage distribution, the average wage of black workers would rise by about 17 percent.

of being born after January 1 of several different years before abortion's legalization. Placebo estimates of reform from 1960 to 1969 yield estimates suggesting that the overall effect of reform may not be statistically distinguishable from zero; however, these tests yield tighter predictions for the interactions between abortion access and race indicators. Using the small number of placebo estimates to calculate a standard error, both interaction terms remain significant at the 5 percent level. Thus, although the effect of abortion estimated for the whole population is tenuous, estimates within minority groups likely represent real changes.

6.2 Effects Over the Wage Distribution

Anecdotal evidence suggests that wealthier women had greater access to abortion prior to its legalization and historical prices indicate that poorer women had easier access to abortion after the New York reform. Therefore, one might expect that abortion's legalization led to larger changes in selection into motherhood for lower-income women and that estimates of eq. (3) might vary across the wage distribution.[35]

Figures 6A and B thus contain the estimates from quantile regressions of log hourly wages and total annual earnings. Figure 6A suggests that abortion legalization caused large wages gains among lower-wage workers. Effects are around 10 log points and statistically significant from roughly the 10th to the 30th quantile. From the 60th wage quantile and up, all estimates are statistically indistinguishable from zero. Further, after the 70th quantile, point estimates are small. The gap between the 10th and the 50th quantiles of the wage distribution is largely unaffected by abortion's legalization but the gap between the 10th and the 90th quantiles significantly narrows.[36] Focusing on earnings in Figure 6B, the results are far noisier. Although point estimates suggest that earnings increased by more in the bottom half of the distribution than at the top, the estimates cannot be statistically distinguished from zero.

One can also estimate eq. (3) using quantile regression within racial groups. Figure 7 con-

[35]This also may explain the differential effects by race discussed in the previous section.

[36]The 90-50 gap narrows by 7 percent with a t-statistic of 1.61.

tains the estimated effect of abortion reform on the hourly wage distributions of whites, blacks, and Hispanics.[37] Each racial group exhibits a different trend. For whites, most quantile effects are statistically indistinguishable from zero but the effect of abortion on lower quantiles is larger than that for higher quantiles. Blacks gain throughout the wage distribution; most estimates are both large (over 15 percent) and significant. The wages of Hispanics in the bottom half of the race-specific wage distribution increase, while the top quartile of the wage distribution exhibits marginally significant losses. Altogether, the results suggest that abortion access led to moderate, short-run decreases in overall and racial inequality. They also suggest that abortion's legalization may have differently affected the fertility choices of women of different backgrounds.

To assess whether my estimates represent permanent changes in the wage distribution, I utilize more broadly-spaced cohorts. Right after legalization, women may use abortion differently than in the long-run. Furthermore, if changes in abortion access influence women's contraceptive choices, social norms, or the marriage market, one might expect the relationship between access and outcomes to fade over successive cohorts. I thus estimate regressions of the form

$$Y_{iatq} = \alpha_{1a} + \alpha_{2t} + \beta_{LR}(abort\_access_{atq}) + \phi_{LR}(pill\_access_{atq}) + \varepsilon_{iatq}, \tag{4}$$

dropping cohorts in which abortion access depends on quarter of birth (and not simply age and survey year). Instead of making age-birth quarter comparisons, this strategy compares individuals of the same age across surveys, essentially assuming that the relationship between age and hourly wages is stable from 2005 to 2010.

Figure 8 contains the quantile effects estimated using eq. (4). The longer time-horizon used in these regressions yields smaller effects, though wages still significantly increase by about 5 percent in the bottom half of the distribution. These estimates suggest that abortion legalization may have had a longer-run effect. That is, the estimates comparing children conceived within a few months of legalization do not simply reflect a one-time change in the wage distribution that was subsequently, quickly undone by changes in women's contraceptive use or social norms.

[37] Earnings regressions yielded very noisy estimates.

6.3 Estimates of Other Variables

Overall, the previous analysis suggests that once women gain access to abortion, mothers become more positively selected. This in turn leads children to have higher wages. To consider more proximate causes for increased wages among grown children, I analyze the effect of abortion reform on a variety of other adult characteristics. Table 4 contains the estimates of abortion's effect calculated using specification (3).

Cohorts whose mothers had access to legal abortion contain more whites and fewer minorities, with the proportion of workers that are black decreasing by about 15 percent. Mother's access to abortion is associated with a worker having an additional quarter-year of education. Further, the proportion of a cohort without a high school degree drops sharply with mother's access. Children whose mothers had legal access to abortion are also less likely to be single parents themselves, while men (but not women) report living with a greater number of own-children if their mothers had access to abortion. Both men and women are also more likely to be married and less likely to be divorced (however, there is no change in the proportion of the population that is single). All of these effects are significant at the 5 percent level and cannot be explained by racial composition.

Additionally, estimates for the whole population imply that abortion reform may have led to decreased dependence on welfare and disability income. The share of a cohort that classifies itself as having a work-limiting disability also significantly declines with mother's access. Furthermore, there may be a small increase in the proportion of a cohort that is working when mothers have access to abortion.

For African American men, this result is far stronger. Idleness of black men is 14 percentage points lower within cohorts whose mothers had access to legal abortion. This change implies that abortion's effect on the wages of African Americans may underestimate abortion's overall effect on income or welfare within this subgroup. Indeed, abortion access caused the average African American to live in a family with 28 percent higher total income (estimate not shown).

Finally, in light of Donohue and Levitt (2001) and related work, I use my identification strategy to examine the impact of New York's abortion reform on institutionalization rates. The ACS does not identify the type of institution for those in group quarters but one can examine institutionalization among black men, as this variable serves as a good proxy for incarceration within this subpopulation.[38] Although I find significant effects of abortion access on many other variables of interest, the results for institutionalization are insignificant.

Despite this null result, these estimates suggest that changes in selection into motherhood after abortion reform yielded cohorts that are different in a variety of ways.

7 The Effect of New York's Legalization on Non-Residents

By focusing on outcomes for native-born New Yorkers, I am able to find support for previous work suggesting that abortion led to positive selection. New York's legalization can also be used to demonstrate why previous analyses comparing early abortion-legalizers (such as New York) to the rest of the country might misrepresent the total effect of abortion reform.[39]

To see how the change in New York law could have impacted selection in other states, I use eq. (3) to estimate the effect of the New York abortion reform on the log hourly wages of those born in states where many women traveled to New York to receive abortions, in states where few women traveled to New York for abortions, and in states not bordering New York. The results (see Table 5) suggest that legalization in New York led to changes in wages for children born in other states.

Someone haling from a state where many women traveled to New York to receive abortions would have higher wages if their mother conceived less than three months before the change in New York law. Unlike the effects for New Yorkers, workers gain throughout the wage distribution, though the average effect is lower within these states than within New York. Those with below- (above-) median wages tend to have 5 (3) percent higher wages if their mother could have had

[38] The ACS only lists whether a person was institutionalized, not incarcerated. The 1980 Census was the last to indicate the type of institution. In that survey, most people between 30 and 45 in an institution were either in a mental institution, nursing home, or other similar facility. Moreover, fewer than two-thirds of the institutionalized younger, New York-born, African American men in the 1980 Census were actually in a correctional facility. Although this was before the period of mass incarceration, the data suggests one should be careful in using ACS institutionalization data to infer incarceration rates.

[39] Also see Joyce, et al. (2012) and Levine, et al. (1999).

access to abortion in New York. Both high transportation costs and lack of Medicaid coverage suggest that New York's reform did not differentially lower the costs of abortion for out-of-state women of different means (unlike the change in costs for New York residents). The more uniform change in the wage distribution matches the more uniform change in costs for women living out-of-state.

If one further considers the impact of New York's reform on all states that do not border New York, the effect of legalization is smaller but still significant and around 3 percent for lower-earning workers. Finally, focusing on the ten states that sent the fewest women (per capita) to New York for legal abortions yields small and insignificant estimates of abortion's effect. Thus, abortion legalization in New York is associated with gains both locally and nationally; however, null results in states where few women obtained New York abortions suggest that the results for New Yorkers are not simply spurious.

8 Conclusion

Three years before the Supreme Court's decision on *Roe v. Wade,* New York became the first state to allow women legal and easy access to abortion. By lowering the total cost of terminating a pregnancy, this reform allowed women to more precisely time their births. This paper suggests that women took the opportunity, and the resulting cohorts were better-off. Birth certificate data implies that children were born into smaller families with more educated mothers and more present fathers. Furthermore, children were less likely to be of dangerously low weight at birth.

Wage data from several decades later suggests that these, and other, improvements in child living conditions resulted in higher wages. On average, the wages of African Americans and Hispanics born in New York increased by 17 and 12 percent. Wages for all workers in the bottom deciles of the wage distribution also increased by about 10 percent. My analysis further suggests that even those born outside of New York gained from New York's reform. Wages increased in states that sent many women to New York to receive legal abortions. But there is no effect of New

York's law on cohorts born in states where few women traveled to New York for this purpose.

As these results compare cohorts spaced very closely together, the estimated effects likely reflect changes in the choices of women to carry their pregnancies to term. That is, children's wages rise because reducing the cost of abortion allows women to give birth during more secure times when they can better care for their babies. Some portion of the estimated effects may also reflect changes in contraception, cohort size, or social norms. But these forces are secondary in short-run comparisons. Using more broadly spaced cohorts implies a smaller effect of mother's abortion access on wages. This suggests that changes in social norms, the marriage market, and contraceptive behavior may counteract some, but not all, of the effect of increased abortion utilization in the long-run.

This paper validates prior work demonstrating that abortion legalization had a positive effect on children carried to term. By showing that wages increased for low-wage workers, I demonstrate that the costs of crime likely increased. This supports (but cannot confirm) the negative relationship between abortion and crime described by Donohue and Levitt (2001). Furthermore, by using a new identification strategy, my analysis reinforces the findings of studies demonstrating that abortion legalization led to increased welfare for children carried to term (e.g., Ananat, et al. 2009 and Gruber, et al. 1999). Finally, my results supplement the previous findings of Joyce, et al. (2012), who demonstrate that abortion reform in New York had national implications.

Despite the congruence of these conclusions with many other studies in the literature, one must be careful in extrapolating these results. I demonstrate that increased abortion access led to changes in cohorts born very close together. Even my longest-run identification strategy compares adults born over the course of only five years. Therefore, this analysis cannot address the permanent effect of legalizing abortion. Moreover, my analysis focuses on a large change in access to abortion. Women went from having to pay high fees, travel long distances, and face much risk to terminate a pregnancy, to having easy access to a safe abortion near home. Thus, one should be careful in applying these results to more modest changes in the costs of abortion.[40] Finally, my results stem

[40] In particular, see Kane and Staiger (1996).

from abortion reform in New York, a relatively liberal state. One might expect the residents of different states to react differently to changes in the costs of abortion.

Altogether, this paper suggests that abortion reform had a real effect on cohorts born just after legalization. Access to abortion allowed women to have children at more desirable and secure times. The result is a cohort typified by higher wages and, very likely, improved welfare.

A Data Appendix

A.1 Access to Abortion and the Pill

A mother is defined as having access to abortion ($abort\_access$) if she completed her first trimester of pregnancy after abortion's legalization in New York (July 1, 1970). In the NCHS, the last week of the first trimester is determined using gestational age and date of birth. In the ACS, I use the day six months prior to birth. Though New York allowed legal abortion until the end of a woman's second trimester, such procedures were more rare, costly, and dangerous (see Gutcheon 1973).

The variable $pill\_access$ indicates if a mother had legal access to the pill if she was unmarried and between 16 and 20 years of age at the date of conception, calculated in a fashion parallel to the determination of the end of her first trimester. Pill access for married women, women over 20, or women under 16 did not vary in New York over the time period of interest. See Bailey (2010) and Goldin and Katz (2002) for details.

A.2 Natality Birth Data

I use the National Center for Health Statistics Vital Statistics Natality Birth Data (NCHS) to demonstrate that abortion legalization led to sharp changes in the size and nature of birth cohorts. This 50 percent sample of births is available starting in 1968 and contains information on month and day of birth and gestational age in weeks. In some cases, day of birth is omitted (in particular, early in the sample). For these observations, I set a child's birthday to the median date of birth for those born in the same month between 1968 and 1975. I drop all observations reporting a gestational age at birth of 45 weeks or more. Those with missing gestational age data are assigned to have a pregnancy lasting the median number of weeks in the sample (40). I then use birthday and gestational age to estimate the week of conception, which implies the final week of a woman's first trimester.

The mothers in my selected sample all reside in New York. I select all births from this dataset where gestational age implies a woman became pregnant between April 1968 and March 1972, or ended her first trimester within two years of New York's legalization of abortion. To avoid potential issues from the estimation of gestational age and birth date, I omit any observations reflecting that a woman would have ended her first trimester of pregnancy within two weeks of the law change. The resulting sample contains 568,764 births.

A.3 The American Community Surveys

The bulk of my analysis uses the 2005 to 2010 American Community Surveys (ACS).[41] From the surveys, I select a sample of people born in New York who identify as white, black, or Hispanic. All observations in the sample have a value of $survey\,year - age$ between 1965 and 1975. Thus, the workers were born in or after 1964 but no later than 1975. I omit any observation with allocated age, race, or ethnicity data. To avoid any concerns about second-trimester abortion, I also remove any observations in which expected access to first and second trimester abortion differ.

[41] The ACS began recording quarter of birth in 2005. My identification strategy utilizes this variable in an important way, leading me to exclude pre-2005 data from my analysis.

From the larger group, I select a sample of workers similar to that used by Autor, et al. (2008). These individuals report non-zero usual hours of work and weeks of work in the past year and identify as civilian, non-self-employed workers. I also omit any observations with allocated earnings data. I calculate the hourly wage as yearly wage and salary income divided by the product of usual weekly hours and weeks worked last year. In cases where weeks worked is only available as an interval variable, I use the mean weeks worked for observations within this interval.

The ACS topcodes income data above the 99.5th percentile by state, and replaces such values with the mean income above this quantile. I omit any observation with an hourly wage that would imply topcoding if the worker reported working more than 1,750 hours per year (but is not topcoded due to limited hours of work). I further omit observations for hourly wages in the first percentile of the wage distribution. The resultant sample contains 95,314 observations. I draw similar samples from the rest of the United States, yielding 1,231,007 observations in total.

A.4 Validation of Estimates of Abortion Access in the ACS

My identification procedure hinges on two key assumptions: ACS interviews are conducted uniformly across the year and births occur uniformly across time.

Although detailed data on date of interview is unavailable, the sampling methodology used by the Census Bureau when conducting interviews for the ACS suggests the validity of the first assumption. Each year of the ACS actually consists of 12 independent monthly surveys, implying equal distribution of interviews over the months of the year. Each month, surveys are mailed out to households, who return them at their leisure. If a household does not return a survey by mail by the end of the month, they are contacted via phone the following month. Finally, some households not available by phone are interviewed in person the next month. Staffing concerns suggest that phone and in-person interviews (about half of responses) occur over the course of the month. One might also assume that mail-in questionnaires are filled out throughout the month. Finally, the monthly nature of the survey and structure of interviews by mail, by phone, and in person implies that time of year is uncorrelated with interview type. Thus, one can reasonably assume that interviews occur roughly uniformly across the year.[42]

To assess the validity of my second assumption (uniformity of births), I suppose that each of the infants recorded in the NCHS survived into adulthood and was interviewed by the ACS in 2005. For each observation, I randomly select a date of interview for the later survey and calculate the observation's corresponding age. I can then compare the actual probability of abortion access to the value estimated using only age, survey year, and birth quarter. Table A1 contains a comparison of the methods.

Small differences emerge. My method may systematically underestimate mother's access to abortion because abortion access decreased the number of pregnancies carried to term (see Table 1). That is, as fewer births occurred after abortion was legalized, a lower proportion of the population should have a mother who had access to abortion.

Despite this discrepancy, I continue to use the assignment rule depicted in Table 2. This rule is based on the null hypothesis that abortion had no effect on the number or type of children born. The actual assignment rule would imply some effect of abortion on births. A test of $\beta = 0$ based on actual access would thus not be interpretable as testing the null hypothesis of interest in its entirety. Moreover, because the probabilistic assignment generally underestimates abortion access, it will only bias estimates of the effect of abortion legalization downward. Therefore, one can view the estimates in Sections 6 and 7 as lower bounds.

Using the actual abortion access calculated from birth certificate data yields results comparable to those

[42]For more detail, see United States Census Bureau (2009).

calculated using predicted access. The coefficients from the OLS regressions increase by about 1 log point. Blacks whose mothers had access to legal abortion have wages 18 percent higher than those whose mothers did not. Likewise, the wages of Hispanics increase by 13 percent on average when mothers gain access to legal and safe abortion. Using this alternative specification, the wages of whites still do not significantly change. Furthermore, Figure A1 demonstrates that using actual and predicted abortion access yield similar results across the quantiles of the wage distribution.

As an additional validation method, I estimate the effect of mother's access to abortion on the log hourly wages of a more limited sample of individuals born from 1968 to 1972 in Figure A2. This narrower range more closely matches the sample used to estimate the effect of abortion access at birth in Section 4. Although the standard deviations of the estimates increase due to lost power, the estimated effect of abortion is at least as large as that computed using adults born over a longer period. Moreover, the results remain significant and still indicate that mother's abortion access increases the wages of those in the lower half of the earnings distribution.

References

Akerlof, George A., Janet L. Yellen and Michael L. Katz. 1996. An analysis of out-of-wedlock childbearing in the United States. *Quarterly Journal of Economics* 111, no. 2:277-317.

Ananat, Elizabeth O.; Jonathan Gruber; Phillip Levine and Douglas Staiger. 2009. Abortion and selection. *Review of Economics and Statistics* 91, no. 1:124-36.

Autor, David H.; Lawrence F. Katz; and Melissa S. Kearney. 2008. Trends in U.S. wage inequality: Re-assessing the revisionists. *Review of Economics and Statistics* 90, no. 2:300-23.

Bailey, Martha J. 2010. *Momma's Got the Pill*: How Anthony Comstock and *Griswold v. Connecticut* shaped U.S. childbearing. *American Economic Review* 100, no. 1:98-129.

Barua, Rashmi and Kevin Lang. 2011. School entry, educational attainment and quarter of birth: A cautionary tale of LATE. Working Paper no. 15236, National Bureau of Economic Research, Cambridge, MA.

Bertrand, Marianne and Jessica Pan. 2013. The trouble with boys: Social influences and the gender gap in disruptive behavior. *American Economic Journal: Applied Economics* 5, no. 1:32-64.

Bitler, Marianne P. and Lucie Schmidt. 2012. Birth rates and the Vietnam draft. *American Economic Review* 102, no. 3:566-69.

Bound, John and Sarah Turner. 2007. Cohort crowding: How resources affect collegiate attainment. *Journal of Public Economics* 91, nos. 5-6:877-99.

Brody, Jane E. 1970. City is planning for abortions at the rate of 25,000 a year. *New York Times* (Rosenthal ed.), July 1.

Buckles, Kasey and Daniel M. Hungerman. 2008. Season of birth and later outcomes: Old questions, new answers. Working Paper no. 14573, National Bureau of Economic Research, Cambridge, MA.

Cameron, A. Colin; Jonah B. Gelbach and Douglas L. Miller. 2006. Robust inference with multi-way clustering. Working Paper no. T0327, National Bureau of Economic Research, Cambridge, MA.

Charles, Kerwin Kofi and Melvin Stephens, Jr. 2006. Abortion legalization and adolescent substance use. *Journal of Law and Economics* 49, no. 2:481-505.

Chetty, Raj; John N. Friedman; Nathaniel Hilger; Emmanuel Saez; Diane Whitmore Schazenbach and Danny Yagan. 2010. How does your kindergarten classroom affect your earnings? Evidence from project STAR. Working Paper no. 16381, National Bureau of Economic Research, Cambridge, MA.

Dahl, Gordon B. and Enrico Moretti. 2008. The demand for sons. *Review of Economic Studies* 75, no. 4:1085-1120.

David, Henry P.; Zdenek Dytrych; Zdeněk Matějček and Vratislav Schüller. 1988. *Born unwanted: Developmental effects of denied abortion.* New York, NY: Springer.

Dellapenna, Joseph W. 2006. *Dispelling the myths of abortion history.* Durham, NC: Carolina Academic Press.

Dills, Angela K. and Jeffrey A. Miron. 2006. A comment on the Donohue and Levitt (2006) reply to Foote and Goetz (2005). Unpublished manuscript, Department of Economics, Wellesley College.

Dills, Angela K.; Jeffrey A. Miron and Garrett Summers. 2008. What do economists know about crime? Working Paper no. 13759, National Bureau of Economic Research, Cambridge, MA.

Donohue, John J. and Steven D. Levitt. 2001. The impact of legalized abortion on crime. *Quarterly Journal of Economics* 116, no. 2:379-420.

____. 2004a. Further evidence that legalized abortion lowered crime: A reply to Joyce. *Journal of Human Resources* 39, no. 1:29-39.

____. 2004b. Understanding why crime fell in the 1990s: Four factors that explain the decline and six that do not. *Journal of Economic Perspectives* 18, no. 1:163-90.

____. 2008. Measurement error, legalized abortion, and the decline in crime: A response to Foote and Goetz. *Quarterly Journal of Economics* 123, no. 1:425-40.

Edmiston, Susan. 1971. A report on the abortion capital of the country. *New York Times* (Rosenthal ed.), April 11.

Foote, Christopher L. and Christopher F. Goetz. 2008. The impact of legalized abortion on crime: Comment. *Quarterly Journal of Economics* 123, no. 1:407-23.

Fredriksson, Peter; Björn Öckert and Hessel Oosterbeek. 2011. The long-term effects of class size. Discussion Paper no. 5879, IZA, Bonn, Germany.

Garrow, David J. 1998. *Liberty and sexuality: The right to privacy and the making of* Roe v. Wade. New York, NY: Macmillan Pub. Co.

Goldin, Claudia and Lawrence Katz. 2002. The power of the pill: Oral contraceptives and women's career and marriage decisions. *Journal of Political Economy* 110, no. 4:730-70.

Gruber, Jonathan; Phillip Levine and Douglas Staiger. 1999. Abortion legalization and child living circumstances: Who is the 'marginal child?'" *Quarterly Journal of Economics* 114, no. 1:263-91.

Gutcheon, Beth (Planned Parenthood of New York City). 1973. *Abortion: A woman's guide*. New York, NY: Abelard-Schuman.

Imbens, Guido and Karthik Kalyanaraman. 2009. Optimal bandwidth choice for the regression discontinuity estimator. Working Paper no. 14726, National Bureau of Economic Research, Cambridge, MA.

Jones, Rachel K. and Kathryn Kooistra. 2011. Abortion incidence and services in the United States, 2008. *Perspectives on Sexual and Reproductive Health* 43, no.1:41-50.

Joyce, Ted. 2004a. Did legalized abortion lower crime? *Journal of Human Resources* 39 no. 1:1-28.

____. 2004b. Further tests of abortion and crime. Working Paper no. 10564, National Bureau of Economic Research, Cambridge, MA.

____. 2009. A simple test of abortion and crime. *Review of Economics and Statistics* 91 no. 1:112-23.

____. 2010. Abortion and crime: A review. In *The Handbook of the Economics of Crime*, ed. B. B. P. Zimmerman. New York, NY: Edward Elger.

Joyce, Ted; Ruoding Tan and Yuxiu Zhang. 2012. Back to the future? Abortion before & after Roe." Working Paper no. 18338, National Bureau of Economic Research, Cambridge, MA.

Kane, Thomas J. and Douglas Staiger. 1996. Teen motherhood and abortion access. *Quarterly Journal of Economics* 111, no. 2:467-506.

Kutinova, Andrea. Paternity deferments and the timing of births: U.S. natality during the Vietnam war. *Economic Inquiry* 47, no. 2:351-65.

Lee, Nancy H. 1969. *The search for an abortionist*. Chicago, IL: University of Chicago Press.

Levine, Phillip B.; Douglas Staiger; Thomas J. Kane and David J. Zimmerman. 1999. *Roe v. Wade* and american fertility. *American Journal of Public Health* 89, no. 2:199-203.

Mincer, Jacob. 1974. *Schooling, experience, and earnings*. New York, NY: Columbia University Press for the National Bureau of Economic Research.

New York Times. 1970. Booklet on legal abortions is available across nation. August 10, p. 19.

New York City Department of Health and Mental Hygiene. 1961-1980. *Summary of Vital Statistics.*

Pakter, Jean and Frieda Nelson. 1971. Abortion in New York City: The first nine months." *Family Planning Perspectives* 3, no. 3:5-12.

Pakter, Jean; Donna O'Hare; Frieda Nelson and Martin Svigir. 1973. Two years experience in New York City with the liberalized abortion law–progress and problems." *American Journal of Public Health* 63, no. 6:524-35.

Planned Parenthood. 1972. *The New York abortion story*. New York, NY: Planned Parenthood Publications.

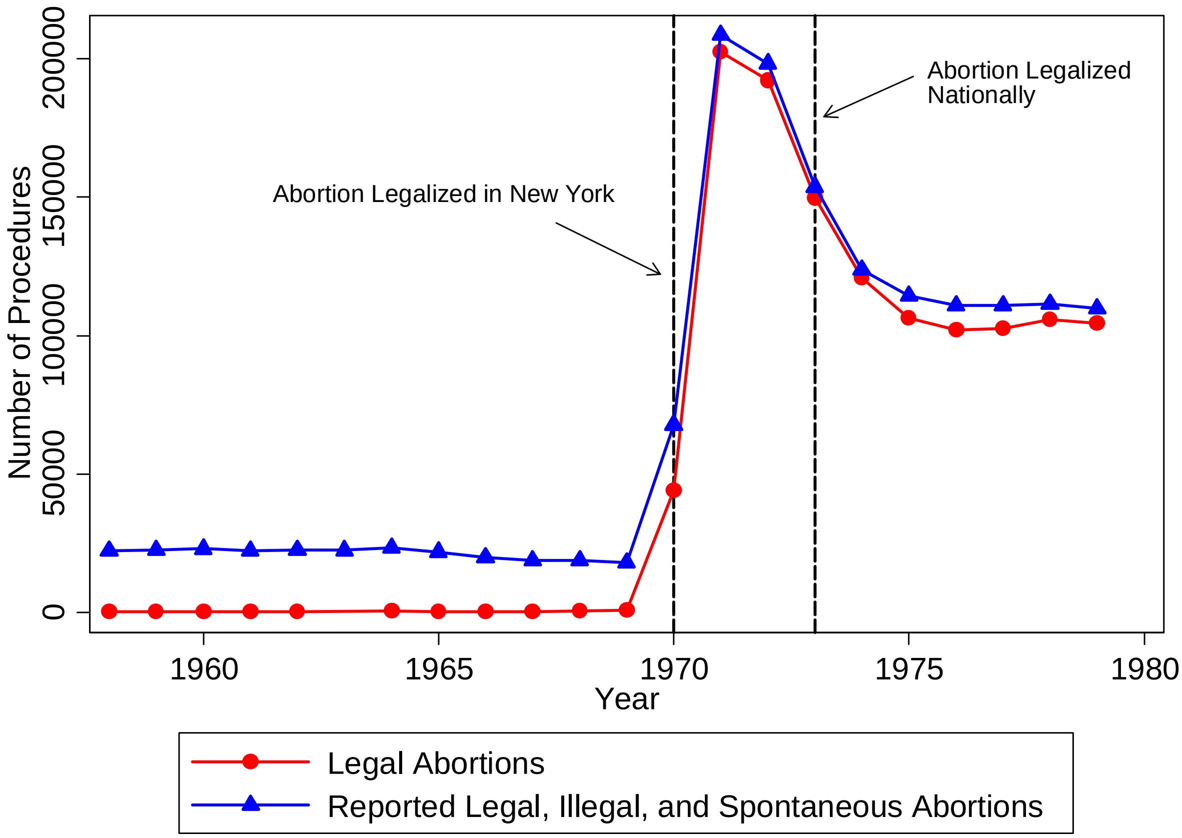
Pop-Eleches, Cristian. 2006. The impact of an abortion ban on socioeconomic outcomes of children: Evidence from Romania." *Journal of Political Economy* 114, no. 4:744-73.

Reuben, David R. 1969. *Everything you always wanted to know about sex (but were afraid to ask).* New York, NY: David McKay Company, Inc.

Roe v. Wade. 1973. 410 U.S. 113.

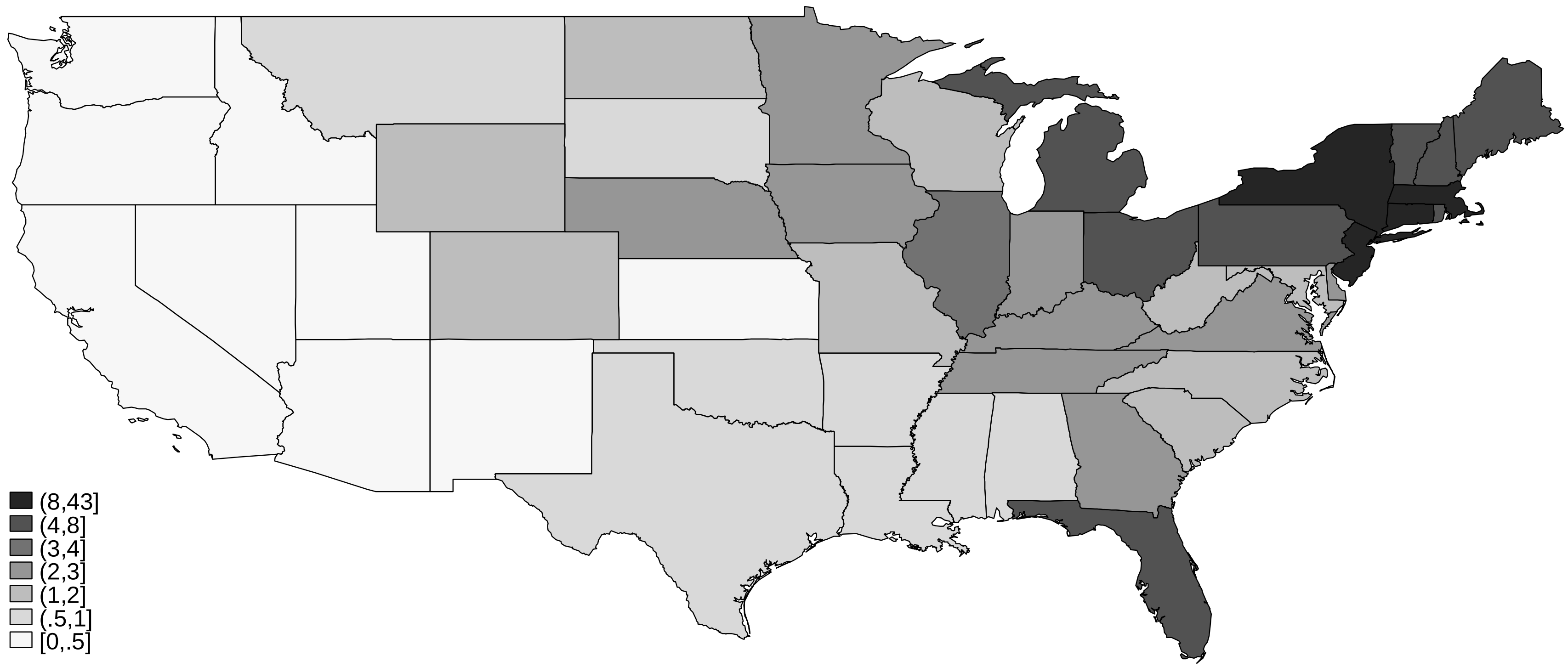
United States Census Bureau. 2009. *Design and methodology: American Community Survey.* Washington, D.C.: U.S. Government Printing Office.

Figure 1: Number of Abortions Preformed in New York City



Source and Notes: NYC Department of Health and Mental Hygiene, *Summary of Vital Statistics, 1961-1980*. Spontaneous abortions include reported and medically documented miscarriages.

Figure 2: New York Abortions per 100 Births to State Residents



Source: Number of abortions from Planned Parenthood (1972), July 1970 to June 1971. Births from NCHS, January 1971 to December 1971.

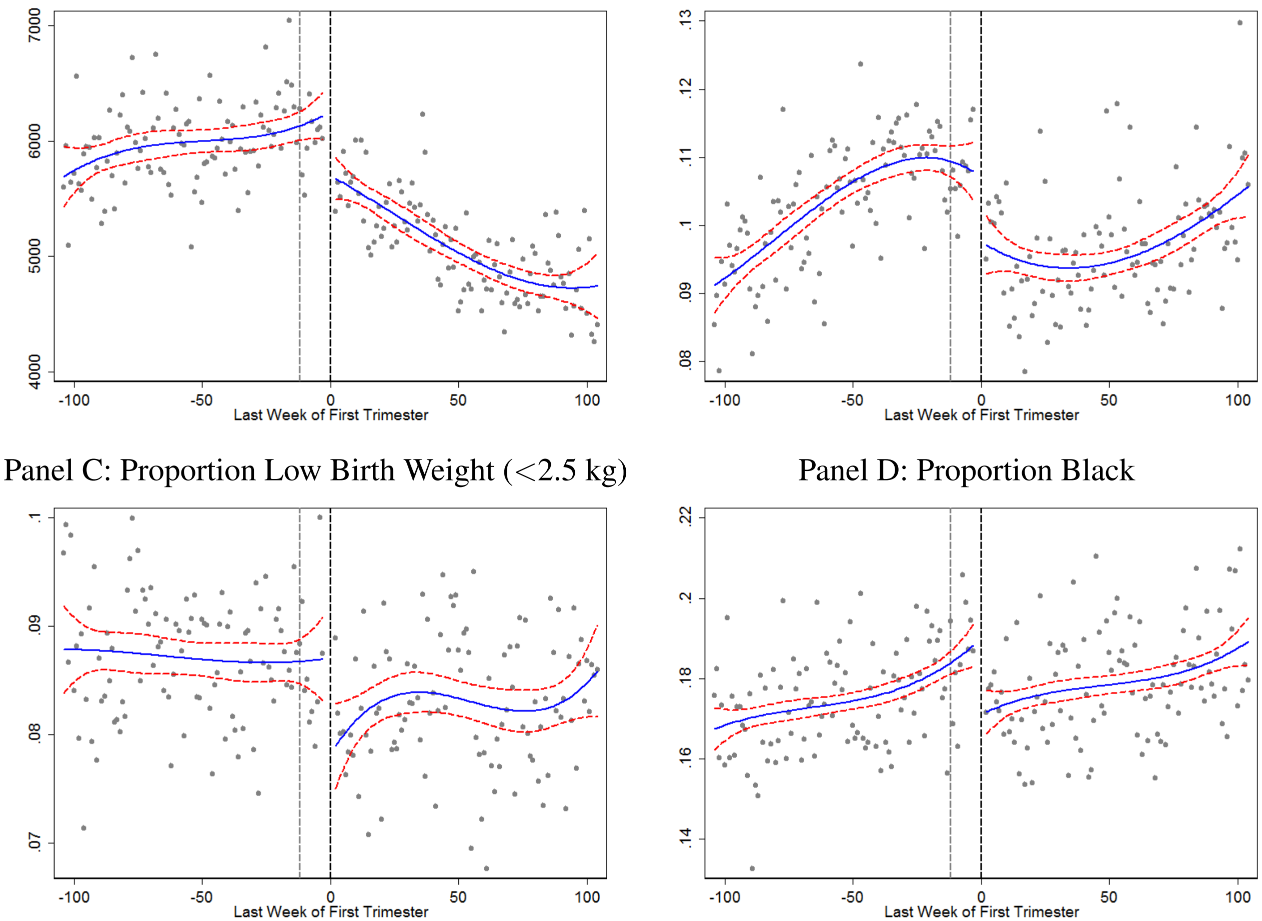
Figure 3: The Effect of Abortion Access on Birth Outcomes

Panel A: Number of Births

Panel B: Proportion Missing Father’s Education

Panel C: Proportion Low Birth Weight (<2.5 kg)

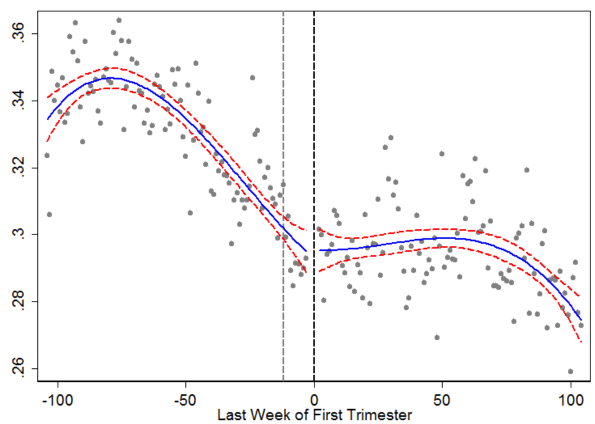
Panel D: Proportion Black



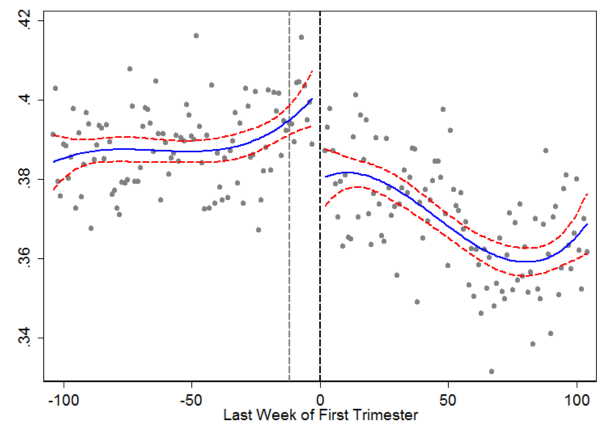
Notes and Sources: Data from NCHS Vital Statistics Natality Birth Data for mothers who were residents of New York and conceived April 1968-March 1972. Abortion access defined as a woman still being in her first trimester or not-yet-pregnant as of July 1, 1970. See Appendix A.2 for details. Split-side polynomial regression using a third-order polynomial, split by treatment, graphed. Robust standard errors used to create 95 percent confidence intervals. Black (gray) dashed line marks availability of first-(second-)trimester abortion.

Figure 4: The Effect of Abortion Access on Birth Outcomes, Continued

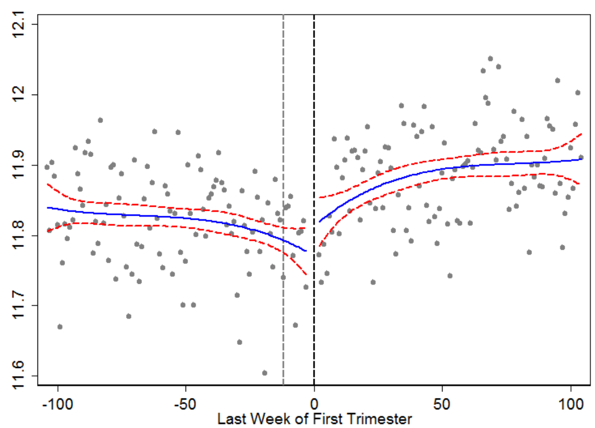
Panel A: Proportion Mother's First Birth



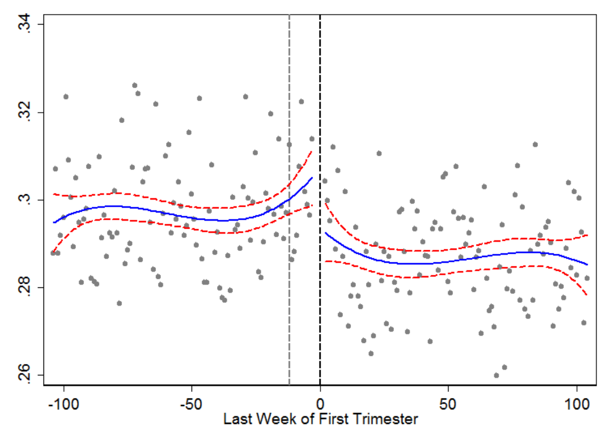
Panel B: Proportion Mother's Third Birth or More



Panel C: Mother's Years of Education



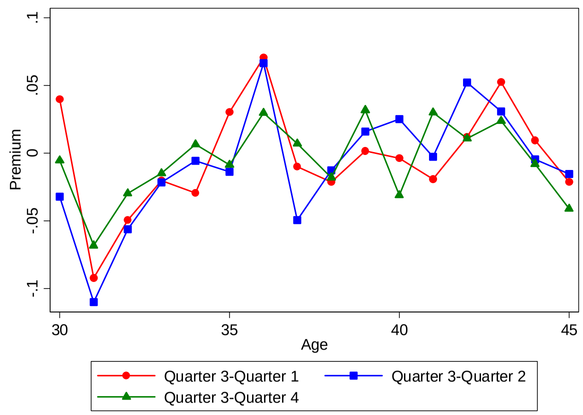
Panel D: Proportion of Mother's Without High School Degree



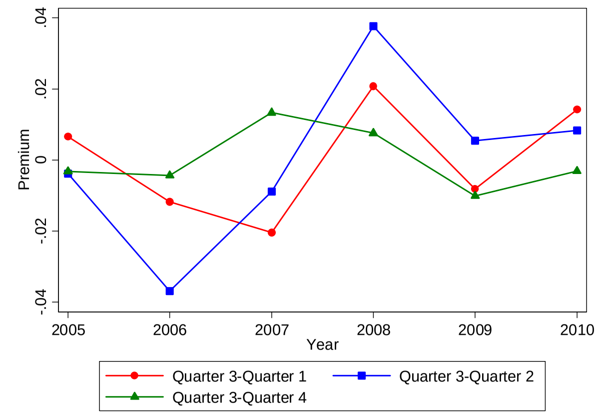
Notes and Sources: Data from NCHS Vital Statistics Natality Birth Data for mothers who were residents of New York and conceived April 1968-March 1972. Abortion access defined as a woman still being in her first trimester or not-yet-pregnant as of July 1, 1970. See Appendix A.2 for details. Split-side polynomial regression using a third-order polynomial, split by treatment, graphed. Robust standard errors used to create 95 percent confidence intervals. Black (gray) dashed line marks availability of first-(second-)trimester abortion.

Figure 5: Trends in the Quarter of Birth Premium

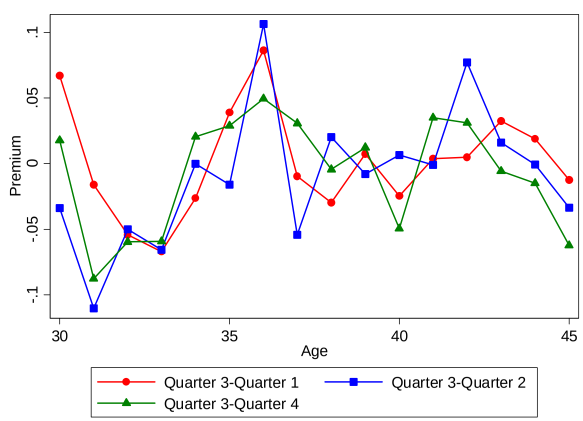
Panel A: Birth Quarter Premium Over Ages
Log(Hourly Wage)



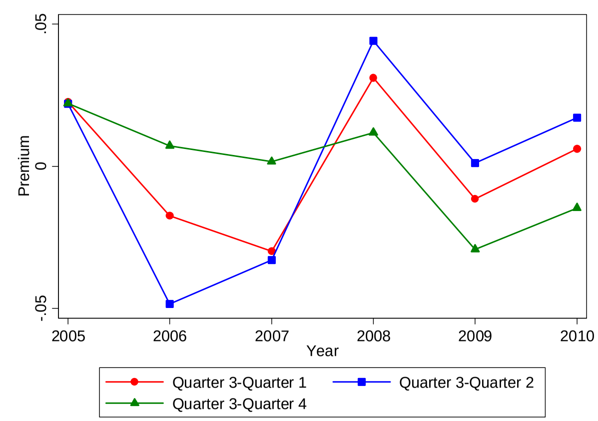
Panel B: Birth Quarter Premium Over Survey Years
Log(Hourly Wage)



Panel C: Birth Quarter Premium Over Ages
Log(Annual Earnings)



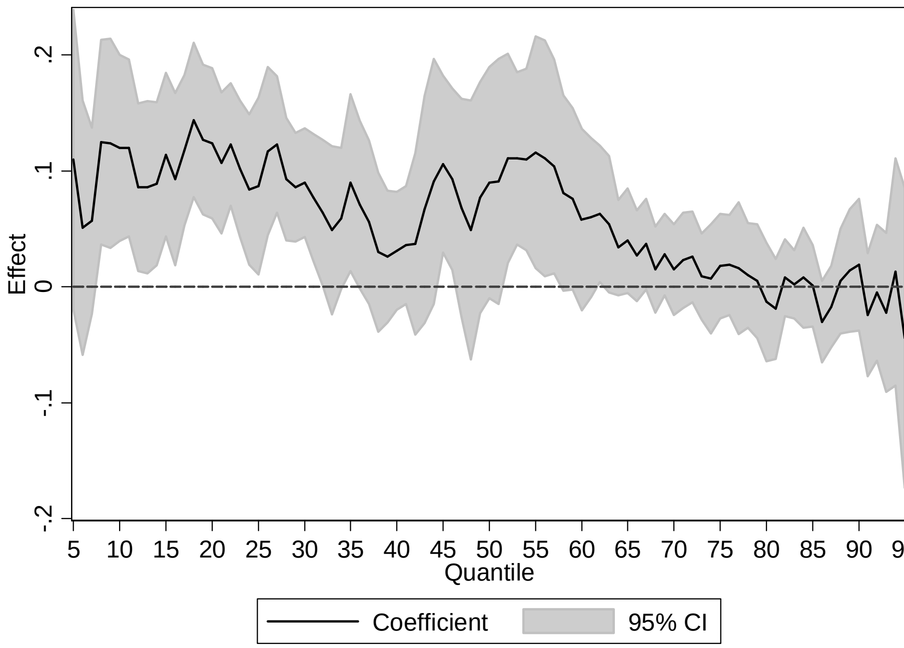
Panel D: Birth Quarter Premium Over Survey Years
Log(Annual Earnings)



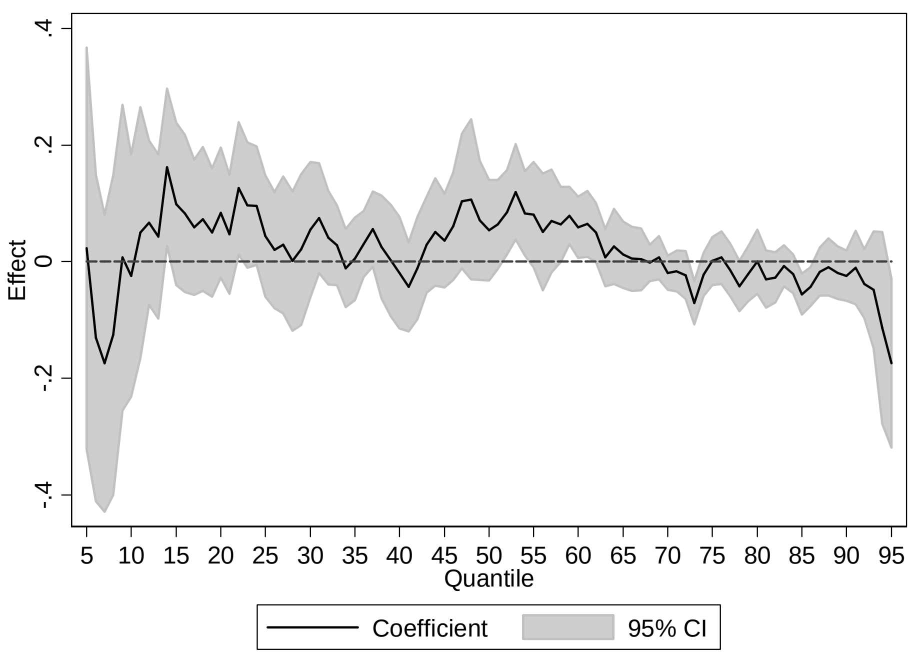
Notes and Sources: Wages from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. Sample excludes survey year-age pairs in which abortion access depends on quarter of birth. See Appendix A.3 for details. Estimates calculated using ACS sample weights. Residuals from regressions of wages or earnings on age-survey year fixed-effects, birth quarter fixed-effects, and mother's pill access.

Figure 6: Quantile Effects of Abortion Access

Panel A: Log(Wages)



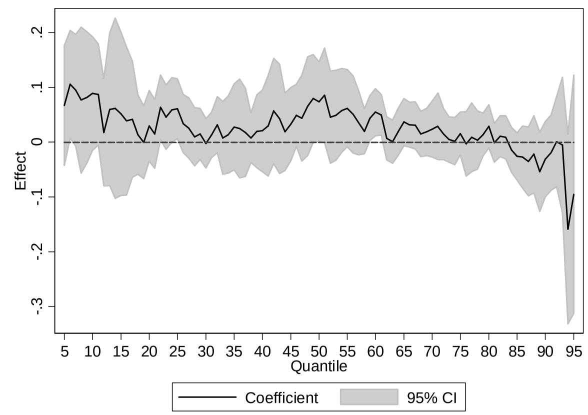
Panel B: Log(Earnings)



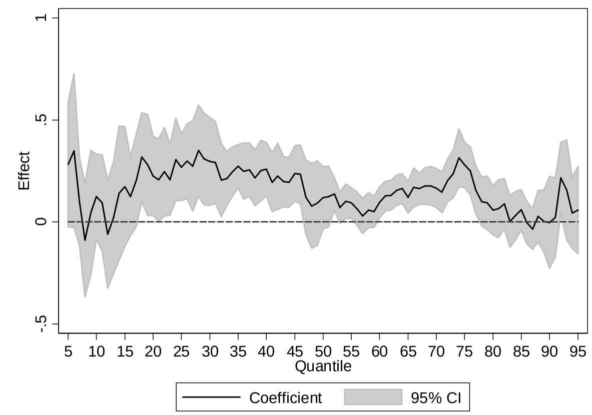
Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. See Appendix A.3 for details. Effects calculated using data collapsed to age-year-birth quarter cells (N=252). Effects from regression including controls for mother's birth control pill access, age-survey year fixed-effects, and birth quarter fixed-effects. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) used to create confidence intervals.

Figure 7: The Effect of Abortion Access Throughout the Wage Distribution, By Race

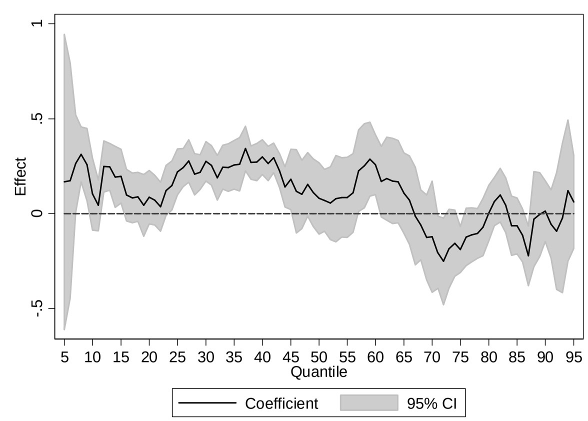
Panel A: Whites



Panel B: Blacks

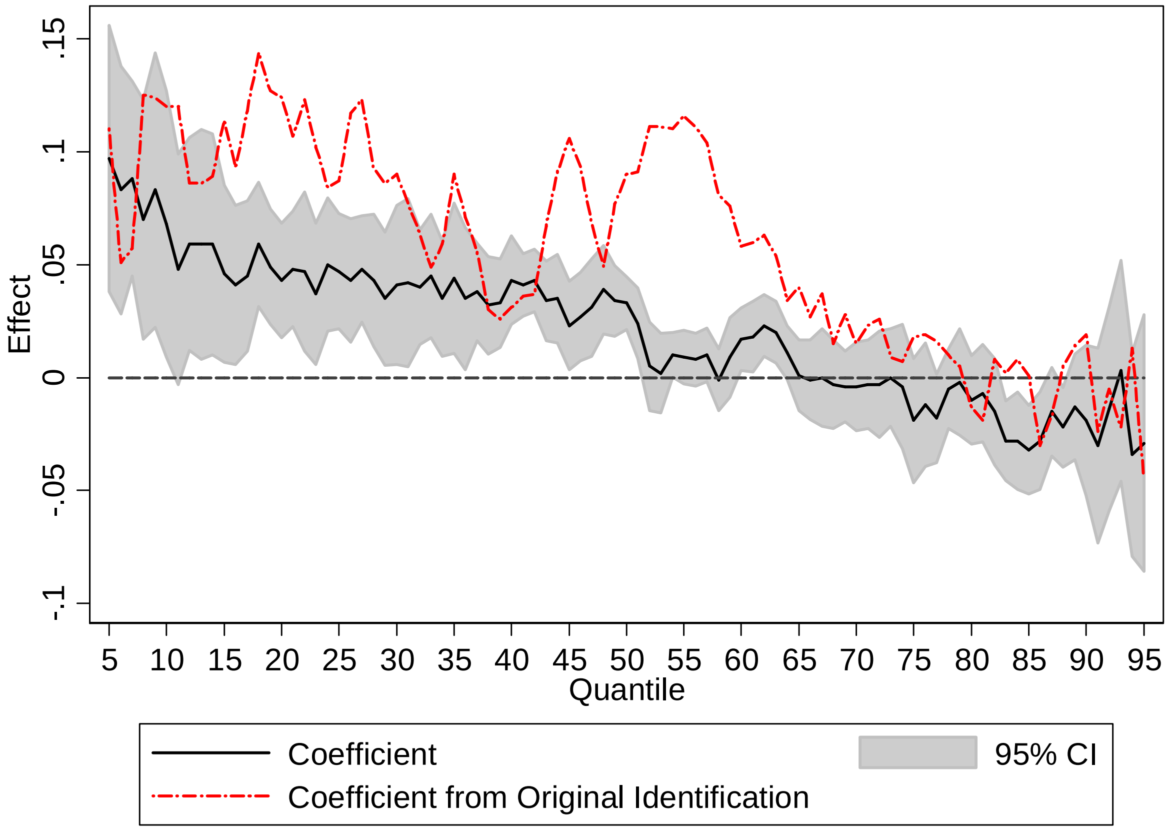


Panel C: Hispanics



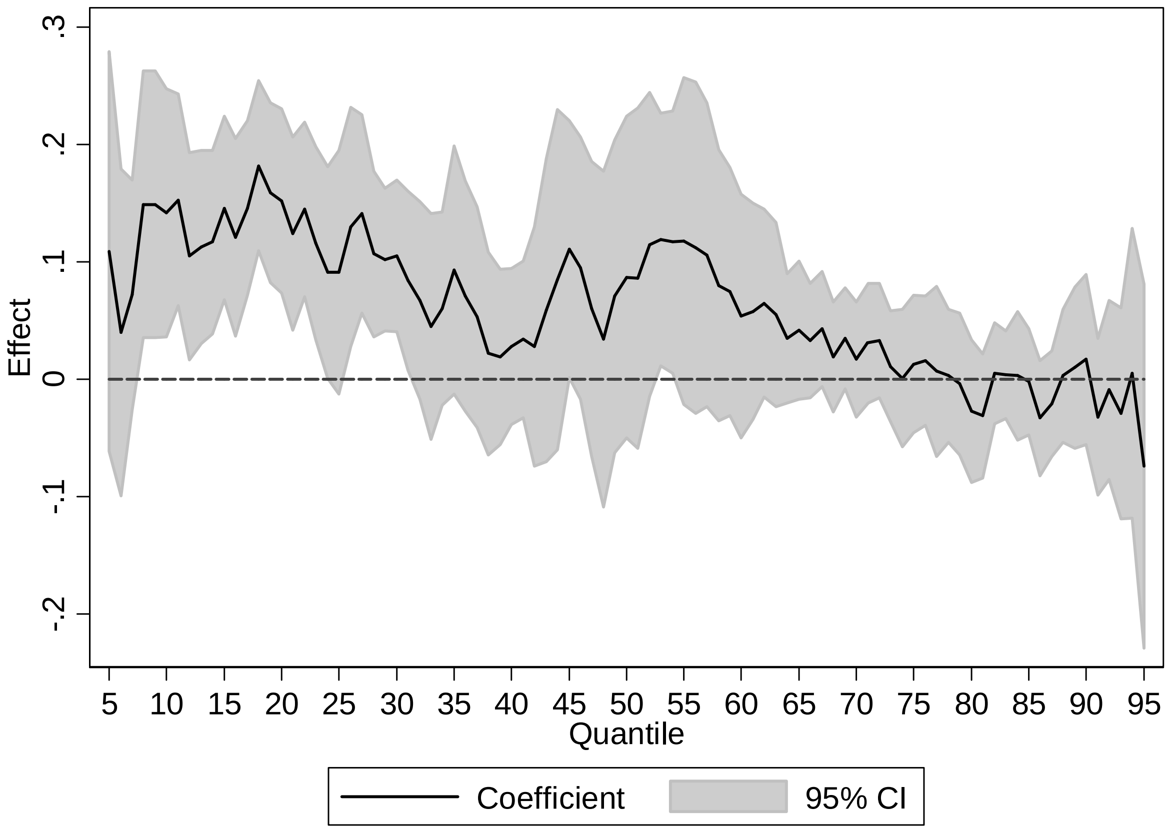
Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. See Appendix A.3 for details. Effects calculated using data collapsed to race-age-year-birth quarter cells (N=756). Effects from regression including controls for mother's birth control pill access, age-survey year fixed-effects, and birth quarter fixed-effects. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) used to create confidence intervals.

Figure 8: Longer-Run Effects of Abortion Access Throughout the Wage Distribution



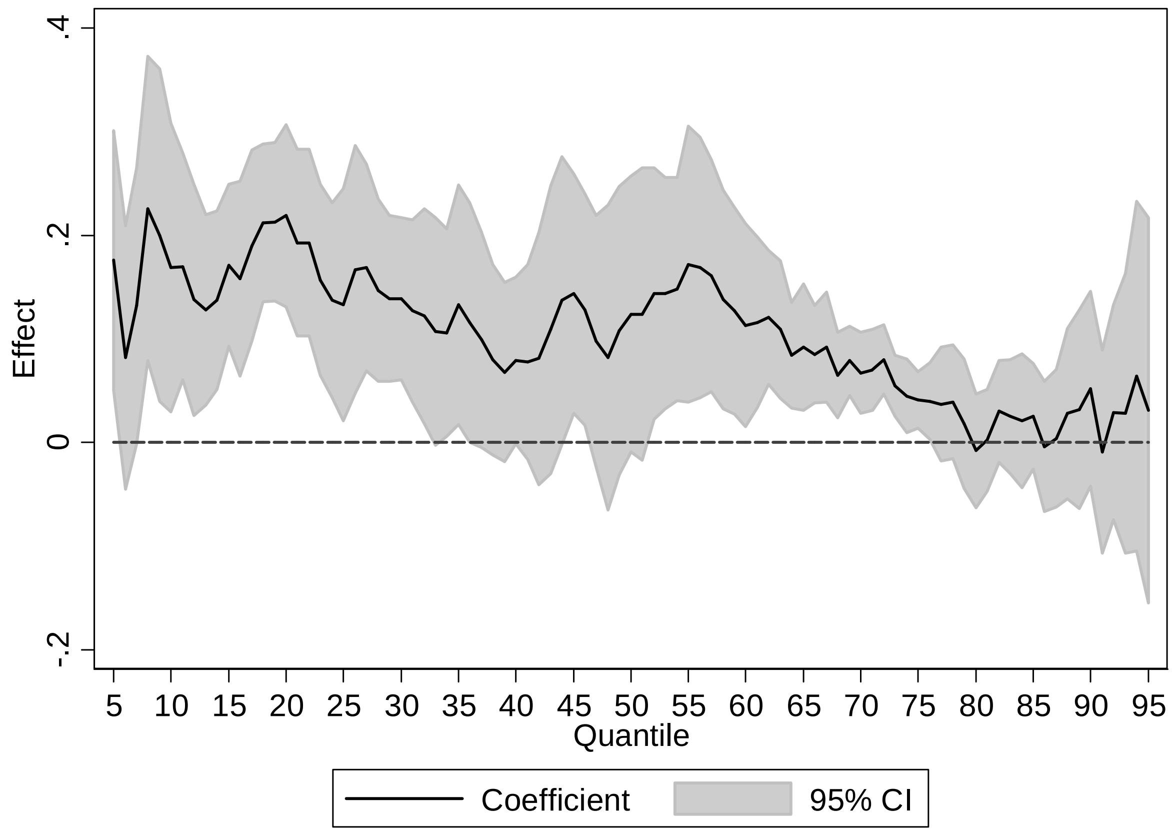
Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. See Appendix A.3 for details. Sample excludes survey year-age pairs in which abortion access depends on quarter of birth. Effects calculated using data collapsed to age-year cells (N=60). Effects from regression including controls for mother's birth control pill access and age and survey year fixed-effects. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year of birth) used to create confidence intervals. Estimates from original identification strategy from Figure 6.

Figure A1: The Effect of Abortion Access Throughout the Wage Distribution
Calculated Using Actual Abortion Access from Birth Certificate Data



Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. Abortion access calculated from birth certificate data. See Appendices A.3-A.4 and Table A1 for details. Effects calculated using data collapsed to age-year-birth quarter cells (N=252). Effects from regression including controls for mother's birth control pill access, age-survey year fixed-effects, and birth quarter fixed-effects. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) used to create confidence intervals.

Figure A2: The Effect of Abortion Access Throughout the Wage Distribution
Calculated Using Workers Born 1968 to 1972 Only



Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1968-1972. See Appendix A.3 for details. Effects calculated using data collapsed to age-year-birth quarter cells (N=120). Effects from regression including controls for mother's birth control pill access, age-survey year fixed-effects, and birth quarter fixed-effects. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) used to create confidence intervals.

Table 1: Estimates of the Effect of Abortion Access from Birth Certificate Data

Panel A: All Births

Method	Weekly Births	Father Ed Missing	Low Birth Weight	Black	Mother's 1st Birth	Mother's 3rd+ Birth	Mother's Yrs of Ed	Mother HS Dropout
Diff.-in-Diff.	-692.0***	-0.0191***	-0.00193	-0.00859***	-0.00134	-0.00592	0.0415***	-0.00853***
	[176.9]	[0.00220]	[0.00144]	[0.00237]	[0.00459]	[0.00370]	[0.0146]	[0.00235]
RD: Split Polynom.	-554.8***	-0.00957***	-0.00891***	-0.0190***	0.00255	-0.0227***	0.0424	-0.0137**
	[169.2]	[0.00367]	[0.00337]	[0.00463]	[0.00550]	[0.00609]	[0.0293]	[0.00563]
RD: Local Linear	-748.0***	-0.00921***	-0.00559*	-0.0138***	0.00803*	-0.0168***	0.0184	-0.00690
(12 Weeks)	[221.1]	[0.00318]	[0.00292]	[0.00378]	[0.00469]	[0.00520]	[0.0250]	[0.00446]
RD: Local Linear	-1,049.6***	-0.0137 ***	-0.00639***	-0.0148***	0.000907	-0.0147***	0.0496***	-0.0126***
(24 Weeks)	[218.1]	[0.00212]	[0.00202]	[0.00269]	[0.00327]	[0.00363]	[0.0176]	[0.00322]
Mean	5,523	0.101	0.0852	0.177	0.313	0.380	11.85	0.293
Observations	205	568,764	564,835	568,764	536,421	536,421	543,895	543,895

Panel B: Black Births

Method	Weekly Births	Father Ed Missing	Low Birth Weight	Black	Mother's 1st Birth	Mother's 3rd+ Birth	Mother's Yrs of Ed	Mother HS Dropout
Diff.-in-Diff.	-165.0***	-0.0355***	-0.000283		0.0046	-0.0199**	0.0918***	-0.00912
	[32.63]	[0.00611]	[0.00403]		[0.0103]	[0.00828]	[0.0244]	[0.00591]
RD: Split Polynom.	-209.5***	-0.003	-0.0263***		0.0285**	-0.0413***	0.00593	-0.00334
	[57.28]	[0.0130]	[0.00986]		[0.0123]	[0.0136]	[0.0575]	[0.0144]
RD: Local Linear	-298.7***	-0.0104	-0.0171**		0.0150	-0.0226*	0.0366	-0.00375
(12 Weeks)	[83.67]	[0.0111]	[0.00860]		[0.102]	[0.0120]	[0.0475]	[0.0112]
RD: Local Linear	-360.9***	-0.0182**	-0.0135**		0.0185***	-0.0283***	0.0446	-0.00272
(24 Weeks)	[71.95]	[0.00815]	[0.00609]		[0.00707]	[0.00882]	[0.0341]	[0.00811]
Mean	978	0.294	0.144		0.284	0.425	11.12	0.459
Observations	205	100,438	99,783		91,001	91,001	95,581	95,581

Notes and Sources: Data from NCHS Vital Statistics Natality Birth Data for mothers who are residents of New York and conceived April 1968-March 1972. Abortion access defined as a woman still being in her first trimester or not-yet-pregnant as of July 1, 1970. See Appendix A.2 for details. Difference-in-differences estimates include month of conception and year of conception fixed-effects and mother's access to the pill at conception. Standard errors clustered by month and year of conception. Split-side polynomial regression uses a third-order polynomial, split by treatment. Local linear regression estimated using Gaussian kernel, standard errors estimated using 500 bootstrap replications. Robust standard errors in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2: Identification of the Probability a Person's Mother Had Legal Access to Abortion

Panel A: Amy, Age 34, Born in Quarter 1, Pr(Mother Had Legal Access)=0.875

	Quarter of Year Interviewed				
	1	2	3	4	Overall
Probability of Interview During Quarter	0.25	0.25	0.25	0.25	1.00
If interviewed this quarter...					
...probability already had birthday	0.50	1.00	1.00	1.00	0.875
...expected age	34.50	34.25	34.50	34.75	34.50
...probability born in 1971	0.50	1.00	1.00	1.00	0.875
...probability born in 1970	0.50	0.00	0.00	0.00	0.125

Panel B: Bob, Age 34, Born in Quarter 2, Pr(Mother Had Legal Access)=0.625

	Quarter of Year Interviewed				
	1	2	3	4	Overall
Probability of Interview During Quarter	0.25	0.25	0.25	0.25	1.00
If interviewed this quarter...					
...probability already had birthday	0.00	0.50	1.00	1.00	0.625
...expected age	34.75	34.50	34.25	34.50	34.50
...probability born in 1971	0.00	0.50	1.00	1.00	0.625
...probability born in 1970	1.00	0.50	0.00	0.00	0.375

Notes: Author's calculations based on the assumptions that birthdays and interviews are evenly distributed across and within quarters and that gestation lasts exactly nine months. See Appendix A.4 for a discussion of these assumptions.

Table 3: The Effect of Legal Access to Abortion on Earnings and Wages

Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log(Earnings)						
Mother Had Access to Abortion	0.00583	0.00575	-0.029	-0.0325	0.0317	-0.0159
	[0.0465]	[0.0470]	[0.0397]	[0.0351]	[0.0626]	[0.0473]
Mother Had Access to Abortion*Black			0.147**	0.0875		
			[0.0646]	[0.0952]		
Mother Had Access to Abortion*Hispanic			0.0492	-0.046		
			[0.121]	[0.124]		
Mother Had Access to Abortion*Female				-0.147	-0.0313	-0.0273
				[0.126]	[0.0533]	[0.0489]
Panel B: Log(Wage Rate)						
Mother Had Access to Abortion	0.0394	0.0429*	-0.0023	-0.00887	0.0135	0.00668
	[0.0241]	[0.0255]	[0.0223]	[0.0182]	[0.0281]	[0.0377]
Mother Had Access to Abortion*Black			0.168***	0.129***		
			[0.0336]	[0.0474]		
Mother Had Access to Abortion*Hispanic			0.120***	0.0361		
			[0.0424]	[0.0303]		
Mother Had Access to Abortion*Female				-0.147	0.0883***	0.0879**
				[0.126]	[0.0279]	[0.0333]
Pill Access Control		Y	Y	Y	Y	Y
Fixed-effects by Race			Y	Y		
Fixed-effects by Gender					Y	Y
Individual Controls				Y		Y
Observations	95,314	95,314	95,314	95,314	95,314	95,314

Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. See Appendix A.3 for details. All specifications include age-survey year and birth quarter fixed-effects. Individual controls include race, gender, and quadratics for years of education and potential experience. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 4: Mother's Abortion Access and Other Outcomes

	All Individuals			Wage Sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Var.	Effect	Effect	Mean	Effect	Effect	Mean
Black	-0.00135		0.141	-0.0205**		0.121
	[0.00925]			[0.00843]		
White	0.0497***		0.751	0.0568***		0.774
	[0.00936]			[0.0174]		
Years of Ed	0.174**	0.120*	14.21	0.268***	0.221***	14.52
	[0.0644]	[0.0595]		[0.0887]	[0.0763]	
High School	-0.0281***	-0.0238***	0.0629	-0.0260***	-0.0234***	0.0374
Dropout	[0.00820]	[0.00766]		[0.00478]	[0.00444]	
Single Parent	-0.0209**	-0.0151*	0.127	-0.0506***	-0.0426***	0.124
	[0.00882]	[0.00894]		[0.00973]	[0.00868]	
Poor	0.00173	0.00625	0.0976	-0.0320***	-0.0295***	0.0431
	[0.00591]	[0.00544]		[0.00714]	[0.00735]	
Receives SSI	-0.0108*	-0.0088	0.0356			
or TANF	[0.00645]	[0.00639]				
Unemployed	-0.00383	-0.00254	0.0486			
	[0.0168]	[0.0168]				
Working	0.0207*	0.0177	0.787			
	[0.0111]	[0.0110]				
Ln(Yearly Hours)	0.0221	0.0222	7.459	-0.0341	-0.0351	7.492
	[0.0333]	[0.0333]		[0.0260]	[0.0259]	
Race Controls		Y			Y	

	All Black Males		
Working	0.138***		0.695
	[0.0301]		
Institutionalized	0.0142		0.0822
	[0.0168]		

Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975 (N=143,254; with 95,314 workers and 6,914 black males). See Appendix A.3 for details. All specifications include age-survey year and birth quarter fixed-effects and a variable indicating mother's pill access at conception. Estimates calculated using ACS sample weights. Robust standard errors clustered by birth cohort (expected year and quarter of birth) in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: The Effect of New York's Abortion Reform on Log(Wages) for Those Born in Other States

	New York		Top Ten Sending States	
	Below Median Wage	Above Median Wage	Below Median Wage	Above Median Wage
Mother Had Access to Abortion	0.0980***	0.0177	0.0463***	0.0278***
	[0.0139]	[0.0123]	[0.0129]	[0.00336]
Observations	39,788	55,525	121,165	134,620

	Non-Border States		Bottom Ten Sending States	
	Below Median Wage	Above Median Wage	Below Median Wage	Above Median Wage
Mother Had Access to Abortion	0.0269***	0.00286	-0.00384	0.0288
	[0.00885]	[0.00472]	[0.0209]	[0.0306]
Observations	483,218	438,329	42,312	35,959

Notes and Sources: Data from whites, blacks, and Hispanics in the ACS 2005-2010, born 1964-1975. See Appendix A.3 for details. Regressions include age-survey year and birth quarter fixed-effects. Robust standard errors clustered by birth cohort (expected year and quarter of birth) and state (when using multiple states) in brackets. Two-way clustering calculated as in Cameron, Gelbach, and Miller (2006). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Top 10 Sending States: CT, FL, MA, ME, MI, NH, NJ, OH, RI, VT.

Bottom Ten Sending States: AR, AZ, ID, KS, MT, NM, NV, OR, SD, UT. States that legalized abortion before 1973 excluded.

Table A1: Mother's Actual and Predicted Abortion Access

		Proportion with 1st Trimester Access	
Age in 2005	Quarter of Birth	Actual Access	Predicted Access
...			
32	3	1	1
32	4	1	1
33	1	0.996	1
33	2	1	1
33	3	1	1
33	4	1	1
34	1	0.750	0.875
34	2	0.609	0.625
34	3	0.349	0.375
34	4	0.145	0.125
35	1	0	0
35	2	0	0
35	3	0.0001	0
35	4	0.002	0
36	1	0	0
			...

Notes and sources: Actual probability of access from the NCHS Vital Statistics Natality Birth Data for mothers who were residents of New York and gave birth from 1968 to 1975. See Appendices A.2 and A.4 for details. Abortion access defined as a woman still being in her first trimester or not-yet-pregnant as of July 1, 1970. Predicted probability from author's calculations based on the assumptions that birthdays are evenly distributed across and within quarters and that gestation lasts exactly nine months.