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ABSTRACT

The Impact of Legal Abortion on Maternal Mortality*

Legal abortion has recently been suggested as an essential healthcare service. In this study, we consider whether abortion legalization over 1969-1973 improved women's health, measured by maternal mortality. Our event-study results indicate that legal abortion substantially lowered non-white maternal mortality by 30-40%, with 113 non-white maternal deaths averted nationally in the first year abortion became legal. We also find that early state-level legalizations were crucial, and more influential than the Roe v. Wade decision itself.

JEL Classification: I18, J13, K38, H75

Keywords: abortion, legal abortion, maternal mortality, Roe v. Wade, maternal health

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1 Introduction

The legal status of abortion remains a contentious topic of debate in the United States. A critical issue in this debate is whether legal abortion impacts health outcomes, specifically through maternal mortality. Causal evidence suggests legal abortion reduces maternal morbidity in Mexico (Clarke and Mühlrad, 2021), but existing studies in the United States are primarily correlational in nature.[1] As debate continues over the legal landscape of abortion rights in the United States, understanding the relationship between legal access to abortion and U.S. maternal health is important.[2]

In this study, we consider whether the legalization of abortion in the United States impacted maternal and abortion-related mortality over 1959-1980. We study state-level abortion legalizations over 1969-1971 as well as the national *Roe v. Wade* decision in 1973. While today, direct abortion deaths account for a small portion of maternal deaths,[3] in 1960, abortion was a major cause of maternal mortality (NCHS, 1960), and even more important for nonwhite women. In the early 1960s in New York City, "one in four childbirth-related deaths among white women was due to abortion; in comparison, abortion accounted for one in two childbirth-related deaths among nonwhite and Puerto Rican women" (Gold, 2003, pg. 10).[4] And while abortions of the nineteenth and early twentieth-century had been quite dangerous, in 1973 when *Roe v. Wade* was decided, the Supreme Court itself noted that legal abortion "is now relatively safe" (Roe v Wade, 1973). In fact, the Court documents emphasize that "mortality rates for women undergoing early abortions, where the procedure is legal, appear to be as low as or lower than the rates for normal childbirth" (Roe v Wade, 1973).

For our primary analysis, we focus on states that repealed their criminal abortion laws and provided legal abortion access, including five states plus the District of Columbia (DC), as well as the *Roe v. Wade* decision in 1973.[5] Using an event-study design, we assess whether

[1]Studies include Tietze (1975, 1984); Cates et al. (1978b); Tietze and Lincoln (1987); Coble et al. (1992); CDC (1999).

[2]These legal changes involved reforming and repealing state-level anti-abortion legislation from the 1800s. Prior to the 1800s-era laws, English common law dictated that abortion was legal until quickening (the first fetal movement felt by the mother) (Gold, 2003; Lahey, 2014a,b; Myers, 2021a).

[3]Over the period 1998-2010, out of "16.1 million abortion procedures, 108 women died, for a mortality rate of 0.7 deaths per 100,000 procedures" (Zane et al., 2015).

[4]The health risks associated with abortion procedures during the pre-legalization period mostly ensued from the clandestine and hazardous nature of illegal abortions (Taussig, 1936; Reagan, 1997). The repression of abortion in the 1950s and 1960s drove the provision of abortion underground, as is characteristic of criminalized activities (Jaros, 2012; Morrison, 2013). This made the procedure "harder to obtain, more expensive, and more dangerous" (Reagan, 1997, pg.193). Illegal *black market* provision of abortion was executed mainly by inadequately trained personnel, under substandard sanitary conditions, and with fewer opportunities for follow-up care (Taussig, 1936; Reagan, 1997; Jaros, 2012).

[5]We refer to state legal status as (1) "repeal/early-legal states" if the state repealed their anti-abortion laws (i.e., adopting full abortion legalization), (2) "reform" if the states allowed abortion only under certain conditions, and (3) *Roe v. Wade* if the state allowed elective abortion only after the 1973 national decision (Myers, 2021a).

the changes to legal abortion impacted maternal and abortion-related mortality (and to what extent). Throughout our results, we rely on both a two-way fixed effects (TWFE) specification, and the Interaction-Weighted (IW) estimator from Sun and Abraham (2020).[6]

The event-study results show that legal abortion reduced non-white abortion-related mortality by 30-60% and non-white maternal mortality by 30-40%. The estimated percentage decline translates into 41 non-white maternal deaths averted in early-legal states and 113 non-white maternal deaths averted nationally in the first year after the passage of legal abortion. Despite the beneficial effects of legal abortion on non-white mortality, our main findings suggest little impact of legal abortion on white mortality. Instead, white abortion-related mortality appears to have been on a preexisting decline before national legalization. This pre-legalization decline aligns with historical narratives, suggesting that white women were better poised to navigate the medical system and obtain a therapeutic abortion (an abortion to prevent medical harm) before formal legalization (Law et al., 1989; Rubin, 1994), or to travel to obtain a legal abortion (Law et al., 1989; Rubin, 1994; Gold, 2003; Joyce et al., 2013). For these reasons, over "1972 to 1974, the mortality rate due to illegal abortion for nonwhite women was 12 times that for white women" (Gold, 2003, pg. 4). Put together, our findings indicate that legal access to abortion was crucial for non-white women, who struggled to obtain a safe abortion before the procedure was legal (Coble et al., 1992; Gold, 2003; Joyce et al., 2013; Solinger, 2017).

Then, we show that the main impact of legal abortion on non-white maternal mortality is robust to a battery of checks. Though, we document one surprising result, the main mortality impact of legal abortion arises primarily from early legalizations. When we separately test the impact of *Roe v. Wade*, we find a less noticeable effect of the national decision. A portion of this smaller effect in *Roe v. Wade* states may be explainable by differing characteristics from early-legal states. For example, *Roe v. Wade* states were more religious, had lower abortion rates, and had lower incomes and educational attainment. While we control for state-level income and education in our main results, there still may be concern over unobservables conflating our estimates for early-legal states. To assess this concern, we follow Oster (2019) and show that the selection on unobservables would have to be almost nine times the selection on observables to explain away the observed impact on abortion-related mortality in early-legal states. To explain away the impact on non-white maternal mortality, selection on unobservables would have to be roughly twice the size of the selection on observables. These maternal mortality estimates are considered robust according to the bounds set in Oster (2019).

We then conclude by considering various mechanisms behind the decline in non-white maternal mortality. Importantly, we demonstrate that the reduction in maternal mortality

[6]The IW estimator compares early-legal states to *Roe v. Wade* states, while the TWFE specification considers the impact of legal abortion in all states.

was due to a change in abortion-related mortality, with other causes of maternal death failing to decline. We also show that counties at or below the state-level median in education, income, and health resources experienced greater benefits from legal abortion. Together, these results reveal a consistent story; disadvantaged groups benefited the most from legal access to abortion. This finding compliments existing studies, that have similarly shown that abortion restrictions impact the less advantaged (e.g., Gold (2003); Joyce et al. (2013); Myers (2017)).

2 Related Literature

Our findings show that early legal abortion substantially improved maternal health for non-white women. Our results add to previous correlational studies considering the impact of abortion on maternal health (Tietze, 1975, 1984; Cates et al., 1978b; Legge, 1985; Tietze and Lincoln, 1987; Coble et al., 1992; CDC, 1999; Cates et al., 2003).[7] Only a few studies have considered the *causal* impact of legal abortion on maternal mortality, with the exception of recent studies set in Mexico (Betancourt, 2017; Clarke and Mühlrad, 2021).[8]

Clarke and Mühlrad (2021) considers the impact of legal abortion (in the first trimester) in the Federal District of Mexico, as well as the increased sanctions on abortion in other Mexican states. Clarke and Mühlrad (2021) finds that legal abortion in the Federal District led to a decline in maternal morbidity (with some evidence for declines in maternal mortality), while sanctions on abortion in other Mexican states produced only weak impacts on maternal health. Our findings add to Clarke and Mühlrad (2021) by being one of the first studies to consider the causal impact of national legal abortion, through both initial state-level legalizations, and the subsequent national legalization through *Roe v. Wade*. Our study also demonstrates important heterogeneous impacts in access to safe and legal abortion by race for the U.S. context.

These findings also add to a broad literature studying the effects of access to fertility control through the pill (Goldin and Katz, 2002; Bailey, 2006; Ananat et al., 2007; Bailey, 2010; Bailey et al., 2012; Zuppann, 2012; Steingrimsdottir, 2016; Myers, 2017) and legal abortion (Bitler and Zavodny, 2001; Hock et al., 2007; Oreffice, 2007; Guldi, 2008; Myers, 2017). Legal access to abortion during the late-1960s and early-1970s has been shown to impact a variety of socio-economic outcomes, including family formation, fertility, crime, schooling, and female

[7] Studies also present evidence of fiscal and legislative restrictions on access to family planning and reproductive health services as contributing to rising maternal morbidity and mortality (Jarlenski et al., 2017; Hawkins et al., 2020; Verma and Scott, 2020; Addante et al., 2021).

[8] Dow and Ronan (1997) is an unpublished manuscript studying the impact of legal abortion on mortality in the United States. Dow and Ronan (1997) focuses on the misclassification of abortion-related mortality into broader female mortality and uses slightly different methods and dates of legalization. In the present study, we focus on a narrower definition of mortality, and document important heterogeneity by race–where legal abortion is essential for non-white maternal and abortion-related mortality. We also use recent innovations in difference-in-differences to unpack the main effect.

labor force participation (Angrist and Evans, 1996; Levine et al., 1999; Donohue and Levitt, 2001; Kalist, 2004; Ananat et al., 2004; Guldi, 2008; Foote and Goetz, 2008; Donohue and Levitt, 2008; Ananat et al., 2009; Lahey, 2014b; Myers, 2017; Jones et al., 2021).[9]

Notably, Myers (2017) demonstrates that legal abortion was of primary importance for demographic changes over the 1960s and 1970s, an impact that had previously been ascribed to the oral contraceptive pill. Myers (2017) uses a difference-in-difference methodology to show that legal abortion substantially reduced the likelihood of becoming a mother or experiencing a shotgun marriage before age 19. Our findings add to Myers (2017) by showing that legal abortion also changed maternal health outcomes, especially non-white maternal health.

A portion of our findings also relate closely to Joyce et al. (2013). Joyce et al. (2013) considers travel to New York state for abortion over 1971-1975, and shows that women traveled hundreds of miles for legal abortion in the period before *Roe v. Wade*. The groups most affected by travel distance were teenagers and non-white women. Joyce et al. (2013)'s findings align with our results, as Joyce et al. (2013) concludes that *Roe v. Wade* was less impactful for unintended childbearing than early legal access to abortion in California, New York, and DC.

3 Background

3.1 Maternal Mortality in the 20th Century

From 1900 to 1982, maternal mortality declined substantially. At the beginning of the twentieth century, between six and nine mothers died for every 1,000 births (CDC, 1999). By 1982, the U.S. maternal mortality rate had declined to 7.5 deaths per *100,000 live births* (Koonin et al., 1988). This 100-fold decline in maternal mortality over the course of 80 years has been attributed to several achievements in medical technology and public health, with legal abortion also suggested as important (Cates et al., 1978b) .[10]

In 1960, at the start of our study, maternal mortality was 97.1 per 100,000 births for non-white mothers and 26 per 100,000 for white women (NCHS, 1960).[11] Over 1960 to 1980, legal abortion has been suggested as a major contributor to the decline in maternal deaths, primarily from abortion-related sepsis (Coble et al., 1992; CDC, 1999). In the pre-legalization

[9] Recent work has also focused on the contemporary supply-side restrictions in abortion access (Fischer et al., 2018; Lindo et al., 2020), parental consent and notification requirements (Kane and Staiger, 1996; Levine, 2003; Klick and Stratmann, 2008; Sabia and Rees, 2013; Sabia and Anderson, 2016; Myers and Ladd, 2020); and mandatory waiting period requirements (Joyce et al., 1997; Lindo and Pineda-Torres, 2021; Myers, 2021b).

[10] For a discussion of the maternal mortality decline before 1960, see Appendix Section G.

[11] 1960 was also the first year that maternal deaths were reported per 100,000 births, indicating a significant decline in the maternal mortality rate (NCHS, 1960).

era, deaths caused by infection from criminal abortion, "which often do not respond to the antibiotics" (Klein and Clahr, 1958, pg. 237), were common. One account from an obstetrician who practiced in New York and Pittsburgh (before 1967) describes that the "complications of illegal abortion were so common that a septic ward was set aside for the infections. Surgery for hemorrhage was a common night duty" (Rubin, 1994, pg. 76).

As abortion became legal and available, the number of septic abortions declined. A single medical center in California reported that while the number of legal abortions from 1966 to 1971 increased from zero to three thousand (per year), the number of septic abortions dropped from 646 to under 150 (per year) (Seward et al., 1973). Septic abortion deaths also declined. Atrash et al. (1987) reports that deaths from legal abortion dropped "from 4.1 per 100,000 abortions in 1972 to 0.8 in 1982." Further, CDC (1999) reports that "the legalization of induced abortion beginning in the 1960s contributed to an 89% decline in deaths from illegal septic abortions during 1950-1973."

However, legal abortion was not the sole change during this period, and other potential drivers of the decline exist. For example, during the 1960s and 1970s, perinatal care was regionalized and specialized, producing advancements in identifying and monitoring high-risk pregnancies (McCormick et al., 1985; Cutler and Meara, 2000; Rowe and Rowe, 2000). Further, advancements in the method of inducing abortions also occurred. Between 1972 and 1981 vacuum aspiration replaced sharp curettage procedures (Kleinman and Senanayake, 1993; Rubin, 1994).[12] This improvement in technology resulted in safer and less expensive abortions (Kleinman and Senanayake, 1993). Combined, these factors all likely contributed to the profound reduction in maternal mortality from 1960-1980.[13]

By the 1980s, abortion was no longer a major cause of maternal death (Lawson et al., 1994), a notable change from earlier decades. During the 1980s, the leading causes of maternal death shifted to "embolism, indirect causes, hypertension in pregnancy, sequelae from ectopic pregnancy, hemorrhage, stroke, and anesthesia-related complications" (Koonin et al., 1988), with abortion and sepsis from abortion absent. By 1982, maternal mortality reached its lowest point and held steady at 7.5 maternal deaths per 100,000 live births (Koonin et al., 1988). Since 1982, maternal mortality has not continued to decline (CDC, 1998, 1999).[14]

[12]Ideally, we would examine the contribution of this technological change to abortion-related mortality. However, data covering abortion methods by state are only available starting in 1971 (and only nine states reported in 1971). Because most of the decline in mortality occurred before 1971, the limited data do not allow us to exploit variation in the change in technology.

[13]Other important drivers do exist. For example, the pill was another major innovation over this period, which we explore in robustness checks. A second notable change was the passage of the Civil Rights Act in 1964 and the onset of federal hospital desegregation campaigns. While there was a noticeable upturn in hospital births among black mothers post-1965 (Chay and Greenstone, 2000); Anderson et al. (2020a) found little evidence of the federal hospital desegregation campaign appreciably accelerating the trend towards in-hospital births among Southern Black mothers or significantly explaining the decline in black maternal mortality.

[14]Maternal mortality declined by roughly 8% per year over 1935 to 1982, and stayed at similar levels from 1982

3.2 Abortion Legislation in the 1960s and 1970s

In 1967, abortion was a felony in the 49 states and DC (Lewis et al., 1981), though 42 states had legal exceptions to preserve the mother's life. Certain state-level legislation also had looser language, which left enforcement open to interpretation, enabling doctors and courts more scope for the application of abortion (Lewis et al., 1981).[15] States began reforming and repealing their anti-abortion legislation in the mid-to-late 1960s (see Table H.3 and H.4). The "repeal" (early-legal) states removed their criminal abortion laws and passed clear legal abortion from 1969-1971. Repeal states made explicit that the decision to obtain an abortion was a matter for the woman in consultation with her physician (Roemer, 1971). Repeal states include California in 1969 and Alaska, Hawaii, New York, and Washington in 1970 (Roemer, 1971; Merz et al., 1996; Myers, 2021a).[16] The District of Columbia also allowed legal abortion beginning in 1971 (based on a court case), with formal abortion outpatient clinics available prior to *Roe v. Wade* (CDC, 1972; Myers, 2021a).[17]

Unlike the broad legalizations in repeal states, "reform" states changed their criminal abortion laws over the 1960s and 1970s (see Table H.4), making abortion accessible only under specific circumstances. Mississippi was the first state to pass an abortion reform, a relatively weak one, which legalized abortion only in the case of rape (Merz et al., 1996). Thirteen other states subsequently adopted abortion provisions from the Model Penal Code (MPC).[18] MPC provisions mandated the abortion procedure to be performed by a licensed physician, and decriminalized abortion procedures in cases of:

1. A pregnancy threatening the mother's physical or mental health
2. When a fetus had a serious defect (physical or mental)
3. A pregnancy that occurred due to rape or incest

Last of the reform states, Vermont and New Jersey, both had court rulings overturning their abortion laws in 1972. However, these states had less clear allowances for legal abortion

to 1999. In 1999, maternal mortality began to increase, which has partially been attributed to a change in the ICD-10 classification of maternal deaths (Singh, 2010, pg. 2). Since 2007, a checkbox for pregnancy has also been added, leading to additional maternal deaths being counted, which otherwise would have been in other causes of death (Singh, 2010, pg. 2).

[15]For example, legislative loopholes included the allowance of abortions to save a woman from "serious and permanent bodily injury," or to preserve her "life and health" (Lewis et al., 1981). Three remaining states also allowed abortions that were not "unlawfully performed" or that were not "without lawful justification", leaving interpretation of those standards to the courts. (Lewis et al., 1981, pg. 2).

[16]Viewpoints differ on whether California repealed its then existing anti-abortion laws in 1969 or 1970. To address this, we consider CA coded as 1970 in the difference-in-differences specification in Section 7.6.

[17]DC had the second-highest abortion-to-live birth ratio in the 1970s, as reported by place of occurrence in CDC (1970). 1970's data is based on voluntary information from all but two hospitals in DC (CDC (1970) Table 2).

[18]These MPC statutes were proposed by American Law Institute (ALI) in their 1962 publication "Model Penal Code on Abortion." See Merz et al. (1996); Roemer (1971); Reagan (1997) for more details on MPC provisions.

following the court rulings.[19] Due to these court rulings in New Jersey and Vermont, we code them as abortion reform states, following Myers (2021a).[20]

Finally, after these state-level reforms and legalizations, the *Roe v. Wade* decision on January 22, 1973, overturned restrictive abortion laws, legalizing abortion throughout the United States. Figure 1 summarizes these legal changes, showing the three major legal status categories. Green shows repeal states, yellow shows reform states, and orange shows *Roe v. Wade*-only states.

3.3 Pre-legalization Access to Abortion

Women were able to obtain abortions before national legalization through illegal abortions, travel to early-legal states, international travel, and by appealing to physicians (in their own residence state) for a therapeutic abortion. The distribution of access was described in one account (by an individual based in Massachusetts) as, "*forty percent of those women in the pre-New York era went to London and had abortions there. Ten percent decided to continue with their pregnancy or had no option but to continue with their pregnancy. Ten percent got abortions under therapeutic laws that were beginning to loosen up in Massachusetts, California and Washington, D.C., although it cost more to go to California than it did to fly to London. Forty percent of that caseload went illegally or, as people say euphemistically, extra legally*" (Rubin, 1994, pg. 50).

Further, while "*almost every woman who looked for an abortion had a difficult time obtaining one*" (Reagan, 1997, pg.193) the process was more arduous and financially burdensome for low-income, non-white women.[21] Women with economic means were able to obtain legal and therapeutic abortions, with private hospitals performing more abortions than public hospitals (Calderone, 1960). Similarly, out-of-state legal abortions were limited by economic means, where access was "*really only available to the small proportion of women who were able to pay for the procedure plus the expense of travel and lodging*" (Gold, 2003, pg.4).

[19]For New Jersey, the US District Court ruled that the New Jersey statute violated the 1st, 9th, and 14th Amendments (CDC (1972) Table 22). According to Myers (2021a) at least one physician began performing abortions publicly in 1972, which could be important for mortality declines.
For Vermont, the Vermont Supreme Court ruled that the law in place, only allowing abortion to save the life of the mother, was discriminatory, and the law should be broadened (CDC (1972) Table 22).

[20]We also run robustness checks in Section 7.6 by excluding these states. Including or excluding Vermont and New Jersey, or including them as repeal states, has little impact on the takeaways from the main results, given these states' relatively small populations.

[21]Repression of abortion in the 1950s and 1960s led to more hospitals cutting access to legal therapeutic abortion and more established clinics shutting down, ultimately restricting access to safe abortions. Consequently, more women seeking abortion had to opt for unsafe, illegal abortion. However, "*low-income women and African American and Latina women suffered more of the ill effects of criminal abortion than white and wealthy women* "(Reagan, 1997, pg. 193).

International travel in the pre-legalization era also appears to have been available to those with financial resources. Women traveled to London, Japan, Sweden, and in other cases, to Mexico (Rubin, 1994, pg. 50). In the years before abortion became legal in London (in 1968), a Massachusetts Planned Parenthood directly referred women to Japan. This referral process opened "*a flood gate of people who felt that they could somehow manage $800 and who came to Planned Parenthood for help and information.*" However, this access was only available for those with financial resources, as suggested by the same account, which goes on to describe, "*even more frustrating, the ones who when you said $800, sat there in utter silence and bewilderment, with tears in their eyes because there was nothing they could do. These were the people who went back out of that office and started the hunt for classic illegal abortions*" (Rubin, 1994, pg. 50).

These historical accounts suggest that pre-legalization, there were deep economic and racial disparities in access to safe and legal abortion. In Appendix Section G.3, we further outline evidence on the occurrence of abortion in legal, reform, and *Roe v. Wade* states.

4 Data

4.1 Maternal and Abortion-Related Deaths

We use the Mortality Data from the Vital Statistics *National Center for Health Statistics Multiple Cause of Death Files* available through the *Centers for Disease Control and Prevention (CDC)* and the *National Bureau of Economic Research (NBER)* to calculate mortality rates (NCHS, 1959-1980a). The mortality data includes all deaths in the United States from multiple causes of death. These national records incorporate the information from the death certificates of each U.S. state beginning in 1959 and continuing to the present. However, we conclude our study in 1980, seven years after the *Roe v. Wade* decision.[22]

Throughout the analysis, we primarily focus on maternal deaths, with abortion-specific deaths as a subset. We identify maternal and abortion-specific deaths according to the underlying cause of death (ucod) and the International Classification of Disease (ICD) codes (see Appendix Section H.1 for more details). Over the analysis, ICD codes were revised twice, and maternal deaths consist of the cause of death codes 640-689 in ICD-7, 630-678 in ICD-8, and 630-676 in ICD-9.[23] Broadly, maternal deaths during the period included any "maternal

[22]Concluding the analysis in 1980 gives us seven years of pre and post-*Roe v. Wade* data, enough time to observe the main impact of legal abortion. Further, by 1980, abortion mortality had reached 13 deaths, and there is limited variation beyond 1980 in that key outcome.

[23]See Table H.1 for details on the ICD codes used to identify *all-cause* maternal deaths (Panel A) and abortion-specific deaths (Panel C).

causes" listed on the death certificate, occurring within a year after the conclusion of the pregnancy (Hoyert, 2007). These maternal causes could include any death related to "complications of pregnancy, childbirth, and the puerperium" (NCHS, 1985).[24] Abortion-related deaths are a subset of maternal deaths, and include those related to the termination of the pregnancy, both spontaneous and induced terminations (WHO, 1970).[25]

A central concern with the measurement of abortion and maternal mortality is mismeasurement, and specifically, the under-counting of maternal and abortion deaths (WHO, 2007; Calhoun, 2013; Elam-Evans et al., 2003). This concern is more salient if under-counting varies by abortion legalization status, which it likely does. Abortion deaths are more likely to be misrepresented on death certificates "because other pregnancy-related deaths and childbearing rarely carry such stigma" (Coble et al., 1992, pg. 3235), and because of the legal ramifications faced by both the physicians and the mothers in states opposing elective abortion (Cates et al., 1978a; Dow and Ronan, 1997). In the literature, the majority of under-counting and misclassification (related to legal abortion) has focused on abortion-related mortality, with abortion deaths estimated to be undercounted by as much as 48% (Cates et al., 1978a). Maternal deaths also suffer from undercounting, by as much as 20%. Maternal deaths are frequently miscoded due to death certificates not mentioning pregnancy, or the death being coded under non-maternal causes (e.g., cardiopulmonary) (Smith et al., 1984; Dow and Ronan, 1997; Calhoun, 2013).[26]

To address the under-reporting of maternal and abortion deaths, in the robustness checks, we add several additional measures of mortality to gauge the measurement problem. First, we add all-cause reproductive-age female mortality, which should capture any death of a female related to abortion, but has the downside of being potentially too broad and confounded with unrelated factors.[27] Second, we consider two potential causes of death (for reproductive-age females) that could have captured misclassified abortion deaths, including septicemia and appendicitis. Third, we construct *broad* abortion-related mortality from maternal causes. *Broad* abortion mortality includes the recategorized sepsis, hemorrhage, and ectopic pregnancies plus the predefined (ICD-coded) abortion-related deaths (See Panel B in Table H.1).

[24] A formal definition of maternal deaths was not introduced until the ICD-9 (adopted in 1979), which defined a maternal death as, a death related to pregnancy or the management of a pregnancy, a period that concludes 42 days after the pregnancy ends (NCHS, 1985; Hoyert, 2007; Singh, 2010)

[25] Ideally, we would also consider abortion-related morbidity. However, variations and inconsistencies in classifying morbidity existed, and there were no national surveillance data (Coble et al., 1992; Binkin, 1986).

[26] Substantial misclassification of the underlying cause of maternal deaths results from physicians, medical examiners, or coroners failing to report if the woman had a recent pregnancy or was pregnant at the time of death in 50% or more of the cases (Atrash et al., 1992; Horon and Cheng, 2001; Calhoun, 2013).

[27] We also consider all-cause reproductive-age male mortality, which may be spuriously related to abortion legalization if the main effect is through another channel, such as general medical progress.

4.2 Population Data and Controls

We combine the death counts with population data covering the state-level size of the population, the number of births, and demographic controls. We calculate the state-level population composition from the U.S. Census data for the years 1950 to 1980 (Ruggles et al., 2021).[28] We combine the population shares from Ruggles et al. (2021) with annual state population totals from Wolfers (2006). We also incorporate further demographic controls, including annual state-level per capita income (from Jordan and Grossmann (2020)) and state per pupil educational spending (from NCES (1959-1980)).

We also add data on the number of births from the *Natality Detailed File* and U.S. Vital Statistics (NCHS, 1968-1980b; NVSS, 1959-1968). Our main policy controls include controls for the passage of unilateral divorce (affecting family dissolutions, Wolfers (2006)), access to the pill and access to the pill by minors (affecting fertility, Bailey (2006); Myers (2021a))[29], state-level equal pay laws (Myers, 2017), and state-level inductions into the Selective Service (Bitler and Schmidt, 2012). We include a complete description of the data sources in Table B.1.

4.3 Mortality Rates

To calculate mortality rates, we use the number of maternal deaths and abortion deaths per 100,000 females aged 15-44 (reproductive-age females). For non-white and white mortality, we use the measures per 100,000 white and non-white reproductive-age females (15-44).[30] We choose per female instead of the traditional measure of per birth because abortion legalization may also affect the fertility rate.[31] While we primarily rely on the reproductive-age females when constructing mortality rates, we use the data on births to test alternative weighting of the population in the robustness tests (see Section 7.5).[32]

Another notable limitation of our data is that the mortality data, the population data, and the birth data all represent different race collection points. Race reporting may vary across these data sources and through time (Mays et al., 2003). Further, even in cases where there is

[28] We linearly interpolate the years between census decades to form annual estimates of the population shares.

[29] Specific dates are from Myers (2021a).

[30] The by-race data is replaced as missing for NJ in 1963 and 1962 due to a substantial number of deaths in the raw data (and codebook) with missing race.

[31] We demonstrate this in Appendix Section I, but this is also supported by Roht et al. (1974); Ananat et al. (2007); Levine and Staiger (2004); Guldi (2008). Moreover, as highlighted by Roht et al. (1974) and Clarke and Mühlrad (2021) the *per live birth* denominator does not fully capture the risk associated with conception and all of its possible terminations.

[32] A limitation of the results weighted by the number of births is that the number of births is endogenously determined, which is why we prefer the use of reproductive-age females in our baseline specification. However, despite the limitations, using the number of births offers a helpful robustness check on the potentially problematic census data.

a consistent collection of race on administrative forms, individuals may report different races over time, even moving from the non-white to the white category (Mays et al., 2003). While different race reporting across data sources is an underlying limitation of our data, we show that the results are robust across both the mortality rates and the death counts in Section 7.5.

4.4 Trends and Summary Statistics

Figure 2 shows the substantial nationwide reduction in both abortion-related and all-cause maternal deaths from 1959-1980. Panels A and B show the largest drop occurred for non-white maternal and abortion mortality. Figure A.1 Panels B and C also show this decline by state-legal status. The rightmost graph illustrates that repeal states had the most substantial declines in abortion-related mortality over the study period, while reform states had the largest reductions in non-white maternal mortality.[33]

Table 1 shows the summary statistics for our primary measures of mortality and the controls. We separate states into states that passed early legalization of abortion (early-legal), those with abortion reforms (excluding CA), and states only treated by the *Roe v. Wade* decision). Table 1 suggests differential state-level characteristics by legal access to abortion. Similar to Figure 2, non-white maternal mortality is highest in reform states (rather than early-legal states), while non-white abortion-related mortality is highest in early-legal states. Repeal (early-legal) states also differ in other characteristics, including having the largest state-level populations and the highest incomes.

5 Empirical Strategy

We primarily rely on an event-study design to consider the impact of legal abortion on maternal mortality and abortion-specific mortality. Formally, we estimate the following equation:

$$\text{Mortality}_{st} = \alpha + \sum_{m=-7}^{6} \beta_m \text{ Legal Abortion}_{sm} + \mathbf{X}'_{st}\gamma + a_s + \eta_t + \epsilon_{st} \quad (1)$$

where Mortality_{st} reflects the mortality rate in state s during year $t = 1959, ..., 1980$. As is

[33] For completeness, we also break out the maternal and abortion-related death counts in Appendix Figures A.2, A.3, and A.4. Figure A.2 shows maternal and abortion death counts, with vertical lines illustrating both the change in the legal framework and changes to the ICD codes during the period. Figure A.2 Panel B displays abortion mortality as a share of maternal deaths, which also declines over the period of analysis. Figure A.3 separately plots the maternal and abortion counts individually for early-legal states (all and by race). Finally, Figure A.4 displays histograms of the number of abortion deaths by age and year.

common in the literature (e.g., Jayachandran et al. (2010); Alsan and Goldin (2019); Anderson et al. (2020b)), we prefer to measure the proportional changes in mortality rather than the linear changes. Ideally, we would take the natural log of the mortality rate,[34] however, in our setting, maternal and abortion-related mortality each include zeros. In our preferred specification, we maintain zeros by relying on the inverse hyperbolic sine of the mortality rate.[35] Still, we demonstrate in Section 7.5 that our general conclusions hold if we use the log of mortality plus one or the log of mortality, as well as in a Poisson model (with linear mortality counts). For our main mortality measures, we use the number of maternal and abortion-specific deaths per 100,000 reproductive-age females (15 to 44).[36]

The passage of legal abortion is captured by the indicator variables Legal Abortion_{sm}. Legal Abortion_{sm} represents the state-level passage of legal abortion in state s during period $m = 0$, where m ranges from seven years before to six years after legalization. More formally, m represents the difference between the observation year (t) and the year legal abortion went into effect (T), where $m = t - T$. The main impact of legal abortion (our 'treatment' effect) is captured by the six post-treatment dummy variables, $m = 0, 1, ..., 6$, which are relative to the pre-legalization year, $m = -1$.

As all states eventually legalized abortion, we lack a never-treated comparison group. To address the lack of a proper control group, we take two approaches. First, in the main TWFE specification, we must either omit two pre-periods or bin the endpoints to avoid collinearity (Borusyak et al., 2018; Schmidheiny and Siegloch, 2020). In the TWFE specification, we choose to bin the left endpoint at $m = -7$ and bin the right endpoint at $m = 6$. Binning, as opposed to excluding two pre-treatment periods, allows us to capture the effect of legal abortion extrapolated from the secular linear trend in maternal (and abortion-related) mortality over our study period (Schmidheiny and Siegloch, 2020).[37] Second, we use the Interaction-Weighted Estimator (IW) (Sun and Abraham, 2020) to compare the effect of early-legalizations relative to the last-treated cohort, *Roe v. Wade* states. Because the IW specification provides a con-

[34]Using the log of mortality instead of linear rates captures the proportional decline in mortality associated with legal abortion. If a linear specification of mortality is implemented, the declines in mortality associated with legal abortion will need to be similar across states in each year after legal abortion passes, instead of the proportional change (based on original levels of mortality). Further, as shown in Figure H.1 the log distribution and inverse hyperbolic sine distribution normalize the skewed distribution of linear rates.

[35]The inverse hyperbolic sine produces results that approximate the natural log of mortality while maintaining zero observations (Bellemare and Wichman, 2020). The inverse hyperbolic sine has the advantage of including zeros and is potentially preferred to more crude measures of including zeros, such as taking the natural logarithm of mortality plus a constant. However, we show both the direct natural log and the log of mortality plus one in Section 7.5.

[36]We choose the mortality rate per reproductive-age female, as legal abortion has been shown to affect the birth rate (e.g., Guldi (2008)), a fact we also demonstrate in Appendix Section I. Though, we show results that do not rely on reproductive-age females as the denominator in Section 7.5.

[37]We have also performed the analysis using -4 and -1 as reference groups and the findings are similar to the binning, the main impact appears to be for non-white women, with pre-trends for white and potentially overall mortality.

trol cohort (the last-treated group), in the IW specification, we leave the event study fully saturated (unbinned).

$\mathbf{X}_{st}$ contains state-level controls. The main set of state-level demographic controls includes the share of reproductive-age females (aged 15-19) who are white, the share of reproductive-age females (aged 15-19) who are non-white, the share of those with 12 years of education or higher (high school graduates), the log of the state-level per capita income, and the log of the state-level per pupil spending. We choose these controls to account for state-level differences in the risk and demand for an abortion. We include income and education as controls to address differences in socioeconomic status by state, which may influence both access to abortion and the necessity of obtaining an abortion.[38] We avoid including controls for the share married, the reproductive-age income, the population under five, and the education level of reproductive-age females as these controls may be affected by abortion itself (e.g., based on the findings in Myers (2017) and Guldi (2008) these would be "bad controls" (Angrist and Pischke, 2008)).

We also control for several policies that coincided with legal abortion over this period. First, we control for states that passed pre-legalization abortion reforms. Because the legalization of abortion is fundamentally different than the MPC abortion reforms, we focus on full legalization while controlling for other abortion reforms (similar to Myers (2017)). However, in subsequent results, we directly test the effect of either passing legal abortion or an abortion reform and find only minimal impacts of legal changes that were not direct legalizations (see Figure C.2). Second, we control for the passage of unilateral divorce, which may affect family dissolutions (Wolfers, 2006; Gruber, 2004; Friedberg, 1998). Third, we control for general access to the pill and access to the pill by minors (Bailey, 2006, 2010; Bailey et al., 2012). Access to the pill may affect who becomes pregnant during the period. Fourth, we control for equal pay laws, which may affect women's income and labor supply. Fifth, we control for the inverse hyperbolic sine of the number of men drafted per 1,000 males 18-25 (the induction rate), to address differential state-level impacts of the Vietnam War on fertility and related household outcomes (Card and Lemieux, 2001a,b; Bitler and Schmidt, 2012; Bailey and Chyn, 2020).

Finally, α_s accounts for the state fixed effects or time-invariant state characteristics. η_t captures the year fixed effects. ϵ_{st} is the regression error. We cluster the standard errors at the state level.[39] All regressions are weighted by the denominator of the rate, which is

[38] These demographic controls also predict early-legalization in Section 7.8.

[39] Throughout our main specification, including both the IW estimator and the TWFE specification, we have 50 clusters, and cluster-robust standard errors are appropriate (Bertrand et al., 2004; Abadie et al., 2017). However, Cameron et al. (2008); MacKinnon and Webb (2017, 2018); Ferman and Pinto (2019) show that having few treated units can be problematic for inference, which is the case for our IW specification. Standard error correction methods when using the relatively new IW estimator have not yet been formally developed. Instead, in the robustness checks, we follow work by Chetty et al. (2009) and Buchmueller et al. (2011) and perform permutations over placebo specifications, assuming that legal abortion was implemented in alterna-

reproductive-age females for the main specification. We choose to weight the regressions so that the estimates reflect the size of the population impacted by the legal framework. In the robustness checks, we also report the unweighted results (see Section 7.6).[40]

5.1 Potential Threats to Validity

There are two main potential threats to our primary specification that are worth outlining upfront. First, we assume that the timing of legal abortion is exogenous. Violation of this assumption would be particularly relevant if states with the worst mortality conditions uniformly adopted early legal abortion. If this were the case, early-adopting states might have converged to the average level of mortality without ever passing legal abortion. Thus, in Appendix Section F.1, we test whether abortion mortality and maternal mortality predict the adoption of legal abortion in a Cox proportional hazard model. The results suggest that mortality does not significantly predict the timing of adoption.

Second, our empirical strategy also assumes that the timing of legal abortion is not correlated with other uncontrolled public policies that might affect mortality. Legal abortion occurred during a time of enormous social change, where a number of other public policies were adopted. In addition to directly controlling for these policies in the main specification, we also test mortality as an outcome of related policies, test the interaction of these policies with legal abortion, and add additional state-level policies in Section 7.7.

6 Main Results

We begin by testing whether legal abortion affects maternal mortality or abortion-specific mortality in an event-study design. Figure 3 presents maternal mortality in green (left graphs) and abortion-related mortality in purple (right graphs). In each graph, the plotted diamonds/circles/squares connected by solid lines reflect point estimates,[41] and the dashed lines reflect the 95% confi-

tive treatment states or treatment years. This allows us to calculate a nonparametric p-value. The results for the full set of early-legal states are robust to this permutation test. However, throughout the main results, the IW specification has only six treated units; and the reported cluster-robust standard errors are potentially more robust in the TWFE than in the IW specification.

[40] We omit state-specific trends from the main event study. In the dynamic specification, the unit-specific trends contaminate the treatment effect (Borusyak et al., 2021). In the IW specification, the fully saturated specification also does not allow the addition of state-specific trends without omitting another period. Still, we include state-level trends in the difference-in-differences specification in Figure 6 and in the TWFE event study as a robustness check (Figure C.3). In both of these checks, our main results are robust to the inclusion of state-specific trends.

[41] Each plotted point estimate leading up to the change in legal abortion (at time $t = 0$) represents the evolution of mortality over the pre-legalization time frame. Post-periods represent the mortality response in each year following the passage of legal abortion (at time $t = 0$).

dence intervals. The vertical line indicates the excluded period, $m = -1$. The graphs show the coefficients from both a canonical two-way fixed effects (TWFE) estimation of Equation 1 and an Interaction-Weighted (IW) estimator from Sun and Abraham (2020). In the IW specification, the early-treated cohorts are compared against states that were treated by the passage of *Roe v. Wade*, and to avoid improper comparisons between treated and already-treated, for the IW specification, we only estimate the effect over the years leading up to *Roe v. Wade* (1973 and beforehand). We also only show the coefficients in the main event window, even though the TWFE specification includes the binned endpoints $m = -7$ and $m = 6$, while the IW specification is fully saturated.

Beginning with maternal mortality (green, left graphs), only non-white maternal mortality declines after abortion legalization, with slightly more noticeable declines in the TWFE specification. In the TWFE specification, the decline in non-white maternal mortality appears consistently statistically significant at the 5% level until the last post-period, and also shows a relatively flat pre-period. For overall maternal mortality, there is a slight dip in period two, but the confidence intervals include zero again, beginning three years after legalization. Finally, white maternal mortality fails to show any evident decline after legalization.

Abortion-specific mortality shows a clear decline (in the right graphs of Figure 3). However, the most apparent decline again appears for non-white women. By contrast, the plotted points for white abortion-related mortality suggest evidence of a preexisting decline before formal legalization. While the TWFE specification for white abortion-related mortality does show a post-period reduction, this decline appears inextricable from the pre-legalization trend. In the IW specification, white abortion-related mortality fails to show a clear significant drop post-legalization.

Figure 3 reveals that legal abortion primarily reduces non-white maternal and abortion-related mortality. To interpret the size of the reduction, we must transform the coefficients into percentage-change effects (see Bellemare and Wichman (2020)). The noteworthy percentage reductions in mortality are displayed in Table B.2. For the main effect on non-white maternal mortality, beginning one year after legalization, legal abortion reduces non-white maternal mortality by 30-40%. For non-white abortion-related mortality, the reduction is 30-60%.

Alongside the percentage decline, we also show the estimated number of deaths averted.[42] For non-white abortion mortality, between five and 16 deaths are averted (per year) for the first few years after legalization. Non-white maternal mortality shows larger declines. In the first year after legalization, there are 41 non-white deaths averted in early-legal states, and

[42] We calculate the deaths averted for each period m using the estimated percentage and the observed death count in period m as: Observed # of $\text{Deaths}_m/(1 + \text{Estimated \% Decline}) - \text{Observed \# of Deaths}_m$. Table B.2 reports the magnitude of the decline, the observed death count for each period t, and the estimated deaths averted based on those counts.

113 non-white deaths averted nationally. In the second year, 17 non-white deaths are averted in early-legal states, and 82 non-white deaths are averted nationally. While these impacts in terms of deaths averted may seem relatively small in magnitude, the percentage decline from the baseline level of deaths is substantial (30-40%). Further, maternal mortality represents the "tip of the iceberg" in terms of the health effects of legal abortion (Clarke and Mühlrad, 2021). Our mortality estimates focus on the worst-case scenario (a death) and only provide a glimpse into the health and well-being achievements of legal abortion (Clarke and Mühlrad, 2021), suggesting, that the number of life-threatening but non-fatal complications averted due to legal abortion are potentially higher (Tietze and Lincoln, 1987).[43]

The importance of legal abortion for non-white maternal and abortion-related mortality aligns with the historical narratives of the time, indicating that poor and non-white women faced the most significant hurdles to obtaining abortions before legalization. Physicians (particularly those in public hospitals) "saw women who needlessly suffered and died as a consequence of illegal abortion" (Rubin, 1994, pg. 71) with these physicians "*disturbed that most of those women were poor and black*" (Rubin, 1994, pg. 71).[44] Another account describes, "*in the first half of the twentieth century, a two-tiered abortion system emerged in which service depended on the class, race, age and residence of the woman. Poor and rural women obtained illegal abortions, performed by people, physicians and others, who were willing to defy the law out of sympathy for the woman or for the fee. More privileged women steadily pressed physicians for legal abortions and many obtained them*" (Law et al., 1989, pg. 18). These observations also suggest a potential explanation for why we observe a preexisting decline in white abortion-related mortality. White women were more likely to have access to therapeutic abortion or travel to early-legal states and international destinations before formal legalization (Rubin, 1994; Reagan, 1997; Joyce et al., 2013). Therapeutic abortion, in particular, had a clear "*class bias inherent in the psychiatric indications*" (Rubin, 1994, pg. 71).[45]

[43]For example, Clarke and Mühlrad (2021) find that legal abortion in Mexico produced a substantial reduction in early-pregnancy hemorrhage (by 35%). Tietze (1984) also outlines three benefits to abortion legalization in the US. First, the mortality decline, which we document. Second, non-fatal pregnancy complications averted, which Tietze (1984) estimates to be in the "several 10s of thousands." Third, Tietze (1984) outlines the general welfare benefits of therapeutic abortion access for women with contraindications in pregnancy, especially poor non-white women.

[44]Poor, non-white women suffered more complications since they "were more likely to try to self-induce abortions and less likely to go to doctors" compared to white, affluent women, "because of poverty or discrimination in access to medical care" (Reagan, 1997, pg. 214). Increasingly repressive abortion policies made illegal abortions more clandestine, less available, riskier, and more expensive - consequently, even "safety of illegal abortion varied by race and class" (Reagan, 1997, pg. 138).

[45]White women of higher socioeconomic class "not only had the necessary ability to pay for consultations, but also enjoyed a more subtle class advantage in gaining the support of their psychiatrists, who were generally of the same class and racial background."(Reagan, 1997, pg.207).

7 Robustness Checks

7.1 Removing States Affected by Spillovers

As a first robustness check, we address spillovers from early (voluntary) repeal states to nearby states. New York, California, and DC, in particular, acted as hubs for women to obtain an abortion. These states may have produced regional abortion and maternal mortality declines through travel from nearby states. To account for spillovers (from early-legal states) contaminating the control group, we eliminate states within 500 miles of the primary early-legalization states (CA/NY/DC, Myers (2017)).[46] Panel A of Figure 4 reveals that even after removing nearby states, non-white maternal mortality and abortion mortality continue to decline after the passage of legal abortion (see Figure C.1 for all and white).

7.2 Early Abortion Reforms

In the primary analysis, we focus on legal abortion. However, certain states adopted abortion reforms before full legalization. In some cases, reform states even had higher abortion rates than repeal (full legalization) states (Myers, 2017). Thus, we explore the possibility that abortion reforms lowered mortality before full legal abortion.[47] Figure 4 Panel B and Figure C.2 present the impact of either an abortion reform or abortion legalization. Including abortion reforms in the treated group produces only a modest decline in non-white maternal and abortion-related mortality (in the IW specification alone). This muted impact is not surprising; while certain reform states, such as Maryland, North Carolina, and Colorado, allowed a substantial number of legal abortions, other states had relatively restrictive access. Thus, *on average* the impact in reform states appears weaker than full legalization alone.

7.3 Tests for State-specific Trends and Pre-trends

In the main findings, we omit state-specific trends,[48] but here, we demonstrate that the findings are similar after adding state-specific linear trends. Panel C of Figure 4 shows that both non-white maternal and abortion-related mortality continue to decline, even with linear trends added to the specification (see Figure C.3 for all and white).

[46]We use average county-level distance, weighted by the reproductive-age female population.
[47]To test abortion reforms, we based the year of legalization on when the state adopted either an MPC reform (plus Mississippi, Vermont, and New Jersey's reforms) or full legalization.
[48]Unit-specific trends contaminate the treatment effect in the dynamic specification (Borusyak et al., 2021). We also cannot include unit-specific trends in the fully-saturated specification (without omitting another period), and we use the fully-saturated event-study specification for the IW estimator.

Next, we directly test for pre-trends, focusing on low-power pre-trends, using the R-package *pretrends* from Roth (2022). We plot the linear and quadratic pre-trends, as well as the hypothesized coefficients after pre-testing. The results from this exercise (for non-white mortality) appear in Figures C.4 (IW) and C.5 (TWFE). These results show a modest pre-trend in the opposite direction of the hypothesized effect, which is quite small compared to our observed post-period coefficients. This pre-trend may cause modest attenuation bias in our estimates, but overall we believe this test supports our identification strategy.[49]

7.4 Placebo Test and Misclassification Tests

Placebo Test. We then implement a placebo test to confirm that our main findings are unlikely to arise from general technological progress, or general medical advancements. While we include year fixed effects, which capture a portion of annual changes in mortality, medical advancements may have appeared first in major U.S. cities (NYC and DC), thereby being correlated with abortion legalization. If this is the case, our main mortality decline would be spuriously correlated with legal abortion through the omitted variable of general medical progress. To test whether this concern is plausible, we consider the impact of legal abortion on all-cause reproductive-age male mortality. Legal abortion should have had no apparent effect on reproductive-age male mortality over this period. As expected, the plotted points suggest little impact of legal abortion on reproductive-age male mortality, the left side of Figure 4 Panel D (non-white only) and Figure C.7.

Misclassification Tests. In the same figures (right), we also test the possibility that legal abortion may have been misclassified into other causes of female mortality. While legal abortion could plausibly cause a decline in all-cause female mortality, the effect should be less noticeable than in maternal and abortion-related mortality. The results in Figure C.7 suggest no general decline in all-cause reproductive-age female mortality. These findings indicate that the declines in abortion-related deaths were not large enough to impact all-cause reproductive-age female mortality during the period, or instead, there were offsetting factors.[50]

[49] Still, there may be a concern that related factors, such as non-white state-level income, are gradually changing over time, thereby impacting maternal and abortion-related mortality. These pre-trends in related factors may be linked to the observed post-period decline in non-white maternal and abortion-related mortality. To test whether this is the case, we plot the income and education of non-white reproductive-age females as outcomes in our primary event study. The plotted coefficients in Figure C.6 show a relatively flat pre-period across both income and education. While there is some effect over the post-period, this is expected based on findings in papers such as Myers (2017).

[50] Figure C.8 and Figure 4 Panel E also present results for two specific causes of death likely to include misclassified abortion deaths, including appendicitis and septicemia mortality (for all reproductive-age females). Neither septicemia nor appendicitis mortality clearly decline after legal abortion in Figure C.8, suggesting, again, little evidence of substantial misclassification.

Then, in Figure C.9, we test whether abortion-related deaths were miscategorized into other non-abortion causes of maternal deaths. We reclassify causes of maternal death likely to be due to abortion, including pre-defined abortion mortality, sepsis, hemorrhage, and ectopic pregnancies (Meyer and Buescher (1994); Walker et al. (2004); Hansen (2010), see Table H.1). This broader classification of abortion-related deaths accounts for any potential misclassification of abortion-related deaths into general maternal deaths. We view potential misclassification as more plausible under illegal abortion as abortion deaths may be less likely to be categorized as an abortion-related death due to the legal ramifications to both the providers and the women (Cates et al., 1978b; Shah and Åhman, 2009).[51] Thus, causes of death from illegal abortions or self-induced abortions may be more likely to be coded under general pregnancy-related deaths. Figure C.9 shows an expected dip in *broad* abortion-related mortality, but the effect is no more apparent than in Figure 3.

Overall, these results help assuage two main concerns. First, general medical progress is unlikely to be generating a spurious correlation between legal abortion and maternal mortality. If general medical progress over this period were strongly correlated with abortion legalization, this progress should show up in other measures of mortality, aside from maternal and abortion-related mortality. Second, we show that the main effect is clearest in the anticipated measures–maternal and abortion-related mortality–helping to assuage concern over miscategorization substantially impacting our findings.

7.5 Alternative Functional Form and Additional Measurement Issues

In our main findings, we take the inverse hyperbolic sine of the mortality rate. Here, we show that our general conclusions are similar if we use alternative functional forms. Appendix Section H.3 also discusses why log transformations are important in our setting.[52]

The Natural Log of Mortality. Figure 5 shows that the declines in non-white maternal mortality and non-white abortion mortality are robust across various functional forms. Panel A presents the log of the mortality rate, and Panel B displays the log of the mortality rate plus one. These alternative functional forms largely reflect the baseline results for maternal mortality. Thus, our main conclusions are robust to alternative functional forms, even those that exclude zeros.

Functional Form and Additional Measurement Issues. When switching to alternative functional forms, we also deal with an underlying measurement issue in the data;

[51] Cates and Rochat (1976) suggests, that deaths from causes relating to illegal procedures were, on average, more likely to be that of non-white women.

[52] For these results, we focus on non-white mortality, and do not place the figures for white mortality in the appendix. We choose to focus on non-white mortality because, for white mortality, the natural logarithm produces too many zero deaths over the post-period.

the fact that the counts of reproductive-age females (in the denominator of the mortality rate) are based on linear interpolations of census years. Because the census year 1970 corresponds with the first treatment year for four out of the six early-treated states, we use the additional functional form results to present the log of the death counts with alternative weights. Figure 5 Panels C, D, and E confirm that the findings are robust to any issues measuring the reproductive-age population. Panel C uses the log of the number of non-white deaths, and weights the specification by non-white births; similarly, Panel D uses the log of the death count plus one, and weights by non-white births. Both Panels C and D mimic the declines in abortion and maternal mortality shown in Panels A and B. Finally, in Panel E, we return to our baseline measure of mortality, but we replace our time-varying measure of reproductive-age females with the state-level number of reproductive-age females from the first year of our sample.[53] Using this constant measure of reproductive-age females for the denominator and weights, the results again appear similar to the baseline.[54]

Overall, all findings in Figure 5 show that the baseline conclusions are not reliant on the linear interpolations of the share of reproductive-age females, especially those results that focus on death counts (instead of rates). The findings using the log of the mortality counts also assuage concerns over race reporting differing between administrative records (an issue discussed in Section 4). These additional findings ensure that our conclusions are not solely dependent on individuals reporting the same race on the census and the death certificate.

Poisson Model. Finally, in Panel F, we conclude by switching to a Poisson model, with the empirical strategy presented in Section H.3.[55] For the Poisson model, we use the linear death count and the exposure population of reproductive-age females. The Poisson results in Panel E show a clear decline in non-white abortion and maternal mortality, including the results across all treated units (including *Roe v. Wade*), and the findings using only early-treated states (analogous to the IW specification).

7.6 Difference-in-Differences

We next show our results are robust to using a difference-in-differences specification (rather than an event study). Within the difference-in-differences results, we display a number of different additional robustness tests.[56] Figure 6 plots the impact of abortion legalization

[53]In Panel E, we also omit census controls; and only include policy controls, as well as the log of per capita income and the log of per pupil spending.

[54]In Appendix Figure C.10 we show two other alternatives for the reproductive-age female population. First, we use the share of reproductive-age females from the first year of the sample multiplied by the state population in each year. Second, we use the reproductive-age female population linearly interpolated over 1960-1980 and omitting 1970 from the interpolation.

[55]Follows related work, e.g., Myers and Ladd (2020); Myers (2021b,c).

[56]For completeness, we outline our difference-in-differences specification in Equation 2 in Appendix Section F.2. We also further decompose these results using a Goodman-Bacon decomposition in Appendix Section F.3.

in a grouped post-period (Legal Abortion$_{st}$ from Equation 2). Throughout Figure 6 we show both maternal mortality (left) and abortion mortality (right) for all (gray circles), white (purple triangles), and non-white (green squares).[57] Figure 6 confirms again that abortion legalization consistently reduced non-white maternal mortality (in all but one specification) and all measures of abortion-related mortality.[58]

7.7 Competing State-level Policies and Additional Controls

Next, we consider whether other competing state-level policies impacted mortality. While we control for related policies in the main results (Figure 3),[59] several alternative policies may have directly impacted maternal and abortion-related mortality (independently). Figures C.11 and C.12 directly test whether related policies led to changes in maternal and abortion-related mortality.[60] The results suggest no persistent effect of any other policy on maternal or abortion-related mortality, helping to lessen concern over competing policies producing the changes in maternal and abortion-related mortality. The main mortality decline appears isolated to legal abortion.

We then test whether our focus on abortion is misdirected away from other family and fertility policies. Table B.4 presents the interaction of state-level policies (for non-white mortality). Most policies show little interacting effect, with a few notable exceptions. In Columns (4) and (9), state fair employment amendments, which prohibit discrimination in employment (Myers, 2021a) show some interacting impact with legal abortion.

7.8 *Roe v. Wade*

The baseline specification heavily relies on early legalizations. The emphasis on early-legal states is especially the case in the IW specification, which captures the impact of early legalizations as compared to *Roe v. Wade* states. Here we test the separate effect of *Roe v. Wade* in Panel F of Figure 4 (for non-white mortality) and Figure D.1 we compare the impact

[57] We also show these difference-in-difference results in Table B.3 for non-white mortality, along with the noted additional controls as well as adding endogenous controls, including reproductive-age female income and education for non-white and white.

[58] While non-white abortion specific mortality results are robust whether we include population weights or not, failing to include population weights attenuates the effect on non-white maternal mortality. The smaller observed effect may be driven by the sheer size of California and New York; these states also have the largest non-white populations nationally. When we remove population weights, we are considering the average state-level effect instead of the population-based effect.

[59] We include policy controls for state-level abortion reforms, access to the pill for minors, access to the pill generally, unilateral divorce legislation, state equal pay legislation, and the inverse hyperbolic sine of state-level inductions per 1,000 males 18-25.

[60] Considered policies include unilateral divorce, Medicaid, access to the pill, minor's access to the pill, equal pay laws, fair employment amendments, equal rights amendments, fair housing laws, and AFDC-UP.

of *Roe v. Wade* to states that legalized abortion in 1969 and 1970.[61] Over the figure, we show three specifications, the baseline with all *Roe v. Wade* states (diamonds), omitting abortion reform states (light circles), and omitting spillover states (dark squares). All plotted points in Panel F of Figure 4 and Figure D show little impact of *Roe v. Wade*. Still, these point estimates reflect an imperfect comparison. The early-legal states in the control group may experience heterogeneous treatment effects over their own post-period, making it difficult to gauge the parallel trends for *Roe vs. Wade* states.

7.9 Selection into Voluntary Early Legalization

Why is the impact of *Roe v. Wade* smaller than the impact of legalization in early-legal states? A few explanations for this smaller impact exist. First, by 1973, abortion mortality had substantially declined; abortion mortality had fallen 90% from 1959 and 87% from 1965.[62] These substantial mortality reductions illustrate that the majority of the decline in abortion deaths occurred before the 1973 *Roe v. Wade* decision.[63]

However, *Roe v. Wade* states also may have systematically differed from early-legalization states. If states were distinct enough, the impact of voluntary legalization might expectedly differ from the national *Roe v. Wade* decision. These differences could ultimately explain the distinct effects of legal abortion in early-legal states versus *Roe v. Wade* states. We next attempt to characterize differences between early-legal states and *Roe v. Wade* states. Then, we balance observable differences using inverse probability weights, and finally, we quantify the remaining degree of selection (on unobservables) necessary to explain away the observed results in early-legal states.

Characterizing Early-Legal States. First, in Table D.1 we show that states that passed legal abortion before *Roe v. Wade* differed across a number of notable characteristics from *Roe v. Wade* states.[64] States that voluntarily passed legal abortion have higher incomes,

[61] For this specification, we use the TWFE estimator because there is no staggered treatment timing. We also limit the pre-period only to consider the period after early legalizations, 1970-1980, and exclude early-legal states that adopted legal abortion after 1970 (DC) from the control group.

[62] In 1959, abortion-related mortality was 0.78 per 100,000 reproductive-age females, and by 1965, before any reforms or legalizations, abortion-related mortality reached 0.59 per 100,000 reproductive-age females (nationally). However, by 1973 abortion-related mortality had dropped to 0.079. In 1974 though, following the *Roe v. Wade* decision, abortion-related mortality hovered at 0.058 in 1974 and 0.056 in 1975.

[63] For example, the smaller effect of *Roe v. Wade* here aligns with the fertility effects observed in Joyce et al. (2013), where the authors conclude that the majority of the changes in unintended childbearing were a result of legal abortion in CA, NY, and DC. More specifically, Joyce et al. (2013) concludes similarly that for fertility, "Roe v. Wade was arguably less important for unintended childbearing than was access to services in California, the District of Columbia and especially New York in the years before Roe."

[64] These differing characteristics potentially influenced the response to the 1973 Supreme Court ruling in *Roe v. Wade* states. Since the *Roe v. Wade* ruling required "neither the states nor the medical community to provide those (abortion) services" (Johnson and Bond, 1982, pg.2), "the provision of services has not been monitored or consistent across the country" (Frankel, 1988, pg.30).

spend more per pupil on education, have a more educated population, and are less religious (as measured by church membership). However, these early-legal states do not appear politically different from *Roe v. Wade* states in either elected state representation or polarization. Early-legal states also do not have significantly different maternal or abortion mortality rates, and they adopted related policies at similar times to *Roe v. Wade* states. Still, the fact that there are notable differences across adoption by income, education, and religious affiliation suggests that the impact of legal abortion may expectedly differ across states. For example, states with more religious adherents per capita may have a lower supply of abortions services or demand for abortions (both legal and illegal).

Matching on Observables and Permutation Tests. In an attempt to make early-legalization and *Roe vs. Wade* states as similar as possible, we next balance observed differences between early-legal and *Roe v. Wade* states using inverse probability weights (IPW).[65] IPW form an improved comparison group by down-weighting dissimilar states in the control group. We focus only on early-legal states over 1959-1972,[66] and refine our control group to include only *Roe v. Wade* states that did not pass abortion reforms.[67]

We create the IPW by first estimating a logistic regression, where the outcome variable represents an indicator for early legalization (equal to one for CA, NY, DC, WA, HI, and AK). Within the logistic regression, we add covariates that are significant predictors of early legalization from Table D.1, including church membership per capita, the share with a high school degree, the log of per pupil education spending, and the log of family income.[68] After estimating the logistic regression, propensity score weights (PSW) are calculated, and we use these PSWs to generate inverse probability weights as $1/PSW$ for the early-treated group and $1/(1-PSW)$ for the *Roe v. Wade* group. After calculating the IPW, the IPW are multiplied by the non-white females 15-44 to recover the population-weighted results.[69]

Table 2 Columns (1)-(2) show the decline in abortion and maternal mortality is robust to adding IPW. Column (1) shows the baseline results, and Column (2) presents the results ap-

[65]We choose inverse probability weights (IPW) instead of propensity score nearest neighbor matching to include all *Roe v. Wade* states in the analysis.

[66]Because we estimate the effect over placebo tests, with *Roe v Wade states*, we strictly limit the post period to avoid any small effects of *Roe v. Wade*.

[67]We avoid including abortion reform states or the year 1973 in the control group due to the permutation tests conducted for the nonparametric p-values. If abortion reforms are included, and abortion reforms have some effect (which they do in Table B.3), then this impact of abortion reforms would potentially be captured in our placebo tests.

[68]We have attempted to include several different covariates, but certain covariates, such as the state population, are problematic. Because the analysis sample is small and there are large differences between groups, certain controls, such as the log of the state population, cannot be included. Because the regression is for a single year, we use the 1960 characteristics. However, for church membership per capita, we use 1971, the first year available.

[69]As shown in Figure 6 population weights are necessary, and the average state-level effect does not yield the same findings as the population-weighted results.

plying IPW. However, because we have few treated units, our cluster-robust standard errors may be improper for the setting (Cameron et al., 2008; MacKinnon and Webb, 2017, 2018; Ferman and Pinto, 2019). Following work by Chetty et al. (2009) and Buchmueller et al. (2011),[70] we perform alternative inference using permutations over placebo specifications, assuming that legal abortion was implemented in alternative treatment states or treatment years.[71] The cumulative distribution of estimates from this exercise is shown in Figure D.2, with the actual estimated coefficient indicated by the vertical line. Using this CDF, we calculate the nonparametric p-values from the permutation test as the number of placebo observations that are less than the estimated effect, divided by the sample size of all permutation estimates.[72] This nonparametric p-value is also displayed at the bottom of Table 2, and while larger than the cluster-robust p-value is still statistically significant for the full sample.

Then, over Columns (3)-(8), we separately consider each treated cohort (based on treatment timing) relative to *Roe v. Wade* states, showing both the baseline and the IPW results. For each treated cohort, there is a clear drop in maternal and abortion mortality across both the baseline and the IPW specifications (with the cluster-robust p-values), excluding abortion-related mortality for the 1971 cohort. Similar to the full sample, we also implement permutation tests across each cohort.[73] With these nonparametric p-values, only the 1970 cohort is statistically significant for maternal mortality. Though, for abortion-related mortality, all cohorts are statistically significant, excluding DC in 1971.

Quantifying Selection on Unobservables. Overall, early-legal states show a robust decline in maternal and abortion-related mortality relative to *Roe v. Wade* states. Here we conclude by quantifying the degree of selection necessary to explain away the observed impact of legal abortion in early-legal states, following Oster (2019) (building on Altonji et al.

[70] Also applied in related settings for similar few treated units, e.g., Ohrn (2018); Baron et al. (2020); Prettyman (2021).

[71] More specifically, Equation 2 is estimated over two different placebo scenarios. First, we estimate Equation 2 over the original legal abortion adoption years, but assign placebo states to these treatment years. In this case, the first placebo state is treated in 1969, the next four in 1970, and the sixth state is treated in 1971. We assign placebo states randomly to this staggered adoption timing, selecting six placebo states from the *Roe v. Wade* control states. in this exercise, we run this simulation 500 times. Second, we use placebo treatment years. In this case, we vary the first legal abortion adoption year from 1960 to 1967 (limiting years to before 1970), but use the same staggered setup, where one state adopts legal abortion in the first year, four in the second year, and one in the final year. Six placebo states are selected from the set of early-legal and *Roe v. Wade* states and randomly assigned across the staggered treatment years. We run 100 permutations over the eight alternative years. Between the two sets of permutations, we have 500+800=1,300 permutations in total.

[72] We follow Ohrn (2018) and Baron et al. (2020) in calculating the p-values.

[73] For each cohort, we again vary both the timing of adoption and the placebo states included. First, we keep only the treated cohort as well as *Roe v. Wade states*, and randomly assign the timing of legal abortion to placebo states, where we select the same number of treated states as in each cohort (e.g., one for 1971 and 1969; four for 1970). For the sample that includes legal abortion states and *Roe v. Wade* states, we consider all years 1960 until the year of legalization (differs by cohort). Second, we run the same analysis with only *Roe v. Wade states* as placebo states from each cohort's treatment year until 1972. In total, we run this permutation for the years 1960 until legalization for the treated cohort states plus *Roe v. Wade* states and *Roe v. Wade* states from the year of legalization until 1972.

(2005)). Oster (2019) uses the observed selection, based on the R-squared, to assess the bias in the treatment effect. Using the R^2 from Table 2 Column (2), and varying levels of R_{max}, we calculate the degree of selection, or δ. δ assesses the unobserved factors relative to observable factors needed to explain away the observed results. Suitable values of δ should be larger than one, meaning that selection on unobservables must be larger than selection on observables to explain away the main findings (Oster, 2019).

Figure D.4 plots δ, the degree of selection, over different values of R_{max}. For both maternal mortality and abortion mortality, δ is above one for all levels of R_{max} (which has a maximum value of one), meaning that the degree of selection on unobserved factors would have to be higher than selection on observables to explain away our findings. Following Oster (2019), we set R_{max} to 1.3 time the observed R^2 in the restricted regression. At this level of R_{max}, in the maternal mortality results $\delta = 1.9$, meaning selection on unobservables would have to be more than 1.9 times the selection on observables (to explain away the effect). For abortion mortality, the results are even stronger; selection on unobservables would have to be 8.7 times the selection on observables to explain away our main findings.

8 Mechanisms for the Maternal Mortality Decline

8.1 Is the Decline in Maternal Mortality from Non-Abortion Maternal Mortality?

Here we consider non-abortion maternal mortality to test whether the main decline in maternal mortality arises from the direct channel of abortion-related mortality, or an indirect channel. Non-abortion causes of maternal death should only be affected by legal abortion through indirect changes in pregnancy characteristics and risk factors.[74] If the main decline in maternal mortality comes from a change in pregnancy characteristics, we should observe a decline in non-abortion maternal mortality (in addition to abortion-related mortality).

Figure 7 Panel A shows the impact of legal abortion on maternal mortality subtracting out abortion-related mortality. The plotted points suggest no decline in non-abortion maternal mortality in the IW specification. The TWFE specification shows some decline for non-white women, but the effect is gradual and only appears several years after treatment. Then, in Panel B, we show the impact of legal abortion on the proportion of maternal deaths due to abortion. Here the share of abortion-deaths-to-maternal-deaths falls for all three measures of mortality, indicating that, abortion as a cause of maternal deaths, falls significantly after legal

[74]Non-abortion maternal mortality is our best approximation of whether pregnancy risk is changing, as we cannot observe everyone who becomes pregnant. While data on who gives birth exists (which we study in Section 8.5) to the best of our knowledge, data on who becomes pregnant does not exist.

abortion.

Together, these findings indicate that the main immediate decline in maternal mortality is primarily explained by the reduction in abortion-related deaths. The failure of non-abortion maternal mortality to decline suggests that other channels, such as a change in pregnancy characteristics, play a minor role. The lack of an apparent reduction in non-abortion maternal mortality also helps to reduce the plausibility of alternative explanations for the decline in maternal mortality, such as improved perinatal care and technological progress in obstetric care. These non-abortion maternal mortality results support our central interpretation of the findings–that the decline in maternal mortality is likely due to lower abortion-related mortality–rather than another factor.

8.2 Heterogeneity by Age and Urban Status

Next, we consider which types of mortality decline the most after legal abortion. We consider the impact of legal abortion on maternal and abortion-related mortality by age and by urban status.[75] Figure E.1 shows these findings. Figure E.1 shows the most apparent decline in non-white maternal mortality is for younger women, those under 30. For abortion-specific mortality, the declines are most apparent for those between 20 and 39. For urban versus rural, non-white mortality declines most clearly for the urban group.

8.3 Heterogeneity by County-level Characteristics

Next, we consider whether the mortality decline varies within each state for non-white women. To examine state-level heterogeneity, we consider the impact of legal abortion at the county level. Since few counties have any maternal or abortion-related deaths in a given year, we adopt a Poisson model, following a county-level version of Appendix Equation 3.[76] A Poisson model is more appropriate than OLS for outcomes with a large number of zeros. Within Equation 3, we interact full legalization with an indicator for below the state-level median of various observable characteristics.[77] Throughout the discussion of results, we focus on maternal mortality, because abortion mortality displays no statistically-significant evidence of heterogeneous impacts, possibly due to the smaller number of abortion deaths throughout

[75] For urban and rural, we use deaths that occurred in a city versus not in a city. By age is grouped into less than 20 (per female population 15-19), 20-29 (per female 20-29), 30-29 (per female 30-39), and over 40 (per female population 40-44).

[76] Of the 68k observations; only 15% have a non-zero number of maternal deaths.

[77] For income, education, share urban, physicians, and hospital deliveries, we use characteristics in 1960, comparing each county to the 1960 state-level median. For religious characteristics, we use church membership in 1971, which is unavailable earlier.

the United States.[78]

Table 3 reveals clear heterogeneity across county-level income and education.[79] In counties below the state-level median in terms of income, average years of schooling, the share of the population with 12 or more years of education, and the share urban, there is a larger decline in non-white maternal mortality due to legal abortion. The observed heterogeneity aligns with our main findings by race, and suggests that the demographic groups most affected by legal abortion likely were disadvantaged groups (counties with lower levels of income and education).

Then, in Columns (6) and (7), we split counties by religiosity, including church members per capita and Catholics per capita. A priori, we might expect that areas with fewer church members or Catholics per capita may have a higher demand for abortion, and, thus, may have a more notable impact of legal abortion. However, the results suggest little heterogeneity by religious affiliation.

Next, in Columns (8)-(11), we test whether the decline in non-white maternal mortality varies across the medical doctors per capita, the share of hospital births, and the share of hospital births by race. Counties with fewer physicians and more out-of-hospital births may be disadvantaged in terms of available medical services. Further, as discussed in Chay and Greenstone (2000); Anderson et al. (2020a), a low share of hospital deliveries for Blacks could also signal persistent discrimination against the Black population in the medical system (despite desegregation and attempts at anti-discrimination policies). For example, Anderson et al. (2020a) cites a survey from the 1950s, suggesting that of 67 urban general hospitals, only seven freely admitted Black patients. While major efforts to desegregate took place over the 1960s, and the share of Black hospital births increased, discrimination still persisted.

The results in Columns (8), (9), and (11) reveal that counties with fewer per capita physicians and a larger share of out-of-hospital births show a clearer decline in non-white maternal mortality due to legal abortion. These findings indicate that counties with fewer medical resources benefited more from legal abortion in terms of lower non-white maternal mortality. To attempt to disentangle discrimination from access to medical care, we add a final specification where we consider the ratio of the share of hospital deliveries for non-white-births-to-white-births.[80] A low relative share of hospital births for non-white deliveries compared to white deliveries should reveal the county-level differences in access to the medical system (by race) rather than general medical access. Column (12) reveals that legal abortion is most impactful

[78] Note that the number of observations is smaller for abortion deaths than maternal deaths. Abortion deaths have more frequent cases of all zero observations, and the Poisson model only includes groups with at least one non-zero outcome.

[79] See Table E.1 for white maternal and abortion mortality.

[80] In other words, the share of deliveries occurring in hospitals for non-white births over the share of deliveries in hospitals for white births. A lower ratio here could suggest discrimination.

in counties with below median non-white hospital deliveries relative to white deliveries. This finding provides suggestive evidence indicating that counties with more discrimination in the medical system may have benefited more from legal abortion.

Overall, while speculative, these county-level findings emphasize the importance of legal abortion for groups likely to be disadvantaged. Non-white maternal mortality declines are clearest after legal abortion in counties with lower incomes, lower levels of education, and limited access to medical services. Counties with potentially more discrimination in the medical system also show larger reductions in non-white maternal deaths.

8.4 Heterogeneity by County-level Travel Distance

Then, we consider whether spillovers from early-legal states potentially impacted nearby non-repeal states. Women frequently traveled to repeal states, which is clear in anecdotal observations from the period. For example, in one account from Massachusetts, the author describes, "*with the advent of the New York law, it was extraordinary what a difference it made. Within 1 month there was not one more illegal case that came through the office. There was not one more trip to London*" (Rubin, 1994, pg. 50). Based on these observations, we consider whether counties in closer proximity to repeal states experienced spillovers from major repeal cities. We calculate the travel time in three steps:

1. We compute the distance from each U.S. county centroid the closest major city in NY, CA, and DC (Los Angeles, San Francisco, Buffalo, New York City, and DC).[81]
2. We construct the inverse travel time (in hours, assuming 60 miles of driving takes an hour), placing a higher treatment weight on states in close proximity to repeal states.
3. We interact the inverse travel time with an indicator for the closest repeal state having passed full legal abortion. This indicator will be equal to one in the year the closest repeal state passes legal abortion and zero beforehand.[82]

Using the travel time (in hours) interacted with the timing of treatment, we consider whether counties closer to repeal states experienced a reduction in maternal or abortion-related mortality when the repeal state passed legal abortion. We use two measures of travel distance–(1) continuous travel time and (2) categorical groupings of driving time. In this analysis, we only consider non-repeal states over the years leading up to *Roe v. Wade*, 1959-1973.

Table E.2 shows the results by travel distance, with clear differences across race. Non-white maternal mortality declines the most in areas closer to repeal states, while white mater-

[81] This approach follows the cities of focus from Joyce et al. (2013), except we also include DC in our travel distance calculation for all states.

[82] For instance, counties in Connecticut will be treated by New York in 1970, counties in Arizona by California in 1969, and counties in Maryland by DC in 1971.

nal mortality declines in both the baseline indicator, and the interaction with distance. White maternal mortality also falls across grouped travel distances (less than 16 hours). These findings suggest that non-white women may have been more sensitive to the travel costs to obtain a safe and legal abortion in nearby repeal states, with higher declines in non-white maternal mortality for counties closer to early-legal states. Despite the importance of travel distance for maternal mortality, there is no clear decline in abortion mortality across travel distance.

We conclude by emphasizing that states nearer and farther from repeal states differed across observable characteristics (similar to early-legal states versus *Roe v. Wade* states). Tables D.1 and E.3 reveal that states closer to early-legal repeal states had a higher abortion rate and were less religious. Thus, we caveat these findings by distance by noting that they reflect associations with other factors, such as abortion demand, rather than causal impacts of travel distance. We also re-emphasize that Table E.2 shows no evidence that abortion mortality declined across any travel distance.

8.5 Did Delivery Characteristics Change?

Finally, we conclude by testing the impact of legal abortion on delivery characteristics for all available U.S. births reported in the Natality Detail Files from 1968 onward.[83] These findings help contextualize our main results by testing whether maternal and infant characteristics shifted in the wake of legal abortion. Ex-ante, we expect younger unmarried mothers to be the most impacted, as they likely had the most limited access to abortion pre-legalization while simultaneously experiencing a large share of unwanted pregnancies. Figure 7 Panel B reveals the expected decline in teen births, a corresponding increase in the average maternal age, and an increase in the share of births to married mothers.[84] As anticipated, in light of existing evidence in works like Cates et al. (2003); Donohue et al. (2009); Ananat et al. (2009), the clearest prevention of unwanted pregnancies is for younger mothers.

9 Conclusion

In this study, we consider whether the 1960s and 1970s legalization of abortion in the United States led to improvements in maternal health. Our findings suggest that legal abortion

[83] These records collected through the Centers for Disease Control (CDC) and the National Center for Health Statistics (NCHS) include microdata of U.S. deliveries for all U.S. states. The data consists of maternal characteristics, including age, education, and marital status, as well as infant health measured by birth weight. We also show additional results measuring infant health, including infant mortality, neonatal mortality, and the fertility rate in Appendix Section I. See Section H.4 for notable data issues.

[84] Until 1978, the birth certificates record whether the newborn was born 'legitimate,' and in 1978 and onward, the certificates report whether the newborn was born to a married mother.

reduced non-white abortion-related mortality by 30-60% and non-white maternal mortality by 30-40%. In the first year after the passage of legal abortion, this percentage decline translates into 41 non-white maternal deaths averted in early-legal states and 113 non-white maternal deaths averted nationally. To ground the magnitude of the deaths averted in present-day maternal deaths, a total of 299 non-white women died from maternal causes of death in 2019, despite a broader classification of maternal deaths today (Hoyert, 2007, 2022). Further, as described in Section 6, the estimated decline in maternal mortality represents the "tip of the iceberg" in terms of the health effects of legal abortion (Clarke and Mühlrad, 2021).

In an era where *Roe v. Wade* no longer determines abortion laws in the United States, we conclude with two facts worth considering for policy today. First, during the period of our study, legal abortion acts primarily through lower abortion-related deaths, rather than a change in pregnancy-related risk factors. The importance of abortion-related maternal mortality indicates that eliminating unsafe and illegal abortion was likely the main driver of mortality declines discovered in this study. Second, legal abortion appears most important for non-white women, and also has the largest impact in counties with lower levels of income, educational attainment, and healthcare resources. Put together, the impact of legal abortion shows marked heterogeneous impacts by race and socioeconomic status, where legal abortion appears most important for less advantaged groups.

Still, based on these observed facts, the maternal mortality impacts of a post-*Roe v. Wade* legal landscape are unclear. A number of factors are different today than in the 1970s. Most notably, the availability of medical abortion, which can be prescribed through telemedicine appointments, sent through the mail, and safely administered at home (Grossman et al., 2011; Kohn et al., 2019; Verma and Shainker, 2020).[85] Instead, we conclude by emphasizing the importance of legal abortion for non-white maternal health during the period of initial legalization. Today, in the U.S., non-Hispanic black women already suffer three times the maternal mortality of white women (Carroll, 2017; Artiga et al., 2020), and if there is a health impact of legal abortion restrictions, it will likely be for this group.

[85] Similar strategies using medical abortion to reduce abortion deaths have been suggested in related settings (e.g., Harper et al. (2007); Fawcus (2008); Baggaley et al. (2010); Jelinska and Yanow (2018)).

10 Tables

Table 1: Summary Statistics, by Legal Status

	Early Legal Pre	Early Legal Post	Reform Pre	Reform Post	Roe v. Wade Pre	Roe v. Wade Post
	Mean	Mean	Mean	Mean	Mean	Mean
Maternal						
Maternal Mortality Rate	3.625	0.885	4.025	1.315	2.602	0.780
Maternal Mortality Rate, White	1.829	0.660	2.169	0.769	1.973	0.586
Maternal Mortality Rate, Non-white	9.022	1.620	13.673	2.986	9.393	2.321
Abortion						
Abortion Mortality Rate	0.826	0.088	0.601	0.102	0.346	0.043
Abortion Mortality Rate, White	0.340	0.051	0.332	0.048	0.239	0.028
Abortion Mortality Rate, Non-white	2.203	0.173	1.851	0.264	1.523	0.244
Demographic Controls						
Share Reproductive Age Females, 15-19 White	0.120	0.128	0.170	0.171	0.196	0.192
Share Reproductive Age Females, 15-19 Non-white	0.061	0.064	0.037	0.044	0.017	0.021
Share High School Educated	0.366	0.477	0.297	0.406	0.327	0.447
Log(Income Per Capita)	8.095	8.921	7.777	8.595	7.940	8.820
Log(Per Pupil Education Expenditure)	6.558	7.649	6.225	7.236	6.408	7.454
Policy Controls						
1(Abortion Reform)	0.030	0.182	0.000	1.000	0.000	0.000
1(Minor's Access to Pill)	0.000	0.485	0.006	0.693	0.045	0.342
1(Pill Access)	0.864	1.000	0.792	1.000	0.810	0.996
1(Unilateral Divorce)	0.167	0.591	0.084	0.403	0.088	0.642
1(Equal Pay Laws)	0.818	0.848	0.351	0.523	0.548	0.733
Inductions Per 1,000 Males 18-25	11.899	1.978	13.136	4.381	15.641	0.337
Population						
State Population (Millions)	6.400	7.835	2.996	3.373	3.601	3.981
State Share Females 15-44	0.212	0.232	0.204	0.222	0.201	0.221
State Share White Females 15-44	0.145	0.158	0.173	0.183	0.186	0.201
State Share Non-white Females 15-44	0.067	0.074	0.031	0.039	0.015	0.020
N	66	66	154	176	420	240

Sources: NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980. State population characteristics are from Ruggles et al. (2021) (shares and means). Population totals use to construct denominators from Wolfers (2006) (also the source of the unilateral divorce laws). Income per capita from Jordan and Grossmann (2020). Per pupil spending from NCES (1959-1980). Reproductive policy laws and equal pay laws from Myers (2017, 2021a). Induction data from Bitler and Schmidt (2012).

Notes: Unweighted means reported. California is the only state that passed a reform and also repealed its anti-abortion legislation. California is included with the early-legal states. Pre and post indicate before and after the legalization, reform, or passage of *Roe v. Wade* (dates vary by state). Maternal mortality and abortion mortality are per 100,000 females 15-44. Non-white maternal mortality and non-white abortion mortality are per 100,000 non-white females 15-44. White maternal mortality and white abortion mortality are per 100,000 white females 15-44.

Table 2: Legal Abortion in Early-treated States, Inverse Probability Weights and Alternative P-values, 1959-1972

	All Early Legal		CA in 1969		NY, WA, HI, AK in 1970		DC in 1971	
	(1) Base	(2) IPW	(3) Base	(4) IPW	(5) Base	(6) IPW	(7) Base	(8) IPW
Panel A: Non-white Maternal Mortality								
1(Legal Abortion)	-0.3361***	-0.4046***	-0.4031***	-0.4485***	-0.3692***	-0.4964***	-0.3868***	-0.3235**
	(0.0616)	(0.1092)	(0.0728)	(0.0907)	(0.0857)	(0.1242)	(0.1209)	(0.1280)
N	504	504	434	434	476	476	434	434
Adjusted R-squared	0.653	0.782	0.612	0.587	0.614	0.778	0.551	0.525
P-value from Permutation Tests	0.024	0.036	0.177	0.149	0.059	0.076	0.255	0.269
Panel B: Non-white Abortion Mortality								
1(Legal Abortion)	-0.5782***	-0.5429***	-1.0624***	-1.0993***	-0.5687***	-0.5461***	-0.2247	-0.1835
	(0.1490)	(0.1782)	(0.1024)	(0.1043)	(0.1206)	(0.1129)	(0.2166)	(0.1939)
N	504	504	434	434	476	476	434	434
Adjusted R-squared	0.593	0.715	0.496	0.503	0.581	0.719	0.437	0.445
P-value from Permutation Tests	0.005	0.005	0.012	0.014	0.015	0.020	0.167	0.162
State FE and Year FE	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X

Sources: See Table 1 and Table B.1 for all data sources.

Notes: Results from a difference-in-differences, with CA, NY, DC, WA, HI, and AK relative to *Roe v. Wade* states without abortion reforms in Column (1)-(2). Columns (3)-(8) show separate linear regressions considering each early-treated cohort relative to *Roe v. Wade* states (excluding those with abortion reforms). State and year fixed effects included. 'Base' indicates the baseline model with population weights (non-white females 15-44). 'IPW' indicates the same results using inverse probability weights (IPW), with the IPW calculated after a logistic regression considering the outcome variable, early legalization, and predictors, church membership per capita, share with a high school degree, the log of per pupil education spending, and the log of family income. Following the logistic regression, propensity score weights (PSW) are calculated. Then, the inverse probability weights are defined as 1/PSW for the early-treated group and 1/(1-PSW) for the *Roe v. Wade* group. After calculating the IPW, the IPW are multiplied by the non-white females 15-44 to recover the population weighted results. For the calculation of the nonparametric p-values see notes in Figure D.2 and the text in Section 7.9. Robust standard errors clustered at the state level. ***, **, * represent statistical significance at 1, 5 and 10 percent levels. Our main set of state-level demographic controls includes the share of reproductive-age females who are 15-19 and white, the share of reproductive age females who are 15-19 and non-white, the log of per capita income, the log of per pupil education spending, and the state-level share with a high school degree. We also include policy controls for state-level abortion reforms, access to the pill for minors, access to the pill generally, unilateral divorce legislation, state equal pay legislation, and the inverse hyperbolic sine of state-level inductions per 1,000 males 18-25. Outcome is the inverse hyperbolic sine of the mortality rate. Rates are per 100,000 reproductive-aged females in each population (all, white, and non-white). Regressions are weighted by either the inverse probability weight or the denominator of the rate in the baseline model.

Table 3: County-level Heterogeneity for Non-white Maternal and Abortion-related Mortality, Above and Below the State-level Median

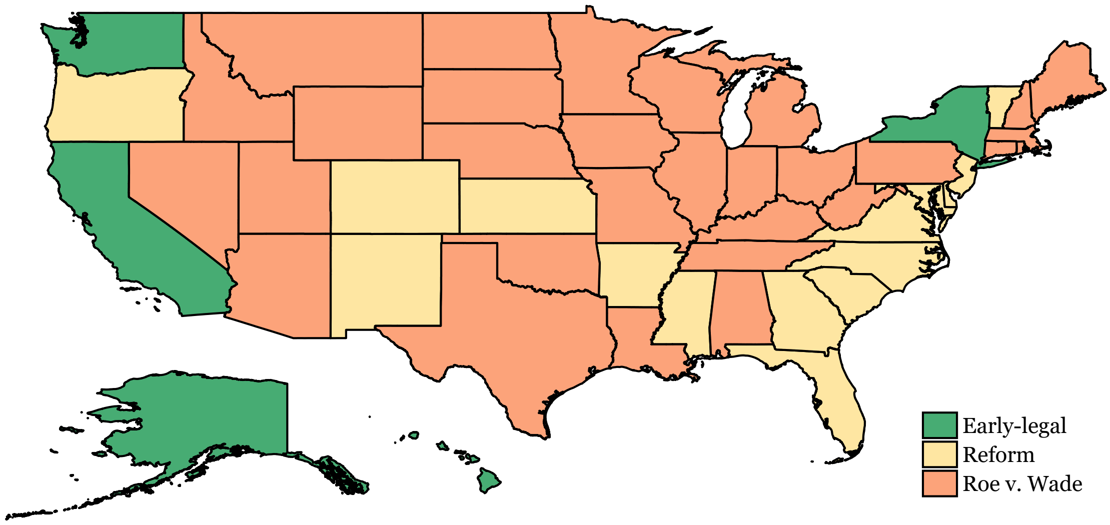
	All	Share Urban	Median Income	Median Years Schooling	Share High School	P.C. Church Members	P.C. Cath-olics	MDs Per 1,000	Hospital Births All	Hospital Births White	Hospital Births Non-white	Ratio Hospital Births Non-white to-White
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Non-white Maternal Mortality												
1(Legal Abortion)=1	-0.2399***	-0.2296***	-0.2240***	-0.2152***	-0.2175***	-0.2299*	-0.2335**	-0.2357**	-0.0950	-0.2137**	-0.0974	-0.1223
	(0.0643)	(0.0644)	(0.0641)	(0.0645)	(0.0644)	(0.1249)	(0.1044)	(0.1049)	(0.0907)	(0.0869)	(0.0872)	(0.0891)
1(Legal Abortion)=1 × 1(Below Median)=1		-0.2167**	-0.2232***	-0.2351***	-0.2110**	-0.0144	-0.0719	-0.1185	-0.2164***	-0.0520	-0.2063***	-0.1729**
		(0.0953)	(0.0838)	(0.0912)	(0.0885)	(0.0844)	(0.0780)	(0.0814)	(0.0685)	(0.0684)	(0.0721)	(0.0728)
N	24,186	24,168	24,168	24,168	24,168	23,944	23,944	23,856	23,750	18,520	18,520	18,520
Panel B: Non-white Abortion Mortality												
1(Legal Abortion)=1	-0.7549***	-0.7606***	-0.7549***	-0.7659***	-0.7681***	-0.8473***	-1.0971***	-1.1210***	-0.6912***	-0.5933**	-0.4919**	-0.5503**
	(0.1890)	(0.1901)	(0.1892)	(0.1925)	(0.1930)	(0.3191)	(0.2501)	(0.2499)	(0.2447)	(0.2439)	(0.2446)	(0.2510)
1(Legal Abortion)=1 × 1(Below Median)=1		0.4607	0.0021	0.2321	0.2683	-0.3054	0.1813	0.3320	-0.1124	-0.2105	-0.3281	-0.2511
		(0.3438)	(0.3371)	(0.2341)	(0.2298)	(0.2562)	(0.2933)	(0.3160)	(0.2031)	(0.2061)	(0.2113)	(0.2139)
N	8,574	8,574	8,574	8,574	8,574	8,442	8,442	8,376	8,420	7,608	7,608	7,608
County FE and Year FE	X	X	X	X	X	X	X	X	X	X	X	X

SOURCES: NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980. County-level population data and county-level characteristics (population of females 15-44) from Bailey et al. (2016); Haines (2010); Ruggles et al. (2021) with specifics discussed in Section H.6 (see Table B.1 for all data sources). Population characteristics based on the 1960 data in Haines (2010). Religious affiliation data from Johnson et al. (1971). Medical doctors from AHRF (1994). Hospital births from NVSS (1959-1968).

NOTES: Results from a county-level Poisson model. County and year fixed effects are included, but no covariates are included. The outcome is the (linear) maternal and abortion deaths, and the exposure is the county-level population of females 15-44. Robust standard errors from xtpoisson are reported, which are equivalent to clustering on the panel variable (county). ***, **, * represent statistical significance at 1, 5 and 10 percent levels. Note that the number of observations is smaller for abortion deaths than the maternal deaths. Abortion deaths have more frequent cases of an individual county having zero deaths, and the Poisson model only includes groups with at least one non-zero outcome. Columns (2)-(12) interact an indicator capturing below the state-level median for noted characteristics. State-level medians and the county characteristics from 1960 for the share urban, median income, median years schooling, share with 12 or more years of education, medical doctors per 1,000, and the share of hospital deliveries. For religious characteristics, we use church membership in 1971 because this data is unavailable earlier.

11 Figures

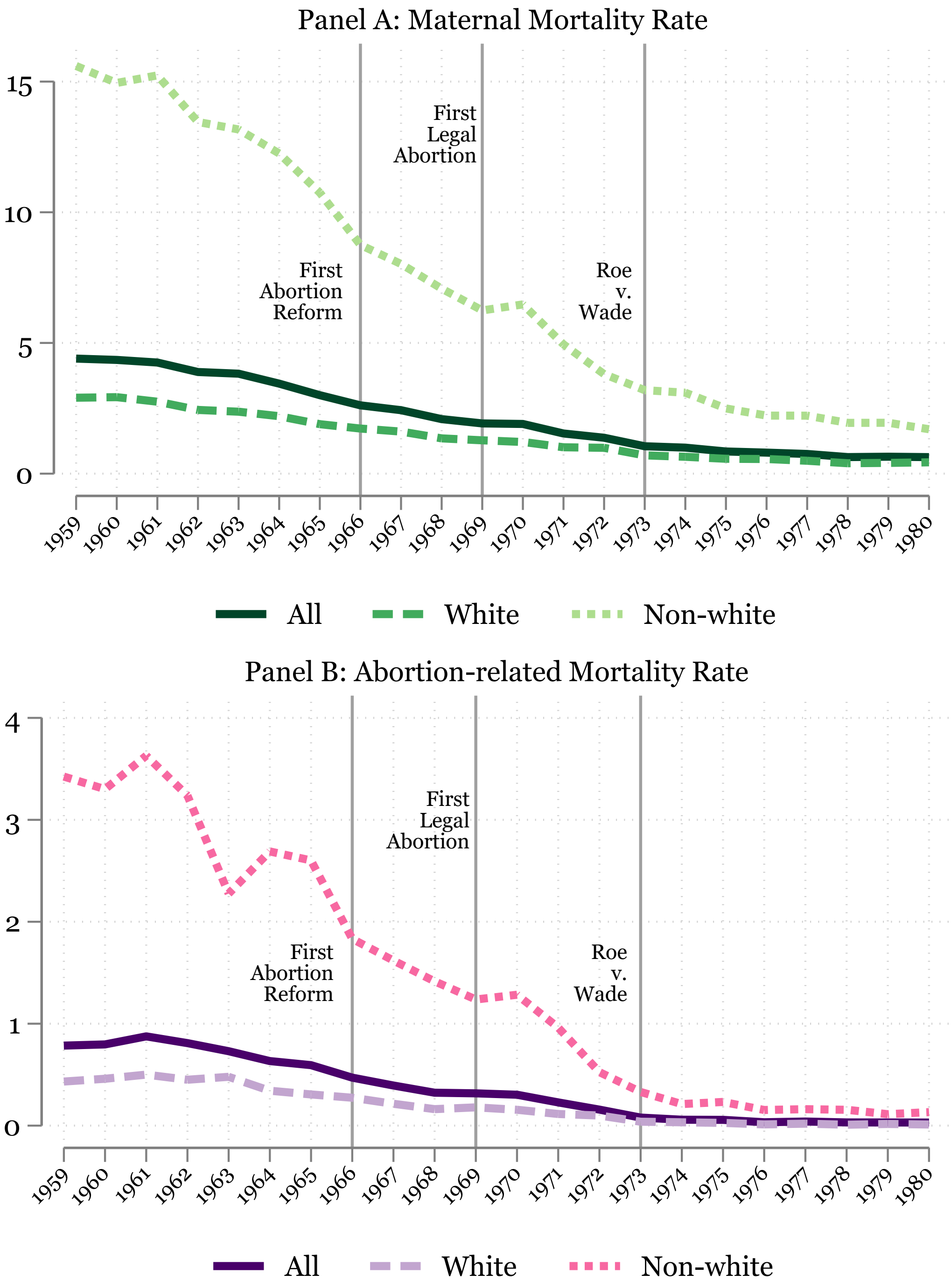
Figure 1: Legal Regimes by State



SOURCES: CDC (1969-1980), Rubin (1994), Merz et al. (1996), Myers (2021a)

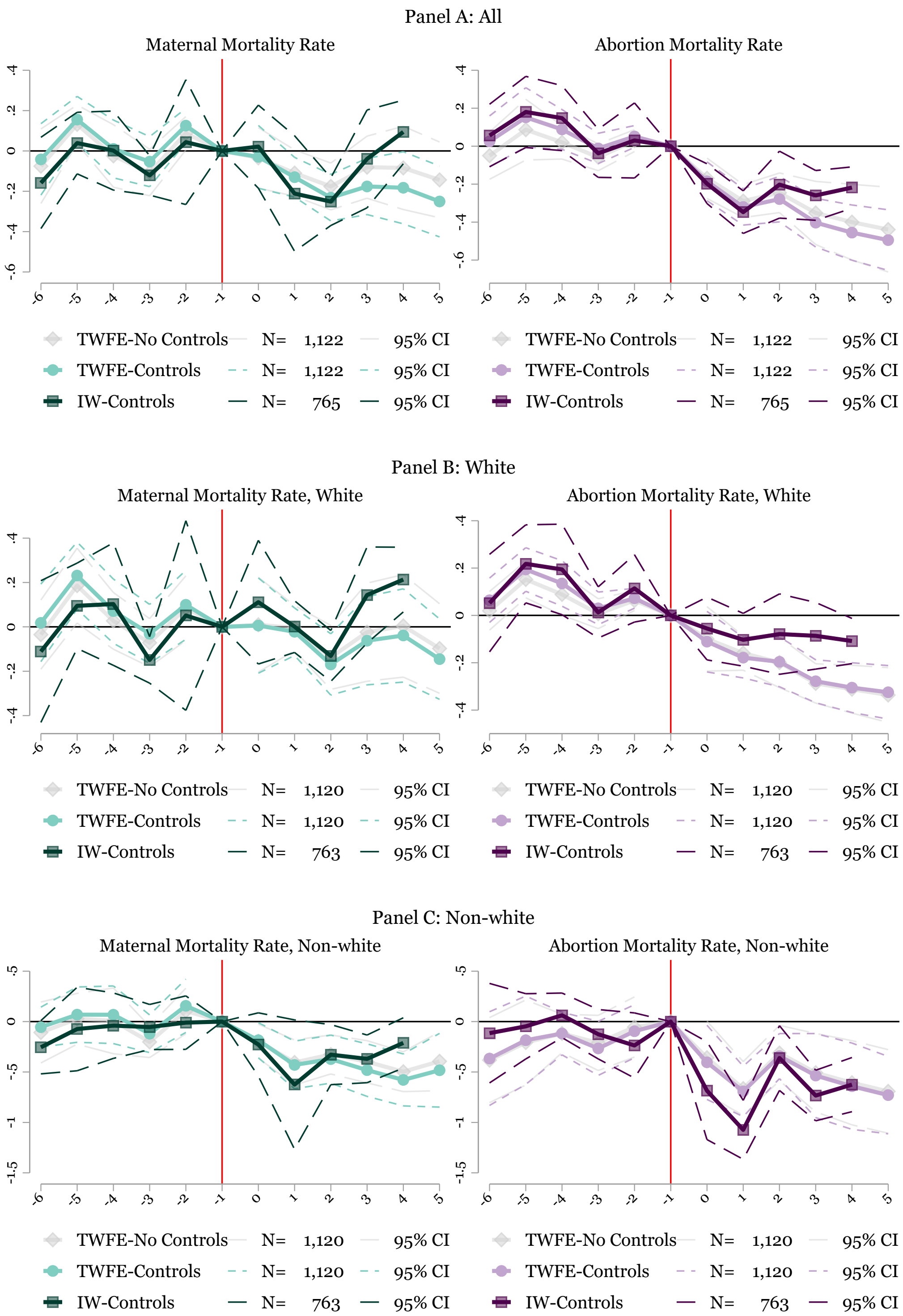
NOTES: Specific legalization dates as follows: 1969–CA; 1970–NY, AK, HI, WA; 1971–DC; 1973–National (*Roe v. Wade*). Specific reform dates as follows: 1966–MS; 1967–CO, NC, CA; 1968–MD; 1969–AR, DE, NM, GA, OR; 1970–SC, KS, VA; 1972–FL, VT, NJ.

Figure 2: Trends in Maternal and Abortion-Related Mortality, 1959-1980



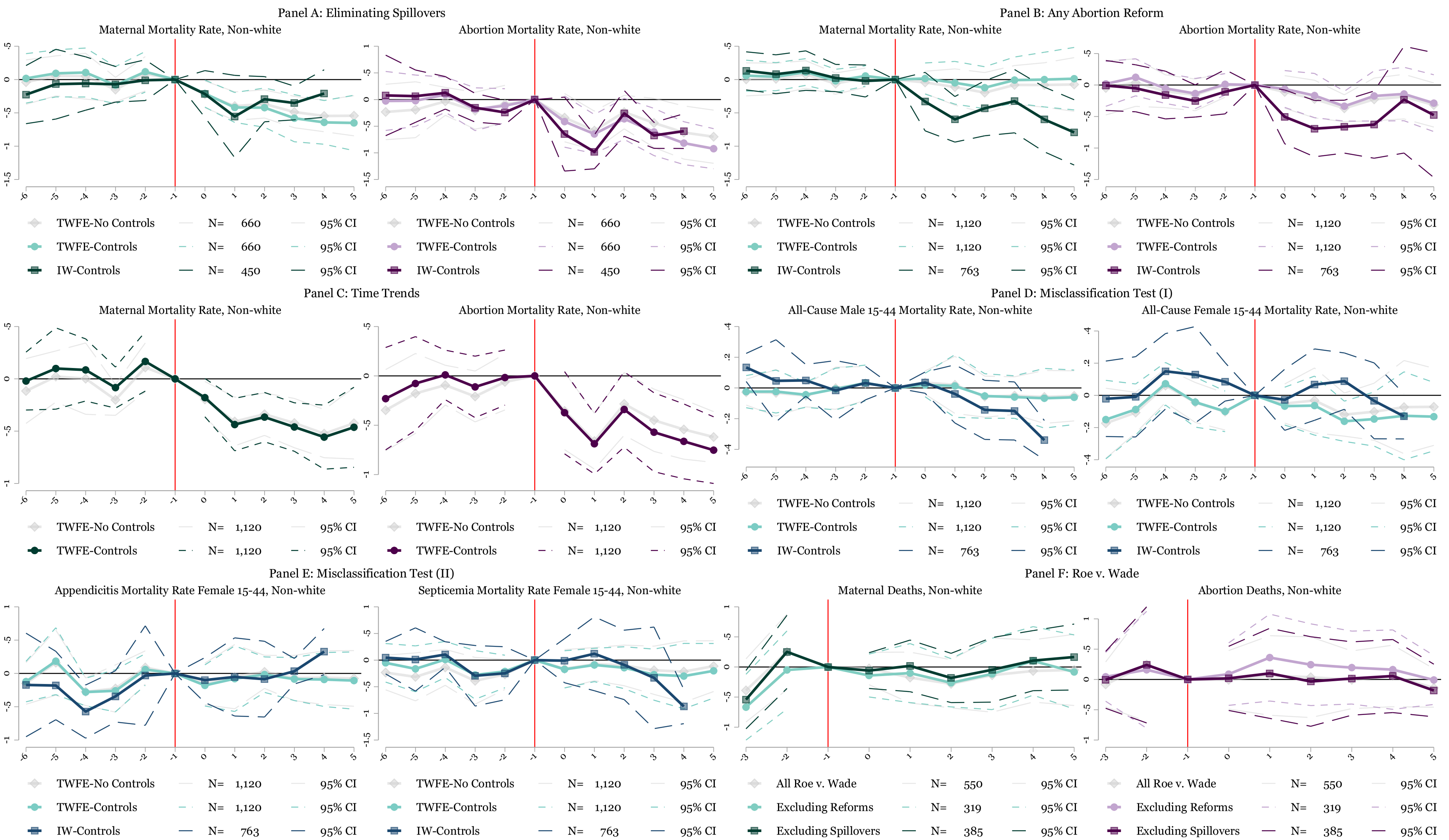
Sources: NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980. Population denominators from Ruggles et al. (2021) and Wolfers (2006).
Notes: Rates are per 100,000 reproductive-aged females in each population (all, white, and non-white).

Figure 3: Event Study: Effect of Legal Abortion on Maternal and Abortion-Related Mortality



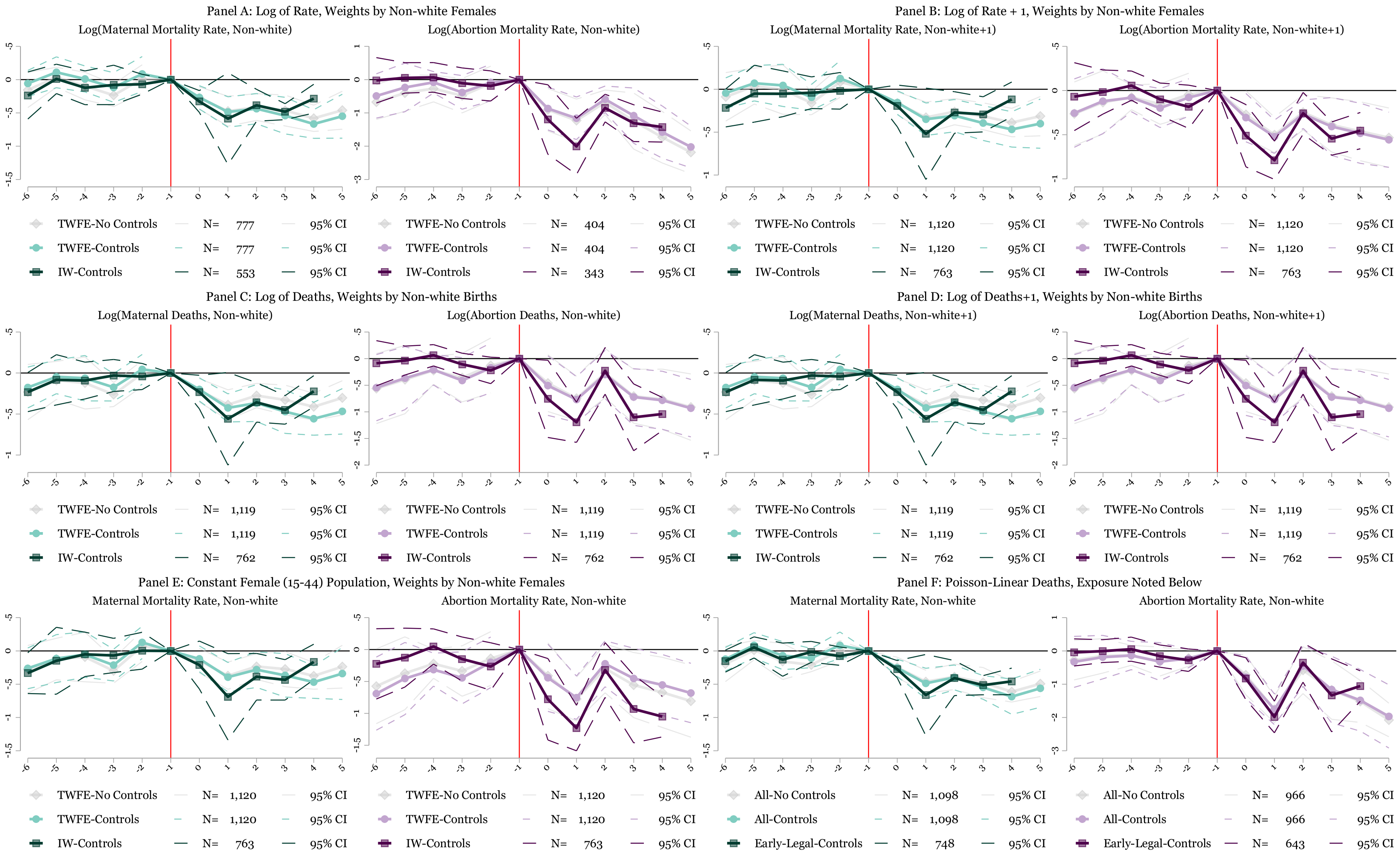
Notes: OLS coefficients presented above. Baseline fixed effects include year fixed effects and state fixed effects. Plotted coefficients are dummy variables on each year before and after the change to abortion policy (see Equation 1). The period just before the legal change is the excluded period (-1)–indicated by the vertical line. For the two-way fixed effects specification (TWFE), the left endpoint is binned at $m = -7$, and the right endpoint is binned at $m = 6$. For the Interaction-Weighted (IW) specification, the event study is fully saturated. The IW specification only considers the years 1959-1973, with *Roe v. Wade* states as the last-treated comparison group. Only the point estimates in the main event window are displayed. Dashed and dotted lines reflect 95% confidence intervals. Robust standard errors clustered at the state level. We take the inverse hyperbolic sine of the mortality rate as the main mortality rate of focus (unless otherwise noted). Maternal mortality and abortion-specific mortality are per 100,000 females 15-44. Non-white (white) rates are per 100,000 non-white (white) reproductive-age females. Estimates are weighted by the denominator of the rate. Our main set of state-level demographic controls includes the share of reproductive-age females who are 15-19 and white, the share of reproductive age females who are 15-19 and non-white, the log of per capita income, the log of per pupil education spending, and the state-level share with a high school degree. We also include policy controls for state-level abortion reforms, access to the pill for minors, access to the pill generally, unilateral divorce legislation, state equal pay legislation, and the inverse hyperbolic sine of state-level inductions per 1,000 males 18-25. See Table 1 for the sources, and Table B.1 for all data sources

Figure 4: Event Study Robustness (I) for Non-white Maternal and Abortion-related Mortality



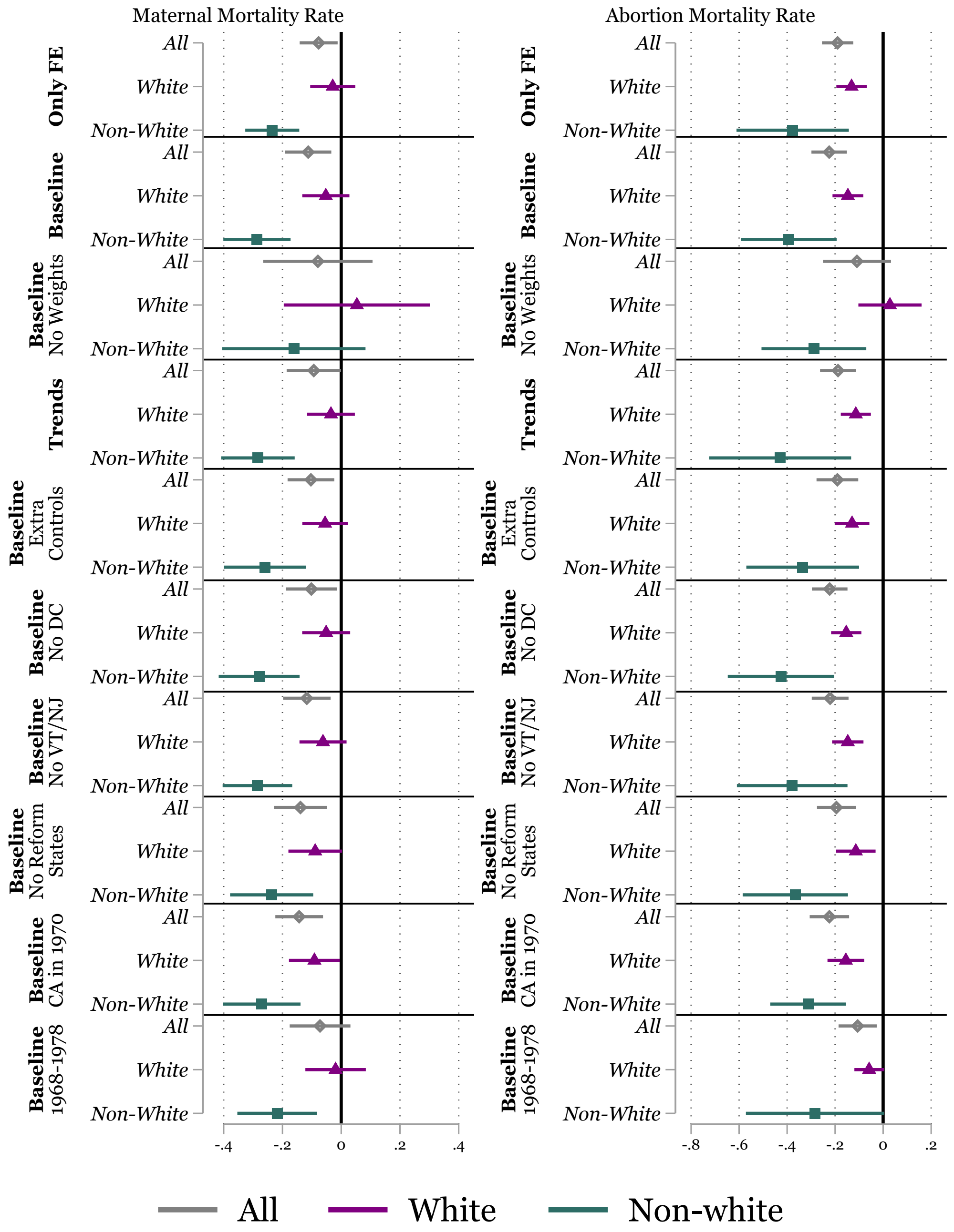
Notes: Reflects Figure 3, except altering the specification in each panel as follows. Panel A omits spillover states. Panel B considers the effect of both abortion reforms and legalization. Panel C adds state-level linear time trends. Panel D presents a misclassification test using all-cause female mortality and a placebo test using all-cause mortality for reproductive-age males. Panel E presents a misclassification test using appendicitis and septicemia mortality for reproductive-age females. Panel F considers *Roe v. Wade* relative to 1969/1970 early-treated states (over 1970-1980), first with all *Roe v. Wade states* then omitting reform states, then excluding spillover states. The *Roe v. Wade* results do not have staggered timing of adoption and we only implement TWFE (with controls).

Figure 5: Event Study Robustness (II): Alternative Functional Form



Notes: Reflects Figure 3, except altering the functional form and weights in Panels A-E. Panel A shows the log of the mortality rate, weighted by the population of non-white females, 15-44. Panel B shows the log of the mortality rate plus one, weighted by the population of non-white females, 15-44. Panel C shows the log of the death counts, weighted by non-white births. Panel D presents the log of the death counts plus one, weighted by non-white births. Panel E uses a constant denominator of females 15-44 from the first year of the sample. In Panel E, we also omit census controls; and only include policy controls as well as the log of per capita income, and the log of per pupil spending. Panel F shows the state-level Poisson model. For the Poisson specification, state and year fixed effects are included. The outcome is the (linear) maternal and abortion deaths, and the exposure is the state-level population of females 15-44. Confidence intervals based on robust standard errors from xtpoisson, which are equivalent to clustering on the panel variable (state).

Figure 6: Difference-in-Differences Results: Maternal and Abortion-related Mortality

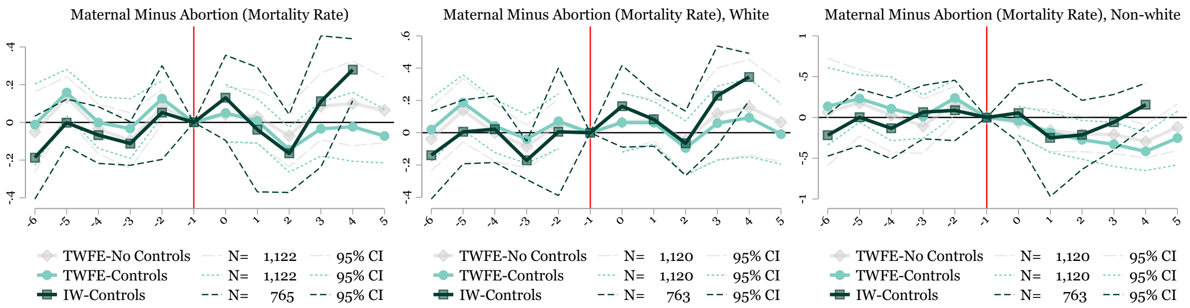


NOTES: Non-white is shown in squares, white in triangles, and all in diamond OLS coefficients presented above. Baseline fixed effects include year fixed effects and state fixed effects. The main binary variable represents legalized abortion, which captures the effect of early legal abortion as well as the 1973 *Roe v. Wade* decision (see Equation 2). We take the inverse hyperbolic sine of the mortality rate as the main mortality rate of focus (unless otherwise noted). Maternal mortality and abortion-specific mortality are per 100,000 females 15-44. Non-white (white) rates are per 100,000 non-white (white) reproductive-age females. Estimates are weighted by the denominator of the rate. Our main set of state-level demographic controls includes the share of reproductive-age females who are 15-19 and white, the share of reproductive age females who are 15-19 and non-white, the log of per capita income, the log of per pupil education spending, and the state-level share with a high school degree. We also include policy controls for state-level abortion reforms, access to the pill for minors, access to the pill generally, unilateral divorce legislation, state equal pay legislation, and the inverse hyperbolic sine of state-level inductions per 1,000 males 18-25.

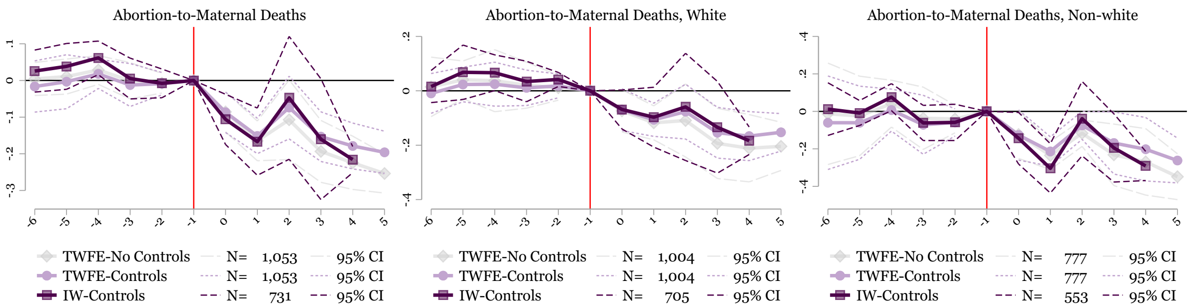
SPECIFICS OF EACH SPECIFICATION: First, we show the specification with only fixed effects (no controls or trends). Second, we add controls, and in the third specification, the same results without weights. Fourth, we add trends to the baseline specification. Fifth, we add additional controls (the state-level economic conditions measured by the unemployment rate from Haines (2010) (before 1975) and Jordan and Grossmann (2020) (after 1975)), a binary variable capturing state-level access to public health insurance through initial Medicaid implementation (Boudreaux et al., 2016), physicians per 1,000 persons (from the Area Health Resource File, (AHRF, 1994), fair housing laws (Collins, 2006), equal rights amendments (Myers, 2017), and the passage of AFDC UP (Winkler, 1995)). Sixth, we drop DC, then seventh, we omit NJ/VT, and eighth, we omit reform states. Ninth, we adjust California to pass legal abortion in 1970 instead of 1969. Tenth, we include only years that relied on the ICD-8 codes, 1968-1978.

Figure 7: Mechanisms

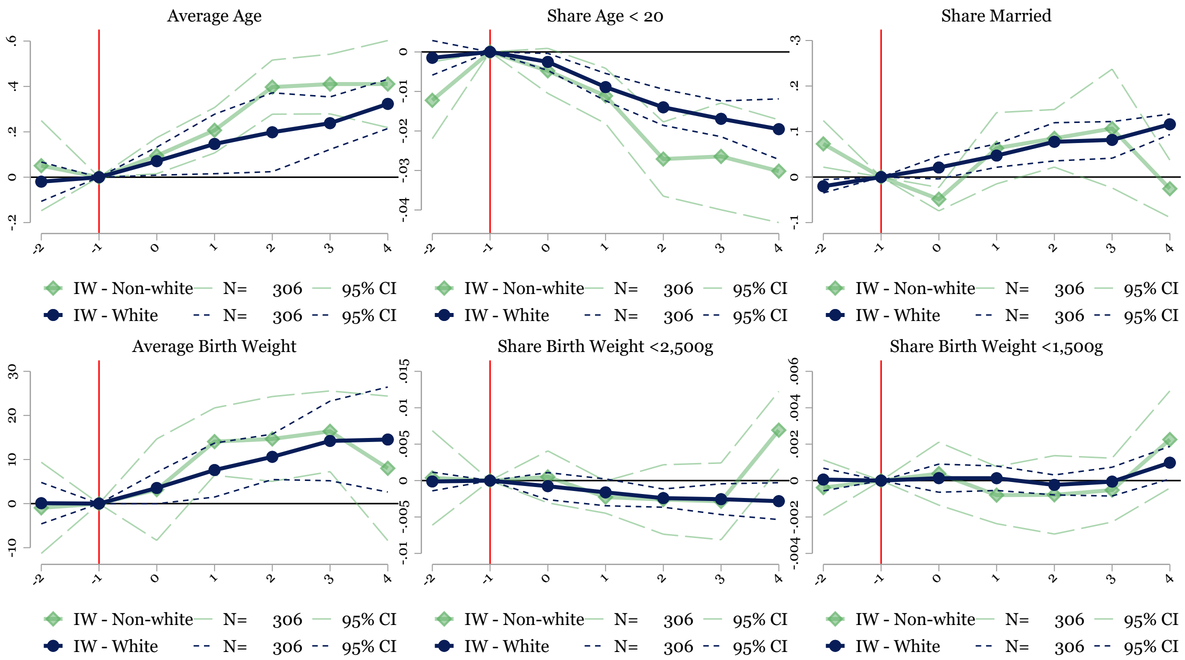
Panel A: Non-abortion Maternal Mortality



Panel B: Proportion of Maternal Deaths Due to Abortion



Panel C: Delivery Characteristics



Notes: Panel A represents the inverse hyperbolic sine of the mortality rate (weighted by the reproductive-age female population). Panel B presents the abortion deaths over the total maternal deaths (weighted by maternal deaths). Panel C shows the average delivery characteristics (weighted by the number of births, white and non-white). For additional information on the specifications in Panels A and B, see notes in Figure 3, the adjusted notes here are for Panel C. OLS coefficients presented above. Baseline fixed effects include year fixed effects and state fixed effects. Plotted coefficients are dummy variables on each year before and after the change to abortion policy (see Equation 1). The period just before the legal change is the excluded period (-1)–indicated by the vertical line. Event study is fully saturated, except we bin the left endpoint at -2 to include a singleton observation. Dashed and dotted lines reflect 95% confidence intervals. Robust standard errors clustered at the state level. Our main set of state-level demographic controls includes the share of reproductive-age females who are 15-19 and white, the share of reproductive age females who are 15-19 and non-white, the log of per capita income, the log of per pupil education spending, and the state-level share with a high school degree. We also include policy controls for state-level abortion reforms, access to the pill for minors, access to the pill generally, unilateral divorce legislation, state equal pay legislation, and the inverse hyperbolic sine of state-level inductions per 1,000 males 18-25. See Table 1 for the sources included, and Table B.1 for all data sources

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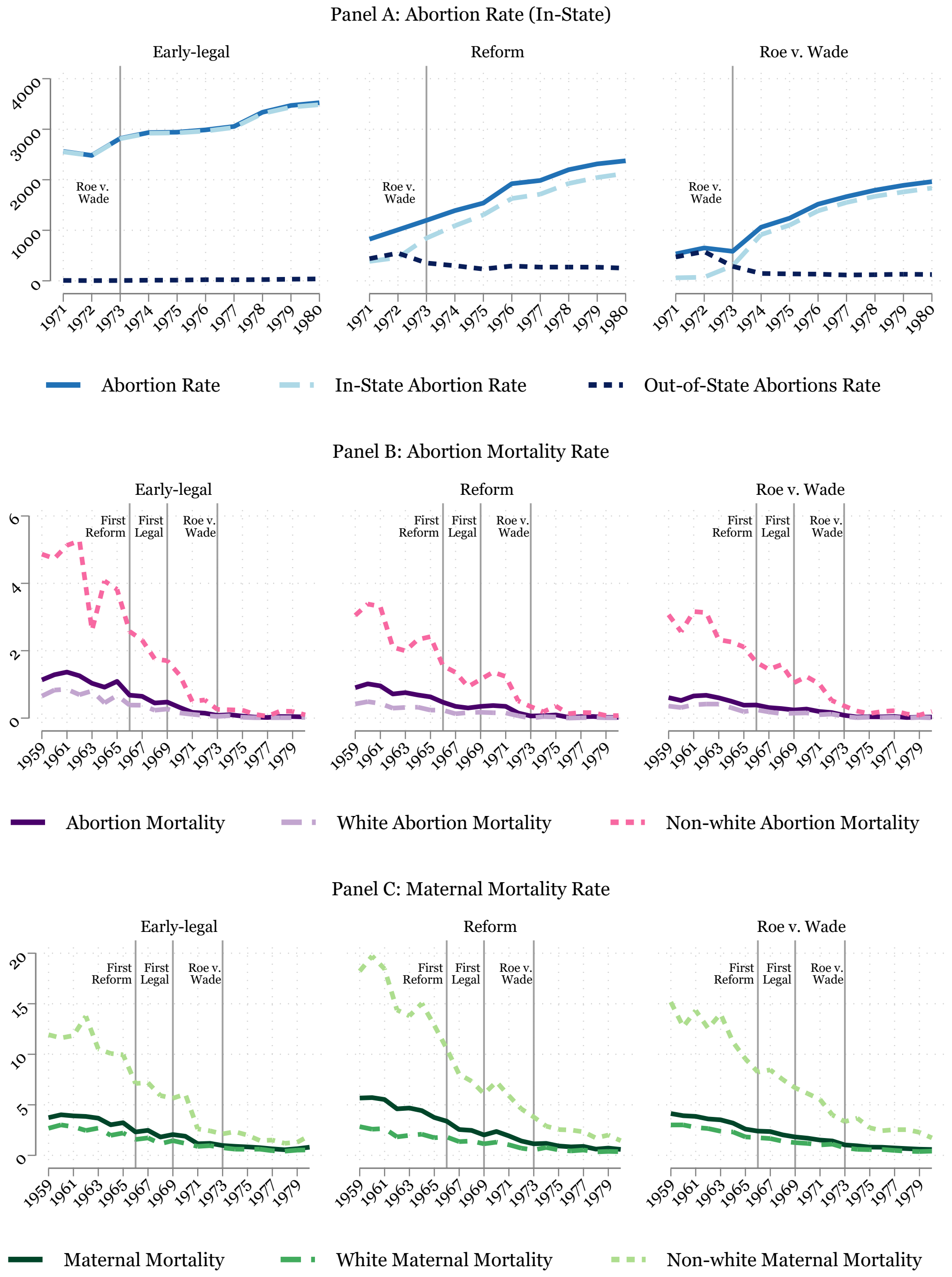
Online Appendices

a) Abortion and Maternal Causes of Death by ICD Code–Section H.1
b) Does the change to the ICD code impact mortality? Section H.2
c) Functional Form–Section H.3
d) Issues and Limitations with the Mortality Data–Section H.4
e) Natality Data–Section H.5
f) County-level Data–Section H.6

9. Additional Results: Fertility, Infant Mortality, and Neonatal Mortality–Section I

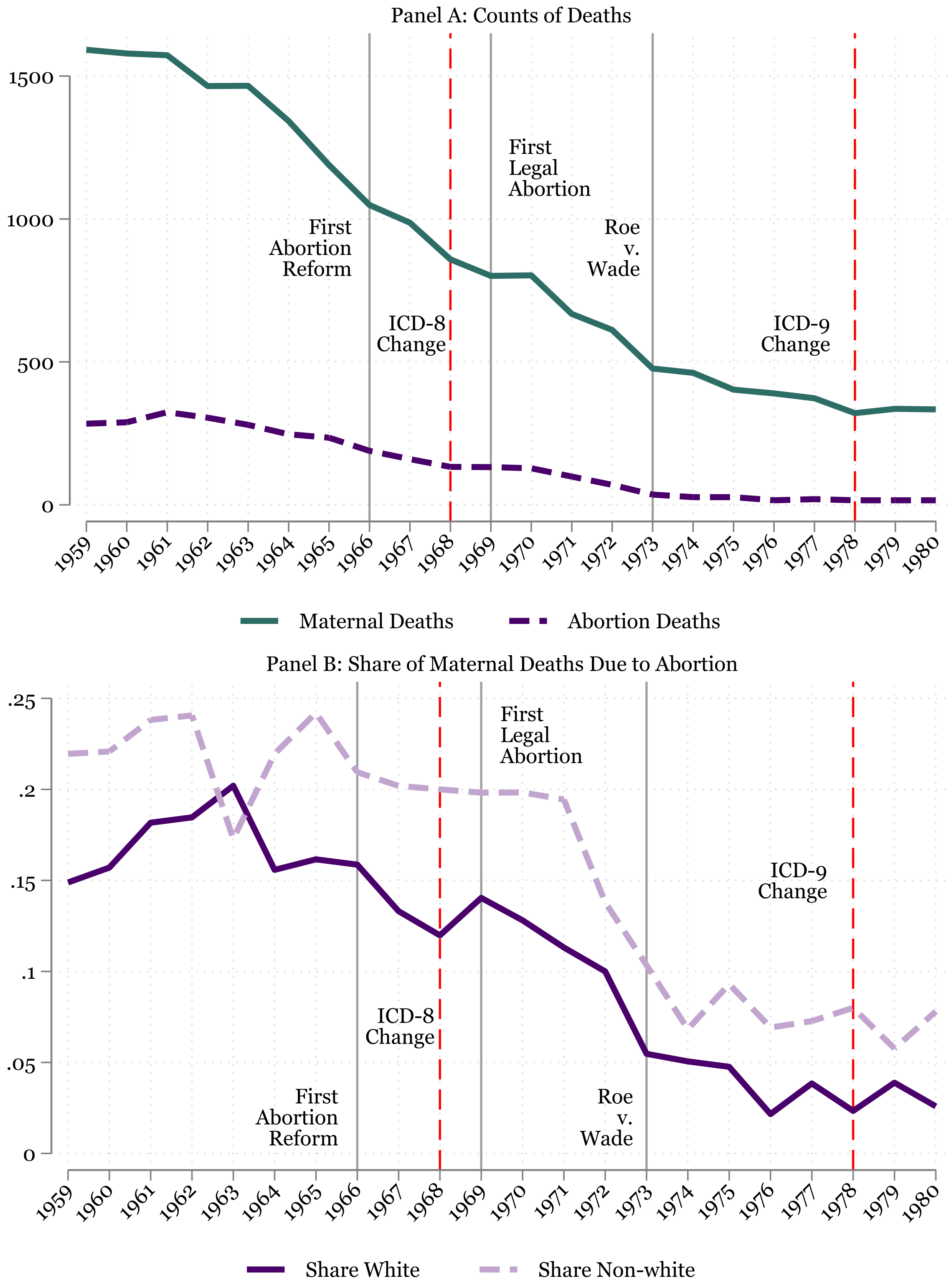
A Additional Descriptive Figures

Figure A.1: Maternal Mortality Rate, Abortion Mortality Rate, and Abortion Rate, by Legal Abortion Status



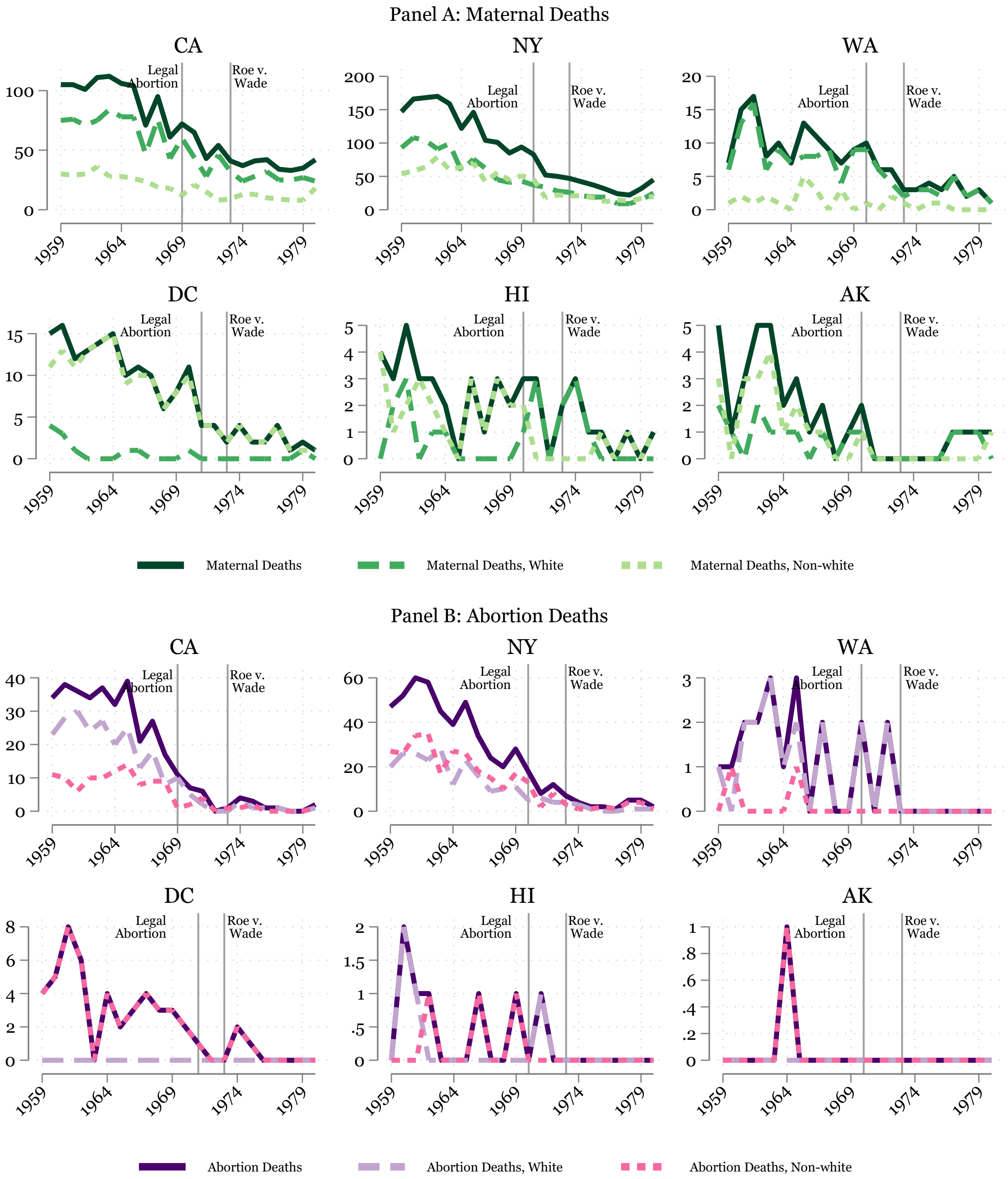
Source: CDC Abortion Surveillance Program, 1971-1980. NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980.
Notes: Rates are per 100,000 reproductive-aged females in each population (all, white, and non-white).

Figure A.2: Trends in Maternal and Abortion-Related Deaths, 1959-1980



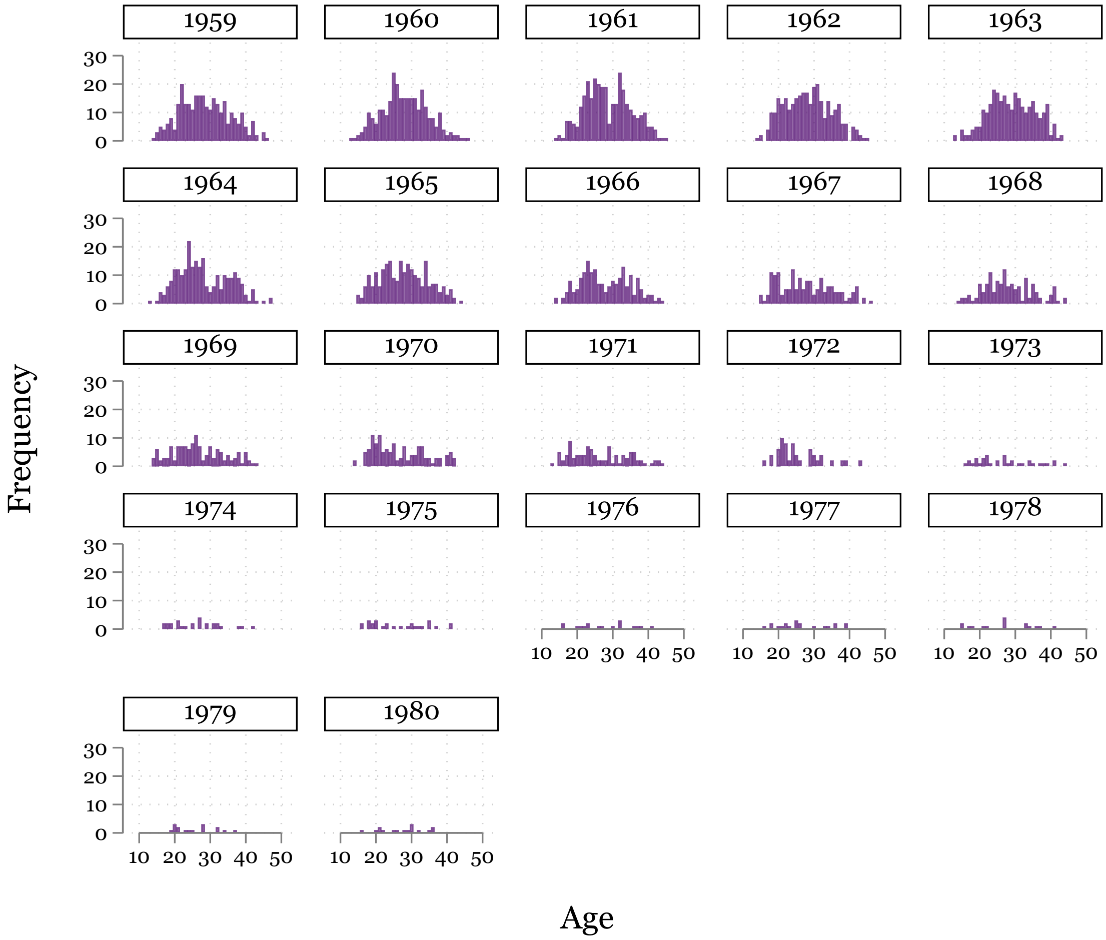
Source: NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980.
Notes: Panel A shows the number of maternal and abortion deaths each year. The vertical lines show the legal changes and the ICD code changes. Panel B shows the proportion of maternal deaths due to abortion.

Figure A.3: State-level Trends in Maternal and Abortion-Related Deaths for Early Legalization States, 1959-1980



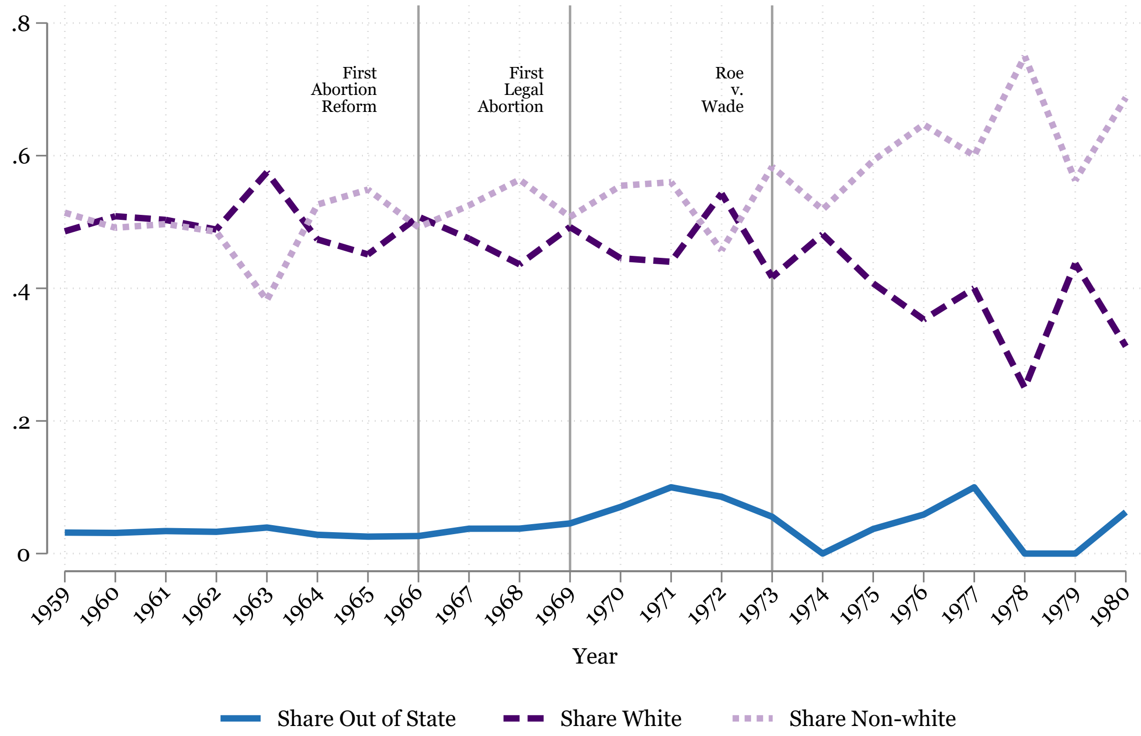
Source: NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980.
Notes: Plotted points represent the number of maternal and abortion deaths.

Figure A.4: Abortion Deaths: Counts over the Age Distribution, 1959-1980



Source: NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980.

Figure A.5: Abortion Deaths: Composition by Race and Resident Status



SOURCE: NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980.

B Additional Tables

Table B.1: Data Sources

Outcomes	
Deaths	NCHS Mortality Files and NBER, 1959-1980 (NCHS, 1959-1980a)
Births I and Delivery Characteristics	NCHS Natality Files and NBER, 1968-1980 (NCHS, 1968-1980b)
Births II	Vital Statistics of the US and NBER, 1959-1968 (NVSS, 1959-1968)
Demographic Controls	
Age Shares	IPUMS USA, 1950-1990 (Ruggles et al., 2021)
Education Shares	IPUMS USA, 1950-1990 (Ruggles et al., 2021)
Population Totals	Wolfers (2006)
Annual Per Capita Income	Bureau of Economic Analysis. 'SA1-3 Personal income summary' via Correlates of State Policy (Jordan and Grossmann, 2020)
Per Pupil Spending	US Department of Education National Center for Education Statistics Statistics of State School Systems (NCES, 1959-1980)
Policy Controls	
Unilateral Divorce	Gruber (2004) via Wolfers (2006)
EPL, ERA, and FEPA	Myers (2017)
Abortion Laws	Myers (2017) and Myers (2021a)
Pill and Minor's Access to Pill	Myers (2021a)
Inductions	Received from Bitler and Schmidt (2012) Combined with 1958/1959 Digitized from Selective Service Records
Additional Data Sources	
Unemployment	Correlates of State Policy (1975+, Jordan and Grossmann (2020)) Historical, Demographic, Economic, and Social Data: The United States, 1790-2002 (Pre-1975, Haines (2010))
Fair Housing Laws	Collins (2006)
Medicaid	Boudreaux et al. (2016)
AFDC UP	Winkler (1995)
1971 Religion Data	Churches and Church Membership in the United States, 1971 (States, Johnson et al. (1971))
Political Alignment	Klarner (2013, 2003) via Correlates of State Policy (Jordan and Grossmann, 2020)
Discrimination Laws	Caughey and Warshaw (2016) via Correlates of State Policy (Jordan and Grossmann, 2020)
Share Urban	Historical, Demographic, Economic, and Social Data: The United States, 1790-2002 (ICPSR 2896, Haines (2010))
MDs	Bureau of Health Professions Area Resource File, 1940-1990: [United States] (ICPSR 9075, AHRF (1994))
Share Hospital Deliveries	Vital Statistics of the US and NBER, 1959-1968 NVSS (1959-1968)
Abortion Counts	CDC Abortion surveillance (CDC, 1969-1980)
County-level Data Sources	
Reproductive-Age Female Population	U.S. County-Level Natality and Mortality Data, 1915-2007 (ICPSR 36603, Bailey et al. (2016))
Missing Reproductive-Age Females	IPUMS USA, 1950-1990 and Historical, Demographic, Economic, and Social Data: The United States, 1790-2002 (ICPSR 2896, Haines (2010))
1971 Religion Data	Churches and Church Membership in the United States, 1971 (Counties, Johnson et al. (1971))
Population Characteristics	Historical, Demographic, Economic, and Social Data: The United States, 1790-2002 (ICPSR 2896, Haines (2010))
Births I	NCHS Natality Files and NBER, 1968-1980 NVSS (1959-1968)
Births II and Share Hospital Deliveries	Vital Statistics of the US and NBER, 1959-1968 NCHS (1968-1980b)
MDs	Bureau of Health Professions Area Resource File, 1940-1990: [United States] (ICPSR 9075, AHRF (1994))

Table B.2: Percent Reduction and Deaths Averted–Estimates from Figure 3, Non-White Maternal and Non-white Abortion Mortality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Non-white Abortion TWFE			**Non-white Abortion IW**			**Non-white Maternal TWFE**			**Non-white Maternal IW**		
	Magnitude of Decline	Observed Number of Deaths	Estimated Deaths Averted	Magnitude of Decline	Observed Number of Deaths	Estimated Deaths Averted	Magnitude of Decline	Observed Number of Deaths	Estimated Deaths Averted	Magnitude of Decline	Observed Number of Deaths	Estimated Deaths Averted
0	-0.34	32	16	-0.51	15	16	-0.17	236	48	-0.21	66	18
1	-0.50	14	14	-0.66	4	8	-0.35	210	113	-0.49	43	41
2	-0.29	24	10	-0.31	12	5	-0.32	179	84	-0.29	41	17
3	-0.43	14	11	-0.52	3	3	-0.39	167	107	-0.31	30	13
4	-0.48	14	13	-0.47	1	1	-0.44	169	133	-0.20	9	2
5	-0.53	10	11				-0.39	161	103			

NOTES: We calculate the deaths averted for each period using the estimated percentage and the observed death count in period m as:
Observed # of Deaths$_m$/(1 + Estimated % Decline) − Observed # of Deaths$_m$.

Table B.3: Difference-in-differences, Additional Controls

	Non-white Maternal Mortality			Non-white Abortion Mortality		
	(1)	(2)	(3)	(4)	(5)	(6)
1(Legal Abortion)	-0.2868***	-0.2595***	-0.2432***	-0.3926***	-0.3351***	-0.3192**
	(0.0570)	(0.0695)	(0.0682)	(0.0990)	(0.1169)	(0.1208)
Share Reproductive Age Females, 15-19 White	-15.0654***	-17.8323***	-16.8626***	-17.1302***	-17.4259**	-16.8420**
	(4.7147)	(4.9291)	(4.8218)	(6.3934)	(7.0600)	(6.6603)
Share Reproductive Age Females, 15-19 Non-white	1.2106	2.1175	3.4320	-20.7604***	-20.4274**	-18.3065**
	(6.9808)	(9.3998)	(7.6531)	(7.5864)	(9.2626)	(8.6018)
Share High School Educated	8.6926***	8.8371**	7.6518**	5.8640	5.6352	6.0672
	(3.2072)	(3.6202)	(3.7497)	(3.6487)	(4.6347)	(5.2220)
Log(Income Per Capita)	-0.3103	-0.8436	-0.3964	0.8073	-0.4588	-0.3799
	(0.7681)	(0.7462)	(0.7584)	(0.7645)	(0.8208)	(0.8218)
Log(Per Pupil Education Expenditure)	-0.4837	-0.2381	-0.0382	-0.6233	-0.2049	-0.0730
	(0.3821)	(0.3953)	(0.3641)	(0.4060)	(0.4051)	(0.4516)
1(Abortion Reform)	-0.1543***	-0.1187**	-0.0690*	-0.1732*	-0.1446	-0.1286
	(0.0520)	(0.0534)	(0.0394)	(0.0877)	(0.0963)	(0.1040)
1(Minor's Access to Pill)	-0.0590	-0.0243	-0.0161	-0.1060	-0.1136	-0.1101
	(0.0639)	(0.0598)	(0.0571)	(0.0957)	(0.0838)	(0.0819)
1(Pill Access)	-0.0021	-0.0436	-0.0531	0.2460*	0.2049	0.2085
	(0.1001)	(0.0869)	(0.0704)	(0.1310)	(0.1441)	(0.1396)
1(Unilateral Divorce)	0.1399**	0.1512**	0.1327**	-0.0946	-0.0750	-0.0847
	(0.0607)	(0.0594)	(0.0576)	(0.0836)	(0.0788)	(0.0796)
1(Equal Pay Laws)	0.0182	0.0743	0.0902	0.0125	0.0401	0.0575
	(0.0651)	(0.0675)	(0.0645)	(0.1304)	(0.1306)	(0.1297)
IHS(Inductions Per 1,000 Males 18-25)	-0.2086**	-0.1767*	-0.1632*	0.1277	0.1132	0.1046
	(0.1029)	(0.0938)	(0.0915)	(0.1096)	(0.1044)	(0.1181)
Unemployment		-0.0110	-0.0147		-0.0431**	-0.0473**
		(0.0282)	(0.0278)		(0.0215)	(0.0200)
1(Medicaid)		0.0159	0.0151		0.1858	0.1927
		(0.0548)	(0.0561)		(0.1203)	(0.1226)
MDs per 1,000		-0.4399	-0.4977		-0.1104	-0.1657
		(0.3122)	(0.3550)		(0.4383)	(0.4803)
1(Fair Housing)		0.0041	0.0103		0.2588**	0.2536**
		(0.0689)	(0.0649)		(0.1100)	(0.1046)
1(Equal Rights Amendment)		0.1383**	0.1532***		0.0730	0.0889
		(0.0540)	(0.0570)		(0.0897)	(0.0893)
1(AFDC-UP)		0.1392*	0.0956		-0.0052	-0.0160
		(0.0806)	(0.0768)		(0.1108)	(0.1063)
Log(Reproductive-Age Family Income, White)			0.5417			-0.0065
			(1.1704)			(1.6075)
Log(Reproductive-Age Family Income, Non-white)			-1.0134*			-0.2402
			(0.5278)			(0.7568)
College Educated, Reproductive Age Females White			-7.2384			-5.2949
			(4.7603)			(4.7765)
College Educated, Reproductive Age Females Non-white			3.6840			3.2557
			(6.8251)			(7.2208)
N	1,120	1,114	1,114	1,120	1,114	1,114
Adjusted R-squared	0.775	0.775	0.776	0.706	0.711	0.711
1965-1968 Mean Dependent	3.006	3.006	3.006	1.457	1.457	1.457
Post-Roe Mean Dependent	1.400	1.400	1.400	0.152	0.152	0.152
State FE and Year FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X

SOURCE: See Table 1, Figure 6, and all sources listed in Table B.1.
NOTES: Reflects baseline specification in Figure 6, with added controls displayed above.

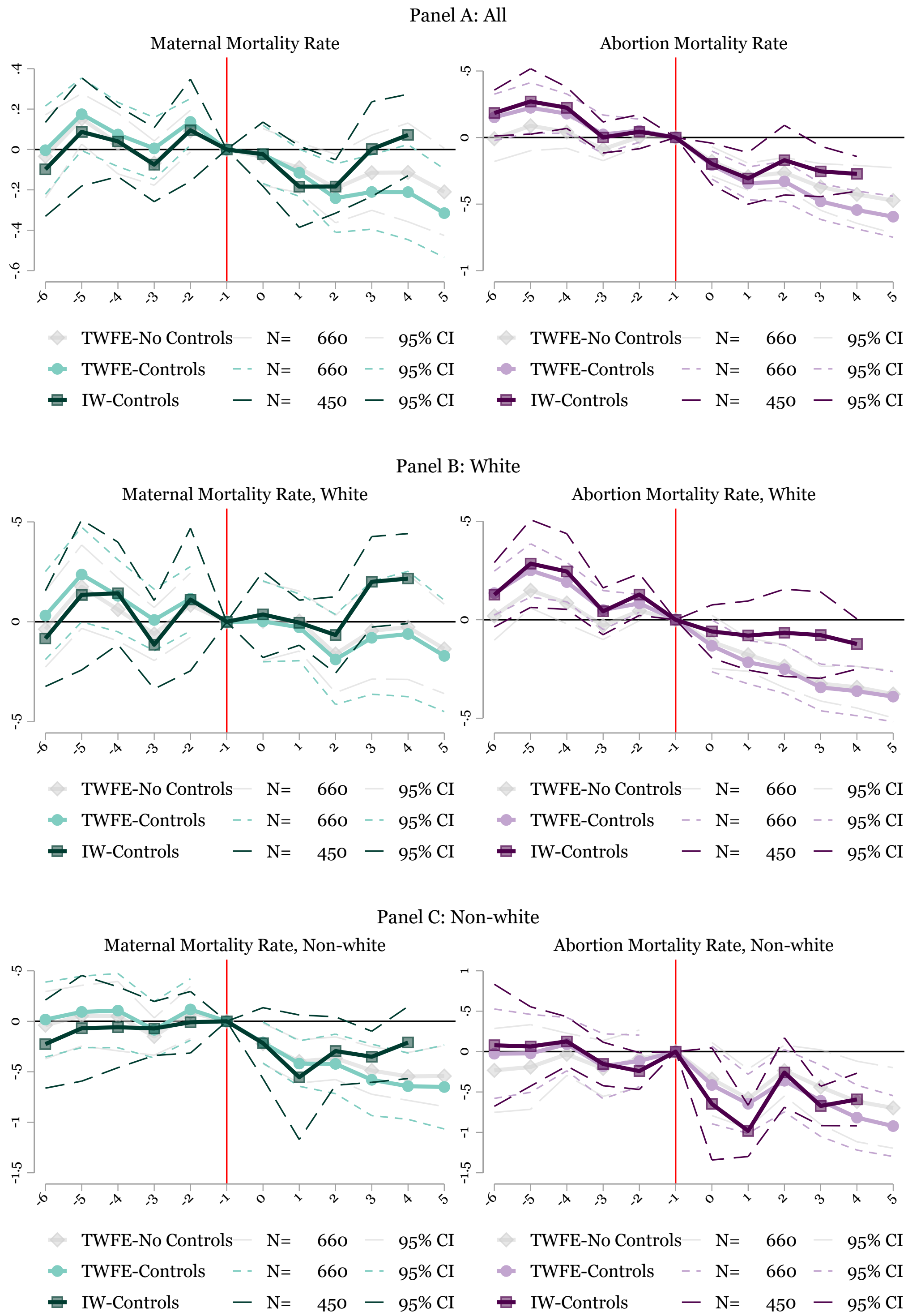
Table B.4: Difference-in-differences, Interaction of Policies

	Non-white Maternal Mortality					Non-white Abortion Mortality				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1(Legal Abortion)=1	-0.2937***	-1.7883***	-0.2277***	-0.2267***	-0.3174***	-0.4224***	-0.5653***	-0.4408***	-0.2862*	-0.3241**
	(0.0675)	(0.0985)	(0.0547)	(0.0686)	(0.0901)	(0.1024)	(0.1609)	(0.1185)	(0.1538)	(0.1347)
1(Legal Abortion)=1 × 1(Unilateral Divorce)=1	0.0348					0.1513				
	(0.1154)					(0.1708)				
1(Legal Abortion)=1 × 1(Pill Access)=1		1.5031***					0.1729			
		(0.0886)					(0.1309)			
1(Legal Abortion)=1 × 1(Minor's Access to Pill)=1			-0.1766					0.1441		
			(0.1111)					(0.1163)		
1(Legal Abortion)=1 × 1(Fair Employment Practices Act)=1				-0.1489*					-0.2565**	
				(0.0864)					(0.1237)	
1(Legal Abortion)=1 × 1(State Bans Discrimination, Public Accommodations)=1					0.0404					-0.1082
					(0.0897)					(0.1109)
N	1,120	1,120	1,120	1,120	1,101	1,120	1,120	1,120	1,120	1,101
Adjusted R-squared	0.775	0.776	0.776	0.776	0.774	0.707	0.706	0.707	0.710	0.709
State FE and Year FE	X	X	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X	X	X

SOURCE: Reflects sources in Table B.3 except Fair Employment Practices Acts (FEPA) from Myers (2021a). Discrimination laws from Jordan and Grossmann (2020).
NOTES: Specification the same as Column (1) of Table B.3, except showing policy interactions with legal abortion.

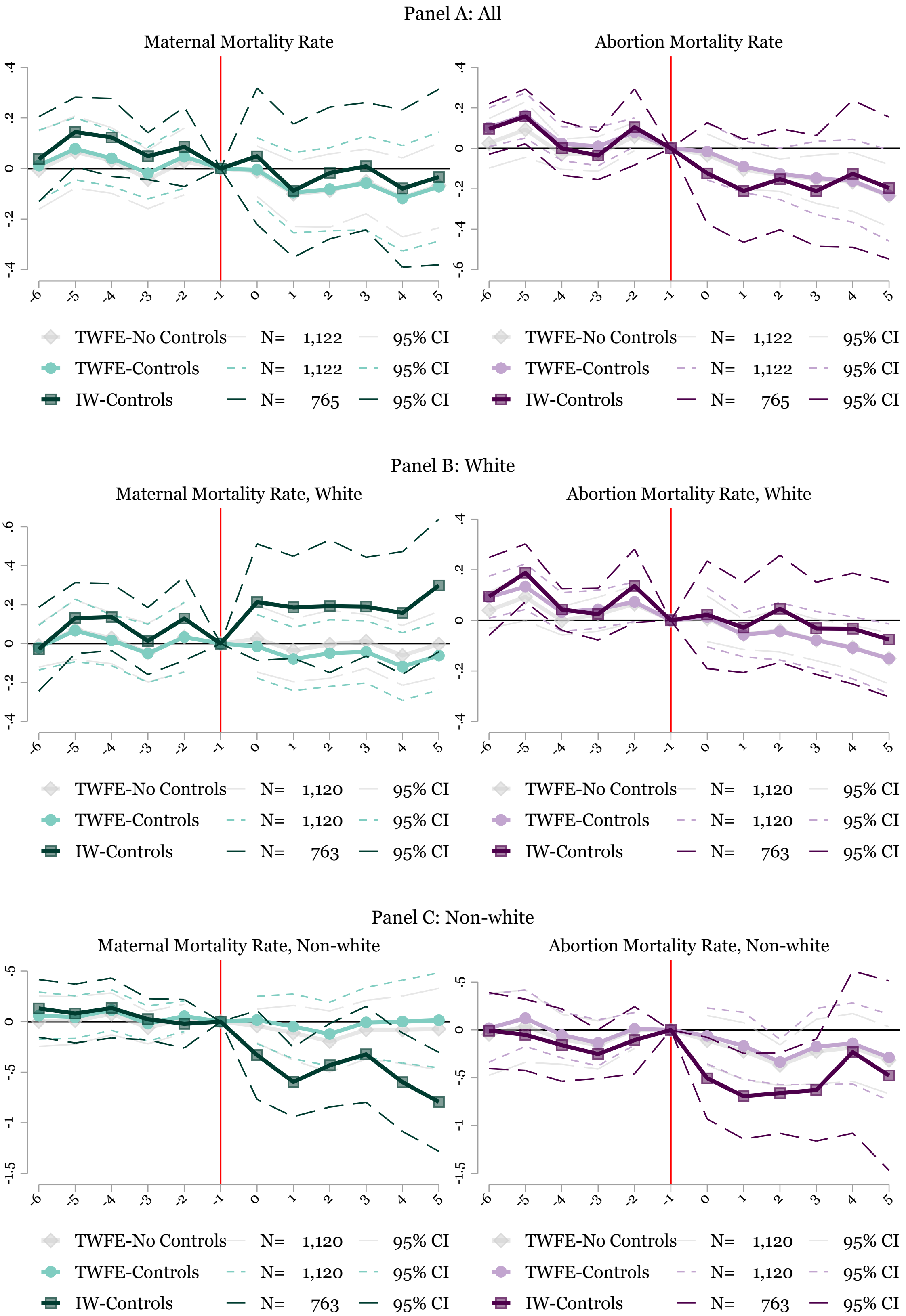
C Additional Event-study Figures

Figure C.1: Effect of Legal Abortion Dropping States within 500 Miles of NY/DC/CA



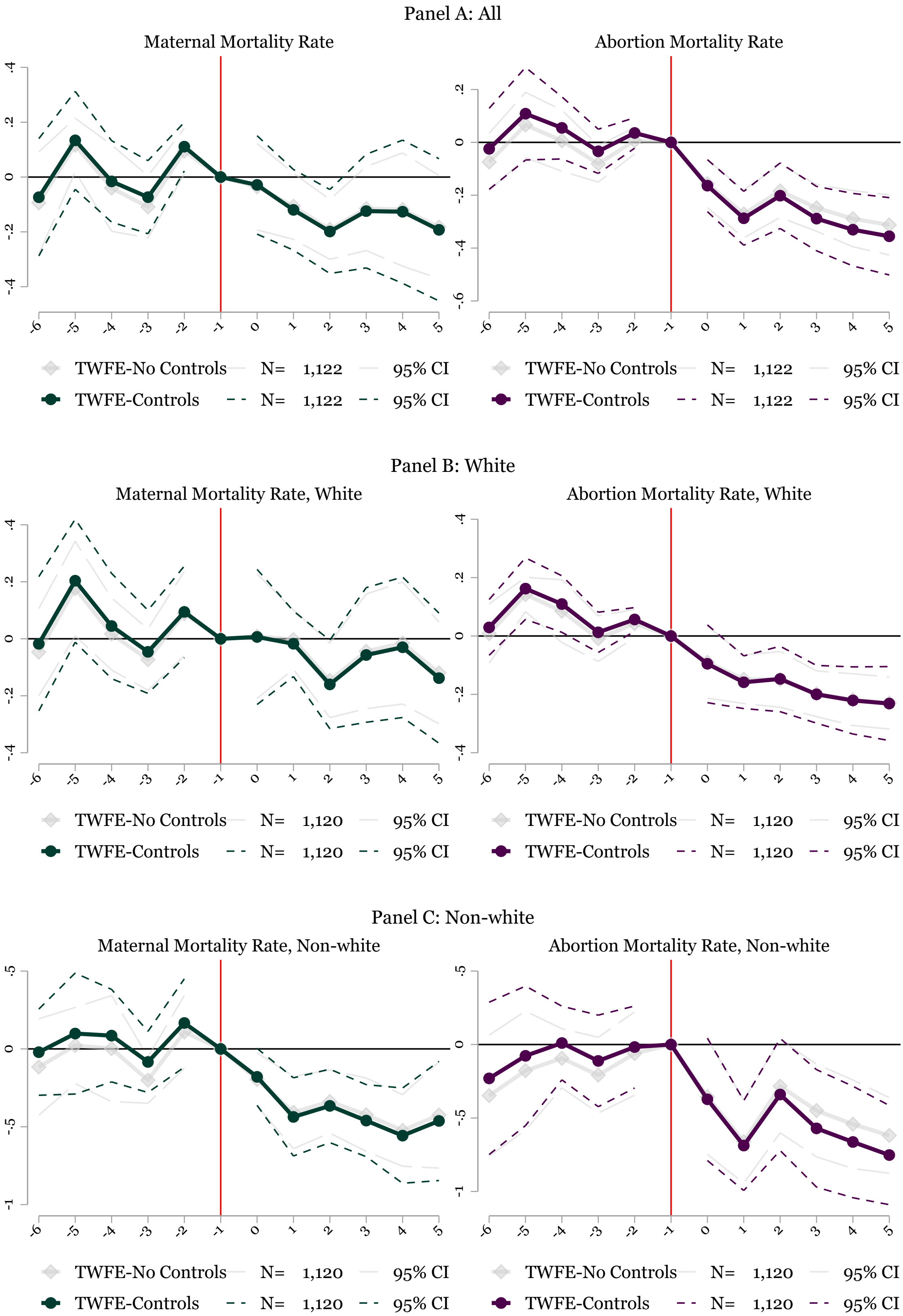
NOTES: Results reflect Figure 3, omitting states within 500 miles of New York, DC, and California. We use the average county-level distance, weighted by the reproductive-age female population from Bailey et al. (2016).

Figure C.2: Combined Impact of Abortion Reforms and Abortion Legalization



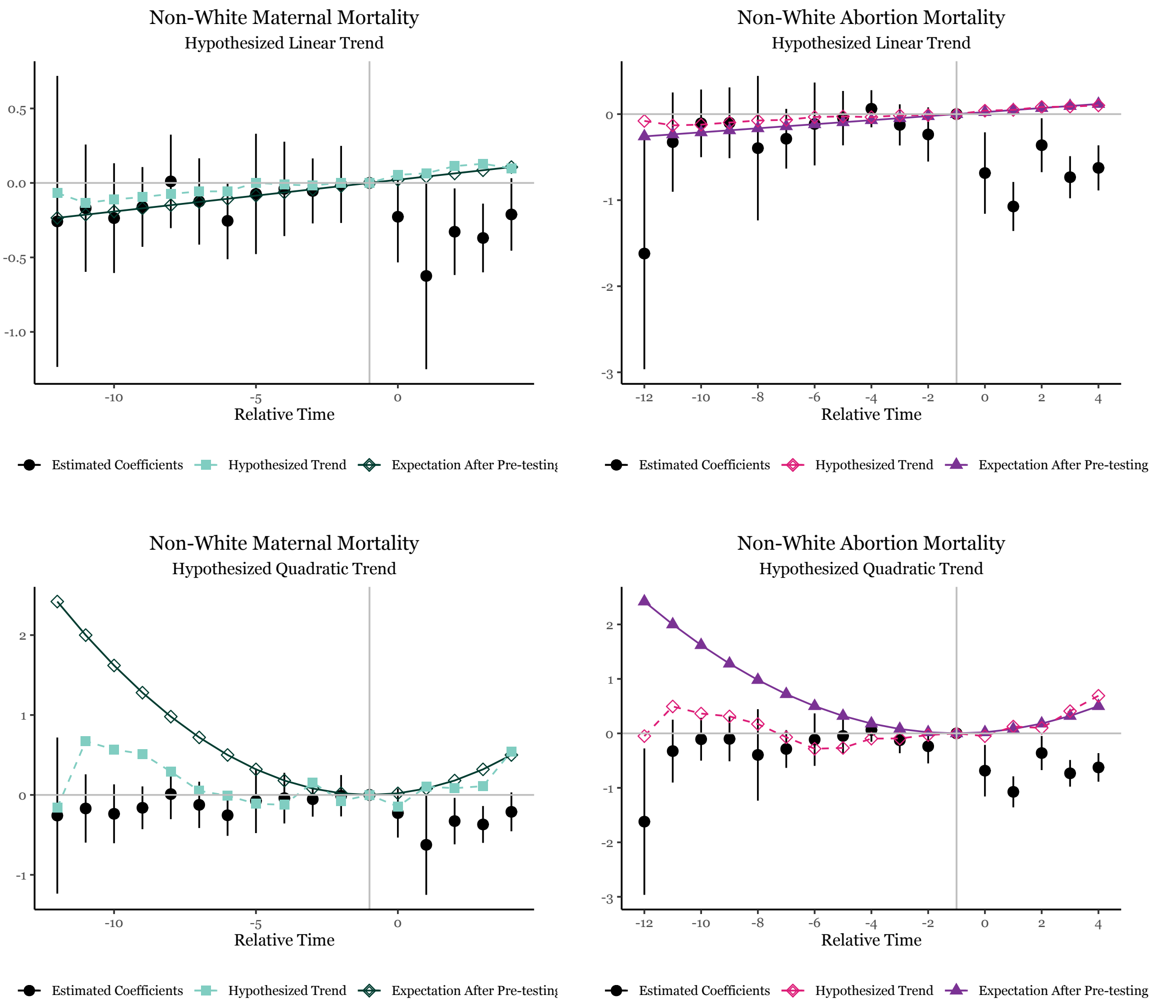
NOTES: Results reflect Figure 3 adding reforms states to the main legalization states.

Figure C.3: Main Results: Adding State-specific Linear Trends



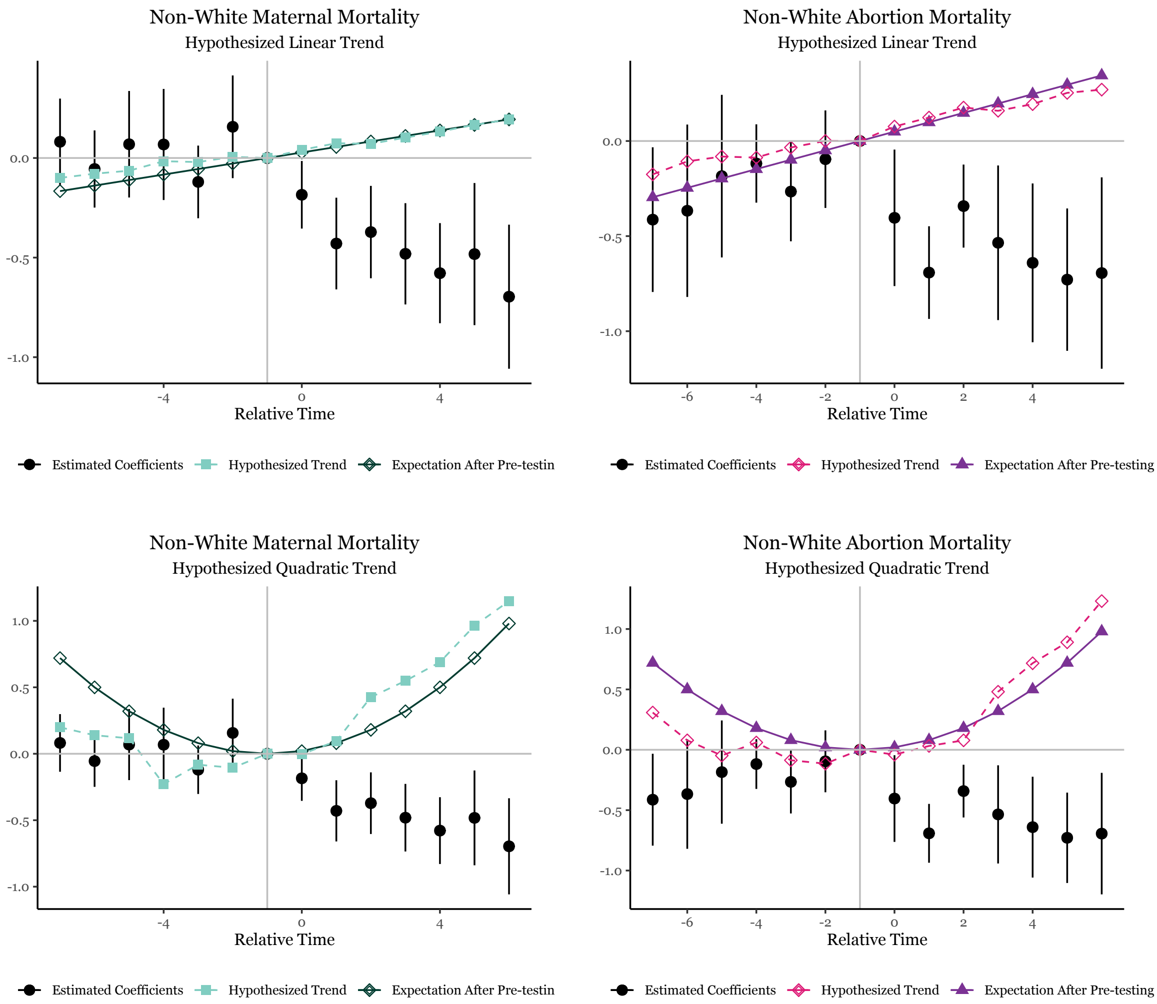
Notes: Results reflect the two-way fixed effects specification from Figure 3, with added state-specific linear trends.

Figure C.4: Main Results: Test for Pre-trends in the Interaction-Weighted Specification



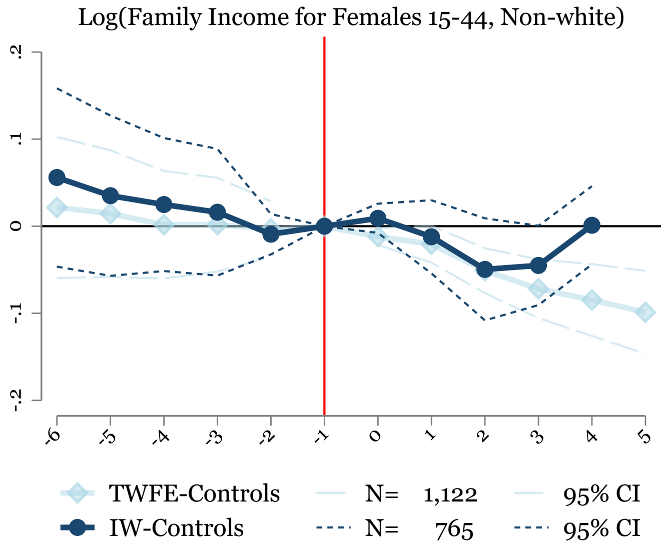
Notes: Reflects estimates from the Interaction-weighted specification in Figure 3. Trends and expectations calculated based on *pretrends* R package associated with Roth (2022).

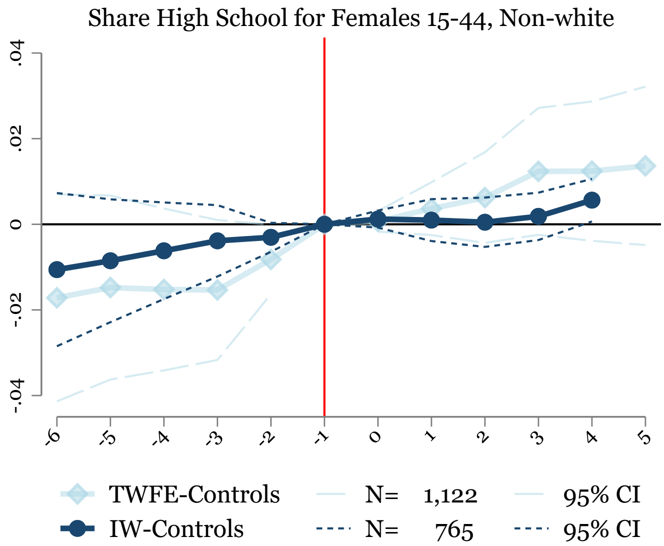
Figure C.5: Main Results: Test for Pre-trends in the Two-Way Fixed Effect Specification (with Controls)



NOTES: Reflects estimates from the TWFE (with controls) specification in Figure 3. Trends and expectations calculated based on *pretrends* R package associated with Roth (2022).

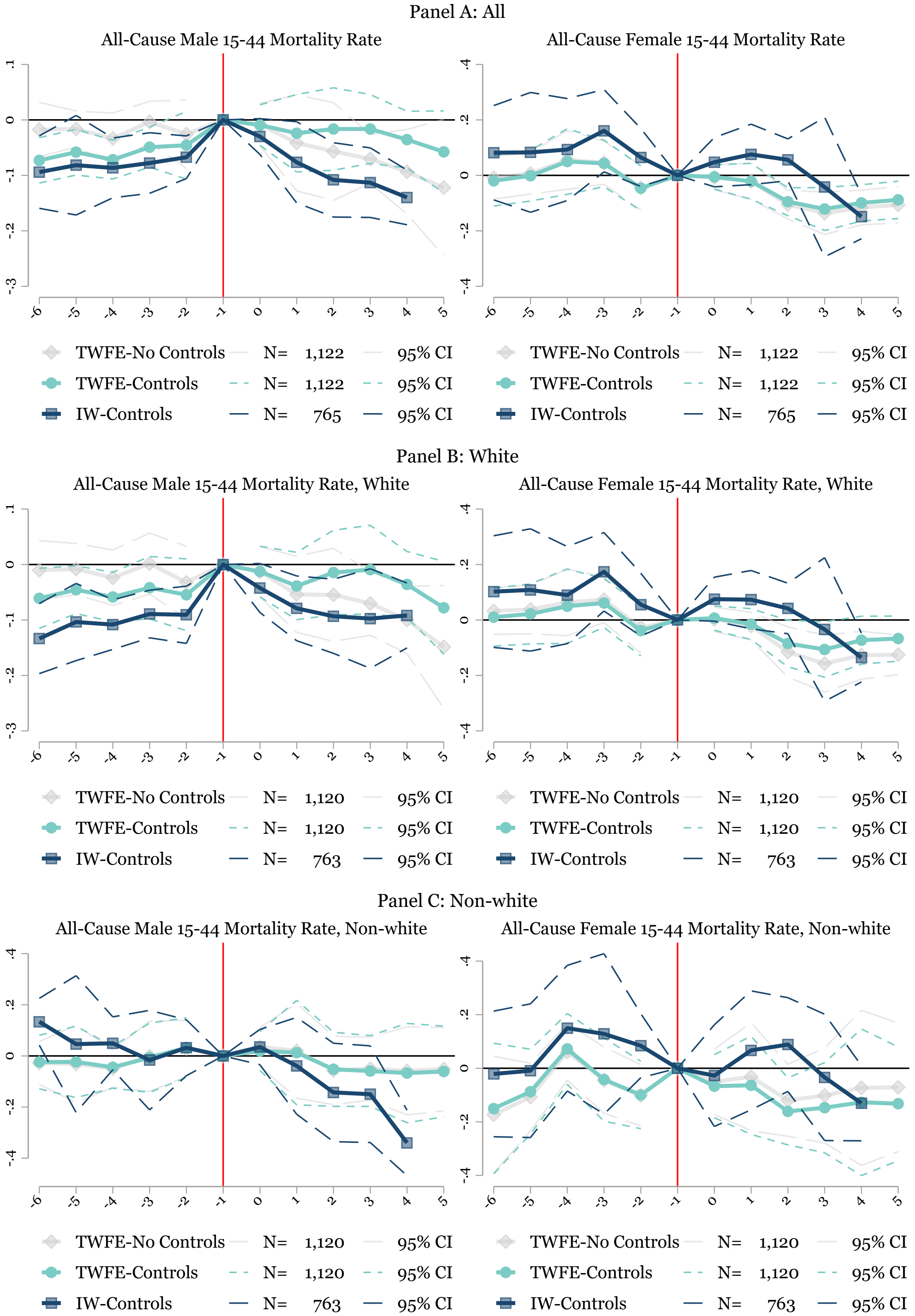
Figure C.6: Event-study Results: Education and Income Non-white Females 15-44





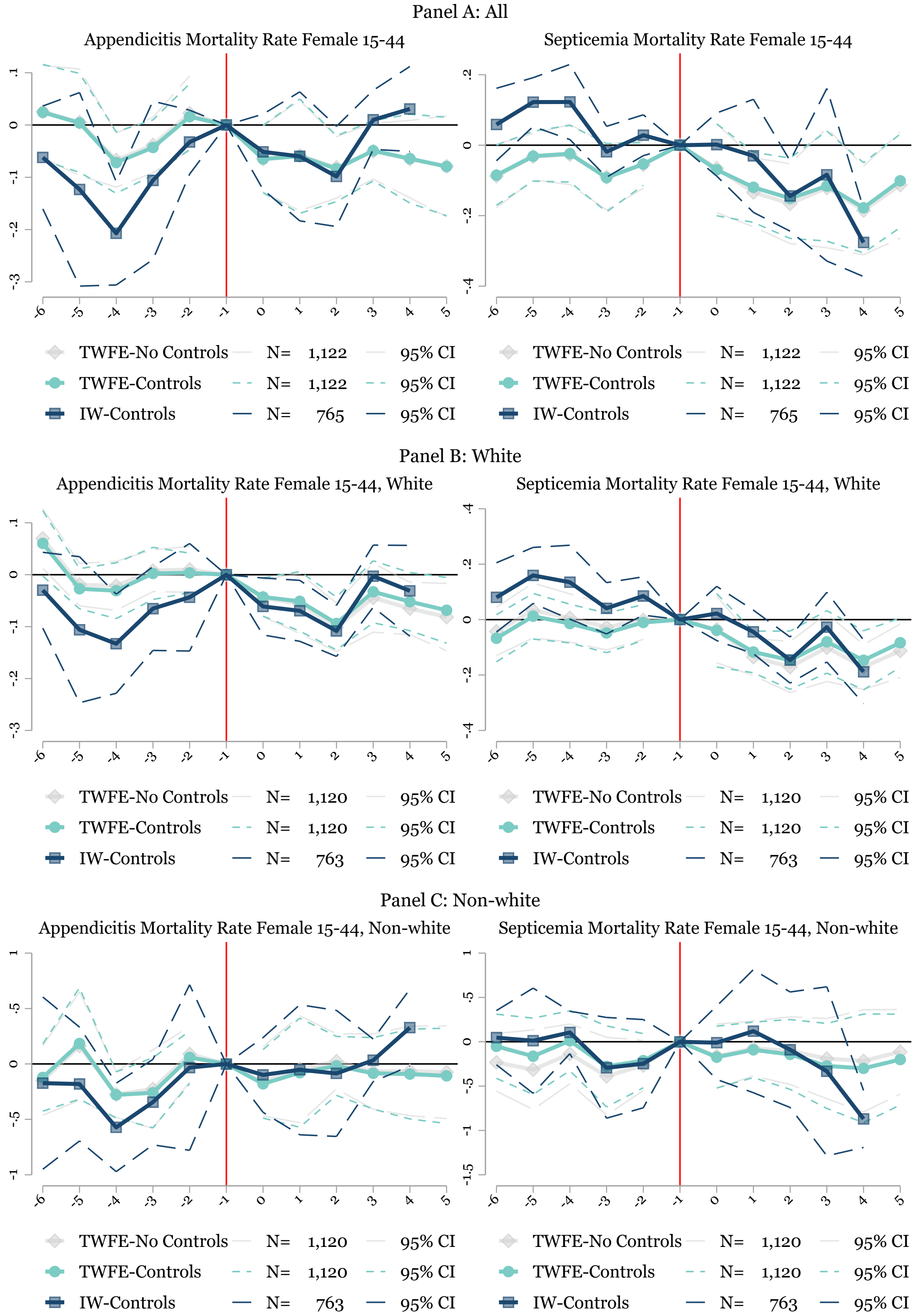
Notes: Reflects the same specification shown in Figure 3, except we adjust the outcome and the controls. For the controls, we include all baseline policy controls. We omit the remainder of the controls (instead considered as outcomes), which may be correlated with the outcome in each specification. Non-white reproductive age female results weighted by non-white females 15-44. Note that the share high school captures the population share with a high school degree.

Figure C.7: Placebo Test and Misclassification Test (I): Reproductive-age Male and Female Mortality



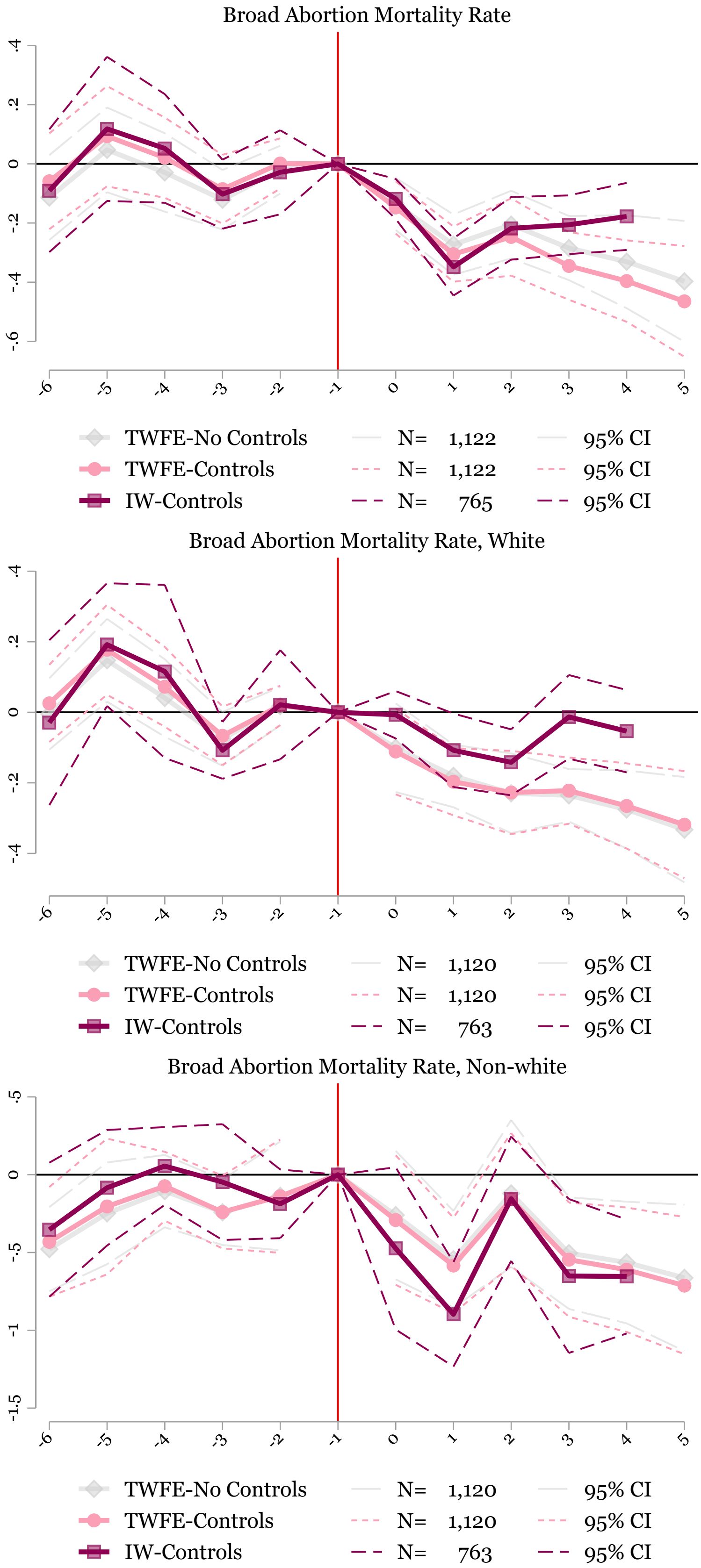
Notes: Results reflect Figure 3, but considering all-cause mortality for reproductive-age males and females. The denominators are the respective male and female reproductive-age populations, with the results weighted by the denominator.

Figure C.8: Misclassification Test (II): Effect of Legal Abortion on Appendicitis and Septicemia Mortality



Notes: Results reflect Figure 3, but considering appendicitis and septicemia deaths for reproductive-age females. The denominator in each mortality rate is the female reproductive-age population, with the results weighted by the denominator.

Figure C.9: Misclassification Test (III): Effect of Legal Abortion on Broad Abortion Mortality

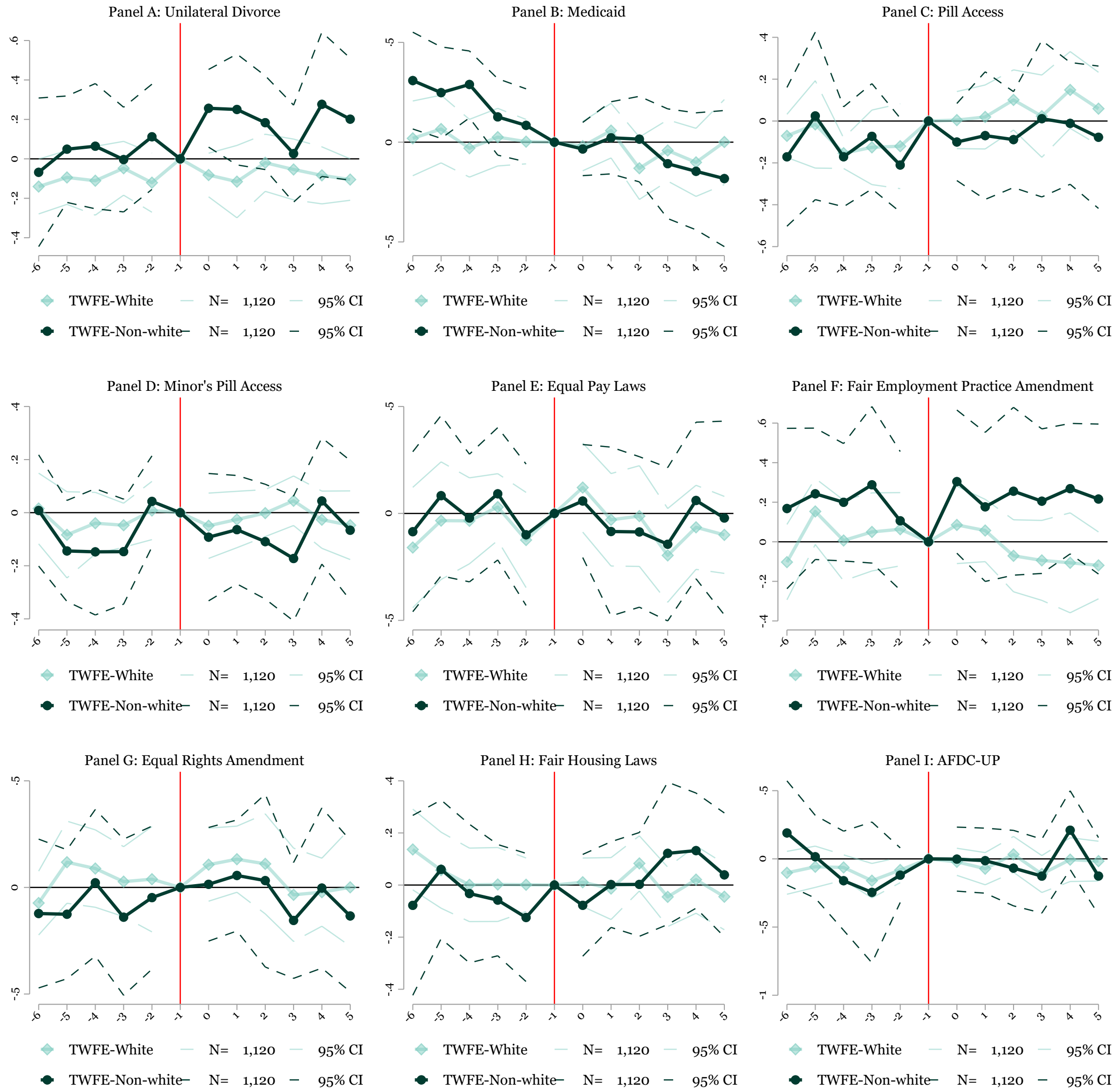


NOTES: Results reflect Figure 3, but considering a broader classification of abortion-related mortality (see Table H.1).

Figure C.10: Alternative Measures of the Reproductive-age Female Population

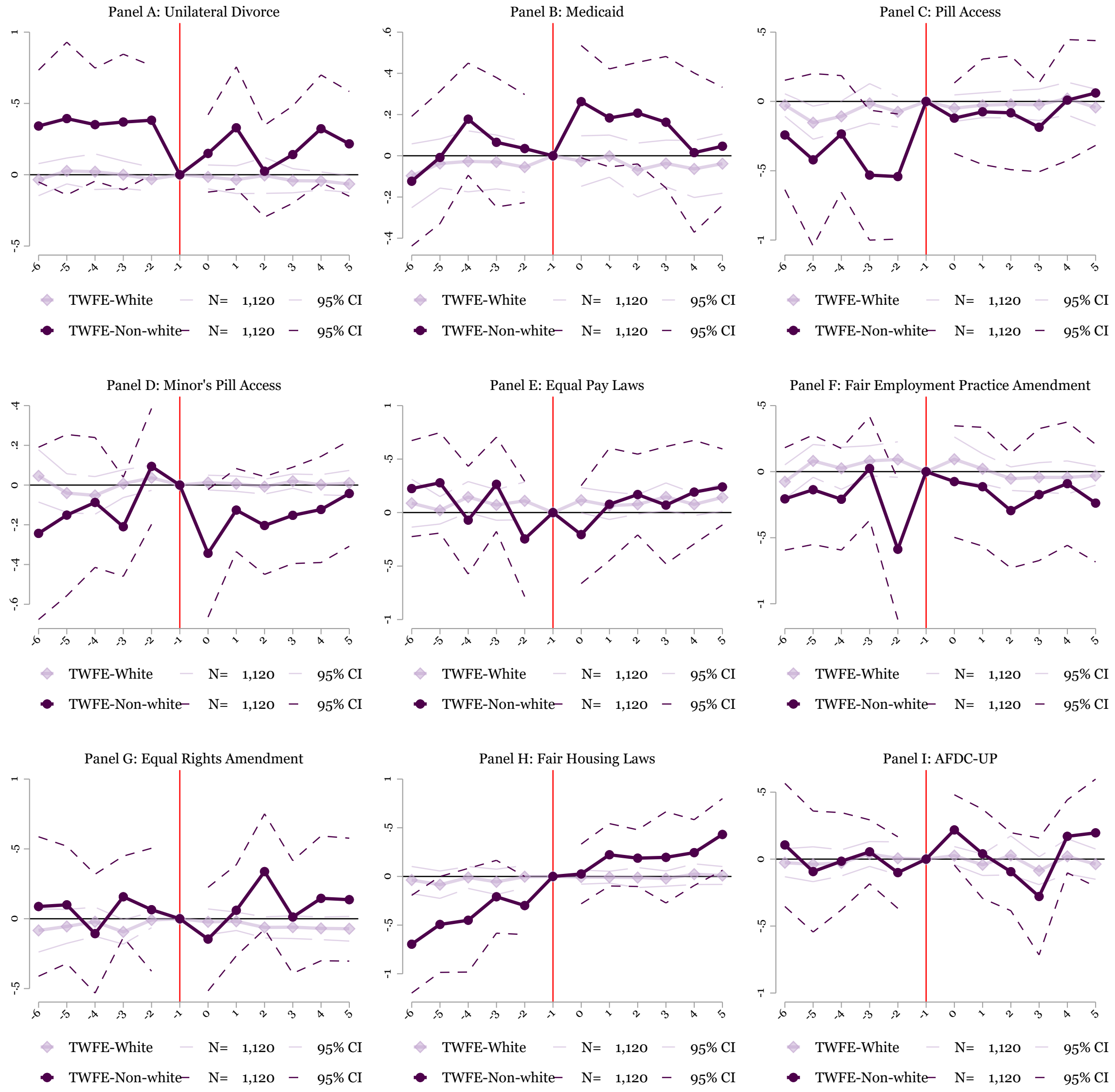
Notes: Results reflect Figure 3, but altering the population of reproductive-age females. Panel A uses a constant measure of reproductive-age females (from the first year of our sample). Panel B shows the share of reproductive-age females from the first year of the sample multiplied by the state population in each year. Panel C shows the reproductive-age female population linearly interpolated over 1960-1980 and omitting 1970 from the interpolation. In each specification, we weight by the denominator of the rate. We also omit census controls; and only include policy controls, as well as the log of per capita income and the log of per pupil spending.

Figure C.11: Impact of Related Policies (I): Maternal Mortality



NOTES: OLS coefficients presented above. Baseline fixed effects include year fixed effects and state fixed effects. Dashed and dotted lines reflect 95% confidence intervals. Robust standard errors clustered at the state level. We take the inverse hyperbolic sine of the mortality rate as the main mortality rate of focus (unless otherwise noted). Maternal mortality and abortion-specific mortality are per 100,000 females 15-44. Non-white (white) rates are per 100,000 non-white (white) reproductive-age females. Estimates are weighted by the denominator of the rate. We control for the main policy and demographic controls.

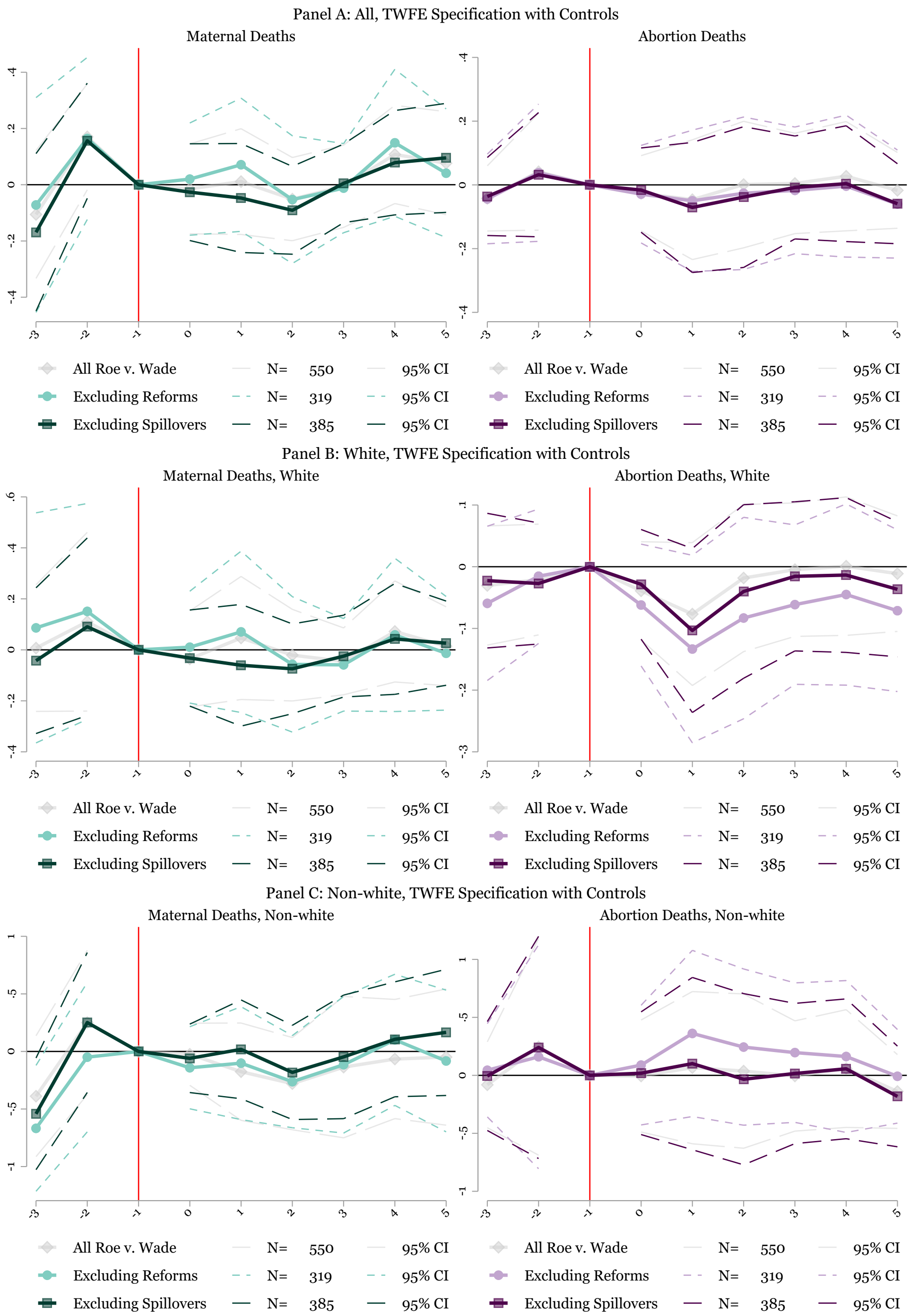
Figure C.12: Impact of Related Policies (II): Abortion-Related Mortality



Notes: OLS coefficients presented above. Baseline fixed effects include year fixed effects and state fixed effects. Dashed and dotted lines reflect 95% confidence intervals. Robust standard errors clustered at the state level. We take the inverse hyperbolic sine of the mortality rate as the main mortality rate of focus (unless otherwise noted). Maternal mortality and abortion-specific mortality are per 100,000 females 15-44. Non-white (white) rates are per 100,000 non-white (white) reproductive-age females. Estimates are weighted by the denominator of the rate. We control for the main policy and demographic controls.

D Roe v. Wade Results

Figure D.1: Effect of Roe v. Wade Relative to Early-Treated States



Source: NVSS/CDC Multiple Cause of Death Files. The years included in the sample are 1970-1980, omitting DC.
Notes: OLS coefficients presented above. Baseline fixed effects include year fixed effects and state fixed effects. Plotted coefficients are dummy variables on each year before and after the change to abortion policy (see Equation 1). The period just before the legal change is the excluded period (-1)–indicated by the vertical line. Event study is fully saturated with endpoints unbinned. Only the point estimates in the main event window are displayed. Dashed and dotted lines reflect 95% confidence intervals. Robust standard errors clustered at the state level. We take the inverse hyperbolic sine of the mortality rate as the main mortality rate of focus (unless otherwise noted). Maternal mortality and abortion-specific mortality are per 100,000 females 15-44. Non-white (white) rates are per 100,000 non-white (white) reproductive-age females. Estimates are weighted by the denominator of the rate. Our main set of state-level demographic controls includes the share of reproductive-age females who are 15-19 and white, the share of reproductive age females who are 15-19 and non-white, the log of per capita income, the log of per pupil education spending, and the state-level share with a high school degree. We also include policy controls for state-level abortion reforms, access to the pill for minors, access to the pill generally, unilateral divorce legislation, state equal pay legislation, and the inverse hyperbolic sine of state-level inductions per 1,000 males 18-25.

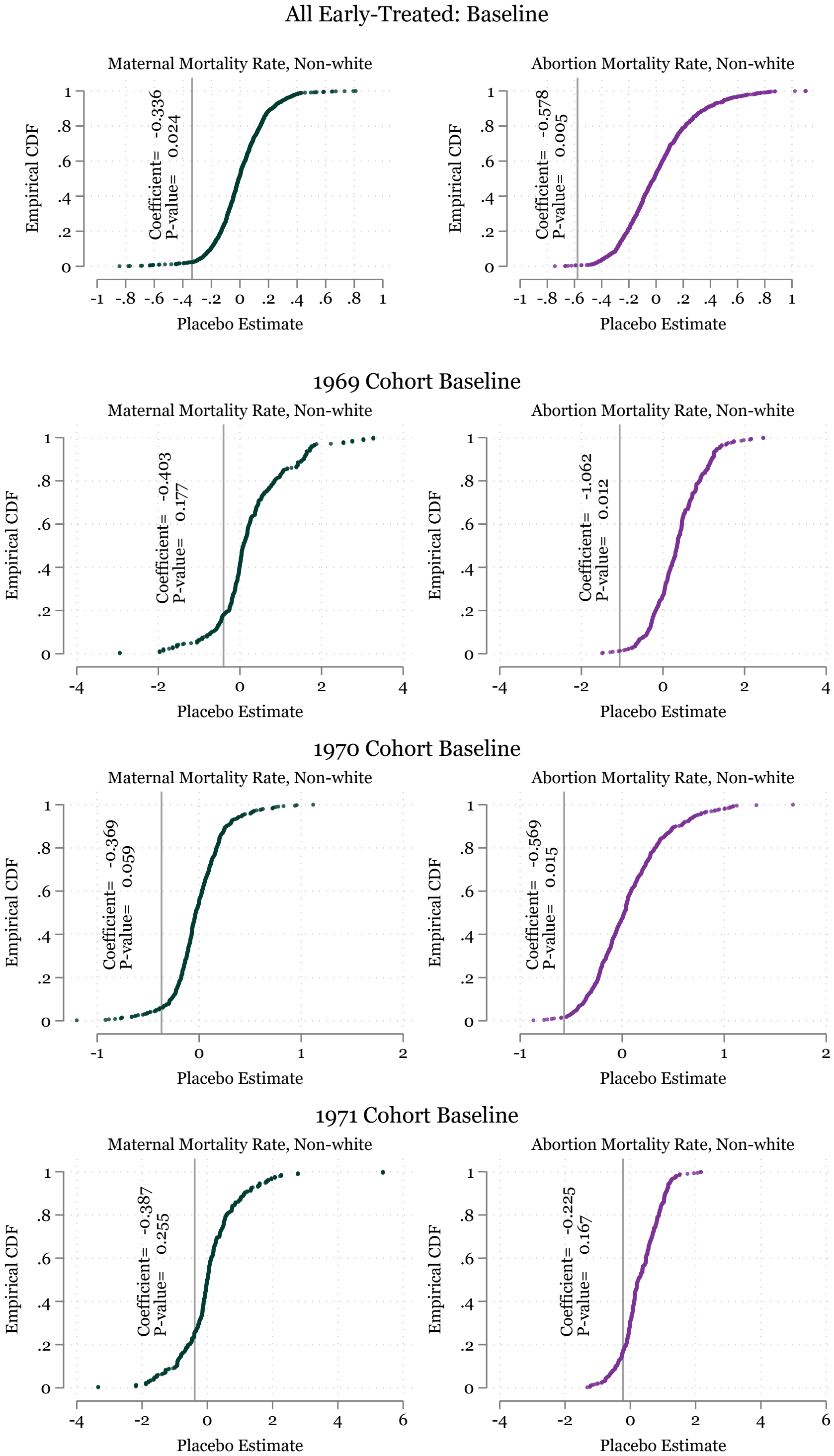
Table D.1: Summary Statistics in 1960, by Legal Status and Driving Time

	Legal Abortion		Roe v. Wade		Diff.	Within 8 Hours to CA, NY, DC		8+ Hours to CA, NY, DC		Diff.
	Mean	S.D.	Mean	S.D.	Est.	Mean	S.D.	Mean	S.D.	Est.
Mortality										
Maternal Mortality Rate	4.08	2.78	4.31	2.33	-0.23	3.93	1.81	4.61	2.67	-0.68
Maternal Mortality Rate, White	3.40	0.97	2.99	1.06	0.41	2.92	1.07	3.05	1.06	-0.13
Maternal Mortality Rate, Non-white	8.31	6.31	10.71	8.55	-2.40	9.67	7.19	11.55	9.56	-1.89
Abortion Mortality Rate	1.21	1.06	0.69	0.65	0.52	0.66	0.58	0.71	0.72	-0.05
Abortion Mortality Rate, White	1.09	1.85	0.45	0.57	0.64	0.46	0.46	0.44	0.65	0.02
Abortion Mortality Rate, Non-white	3.42	2.84	2.40	3.02	1.02	2.02	2.21	2.70	3.55	-0.68
State Characteristics										
Share High School Educated	0.33	0.03	0.27	0.05	0.06**	0.27	0.04	0.27	0.05	-0.00
Share College Educated	0.06	0.02	0.04	0.01	0.02	0.04	0.01	0.04	0.01	0.00
Unemployment	5.95	3.94	5.03	1.05	0.92	5.24	1.13	4.86	0.97	0.38
MDs per 1,000	1.50	0.90	1.02	0.23	0.48	1.09	0.25	0.96	0.20	0.13
Share Urban	0.71	0.18	0.61	0.14	0.10	0.63	0.17	0.59	0.12	0.05
State Population (Millions)	6.20	7.92	3.17	2.80	3.03	3.56	3.18	2.86	2.49	0.70
Log(Income Per Capita)	7.90	0.11	7.62	0.19	0.28***	7.68	0.20	7.57	0.17	0.10
Log(Per Pupil Education Expenditure)	6.21	0.19	5.94	0.21	0.28*	5.96	0.22	5.92	0.21	0.04
Females, 15-44										
State Share Females 15-44	0.21	0.01	0.20	0.01	0.01	0.20	0.01	0.19	0.01	0.01**
State Share White Females 15-44	0.14	0.05	0.18	0.02	-0.03	0.18	0.01	0.17	0.02	0.01
State Share Non-white Females 15-44	0.06	0.06	0.02	0.02	0.04	0.02	0.02	0.02	0.02	-0.00
High School, Reproductive Age Females 15-44	0.56	0.03	0.51	0.08	0.06**	0.50	0.06	0.52	0.09	-0.02
College Educated, Reproductive Age Females 15-44	0.07	0.02	0.05	0.01	0.02*	0.05	0.01	0.05	0.01	0.00
Log(Reproductive-Age Family Income)	8.90	0.08	8.70	0.16	0.19***	8.76	0.17	8.66	0.14	0.09
Log(Reproductive-Age Family Income, White)	8.95	0.10	8.75	0.13	0.20**	8.80	0.15	8.71	0.10	0.09*
Log(Reproductive-Age Family Income, Non-white)	8.59	0.21	8.23	0.29	0.35**	8.27	0.32	8.21	0.27	0.06
Policies										
Inductions Per 1,000 Males 18-25	6.84	3.48	9.36	3.09	-2.52	8.98	3.80	9.66	2.42	-0.68
Year Medicaid	19.67	0.02	19.68	0.03	-0.01	19.68	0.04	19.67	0.01	0.01
Year Equal Rights Amendment	19.41	0.54	19.66	0.30	-0.25	19.73	0.02	19.62	0.38	0.11
Year Equal Pay Laws	19.49	0.06	19.57	0.13	-0.08	19.55	0.13	19.59	0.13	-0.04
Year Pill Access	19.61	0.01	19.62	0.03	-0.01	19.62	0.03	19.61	0.03	0.00
Year Minor's Access to Pill	19.77	0.08	19.73	0.06	0.04	19.73	0.03	19.73	0.08	-0.01
Fair Housing Year	19.65	0.03	19.66	0.03	-0.01	19.65	0.03	19.67	0.03	-0.02
Year Unilateral Divorce	19.63	0.18	19.71	0.09	-0.09	19.72	0.03	19.71	0.11	0.01
Religion (1971)										
Churches Per 1,000	0.57	0.28	1.18	0.55	-0.60**	1.02	0.54	1.30	0.53	-0.28
Catholic Churches Per 1,000	0.11	0.08	0.15	0.11	-0.04	0.12	0.07	0.17	0.13	-0.04
P.C. Church Members	0.11	0.03	0.24	0.11	-0.13***	0.19	0.11	0.27	0.10	-0.07*
P.C. Church Adherents	0.37	0.06	0.53	0.11	-0.16***	0.50	0.09	0.55	0.12	-0.05
P.C. Catholic Adherents	0.20	0.09	0.20	0.14	0.00	0.25	0.17	0.16	0.11	0.09
Abortions										
Abortion Rate-1972	25.66	8.05	7.12	4.45	18.54**	9.83	4.01	4.95	3.54	4.89***
Abortion Rate-1976	32.94	21.25	14.37	6.12	18.58	16.85	6.33	12.39	5.27	4.46*
Abortion Rate-1980	38.75	22.73	19.12	6.50	19.63	21.23	7.38	17.42	5.25	3.81
Political										
Republican Governor	0.33	0.52	0.31	0.47	0.02	0.40	0.50	0.24	0.44	0.16
State Senate-Proportion Republican	0.37	0.20	0.35	0.26	0.03	0.38	0.25	0.32	0.27	0.05
State House-Proportion Republican	0.36	0.17	0.30	0.22	0.06	0.33	0.22	0.28	0.22	0.06
Democrat Governor	0.50	0.55	0.67	0.48	-0.17	0.60	0.50	0.72	0.46	-0.12
State House-Proportion Democrat	0.63	0.17	0.70	0.22	-0.07	0.67	0.22	0.72	0.22	-0.06
State Senate-Proportion Democrat	0.63	0.20	0.65	0.26	-0.02	0.62	0.25	0.68	0.27	-0.06
Proportion Legislature Same Party as Governor	0.65	0.16	0.64	0.25	0.01	0.60	0.25	0.68	0.26	-0.08
N	6		45		51	20		25		45

Source: NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980. State population characteristics are from Ruggles et al. (2021) (shares and means). Population totals use to construct denominators from Wolfers (2006) (also the source of the unilateral divorce laws). Abortion access, equal rights amendments, and equal pay laws from Myers (2021a). Induction data from Bitler and Schmidt (2012). State-level economic conditions are measured by the unemployment rate from Haines (2010). Medicaid implementation from Boudreaux et al. (2016). Physicians per 1,000 persons from the Area Health Resource File, (AHRF, 1994). Fair housing laws from Collins (2006), Myers (2021a). The log of per pupil spending from National Center NCES (1959-1980). The passage of AFDC UP from Winkler (1995). Religious affiliation data from Johnson et al. (1971). Abortion counts from CDC (1969-1980), and reported per 1,000 reproductive-age females. Political alignment from Jordan and Grossmann (2020). Fair housing laws from Collins (2006). The log of per pupil spending from NCES (1959-1980).

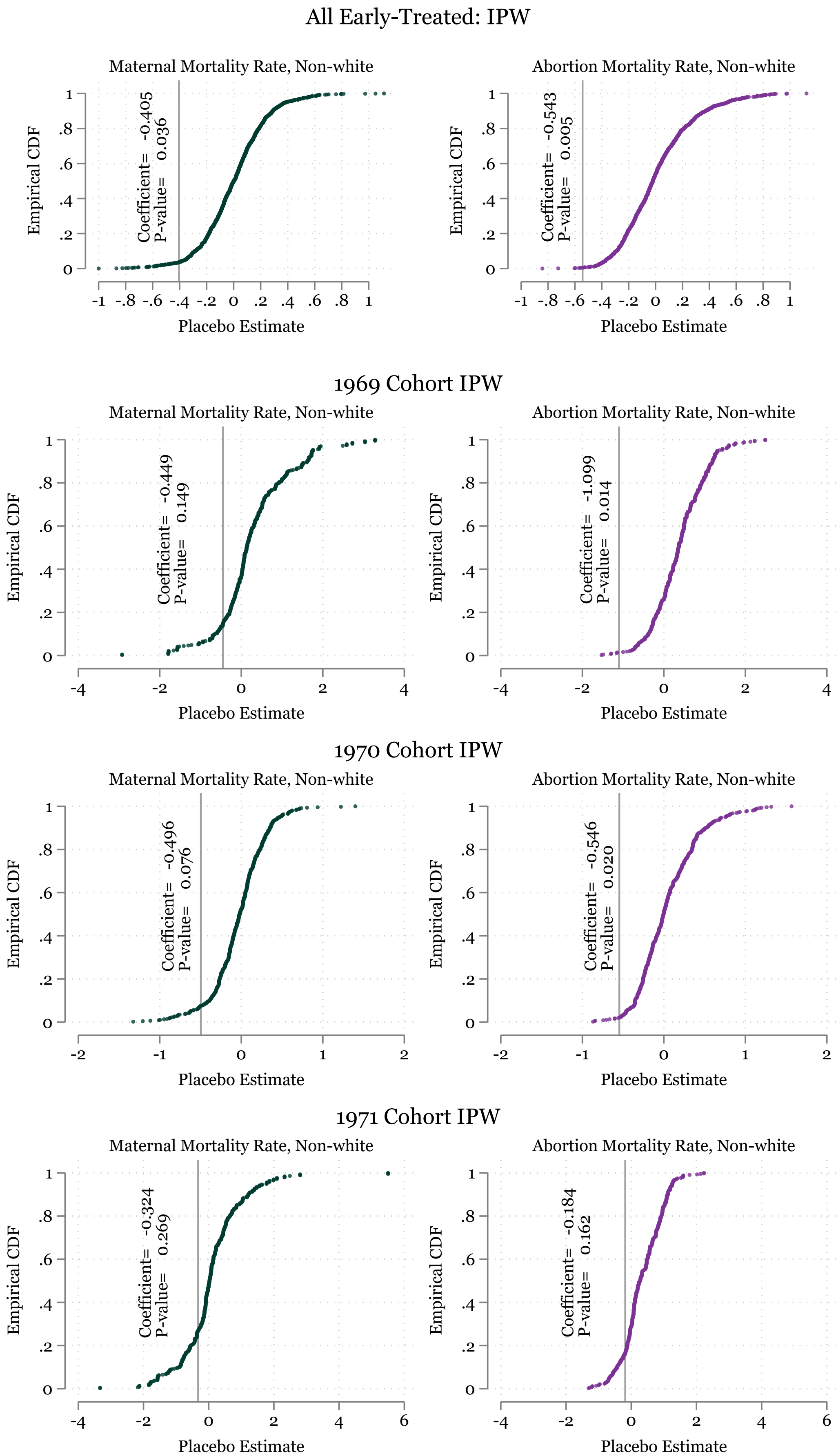
Notes: Unweighted means presented. All characteristics from the 1960 census year unless otherwise noted (religion and abortion rates). ***, **, * represent statistical significance at 1, 5 and 10 percent levels.

Figure D.2: Permutation Tests: Baseline



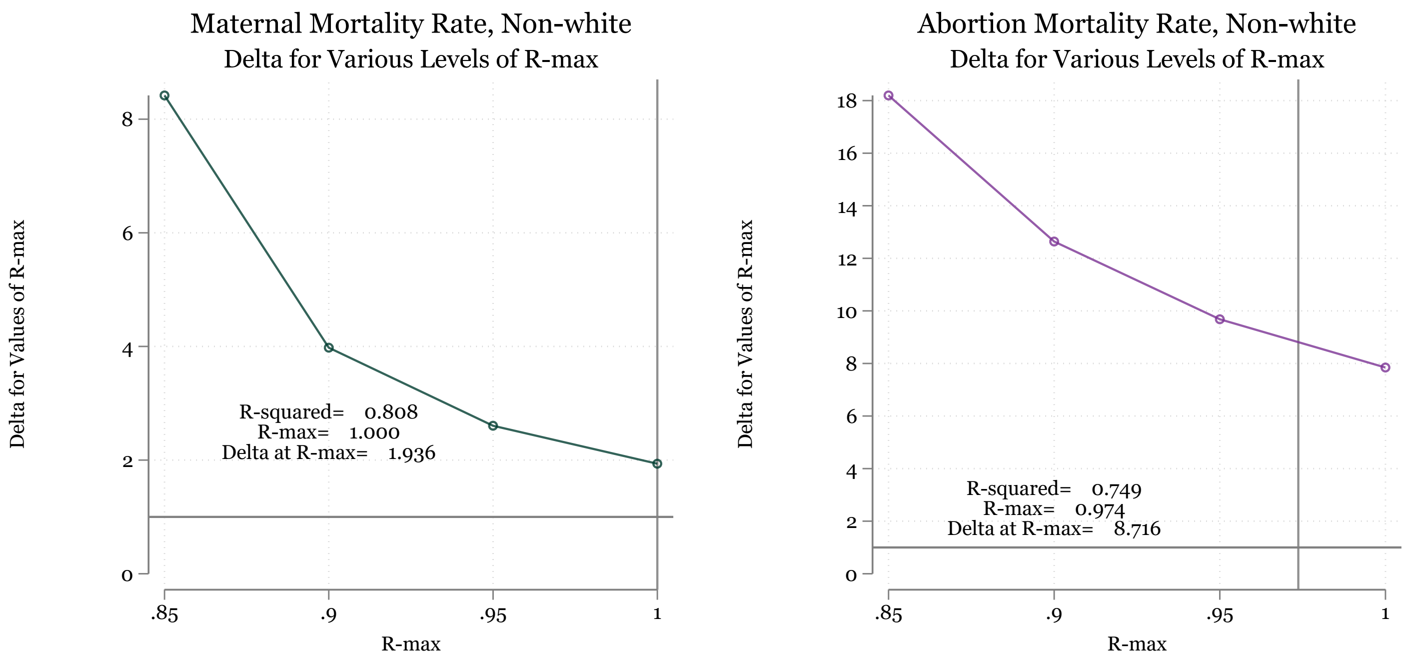
NOTES: Permutation tests based on the estimates provided in Table 2. Each cumulative distribution function is constructed from a different set of permutation tests. In the top two panels, Equation 2 is estimated over two different scenarios. First, we vary the treatment states over identical treatment years, with *Roe v. Wade* control states as placebo-treatment states (only those without abortion reforms). In this case, we assume a similar timing of legal abortion, with a staggered treatment set up of one state treated in 1969, four in 1970, and one in 1971. in this exercise, we randomly choose four of the *Roe v. Wade* states for each treatment year and run this simulation 500 times (choosing a different state combination each time). Second, we use placebo treatment years. In this case, we vary the start year of legal abortion from 1960 to 1967 (stopping the sample at 1969), but use the same staggered setup with one state treated in the first year, four in the second year, and one in the final year. Placebo treatment states are selected from the set of early-legal and *Roe v. Wade* states and randomly assigned to the treatment years. We run this simulation 100 times for eight years. Between the two sets of permutations, we have 500+800=1,300 permutations in total. For the bottom six panels, we again vary both treatment timing and treatment states for each cohort. For each cohort, we randomly select the same number of treated states as in each cohort (e.g., one for 1971 and 1969; four for 1970). We then keep only the treated cohort as well as *Roe v. Wade states*, and randomly assign the timing of legal abortion (for each state) from 1960 until just before the actual treatment year (differs by cohort). Then we run the same analysis with only *Roe v. Wade states* from each cohort's treatment year until 1972. The number of observations in this set of permutations depends on the cohort. To calculate the nonparametric p-value, we take the number of observations with a coefficient less than the baseline estimate, divided by the sample size of all permutations.

Figure D.3: Permutation Tests: Inverse Probability Weights



NOTES: See description in Figure D.2.

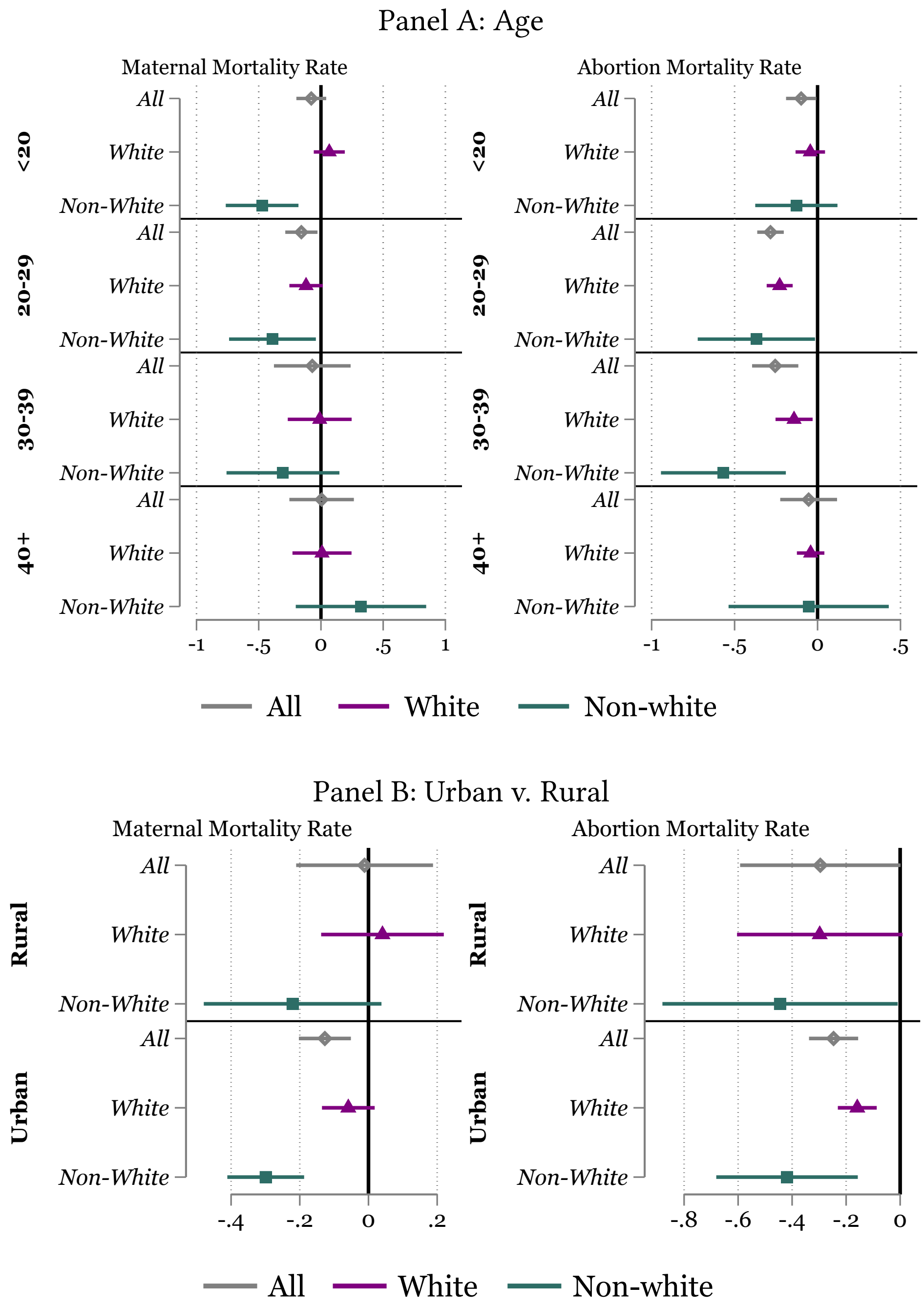
Figure D.4: Degree of Selection on Unobservables at Different Levels of R_{max}



Notes: Results show different levels of R-max from the Stata routine *pscalc*, using the results from the inverse probability weighted results from Column (2) of Table 2. Delta represents the degree of selection on unobservables relative to observables that would be needed to explain away the observed findings. Oster (2019) sets a reasonable value of R_{max} equal to 1.3 multiplied by the observed R-squared from the restricted regression, denoted in each specification by the vertical line. The minimum robust level of δ is one, and indicated by the horizontal line (Oster, 2019).

E Mechanisms-Additional Results

Figure E.1: Difference-in-Differences Results: Maternal and Abortion-related Mortality By Age Group and Urban Status



NOTES: Results reflect the same specification as Figure 6. Note that we use the age-specific rates per the respective populations. However, the rural versus urban counts are per the total females 15-44 multiplied by the share urban (Haines, 2010).

Table E.1: Poisson Regression Model: County-level Heterogeneity for White Maternal and Abortion-related Deaths

Panel A: White Maternal Mortality

	All	Share Urban	Median Income	Median Years Schooling	Share High School	P.C. Church Members	P.C. Cath-olics	MDs Per 1,000	Hospital Births All	Hospital Births White	Hospital Births Non-white	Hospital Births Non-white to-White
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1(Legal Abortion)=1	-0.0350	-0.0270	-0.0327	-0.0131	-0.0172	0.0109	-0.0315	-0.0058	-0.0379	-0.1588*	-0.1463	-0.1511*
	(0.0747)	(0.0752)	(0.0762)	(0.0760)	(0.0764)	(0.0925)	(0.0892)	(0.0869)	(0.0772)	(0.0861)	(0.0896)	(0.0899)
1(Legal Abortion)=1 × 1(Below Median)=1		-0.0859	-0.0151	-0.1268**	-0.0983*	-0.0521	0.0406	-0.1566**	0.0051	0.1062	0.0846	0.0930
		(0.0709)	(0.0639)	(0.0599)	(0.0584)	(0.0617)	(0.0657)	(0.0614)	(0.0557)	(0.0703)	(0.0725)	(0.0718)
N	45,876	45,839	45,839	45,839	45,839	45,542	45,542	45,421	45,377	15,491	15,491	15,491
County FE and Year FE	X	X	X	X	X	X	X	X	X	X	X	X

Panel B: White Abortion Mortality

	All	Share Urban	Median Income	Median Years Schooling	Share High School	P.C. Church Members	P.C. Cath-olics	MDs Per 1,000	Hospital Births All	Hospital Births White	Hospital Births Non-white	Hospital Births Non-white to-White
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1(Legal Abortion)=1	-0.6036***	-0.5904***	-0.5885***	-0.6233***	-0.6278***	-0.7617**	-0.7376***	-0.7161***	-0.1984	-0.7005**	-0.7315***	-0.6650**
	(0.1826)	(0.1831)	(0.1841)	(0.1902)	(0.1913)	(0.3198)	(0.2215)	(0.2194)	(0.2669)	(0.3406)	(0.2598)	(0.3232)
1(Legal Abortion)=1 × 1(Below Median)=1		-0.2115	-0.0956	0.2222	0.2467	0.0443	0.0783	-0.2857	-0.5704**	0.0183	0.0550	-0.0253
		(0.4223)	(0.3148)	(0.2754)	(0.2656)	(0.2786)	(0.3243)	(0.3443)	(0.2367)	(0.3198)	(0.2806)	(0.3146)
N	11,937	11,918	11,918	11,918	11,918	11,764	11,764	11,742	11,720	5,276	5,276	5,276
County FE and Year FE	X	X	X	X	X	X	X	X	X	X	X	X

NOTES: Reflects Table 3 except considering white maternal and abortion deaths.

Table E.2: Pre-Roe Treatment by Inverse Travel Distance to Nearest Major Repeal State, NY/CA/DC

Omitting Repeal States, 1959-1973

	White Maternal		Non-white Maternal		White Abortion		Non-white Abortion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(Nearest Repeal Legal)=1	-0.1558*	0.0100	0.1633	0.0818	0.0724	0.3747	0.1476	-0.6006
	(0.0940)	(0.1158)	(0.1145)	(0.1829)	(0.2407)	(0.3013)	(0.2316)	(0.5607)
1(Nearest Repeal Legal)=1 × 1/Travel Time	-0.1157*		-0.0835***		-0.0371		0.0525	
	(0.0637)		(0.0296)		(0.1507)		(0.0597)	
1(Nearest Repeal Legal)=1 × 1(Within 4 Hours)		-0.3335***		-0.0123		-0.3844		0.7856
		(0.1216)		(0.1738)		(0.3077)		(0.5322)
1(Nearest Repeal Legal)=1 × 1(4-7.99 Hours)		-0.2323**		0.0867		-0.4476		1.1189**
		(0.1096)		(0.1733)		(0.3483)		(0.5517)
1(Nearest Repeal Legal)=1 × 1(8-15.99 Hours)		-0.2311**		0.0884		-0.4560		0.5973
		(0.1058)		(0.1732)		(0.3259)		(0.5735)
N	27,521	27,521	14,752	14,752	7,021	7,021	5,072	5,072
County FE and Year FE	X	X	X	X	X	X	X	X

Source: NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980. County-level population data and county-level characteristics are from Bailey et al. (2016) and Haines (2010).

Notes: Results from a county-level Poisson model. County and year fixed effects are included, but no covariates are included. The outcome is the (linear) maternal and abortion deaths, and the exposure is the county-level population of females 15-44. Robust standard errors from xtpoisson are reported, which are equivalent to clustering on the panel variable (county). ***, **, * represent statistical significance at 1, 5 and 10 percent levels. County-level travel time to the nearest of the major cities in NY/CA/DC, with the timing of the legal reform based on the nearest repeal state. We calculate the distance in miles and assume individuals can travel roughly 60 miles per hour to calculate the travel time. Note that the number of observations is smaller for abortion deaths than the maternal deaths. Abortion deaths have more frequent cases of an individual county having zero deaths, and the Poisson model only includes groups with at least one non-zero outcome.

Table E.3: Correlations of Travel Time and Abortion Rates

	Travel Time				
	(1)	(2)	(3)	(4)	(5)
	1972	1974	1976	1978	1980
Abortion Rate	-0.9028***	-0.5057***	-0.5144***	-0.3468***	-0.2450
	(0.1258)	(0.1155)	(0.1467)	(0.1088)	(0.1534)
N	45	45	45	45	45

NOTES: Linear regression of travel time on abortion rate per 1,000 reproductive age females 15-44. Only *Roe v. Wade* states are included. Robust standard errors shown. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.

F Other Robustness Checks

F.1 Factors that Predict Adoption

In this section, we use a Cox proportional hazard model to test whether changes in mortality predict the state-level implementation of legal abortion. To consider this, we take a similar specification to Equation 2. However, we use a Cox Proportional Hazard model and consider whether the lag of mortality predicts the adoption of legal abortion. We use the lag of mortality to avoid capturing the effect of legal abortion on mortality. We also include our standard set of controls. Table F.1 shows the hazard rate of adoption of legal abortion by state and over time. The prior year's mortality fails to significantly predict future adoption of legal abortion. This analysis bolsters our primary empirical strategy, by validating that adoption is not conditional on mortality. States did not systematically adopt legal abortion based on the evolution of mortality. While states with higher mortality overall may have adopted abortion earlier, this time-invariant level of mortality is accounted for by the state fixed effects.

Table F.1: Cox Proportional Hazard Model

Panel A: Maternal Mortality

	Adoption of Legal Abortion					
	(1)	(2)	(3)	(4)	(5)	(6)
L.Maternal Mortality Rate	0.0707 (0.0748)	-0.0884 (0.1581)				
L.Maternal Mortality Rate, White			0.0499 (0.1025)	0.0722 (0.1763)		
L.Maternal Mortality Rate, Non-white					-0.0262 (0.0380)	-0.0716 (0.0765)
N	696	696	694	694	694	694
State FE and Year FE	X	X	X	X	X	X
Controls		X		X		X

Panel B: Abortion Mortality

	Adoption of Legal Abortion					
	(1)	(2)	(3)	(4)	(5)	(6)
L.Abortion Mortality Rate	0.4581 (0.3434)	0.0334 (0.3798)				
L.Abortion Mortality Rate, White			-0.2322 (0.2830)	-0.0631 (0.3538)		
L.Abortion Mortality Rate, Non-white					0.1455 (0.1200)	0.0251 (0.1500)
N	696	696	694	694	694	694
State FE and Year FE	X	X	X	X	X	X
Controls		X		X		X

Notes: Results from a Cox Proportional Hazard model, until all states have adopted legal abortion. The 'failure year' is the year after legal abortion passes (goes into effect) in each state. Rates represent the inverse hyperbolic sine of the rate and are per 100,000 reproductive-aged females in each population (all, white, and non-white). Our main set of state-level demographic controls includes the share of reproductive-age females who are 15-19 and white, the share of reproductive age females who are 15-19 and non-white, the log of per capita income, the log of per pupil education spending, and the state-level share with a high school degree. We also include policy controls for state-level abortion reforms, access to the pill for minors, access to the pill generally, unilateral divorce legislation, state equal pay legislation, and the inverse hyperbolic sine of state-level inductions per 1,000 males 18-25. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.

F.2 Specifications for Difference-in-differences and Poisson Model

Throughout the main results, we prefer the event study to other approaches, such as a difference-in-differences specification, for several reasons (see Section 7.6 for the difference-in-differences results). The event study allows us to visualize the pre and post-period effects of legal abortion, allowing us to determine (1) whether there are pre-existing trends (or pre-trends) leading up to the passage of legal abortion, (2) whether there is a clear break in mortality with the passage of legal abortion, and (3) whether the treatment effect varies after the passage of legal abortion (Wolfers, 2006; Goodman-Bacon, 2021). However, throughout the results, we also show the difference-in-differences results, and outline this alternative specification here.

Difference-in-differences We use a difference-in-differences specification to consider the impact of legal abortion in a grouped post-period. Formally, our difference-in-differences specification is expressed as:

$$\text{Mortality}_{st} = \alpha + \beta \text{ Legal Abortion}_{st} + \mathbf{X}'_{st}\gamma + a_s + \eta_t + \epsilon_{st} \tag{2}$$

where $\text{Legal Abortion}_{st}$ captures the effect of legal abortion and includes both the early legalization of abortion in repeal states as well as the 1973 passage of Roe v. Wade. $\text{Legal Abortion}_{st}$ is a dummy variable that is equal to one in the year t (on onward) that state s passed legal abortion, and zero in the years before the abortion law passed.[86] All other features of Equation 2 reflect Equation 1.

Poisson Model In additional results, due to the substantial number of zeros in abortion-related mortality, we also show the difference-in-differences results assuming a Poisson distribution following related work (Myers and Ladd, 2020; Myers, 2021b,c). Thus, we estimate:

$$E[M_{st}|\text{Legal Abortion}_{st}, \mathbf{X}_{st}, a_s, \eta_t] = exp(\beta \text{ Legal Abortion}_{st} + \mathbf{X}'_{st}\gamma + a_s + \eta_t) \tag{3}$$

where all notation reflects Equation 2, except we model the mortality M_{st} deaths (as a count) based on the population of exposure (females 15-44).

We also make too modifications to this strategy throughout the results. First, we generalize Equation 4 to consider the dynamic effect, reflecting our main event study. Second, we use a county-level version of Equation 3, where we observed mortality at the county level, M_{ct}, and replace state fixed effects, a_s, with county-level fixed effects, a_c.

[86]We have also defined this specification as equal to one in the year *after* the abortion law passes, and the results are largely the same. However, because the effect for abortion-related mortality is immediate in the event-study specification, we choose to include the year of the passage of the abortion law in the treated period. The full two-way fixed effects results are insensitive to this choice, but the results for the early-treated are more dependent on this specification choice.

F.3 Goodman-Bacon Decomposition

Based on the importance of early-legalization states in the main results, we decompose our main difference-in-differences results by treatment timing using a Goodman-Bacon decomposition (Goodman-Bacon, 2021) in Table F.2. The results presented are without weights or controls.

The findings in Table F.2 illustrate that, as suspected, the general conclusions on the impact of abortion legalization depend on the comparison being made. For non-white maternal mortality, the main effect is through early abortion legalization, with later legalization having a positive coefficient. For white maternal mortality, the reverse is true, with later-treated showing a reduction while early-treated fails to show a decline in maternal mortality. However, the effect on white maternal mortality is relatively small in magnitude.

Then, focusing on abortion-related mortality, the results suggest similar heterogeneous treatment effects to maternal mortality. Abortion-related mortality declines are highest in early-treated states, with the impact most apparent for non-white abortion-related mortality. White abortion-related mortality actually is higher after legalizations in later-treated states.

These heterogeneous effects across comparison groups align with the findings from Section 7.8, suggesting the main effect is most pronounced in early-legal states and for non-white mortality. To more explicitly test the separate effect of all legalization states, we present the heterogeneous treatment effects from the Goodman-Bacon Decomposition excluding states treated by *Roe v. Wade* in Table F.3. The results indicate that all voluntary abortion legalizations produced declines in non-white maternal and non-white abortion-related mortality, where the effect appears symmetric in early versus later-treated states.

Table F.2: Goodman-Bacon et al. (2019) Decomposition

DD Comparison	(1) Weight	(2) DD Estimate
Maternal Mortality Rate		
Earlier Treated v. Later Control	0.575	-0.103
Later Treated v. Earlier Control	0.425	-0.025
Average DD Estimate		-0.070
Maternal Mortality Rate, White		
Earlier Treated v. Later Control	0.575	0.107
Later Treated v. Earlier Control	0.425	-0.053
Average DD Estimate		0.039
Maternal Mortality Rate, Non-white		
Earlier Treated v. Later Control	0.575	-0.238
Later Treated v. Earlier Control	0.425	0.108
Average DD Estimate		-0.091
Abortion Mortality Rate		
Earlier Treated v. Later Control	0.575	-0.181
Later Treated v. Earlier Control	0.425	0.000
Average DD Estimate		-0.104
Abortion Mortality Rate, White		
Earlier Treated v. Later Control	0.575	0.000
Later Treated v. Earlier Control	0.425	0.059
Average DD Estimate		0.025
Abortion Mortality Rate, Non-white		
Earlier Treated v. Later Control	0.575	-0.426
Later Treated v. Earlier Control	0.425	-0.146
Average DD Estimate		-0.307
Notes: controls and weights excluded		

Table F.3: Goodman-Bacon et al. (2019) Decomposition - Excluding *Roe v. Wade*

	(1)	(2)
DD Comparison	**Weight**	**DD Estimate**
Maternal Mortality Rate		
Earlier Treated v. Later Control	0.500	-0.075
Later Treated v. Earlier Control	0.500	-0.357
Average DD Estimate		-0.216
Maternal Mortality Rate, White		
Earlier Treated v. Later Control	0.500	-0.358
Later Treated v. Earlier Control	0.500	-0.196
Average DD Estimate		-0.277
Maternal Mortality Rate, Non-white		
Earlier Treated v. Later Control	0.500	0.070
Later Treated v. Earlier Control	0.500	-0.003
Average DD Estimate		0.034
Abortion Mortality Rate		
Earlier Treated v. Later Control	0.500	-0.121
Later Treated v. Earlier Control	0.500	-0.396
Average DD Estimate		-0.259
Abortion Mortality Rate, White		
Earlier Treated v. Later Control	0.500	-0.202
Later Treated v. Earlier Control	0.500	0.144
Average DD Estimate		-0.029
Abortion Mortality Rate, Non-white		
Earlier Treated v. Later Control	0.500	-0.671
Later Treated v. Earlier Control	0.500	-0.638
Average DD Estimate		-0.654
Notes: controls and weights excluded		

G Additional Background Information

G.1 Declines in Maternal Mortality, 1900-1960

The leading cause of maternal mortality at the start of the twentieth century was "childbed" or puerperal fever (Anderson et al., 2020b). Until 1937 (with the advent of sulfa drugs), there was no cure for puerperal fever, only preventative measures through hand-washing and the cleaning of instruments. In 1920, 40% of maternal mortality was caused by puerperal sepsis (or septicemia) (CDC, 1999; Albanesi and Olivetti, 2016). CDC (1999) reports that half of the cases of sepsis occurred directly following delivery while the other half occurred after an illegal abortion. The remaining major causes of maternal deaths included hemorrhage, toxemia, and obstructed labors (CDC, 1999; Albanesi and Olivetti, 2016). Over 1900-1930, maternal mortality showed few improvements, hovering around seven deaths per 1,000 (or 700 deaths per 100,000) (Albanesi and Olivetti, 2016).

Before the medical advancements of the 1930s and onwards, the largest contributor to improved maternal mortality occurred through public health preventive measures. Public health measures include the advent of prenatal care, which starting in the 1920s lowered deaths from toxemia (Albanesi and Olivetti, 2016). Regulatory reforms also targeted maternal mortality over this period. Hospital and state maternal mortality review boards helped to monitor maternal health conditions (CDC, 1999). Further, state-level occupational licensing of midwives led to a reduction in maternal mortality by 6-7% over 1900-1940 (Anderson et al., 2020b).

Then, between 1930 and 1950, significant medical progress produced substantial reductions in maternal mortality. In 1936, the establishment of blood banks allowed mothers to survive maternal hemorrhage for the first time (Albanesi and Olivetti, 2016). The most significant contributor to the decline in maternal mortality occurred through the discovery of sulfa drugs (between 1937 to 1943) (Thomasson and Treber, 2008; Jayachandran et al., 2010; Albanesi and Olivetti, 2016). Jayachandran et al. (2010) shows that the discovery of sulfa drugs reduced maternal mortality by 24-36%. Sulfa drugs not only lowered deaths from puerperal fever, but they also improved the survival rate from live-saving medical procedures such as cesarean section (Thomasson and Treber, 2008). Finally, the medical advancement of penicillin in the early 1940s helped further reduce maternal deaths from sepsis (Albanesi and Olivetti, 2016).

G.2 A Brief History of Abortion Laws in the United States

Abortion at the founding of the United States was legal until quickening (the first fetal movement felt by the mother) (Roe v Wade, 1973; Law et al., 1989; Rubin, 1994). This focus on abortion only until quickening was a practice based on English common law (Mohr, 1979; Gold, 2003). After quickening, abortion was considered a criminal offense (Mohr, 1979; Gold, 2003; Lahey, 2014a). The 1830s and 1840s brought the first U.S. laws regulating abortion. The new laws restricting abortion started as medical malpractice laws that targeted abortion practitioners instead of the mothers (Rubin, 1994; Reagan, 1997; Lahey, 2014a,b). Connecticut was the first state to pass an anti-abortion law, and "made it a crime to give a poisonous substance to a woman in order to cause a miscarriage" (Rubin, 1994, pg. 2). States that followed over the early 1800s passed "anti-poisoning statutes" (Law et al., 1989, pg. 66) and it became a crime to "administer such remedies" (Law et al., 1989, pg. 66).

As the 1800s progressed, state regulation became more stringent so that by the 1860s, many states were actively outlawing abortion (Mohr, 1979; Lahey, 2014a). Still, over the nineteenth century, abortion was common enough that performing "abortions became one of the first specialties in American medical history" (Law et al., 1989, pg. 63). But as the American Medical Association (AMA) grew in influence, physicians attempted to distinguish themselves from non-physician providers (Mohr, 1979; Lahey, 2014a). Thus, the AMA became the "single most important factor in altering the legal policies towards abortions in this country" (Law et al., 1989, pg. 63).

State laws outlawing abortion spanned 1840 to 1899, with women facing potential prosecution for obtaining an abortion (Mohr, 1979; Lahey, 2014a,b). As these criminal abortion laws went into effect, the years spanning 1880 to 1960 were "labeled 'the silent decades"' for abortion (Rubin, 1994, pg. 2). During this period, abortion was forced underground and ill-reported in public records. "Despite the criminalization of abortion nationwide, abortion continued" (Reagan, 1997, pg.20) and a substantial number of abortions still occurred, with some estimates suggesting as many as "one million illegal abortions a year" (Rubin, 1994, pg. 2). In fact, in 1871 NYC, the city's population of less than a million "supported two hundred full-time abortionists, not including doctors who sometimes performed abortions" (Law et al., 1989, pg. 64). The legal restrictions on abortion, "did not stop abortion, but made it furtive, humiliating, and dangerous" (Law et al., 1989, pg. 66).

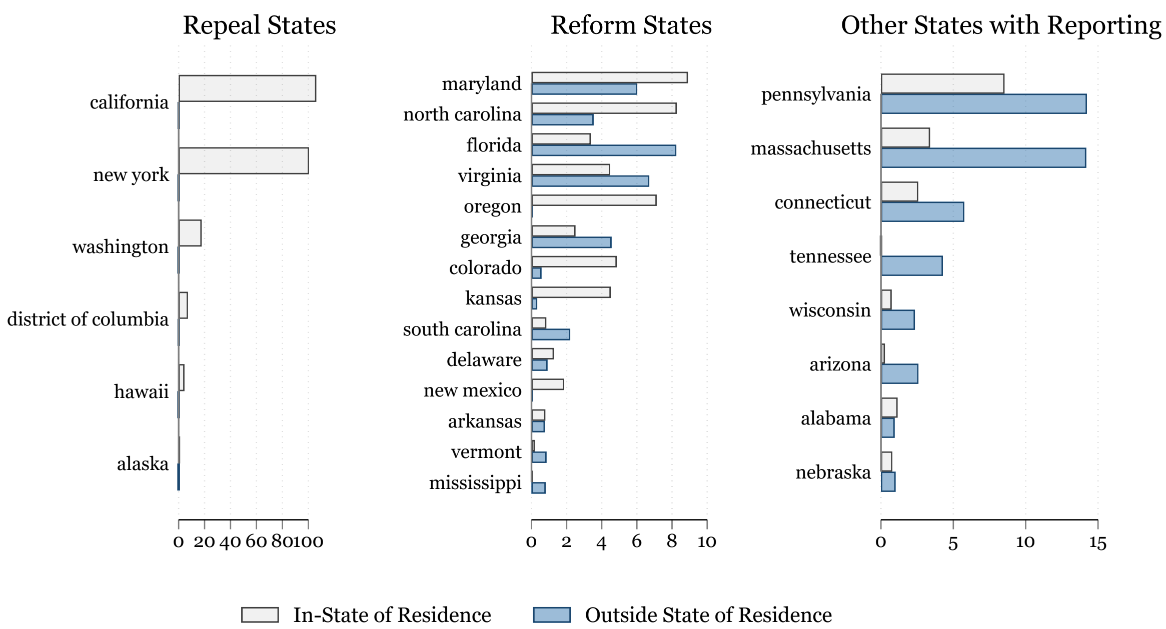
G.3 Speculative Evidence on Abortion Access in the Early 1970s

In 1969, the Centers for Disease Control and Prevention (CDC) began separately compiling, analyzing and disseminating statistics on legal abortion from each reporting area throughout the United States in the form of periodic surveillance reports, under the Abortion Surveillance Program (Smith and Bourne, 1973; Cates et al., 1977). By 1972, the abortion surveillance program included data from 20 states (plus DC) with state-wide abortion data, as well as abortion data from single hospitals in eight non-reporting states (CDC, 1972).[87] An important

[87]Reporting states with legal abortion include: Alaska, California, District of Columbia, Hawaii, New York, and Washington. Reporting States with abortion reforms include Arkansas, Colorado, Delaware, Georgia, Florida, Kansas, Maryland, North Carolina, Oregon, South Carolina, Virginia, Vermont, and Mississippi. Other reporting states include Massachusetts. Single hospitals reporting include Alabama, Arizona, Con-

relevant limitation of the data from the CDC abortion surveillance reports is that it does not span the nation, and thus, may depict incomplete and undercounted information (Koonin et al., 1993; Henshaw and Feivelson, 2000; Kortsmit et al., 2020; Myers, 2021b).[88] Still, this data provides the best historical picture of abortions occurring in the early 1970s. We digitize this CDC abortion surveillance program data to demonstrate that in the years before *Roe v. Wade*, abortion was still quite prevalent.

Figure G.1: Reported Abortion Counts by Residence State, Split by Abortions Occurring In-Residence State and Out-of-Residence State–CDC (1972)



Source: CDC (1972).
Notes: Counts reported in 1,000s. States sorted by total reported abortions, note the scaling differences between state groups. Reporting states with legal abortion include: Alaska, California, District of Columbia, Hawaii, New York, and Washington. Reporting States with abortion reforms include Arkansas, Colorado, Delaware, Georgia, Florida, Kansas, Maryland, North Carolina, Oregon, South Carolina, Virginia, Vermont, and Mississippi. Other reporting states include Massachusetts. Single hospitals reporting include Alabama, Arizona, Connecticut, Nebraska, New Mexico, Pennsylvania, Tennessee, and Wisconsin.

In 1972 there were 586,760 known cases of legal abortion (versus 1,864,064 births) in 27 states and DC, and the legal abortion to live birth ratio was 0.18 (CDC, 1972).As shown in Panel A of Figure A.1 abortions per reproductive-age female were highest in repeal (fully legal) states (both before and after *Roe v. Wade*). While abortion rates were more similar between reform states and states that never adopted any legal changes, reform states had slightly higher abortion rates. States without repeals or reforms also had the highest share of abortion occurring outside of the women's state of residence (as demonstrated by the dashed line in the darkest shade of blue in Figure A.1). In 1972, just before *Roe v. Wade*, 43% of all abortions occurred outside the individual's state of residence (CDC, 1972).

necticut, Nebraska, New Mexico, Pennsylvania, Tennessee, and Wisconsin.

[88] The CDC abortion surveillance program, by design, relies on the voluntary cooperation of the state and local health departments to report the data on legal induced abortions (Smith and Bourne, 1973). This leaves scope for a wide range of variation in the reporting mechanism - some states may choose not to survey or report abortions, while some states may not require all abortion providers to report data, leading to underreporting (Saul, 1998).

The prominence of out-of-state abortions is also demonstrated clearly in the 1972 abortion counts by residence state, shown in Figure G.1. Figure G.1 presents the total number of *known* abortions by state of residence, split into abortion in the state of residence and abortions outside the residence state. The lighter gray bars show abortions performed in the state of residence (for each state), and the darker blue bars show abortions performed outside the state of residence. The prominence of the darker blue bars in non-repeal states demonstrates that women commonly traveled outside their state of residence to obtain an abortion. The presence of lighter gray bars in non-repeal states also reveals that abortion access did exist in non-repeal states in the year just before *Roe v. Wade*.

H Data Appendix

H.1 Abortion and Maternal Causes of Death by ICD Code

We include deaths that occurred due to maternal causes (which includes abortion) and abortion-specific causes over the period of our analysis. The major ICD codes included in each of our mortality measures are shown in Table H.1.

Over the period of analysis, two major revisions to the ICD codes occurred. During the seventh revision, in place for 1958–67, maternal causes of death included ICD-7 codes 640–689 (Hoyert, 2007). In the eighth revision, applicable for 1968–78, maternal causes of death included ICD-8 codes 630–678. In the ninth revision, occurring in 1979–1998, maternal causes of death come from ICD-9 codes 630–677.[89]

For abortion-specific causes of death, during the 7th revision, abortion includes ICD-7 codes 650–652. During the 8th revision, abortion-related deaths include ICD-8 codes 640–645. Finally, in the ninth revision, abortion-related deaths include 634–639 (Hoyert, 2007; WHO, 2019). Abortion classifications change slightly between revisions to reflect the changing nature of abortion. To account for these adjustments, we take the larger header of "abortion deaths" as encompassing these changes in finer causes of abortion-related deaths.

Due to the changes in the specific causes of death, it is difficult to follow classifications of legal versus illegal abortion over time. For instance, in the ICD-9 version of the causes of death, abortion is separated into "spontaneous abortion," "legally induced abortion," and "Illegally induced abortion." However, in the ICD-7 codes, abortion is classified only as "Abortion without mention of sepsis or toxaemia," "Abortion with sepsis," "abortion with toxaemia, without mention of sepsis." These changes in the classifications of abortion deaths are a limitation of this data, making it difficult to track individual causes of abortion deaths.

Another important feature of the multiple cause of death data is the difference between death by residence and death by occurrence. For our main results, we show the results by residence instead of occurrence. If women traveled from their residence to obtain an abortion, we would want to capture the decline in these deaths based on the residence state due to their residence states' illegal status. Despite the concern over differences between deaths in the state of residence versus occurrence, the results are similar between the use of both. We suspect this is due to the fact that only a small share of deaths occur outside the resident state (Figure A.5).

In addition to the maternal and abortion-related deaths, we also show the effect of abortion on infant and neonatal mortality. We use the age at the time of death reported on the death certificates to compute the infant and neonatal rates. Infant mortality is measured as any death occurring to infants under one year of age. Neonatal mortality is defined as the death of an infant in the first month of life.

[89] In addition to the underlying causes of death, the data includes grouped causes of death, which can also be used to ascertain maternal mortality (separately from the ICD codes).

Table H.1: ICD-10 Codes in Each Cause of Mortality

7th Revision	8th Revision	9th Revision
1959-1967	**1968-1978**	**1979-1998**
Panel A: ICD Definition of Maternal Deaths (All-cause)		
Complications of pregnancy **(640-649)**	Complications of pregnancy **(630-634)**	Ectopic and molar pregnancy **(630-633)**
Abortion **(650-652)**	Urinary infections and toxaemias of pregnancy and the puerperium **(650-652)**	Other pregnancy with abortive outcome **(634-639)**
Delivery without mention of complication **(660)**	Abortion **(640-645)**	Complications mainly related to pregnancy **(640-648)**
Delivery with specified complication **(670-678)**	Delivery **(650-662)**	Normal delivery and other indications for care in pregnancy, labour and delivery **(650-659)**
Complications of the puerperium **(680-689)**	Complications of the puerperium **(670-678)**	Complications occurring mainly in the course of labour and delivery **(660-669)**
		Complications of the puerperium **(670-677)**
Panel B: *Broad* Definition of Abortion-specific Deaths		
Other infections of genito-urinary tract during pregnancy **(641)**	Infections of genital tract during pregnancy **(630)**	Hydatidiform mole **(630)**
Other haemorrhage of pregnancy **(644)**	Ectopic pregnancy **(631)**	Other abnormal product of conception **(631)**
Ectopic pregnancy **(645)**	Haemorrhage of pregnancy **(632)**	Missed abortion **(632)**
Anaemia of pregnancy **(646)**	Anaemia of pregnancy **(633)**	Ectopic pregnancy **(633)**
Abortion **(650-652)**	Abortion **(640-645)**	Abortion **(634-639)**
Puerperal urinary infection without other sepsis **(680)**	Sepsis of childbirth and the puerperium **(670)**	Hemorrhage in early pregnancy **(640)**
Sepsis of childbirth and the puerperium **(681)**		Antepartum hemorrhage, abruptio placentae and placenta praevia **(641)**
		Infective and parasitic conditions in the mother classifiable elsewhere but complicating pregnancy, childbirth and the puerperium **(647)**
		Major puerperal infection **(670)**
Panel C: ICD Definition of Abortion-specific Deaths		
Abortion without mention of sepsis or toxaemia **(650)**	Abortion induced for medical indications **(640)**	Spontaneous abortion **(634)**
Abortion with sepsis **(651)**	Abortion induced for other legal indications **(641)**	Legally induced abortion **(635)**
Abortion with toxaemia, without mention of sepsis **(652)**	Abortion induced for other reasons **(642)**	Illegally induced abortion **(636)**
	Spontaneous abortion **(643)**	Unspecified abortion **(637)**
	Abortion not specified as induced or spontaneous **(644)**	Failed attempted abortion **(638)**
	Other abortion **(645)**	Complications following abortion and ectopic and molar pregnancies **(639)**

H.2 Does the change to the ICD code impact mortality?

Over the period of analysis, two major revisions of the ICD codes occurred, ICD-7 to ICD-8 over 1968–78 and ICD-8 to ICD-9 over 1979–1998. This change in classification poses some concern in our baseline specification, where the switch from ICD-7 to ICD-8 occurred just before legal abortion took hold in the United States. In the literature, this change from ICD–7 to ICD–8 has been thought to have little impact on maternal mortality statistics (Hoyert, 2007). Hoyert (2007) also demonstrated that changes in ICD definition did not lead to major jumps in the maternal mortality rates, except for the change from the revision of ICD-9 to ICD-10, which is not included in our period of analysis. In the difference-in-differences results in Figure 6, we also show that omitting years before ICD-8 codes, and focusing on 1968-1978, has little impact on the point estimate of our results. Still, we further analyze whether the change to ICD-8 led to any noticeable change in mortality.

To consider whether ICD changes produced a change in mortality, we estimate the year-over-year change in mortality as:

$$\text{Mortality}_{st} = \alpha + \beta\ \text{ICD}_{st} + \mathbf{X}'_{st}\gamma + \alpha_s + \epsilon_{st} \tag{4}$$

where all features of Equation 4 reflect Equation 2 except for the following. We limit the sample to two years, where the dummy variable ICD_{st} will equal one in 1968 and zero in 1967.

Table H.2 shows two different specifications considering whether the mortality rate changes from ICD-7 to ICD-8. First, in Panel A, we follow Equation 4 and show the year-over-year change in mortality from 1967 to 1968. In the year-over-year specification from 1967-1968, the ICD-8 change suggests little evidence of a jump in mortality.

Second, in Panel B, we consider whether there is a break from the state-level trend from 1963-1972. The difference from Panel A is that multiple years are considered, and a state-level linear trend is added to the specification. Here again, the ICD-8 code produces little change in the number of deaths collected. Together, these findings suggest that the ICD-8 change is not producing a statistically significant change in maternal or abortion-related mortality.

Table H.2: ICD Changes

Panel A: Year-over-Year Change from 1967-1968

	Maternal			Abortion		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	White	Non-white	All	White	Non-white
1(Adoption of ICD-8)	-0.4848	-0.6217	-0.9582	0.5474	0.6788	-0.0983
	(0.5682)	(0.7712)	(1.2650)	(0.4856)	(0.4491)	(1.5068)
N	102	102	102	102	102	102
State FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X

Panel B: Trend over 1963-1972

	Maternal			Abortion		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	White	Non-white	All	White	Non-white
1(Adoption of ICD-8)	-0.0137	-0.0340	0.0548	0.0283	0.0369	0.0262
	(0.0550)	(0.0681)	(0.0898)	(0.0434)	(0.0397)	(0.1497)
N	510	509	509	510	509	509
State FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X
State-level Linear Trend	X	X	X	X	X	X

Source: NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980.

Notes: OLS coefficients reported. Baseline fixed effects include state fixed effects. The binary variable of interest captures the change in the ICD classification in 1968. Panel A shows year-over-year changes, and Panel B shows the results over 1963-1972, controlling for state-level linear trends. Our main set of state-level demographic controls includes the share of reproductive-age females who are 15-19 and white, the share of reproductive age females who are 15-19 and non-white, the log of per capita income, the log of per pupil education spending, and the state-level share with a high school degree. We also include policy controls for state-level abortion reforms, access to the pill for minors, access to the pill generally, unilateral divorce legislation, state equal pay legislation, and the inverse hyperbolic sine of state-level inductions per 1,000 males 18-25. Outcome is the inverse hyperbolic sine of the mortality rate. Rates are per 100,000 reproductive-aged females in each population (all, white, and non-white). Robust standard errors clustered at the state level. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.

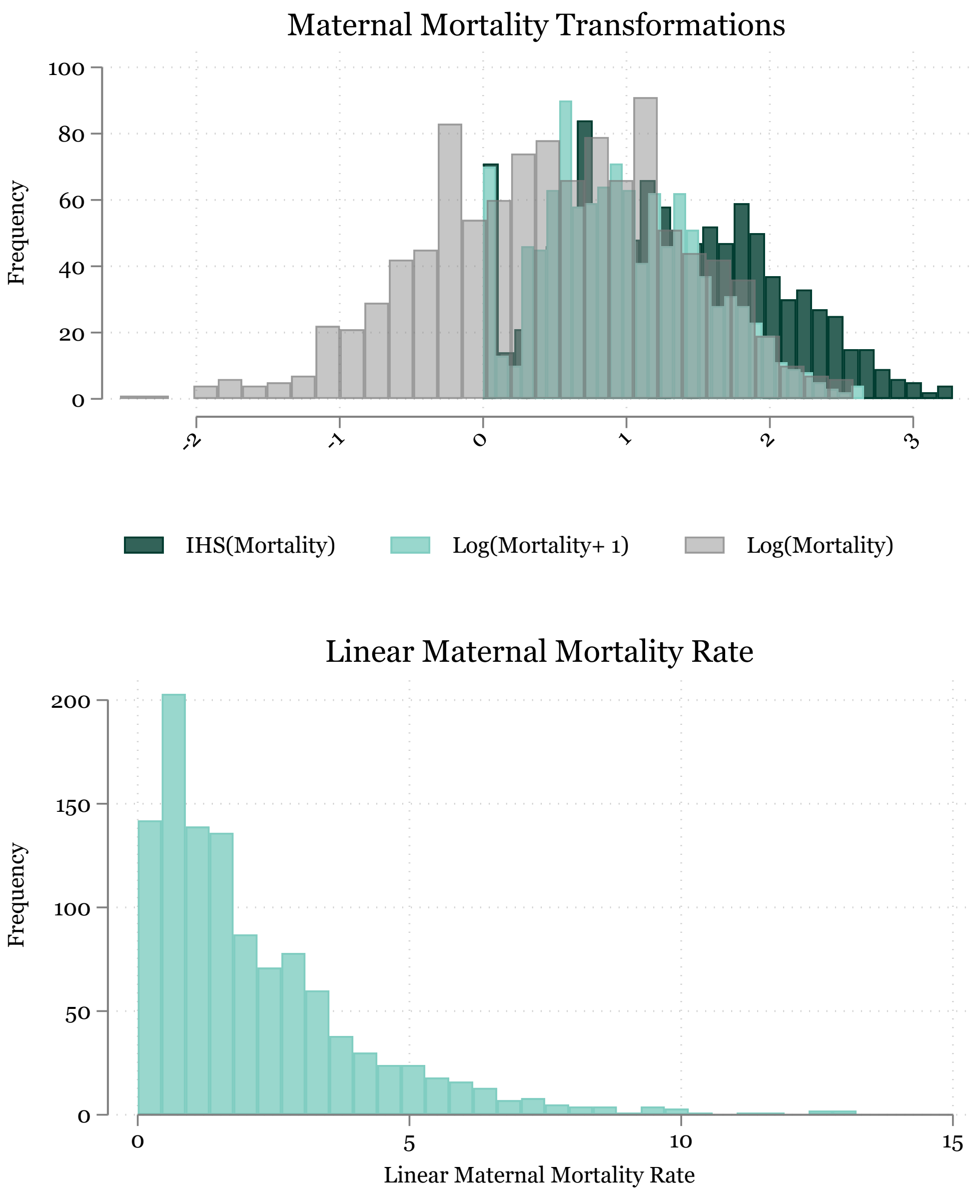
H.3 Functional Form

In our main findings, we take the inverse hyperbolic sine of the mortality rate. We choose the inverse hyperbolic sine as it approximates the natural log of mortality while maintaining zero observations (Bellemare and Wichman, 2020). While in the robustness, we show our general conclusions are similar if we use alternative functional form, in this section, we motivate why a log transformation is important.

First, Figure H.1 shows that the natural log of mortality, the inverse hyperbolic sine, and the log of mortality plus one all are closer to a normal distribution while the linear mortality rates are skewed towards zero. We prefer the natural log distributions, which are closer to normal, and will perform better in our OLS specification. Second, when using the natural log, our estimates reflect proportional changes in mortality rather than mean absolute levels. As maternal and abortion mortality vary substantially from state to state and over time, a specification that accounts for proportional changes rather than linear changes will be preferred in our context.

Third, the inverse hyperbolic sine performs better in cases where the transformed variable of interest is non-negative (Ravallion, 2017) and (roughly) more than one-third of the observations are greater than zero (Bellemare and Wichman, 2020). In our case, all measures of mortality clearly satisfy the non-zero condition. However, for abortion-related mortality, more than 1/3 of observations are zero (e.g., Figure H.2). Thus, for abortion-related mortality (in particular), a Poisson model may be preferred.

Figure H.1: Distribution of Maternal Mortality



Source: NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980.
Notes: Rates are per 100,000 reproductive-aged females in each population (all, white, and non-white).

Figure H.2: Average (Linear) Maternal and Abortion-Related Mortality by State, 1959-1968

Source: NCHS/NVSS/CDC Multiple Cause of Death Files, 1959-1980.

H.4 Issues and Limitations with the Mortality Data

Two notable data issues exist in the raw mortality data. First, NJ has a substantial number of missing race values for 1962 and 1963. We replace the mortality by race as missing for NJ in 1963 and 1962 to account for the substantial number of observations with missing race. This missing race issue is present in the underlying raw data and noted in the codebook. Another issue is that in 1972 only, the mortality data represent a 50% sample. We thus, replace 1972 with double the underlying deaths to smooth the rates over time.

H.5 Natality Data

We also add data on the number of births from the *Natality Detailed File* and U.S. Vital Statistics (NCHS, 1968-1980b; NVSS, 1959-1968). Pre-1968, the data is available at the state level from NVSS (1959-1968), but for 1968 and onward, we aggregate post-1968 data to the state level from individual birth certificates (NCHS, 1968-1980b). Two notable limitations of the Natality Detail File exist for our sample time frame. First, the microdata is only available from 1968 onward, limiting our ability to consider an extensive pre-period. Still, since all legalizations occurred from 1970 onward, the data allow us at least one pre-period for each state. Thus, due to the limited pre-legalization years in the sample, we consider the impact of early legalizations relative to *Roe v. Wade* using the IW specification (rather than TWFE). Second, the data are based on a 50% sample for specific years, with states gradually expanding from 1973 onward. This 50% sample does not affect our results because we use the average delivery characteristics by state for each year. Though, when we calculate the birth counts, we use double the births for the specific year noted as 50% samples in the NCHS codebooks NCHS (1968-1980b).

H.6 County-level Data

In the mechanism section, Section 3, we perform a county-level analysis. For this analysis, we adjust the main data sources. First, we use county-level estimates of births and deaths from NCHS (1959-1980a), NCHS (1968-1980b), NVSS (1959-1968) (instead of the state level). Then, we use Bailey et al. (2016) to obtain the NCHS county codes and the county-level population of reproductive-age females. Though, a notable data limitation of Bailey et al. (2016) is that certain counties are missing population information.[90] To replace the missing population counts, we estimate the county population of reproductive-age females using state-level population shares from the Ruggles et al. (2021) combined with the county-level populations from Haines (2010). This population replacement only affects around 2,000 out of 68,000 observations. Replacing the missing population information is important, as DC is notably missing the reproductive age-females for a portion of the analysis period. For the heterogeneity analysis, we use the 1960 population characteristics from Haines (2010); AHRF (1994), the 1960 information on hospital deliveries available in NVSS (1959-1968), and the 1971 information from Johnson et al. (1971).

[90] Alaska is missing for the entire sample, and we are unable to remove this information.

H.7 Specifics of Abortion Legislation

Table H.3: Abortion Legalizations–States that Repealed their Anti-abortion Statutes

	Year	State	ACTION
1	1969	California	Legalized abortion
2	1970	New York	Legalized abortion
3	1970	Alaska	Legalized abortion
4	1970	Hawaii	Legalized abortion
5	1970	Washington	Legalized abortion
6	1971	District of Columbia	Legalized abortion
	January 22, 1973	All states	Supreme Court decisions in Roe v. Wade

SOURCES: CDC (1969-1980), Rubin (1994), Merz et al. (1996), Myers (2021a)

Table H.4: Abortion Reforms pre-Roe v. Wade

	Year	State	Action
1	1966	Mississippi	Legalized abortion in cases of rape.
2	1967	Colorado	MPC reform
3	1967	North Carolina	MPC reform
4	1967	California	MPC reform
5	1968	Maryland	MPC reform
6	1969	Arkansas	MPC reform
7	1969	Delaware	MPC reform
8	1969	New Mexico	MPC reform
9	1969	Georgia	MPC reform
10	1969	Oregon	MPC reform
11	1970	South Carolina	MPC reform
12	1970	Kansas	MPC reform
13	1970	Virginia	MPC reform
14	1972	Florida	MPC reform
15	1972	Vermont	Court case on abortion
16	1972	New Jersey	Court case on abortion

SOURCES: CDC (1969-1980), Rubin (1994), Merz et al. (1996), Myers (2021a)
NOTES: MPC decriminalized abortion in cases of: danger to the mother's physical or mental health, a fetus with a physical or mental defect, and a case of rape or incest.

I Fertility, Infant Mortality, and Neonatal Mortality

In this section, we test the impact of abortion policy on infant deaths, neonatal deaths, and the fertility rate. We measure the fertility rate as the number of births per 1,000 reproductive-age females. However, for infant and neonatal mortality, we compute these measures per 1,000 births (the standard measure). We maintain the infant and neonatal deaths per birth since these deaths are a direct function of the number of infants born in a given year. Thus, for infant and neonatal mortality, both the numerator and denominator will be affected by any change in the fertility rate.

I.1 Fertility

We test the effects of abortion legalization on the state-level fertility rate, which has been studied previously (e.g., in Guldi (2008)). While these effects have been previously documented, we present the fertility effects to emphasize two points. First, we want to test whether legal abortion affects fertility, which validates our use of reproductive-age females in the denominator of maternal and abortion mortality (instead of traditional measures of abortion and maternal deaths per birth). If we use births, mortality may change as a result of the denominator (births) adjusting in response to the passage of legal abortion. Second, the characteristics of pregnancies themselves may be changing due to a reduction in the fertility rate.

Figure I.1 shows the impact of abortion legalization on the number of births per 1,000 females 15-44 in the left-most specification (in purple). Following abortion legalization, there is a substantial reduction in the fertility rate, especially for non-white women. White women show a more transient decline. The clear reduction in the non-white fertility rate is also robust to alternative specifications in the difference-in-differences specification shown in Figure I.2.

Given the magnitude of the number of abortions performed in the wake of Roe v. Wade (discussed in Section 3), the importance of abortion to the fertility rate is not surprising. Over 1973 to 1980, abortions appear to have doubled in counts, rising from 616,000 in 1973 to 1,298,000 abortions in 1980 (CDC, 2011). Based on the increase in abortions over this time period, the substantial decline in the fertility rate is quite plausible and aligns with prior findings.[91]

I.2 Infant and Neonatal Mortality

Based on evidence suggesting that access to abortion may have impacted infant mortality (Grossman and Jacobowitz, 1981; Krieger et al., 2015), we consider the effect of legal abortion on infant mortality and neonatal mortality. We show the event-study results in Figure I.1 in the middle (in green) for infant mortality and on the right (blue) for neonatal mortality. The results only show some reduction in white infant and neonatal mortality, but there is no impact on non-white infant or non-white neonatal mortality.

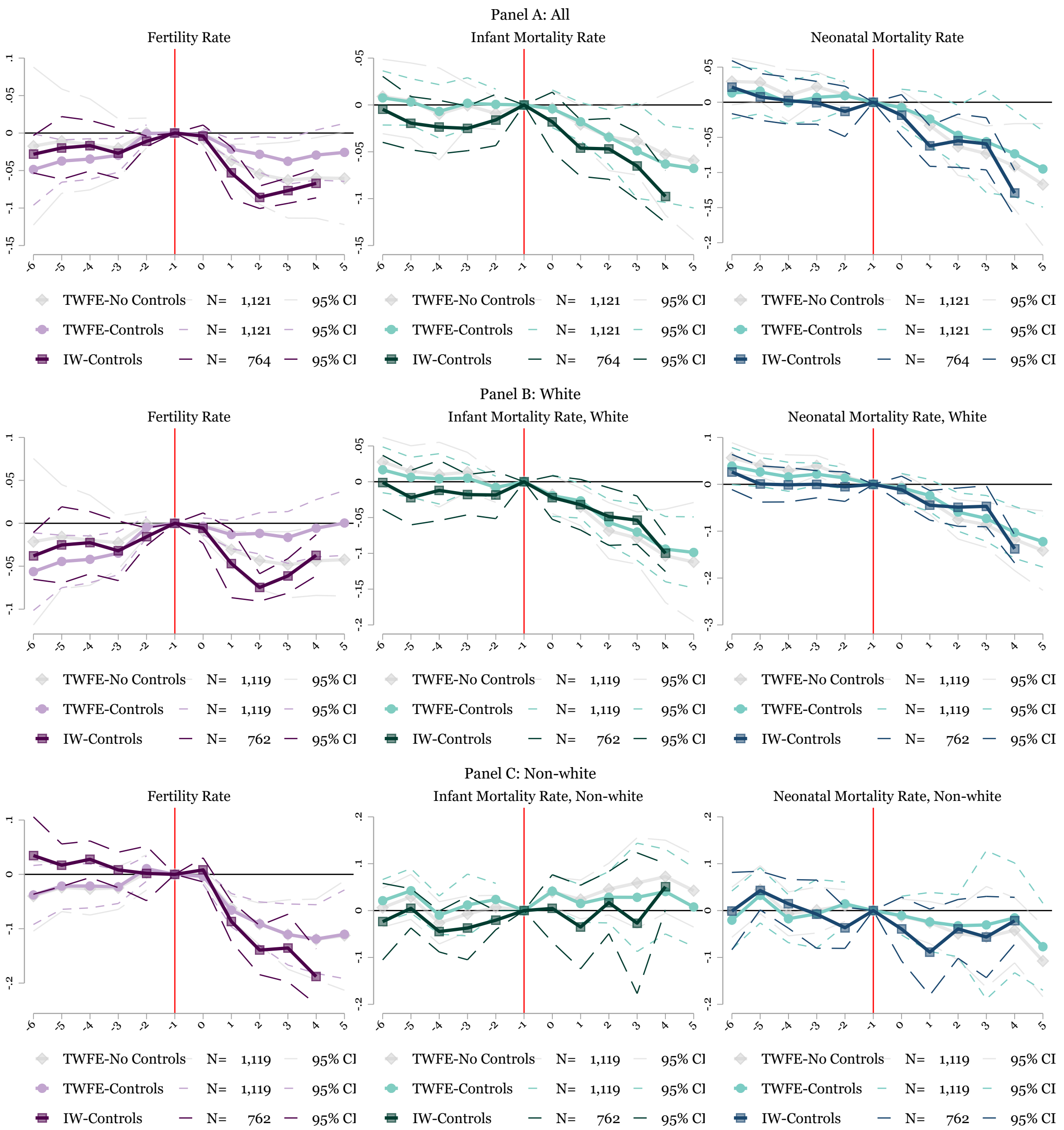
Figure I.2 then displays the difference-in-differences specification for neonatal and infant

[91] In addition to Guldi (2008), our findings align with Myers (2017), though we consider different outcomes.

mortality. The difference-in-differences results show a less noticeable decline in neonatal and infant mortality, suggesting that the event-study findings are not robust to alternative specifications. These findings place some skepticism on the conclusions in (Krieger et al., 2015), indicating little causal impact on infant or neonatal mortality.[92]

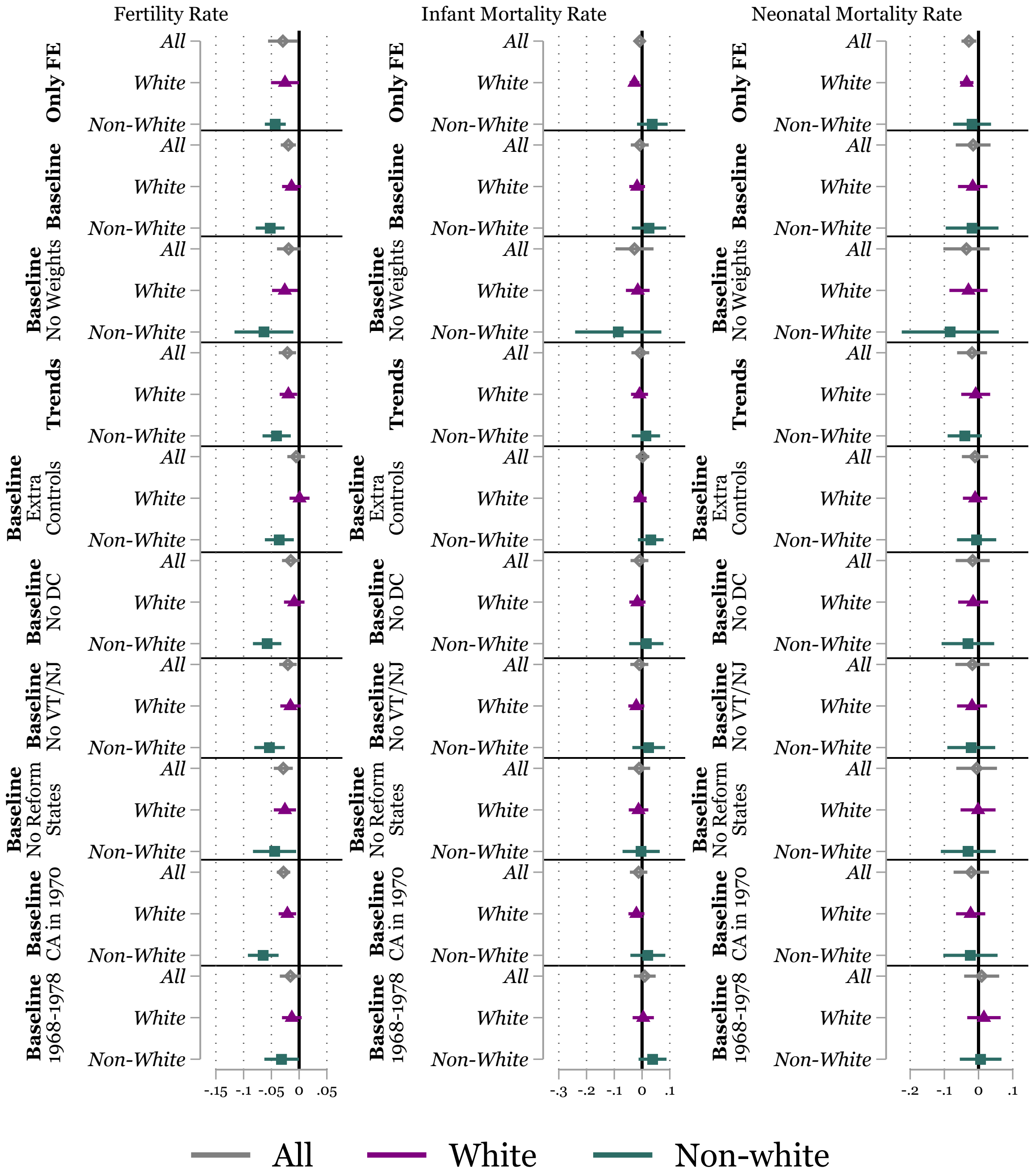
[92]These results also fall somewhat in line with previous work in Bauman and Anderson (1980); Miller et al. (1988), where each study found no discernible impact on neonatal mortality or infant mortality. However, Bauman and Anderson (1980) did find a modest and brief contribution of the 1970s legalizations to reducing fetal deaths.

Figure I.1: Additional Results: Effect of Legal Abortion on Infant and Neonatal Mortality



Notes: OLS coefficients presented above. Baseline fixed effects include year fixed effects and state fixed effects. Plotted coefficients are dummy variables on each year before and after the change to abortion policy (see Equation 1). The period just before the legal change is the excluded period (-1)–indicated by the vertical line. For the two-way fixed effects specification (TWFE), the left endpoint is binned at $m = -7$, and the right endpoint is binned at $m = 6$. For the Interaction-Weighted (IW) specification, the event study is fully saturated. The IW specification only considers the years 1959-1973, with *Roe v. Wade* states as the last-treated comparison group. Only the point estimates in the main event window are displayed. Dashed and dotted lines reflect 95% confidence intervals. Robust standard errors clustered at the state level. In the main specification, we use the inverse hyperbolic sine of the mortality and fertility rates. Infant and neonatal mortality are per 1,000 births. The fertility rate is per 1,000 reproductive-age females. Non-white (white) rates are per non-white (white) 1,000 births (infant/neonatal) or 1,000 reproductive-age female (births). Estimates are weighted by the denominator of the rate. Our main set of state-level demographic controls includes the share of reproductive-age females who are 15-19 and white, the share of reproductive age females who are 15-19 and non-white, the log of per capita income, the log of per pupil education spending, and the state-level share with a high school degree. We also include policy controls for state-level abortion reforms, access to the pill for minors, access to the pill generally, unilateral divorce legislation, state equal pay legislation, and the inverse hyperbolic sine of state-level inductions per 1,000 males 18-25. For the sources, see Table 1.

Figure I.2: Difference-in-Differences Results: Infant and Neonatal Mortality



Notes: OLS coefficients presented above. Baseline fixed effects include year fixed effects and state fixed effects. The main binary variable represents legalized abortion, which captures the effect of early legal abortion as well as the 1973 *Roe v. Wade* decision (see Equation 2). In the main specification, we use the inverse hyperbolic sine of the mortality and fertility rates. Infant and neonatal mortality are per 1,000 births. The fertility rate is per 1,000 reproductive-age females. Non-white (white) rates are per non-white (white) 1,000 births (infant/neonatal) or 1,000 reproductive-age female (births). Estimates are weighted by the denominator of the rate. Our main set of state-level demographic controls includes the share of reproductive-age females who are 15-19 and white, the share of reproductive age females who are 15-19 and non-white, the log of per capita income, the log of per pupil education spending, and the state-level share with a high school degree. We also include policy controls for state-level abortion reforms, access to the pill for minors, access to the pill generally, unilateral divorce legislation, state equal pay legislation, and the inverse hyperbolic sine of state-level inductions per 1,000 males 18-25. For the sources and description, see Table 1 and Figure 6.