Maternal mortality, race, and the abortion laws of the 1960s and 1970s

Tia M. McDonald, USDA ERS, tia.mcdonald@usda.gov

Selected Paper prepared for presentation at the 2023 Agricultural & Applied Economics Association Annual Meeting, Washington DC; July 23-25, 2023

The findings and conclusions in this article are those of the author and should not be construed to represent any official USDA or U.S. Government determination or policy. This research began prior to the author's employment at the USDA.

Abstract

The following study examines the effect of access to legal abortion by race on maternal mortality using the changes to abortions laws of the 1960s and 1970s. These legal changes were comprised of states who expanded access to abortions and others that removed all legal barriers either due to a change in state law or the result of Roe v Wade. The results of this study suggest disparate impacts depending on the legal mechanism and the race of the patient. Using a panel of deaths due to pregnancy related complications and a Poisson pseudo maximum likelihood estimator, the results suggest a pronounced decrease in maternal mortality for Nonwhite women in response to abortion decriminalization. For White women, what may be an anticipatory decline in maternal mortality prior to abortion decriminalization which makes assessing the effects of abortion decriminalization on maternal mortality for this population difficult since there is a clear violation of the parallel trend assumption. Based on data and methods used in our analysis, abortion reforms seem to have no detectable effect on maternal mortality for women regardless of race.

On June 24, 2022, the Supreme Court of the United States, in their ruling in the case of Dobbs v. Jackson Women's Health Organization, overturned the precedent established in Roe v. Wade. Similar to abortion access prior to Roe v. Wade, this 2022 ruling made state statutes of paramount importance in establishing women's access to legal abortion. As the landscape changes around abortion access, researchers are addressing many important questions regarding the impact of abortion restrictions. The research pursued in this paper addresses the degree to which differences in the legality of abortion in the 1960s and 1970s affected maternal mortality. We further assess how abortion access affected maternal mortality by race.

The 1960s ushered in two types of substantial changes to many states' legal codes regarding abortion access. One type was a reform of existing laws that expanded the legal exception to allow abortions if "there is substantial risk that continuance of the pregnancy would gravely impair the physical or mental health of the mother" (Moyers 1970). The language of the reform was developed by the American Law Institute (ALI) and published as part of their 1962 Model Penal Code (Pendleton 1967). The purpose of the change in the language was to provide a broader range of exceptions that would lessen the risk of prosecution for the physician. The ALI Model Penal Code also expanded abortions to cases of rape, incest, and fetal deformity. While the purpose of the ALI Model Penal Code was to expand access to abortions for women at risk of impaired health, the law included additional provisions as to how the proposed abortion would be deemed necessary including a provision that hospitals should have a three doctor panel review cases for approval. This provision has important theoretical implications which we develop as part of our theoretical model.

The second type of legal change was full decriminalization of abortion, whether by state legislation or by judicial ruling. Full decriminalization withdrew all penalties for physicians performing abortions. Five states (plus the District of Columbia) decriminalized abortion between 1969 and 1971. Alaska, Hawaii, and New York passed state legislation to this effect; California and Washington DC experienced de facto decriminalization after judicial rulings; and Washington passed a referendum decriminalizing abortion. The U.S. broadly decriminalized abortion in 1973 due to the Supreme Court ruling, however, researchers have noted that the legality of abortions is North Dakota was not settled until 1974 (Myers 2022).

Previous literature that examines the impact of changes in access to abortion can be organized into first-order and second-order effect studies. First order effects of abortion access include legalization's effect on utilization and fertility rates. Among these studies, some have employed difference-in-difference estimation to examine the role of widespread decriminalization of abortion brought about by Roe v. Wade using states that decriminalized prior to Roe v. Wade as a control group (Levine 1999; Bailey 2010). Another article used a unique dataset from New York state to demonstrate the large distances women had traveled to receive abortions in this state after decriminalization in 1970 (Joyce 2013). The Joyce (2013) article highlights the need to account for proximity to states like New York, where abortions were legalized prior to Roe v. Wade and where there were no residency requirements for those seeking an abortion. Another important paper by Blank et al. (1996) shows the effect of Medicaid restrictions on abortion rates. This paper has important findings with regard to the nature of abortion constraints such as cost and availability. Other papers have taken the research a step further to investigate second order effects on crime, poverty, education, labor market outcomes, low birth weights, and substance abuse (Angrist 2000: Ananat et al. 2012; Charles 2006; Donohue 2001; Foote 2008; Joyce et al. 1990).

There have been a number of articles that link more recent restrictions in abortion access to increased maternal mortality (Vilda et al. 2021; Stevenson 2019; and Hawkins et al. 2019). The research by Vilda et al. (2021) and Hawkins et al. (2019) use state-level data and Poisson and negative binomial estimators, respectively which is similar to the estimation strategy employed in this article and find an increase in maternal mortality associated with restricted access to abortion. To the author's knowledge, there has yet to be an analysis of maternal mortality related to abortion restrictions at the time of Roe v. Wade.

Background

As of 1965, most states and the District of Columbia had statutes to allow abortions if the abortion was necessary to save the life of the mother[1] (George 1965) The state laws banning abortions were criminal statues, where doctors convicted of performing an unnecessary abortion were subject to felony conviction and the loss of their medical licenses. The late 1960s ushered in two types of substantial changes to many states' legal codes. One type was a reform of existing laws that expanded the legal exception to allow abortions if "there is substantial risk that continuance of the pregnancy would gravely impair the physical or mental health of the mother" (Moyers 1970). The theoretical impact of this change will be discussed below. The language of the reform was developed by the American Law Institute (ALI) and published as part of their 1962 Model Penal Code (Pendleton 1967). The purpose of the change in the language was to provide a broader range of exceptions that would lessen the risk of prosecution for the physician. The ALI Model Penal Code also expanded abortions to cases of rape, incest, and fetal deformity. While the purpose of the ALI Model Penal Code was to expand access to abortions for women at

[1] Four states (Louisiana, Massachusetts, New Jersey, Pennsylvania) provided no explicit legal exceptions allowing an abortion to save the life of the mother although Massachusetts and New Jersey Supreme Court decisions in 1961 and 1944, respectively, provided such an exception (George 1965).

risk of impaired health, the law included additional provisions as to how the proposed abortion would be deemed necessary including a provision that hospitals should have a three doctor panel review cases for approval. This provision has important theoretical implications which we will discuss below.

The second type of legal change was full decriminalization of abortion, whether by state legislation or by judicial ruling. Full decriminalization withdrew all penalties for physicians performing abortions. Five states (plus the District of Columbia) decriminalized abortion between 1969 and 1971. Alaska, Hawaii, and New York passed state legislation to this effect; California and Washington DC experienced de facto decriminalization after judicial rulings; and Washington passed a referendum decriminalizing abortion. The U.S. broadly decriminalized abortion in 1973 due to the Supreme Court ruling in 1973, however, researchers have noted that the legality of abortions is North Dakota was not settled until 1974 (Myers 2022). Table 1 lists the states, including Washington DC, that changed their abortion policies during this time frame by type of change and the year adopted.

Theoretical Model

The theoretical underpinnings that we use to understand maternal mortality and the decision to end a risky pregnancy is that of a constrained decision. We then overlay this model with the existence of racial bias. To begin, the decision to end a risky pregnancy may be constrained by legal statute. We assume there are two decision makers in this process. The first is the pregnant woman. The second is the physician since the physician is the party for whom the legal consequences fall in most cases. Whether or not the constraint is binding for the woman depends on the woman's views, the costs she faces, and her willingness to seek an abortion. These additional constraints are informed by Blank et al. (1996) who found that public funding and the number of abortion providers were associated with abortion rates as were certain demographic and economic factors like marriage and unemployment rates.

Since there are many factors that influence the abortion decision, the relaxing of a legal constraint may or may not have any detectable impact if the legal constraint was never binding. In the event of a binding constraint for the woman, the likely impact of these laws may not be apparent with regard to pregnancy related complications, especially if there has always existed an exception for the life of the mother. For the sake of simplicity let us assume there are two types of pregnancy related complications, one that will result in death with perfect certainty, and others that may result in death with uncertainty. For cases of perfect certainty and where the need for an abortion to save the life of the mother is evident to external observers, we would not expect any repeal or reform laws to have an impact on these cases, since there was never risk of prosecution to the physician. Instances of uncertainty are where we might expect the repeal and reform laws to have an effect. When there is uncertainty as to whether continuing the pregnancy will result in death or it is not evident to an outside observer, then expanded exceptions should provide physicians expanded cover to avoid prosecution and increase their willingness to perform an abortion. Therefore, we expect to find that states who adopted laws to expand access to abortions will show a decrease in maternal mortality relative to those who did not adopt these laws.

In order to take racial bias into account, we need to consider the two legal changes separately. Decriminalization, since it does not rely on subjective approval, should have the effect of decreasing maternal mortality for Nonwhite and White women, assuming that there is a binding legal constraint. The degree of the relative impact is not likely to be the same and depends on relative access to live saving abortions prior to legalization and the degree of access afforded by legalization, which can be constrained by costs, availability and other factors. These factors likely differed by race for women in the 1960s and 1970s. Since pre and post legalization access was likely less for women of color we cannot offer a hypothesis as two the relative impact of legalization on women of color relative to white women.

The case of abortion reforms that expanded access via the ALI Model Penal Code might have had a similar effect save for the use of a three-doctor panel to approve the procedure. We posit that the effect of a three-doctor panel would be negligible for white women and adverse for non-white women assuming the existence of racial bias[2]. Further, we posit that the impact of having a three-doctor panel would be worse for non-white than the prior system of seeking approval from just one doctor. To illustrate, let us say there is probability p that any one doctor is free of racial bias, then the probability of a three-doctor panel being free from racial bias is p^3 where $p^3 < p$.[3] Therefore, we expect that the three doctor panel would lower the likelihood of approval for an abortion for non-white women relative to white women and relative to the previous policy of approval from one doctor. This means that for white women, who are not subject to racial bias, we hypothesize the abortion reforms should have the intended effect of reducing maternal mortality. For non-white women, we hypothesize that the abortion reforms fail to decrease, and may even increase, maternal mortality for non-white women.

[2] The race of the doctor is potentially important to mention here since racial bias is likelier when the race of the doctor is different than the race of the patient. During this the time period of interest in this study, it was likely that if a racial difference existed between patient and doctor and it was more likely to occur for non-white patients than white. In fact, according to Sorensen (1972) in 1967, the ratio of white doctors to non-white doctors was almost 7:1.

[3] These are the relevant probabilities if the decision rule requires unanimity. If the decision rule is majority, then the relevant probability is the probability that at least two out of the three doctors are free from racial bias, $p^3 + 3p^2(1-p)$ where $p^3 + 3p^2(1-p) < p$.

Methodology

Prior to the Supreme Court ruling of Roe v. Wade, there were six states that decriminalized abortion access and thirteen that adopted the MCI reform law (table 1). We use variation in adoption of these laws to identify treatment effects against never treated or not-yet-treated. In the case of MCI reform law adoption, we consider the effect of adoption compared with states that had no change to the legal status of abortion. Therefore, the timeframe of this analysis is constrained to years prior to 1973's Supreme Court ruling. For the abortion decriminalization analysis, reform states are excluded.

[Insert Table 1 here]

The following empirical study exploits the quasi-experimental framework of states adopting repeal and reform laws. We may be concerned that the adoption of repeal or reforms laws may depend on the political climate within a given state. To this point, as Bailey (2010) points out, there is no consistent pattern between bans on contraception and the likelihood of adopting abortion reform laws, suggesting that it is not altogether true that political climate determined these laws.

In order to understand chances to risk of pregnancy death, which is count data, we use a Poisson pseudo-maximum-likelihood fixed effects estimator (Correia et al. 2020) which can accommodate complex and multilevel fixed effects which were employed in this analysis to check for robustness. Additionally, the Poisson pseudo-maximum likelihood estimator is robust to zero values in the dependent variable (Silva and Tenreyro 2011). Exposure in these models allows us to account for the rates of pregnancy death by number of live births. We therefore include number of live births in each state by race as the exposure variable.

The two-way fixed effect estimation of staggered treatments used in the is study is an extrapolation of the canonical two-period difference-in-difference study for causal inference. Causal inference from these types of studies relies on an assumption of parallel trends prior to treatment. In our case, this means that the treated and untreated populations should have similar maternal mortality trends prior to treatment. The parallel trend assumption allows for an extrapolation of the untreated trend to form the counterfactual for the effect on the treated population. Several recent advancements in this area of econometrics have allowed for testing results' robustness to violations in the parallel trends assumption (Rambachan and Roth 2023; Blilinski and Hatfield 2018) and if violations of the parallel trends assumption are found using an instrumental variable approach to account for differences in the pre-trend (Freyaldenhoven et al. 2019). We use the method developed by Rambachan and Roth (2019), to assess the robustness of our parallel trend assumption. Their method involves allowing for deviations in the parallel trend assumption and testing to see the effect on treatment coefficients[4].

The two-way fixed effect estimator of staggered treatments employed in this study have been shown to be vulnerable to bias resulting from heterogeneous time effects (Goodman-Bacon 2019; Callaway and Sant'Anna 2020; Sun and Abraham, 2020). Recent econometric advances allow for dynamic estimation of two-way fixed effects models that can accommodate heterogeneous time effects (Goodman-Bacon 2019; Callaway and Sant'Anna 2020; Sun and Abraham, 2020). Currently these dynamic models are only available for linear estimators. In order to assess whether states that are treated early differ from later treated states, we replicate Goodman-Bacon's method of assessing heterogeneous treatment effects by treatment cohort. The cohorts are defined by the year of treatment and 2 x 2 treatment effects are estimated for each

[4] We use the Stata package, Honestdid (Bravo et al. 2022) to implement this robustness check.

cohort. Then we assess the degree to which these early treated states differ from later treated states[5].

Additional controls were included to account for nearness to states that decriminalized abortion. As Joyce (2013) points out, some women were willing to travel large distances to seek abortions in New York, but the largest number came from states that border New York. For this reason, we include treatment variables for the nearest neighbors to New York, Washington DC, and California since these states had no residence requirements. Neighbors for Hawaii and Alaska were not included since they do not border any other US states and Washington state was not included since their decriminalization statute had a residency requirement. We also include the amount of health spending, average income, and share of deliveries that took place in the hospital as additional controls on maternal mortality.

Data

The mortality and natality data were retrieved from the Centers for Disease Control (CDC) Vital Statistics.[6] The Vital Statistics database contains death statistics due to pregnancy related complications by state and year. It is important to note that the CDC data list separately maternal deaths resulting from an abortion procedure and maternal deaths resulting from pregnancy related complications. These data were compiled for every US State from 1965 to 1978. Data regarding the number of live births as well as the percentage of births taking place in a hospital by state and year for the same time frame were compiled from the same Vital Statistics records.

[5] The results of robustness checks of heterogenous time effects are still in progress.
[6] The data are available in pdf files that are not digitally rendered. The process of converting the data was conducted in two troughs. The first trough was entered manually. Two people each entered all the natality and mortality data to ensure against mistakes. The second trough used R program pdftools to read the data into analyzable files. All data have columns where individual subcategories sum to the total. These were used to check for data quality and ensure that the data were internally consistent.

The percentage of hospital births allows us to measure the role of access to health care on maternal mortality. All of the data from vital statistics were available by race (White and Nonwhite) which formed the basis for our disaggregated analysis. The income and health spending data were retrieved from the Bureau of Economic Analysis (BEA). The health spending data comes from the dataset of GDP by state and industry.

The sample years (1965-1978) were chosen to balance the trade-off between more observations and increased exposure to unobserved heterogeneity. We decided to begin the sample in 1965 because in that year contraception was made legal for married women throughout the US. If we included any years prior to 1965, we would have to account for differences with regard to the legality of contraception across states. The ending time period, 1978, was chosen in order to allow time for institutional change to occur after the 1973, Roe v. Wade decision. Additionally, Blank et al. demonstrate that abortions were not widely available until 1974 and further, broad changes to Medicaid restricted access to abortions for low-income women began in 1978. Table 2 lists the variables used in this analysis and their descriptions.

[Insert table 2 here]

Results

Maternal mortality, measured as number of pregnancy deaths per 1,000 live births, decreased from 1965 to 1978, though the decrease is more marked for Nonwhite women (figure 1). Between 1965 and 1978, the average number of pregnancy related deaths was 11.4 and the average number of live births was approximately 67,000 (table 3). Figure 1 shows maternal mortality, calculated as number of deaths per 1,000 live births, decreasing over this time frame with a higher starting point and steeper decline for Nonwhite women than White women.

[Insert figure 1 here]

[Insert table 3 here]

The average percent of live births in hospital was 98.7; however, hospital deliveries ranged from 55 percent to 100 percent. The share of deliveries in hospital increased dramatically for Nonwhite women over this time frame (Figure 2) with less than 90 percent of Nonwhite women delivering in hospital in 1965 converging by 1978 to the share of White women delivering in hospital (98 percent).

[Insert figure 2 here]

We present the results from abortion reforms first followed by the results of abortion decriminalization. Table 4 shows the results of the estimated effect abortion reforms on maternal mortality. The average pre-trend is positive and significant for all regression (table 4). The average post trend effect of adoption on maternal mortality is small and statistically insignificant. In figures 3 and 4, the coefficient on time periods after treatment (lags) show little impact in the first four years after adoption for Nonwhite and White women respectively. In both cases, there appears to be a negative impact in the fifth year of adoption. The reason for this apparent decrease may be due to the fact that we only observe 5 periods post adoption for the earliest adopters. This decrease may be due more to fewer states observed in the time period rather than any delayed effect of the policy. Among the control variables, only share of deliveries in hospital were found to be statistically significant during this time frame, and this result only held for White women and was not statistically significant for Nonwhite women. The reason for this may be related to the time frame of the analysis. Because we are considering reforms separately, the time frame for this analysis is limited by Roe v. Wade, since this 1973 court decision ushered in a different treatment for all states.

[Insert table 4 here]

[Insert figures 3 and 4]

The effect of abortion decriminalization is considered separately and is displayed in table 5. We present three models each for Nonwhite women and White women. The first model includes treatment lag and leads and control variables for health spending, income, and hospital delivery. The second model includes binary variables for neighbor states to those that decriminalized. The third model includes region by year fixed effects as a robustness check to ensure that any unobserved regional trends are not driving the results.

[Insert table 5 here]

The average pre-trend for each regression was insignificant, offering evidence in support of the parallel trends assumption. The average post-treatment trend was negative and significant for Nonwhite women (all models) and White women (first model). For Nonwhite women, the results suggest an 18 percent decrease in risk of pregnancy death relative to the control group in the first year of decriminalization. The magnitude of this effect is lower in the third model which includes region-by-year fixed effects, suggesting that some of this treatment effect is correlated with this regional variation.

The control variables suggest that increased hospital delivery decreased the risk of pregnancy related death for Nonwhite and White women. For Nonwhite women, a percentage increase in the share of deliveries made in the hospital decrease the risk of pregnancy related death by 1 percent. For White women, the effect was higher where a percentage increase in the share of deliveries made in the hospital decreased the risk of pregnancy related death by 8.8 percent. The coefficients on the variables indicating states that neighbored states that decriminalized pre-Roe

v. Wade were largely not significant.

The event study graphs presented for Nonwhite women (figure 5) and White women (figure 6) show markedly different pre-trends. For Nonwhite women, the pre-trend shows little difference between maternal mortality for Nonwhite women in adopting states compared with Nonwhite women in non-adopting states, providing visual evidence in support of the parallel trends assumption. The post-adoption effect of decriminalization on maternal mortality for Nonwhite women shows a decrease beginning in the first year after adoption. For White women, the pre-trend shows higher maternal mortality for White women in adopting states prior to decriminalization, which is clear violation of the parallel trend assumption. Beginning 3 years prior to treatment, maternal mortality in adopting states is lower than non-adopting states and remains lower throughout and into the post treatment time period. This may suggest anticipatory access to abortions for White women in adopting states or there may be unobserved factors that are accounting for this overall downward trend.

[Insert figures 5 and 6]

Robustness

In order to assess the robustness of results to potential violations of the parallel trends assumption, we use the method developed by Rambachan and Roth (2019). This method calls for assessing the robustness of the estimated treatment effects under conditions where the pre-trends differ between the treated and untreated groups. Using Bravo et al.'s Honest DID Stata program, we estimated the effect of an increasing deviation in the parallel trends assumption.

Figure 7 shows that the estimated treatment effect of decriminalization on maternal mortality for Nonwhite women is robust to violations of the parallel trends assumption, up to 9 percent deviation. While the event study results suggest that the pre-trend was similar between treated and untreated groups, we find that these additional checks add confidence that abortion decriminalization decreased maternal mortality for Nonwhite women.

[Insert figure 7 here]

Conclusions

The analysis presented in this article considers the possible effects of increased access to abortion on maternal mortality during the 1960s and 1970s. There were two main changes to legal access to abortion. The first was expanded abortion access in cases where the life or health of the mother was at risk. The legal framework for this expanded access was developed as part of the American Law Institute's 1962 Model Penal Code. Our analysis of the effect State adoption of the Model Penal Code did not find any evidence of an effect on maternal mortality regardless of race.

The second policy change that we examined was a full decriminalization, whether by court ruling or State legislation. The preliminary results presented in this article provides evidence that abortion decriminalization that occurred in the 1960s and 1970s decreased maternal mortality, particularly for Nonwhite women.

This analysis is limited by the data available to us at this point in time. We were able to include the share of deliveries that took place in the hospital to control for expanding access to medical services. We also include average state income and amount of GDP allocated to health spending as additional controls. These aggregations are imperfect indicators of level of income and health infrastructure available in each state. The effect of abortion decriminalization on maternal mortality for Nonwhite women is robust to the inclusion of region by year fixed effects, which controls for unobserved regional shocks each year. These results are also robust to modest violations of the parallel trends assumption.

Future research could examine the extent to which there may be heterogenous time effects, which can bias the results from these staggered treatment models. One can explore the degree to which declines in maternal mortality for White women that preceded abortion decriminalization may have been the result of anticipatory effects.

References

E. O. Ananat and D. M. Hungerman. The power of the pill for the next generation: oral contraception's effects on fertility, abortion, and maternal and child characteristics. Review of Economics and Statistics, 94(1):37{51, 2012.

J. D. Angrist and W. N. Evans. Schooling and labor market consequences of the 1970 state abortion reforms. In Research in labor economics, pages 75{113. Emerald Group Publishing Limited, 2000.

M. J. Bailey. Momma's got the pill: How Anthony Comstock and Griswold v. Connecticut shaped US childbearing. American Economic Review, 100(1):98{129, 2010.

R. M. Blank, C. C. George, and R. A. London. State abortion rates the impact of poli cies, providers, politics, demographics, and economic environment. Journal of Health Economics, 15(5):513, 1996. ISSN 0167-6296.

Bilinski, Alyssa, and Laura A. Hatfield. "Nothing to see here? Non-inferiority approaches to parallel trends and other model assumptions." arXiv preprint arXiv:1805.03273 (2018).

Callaway, B., & Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, *225*(2), 200-230.

Bravo, M. C. Rambachan, A., & Roth, J (2022). HONESTDID: Stata module implementing the HonestDiD R package.

G. Burns. The moral veto: Framing contraception, abortion, and cultural pluralism in the United States. Cambridge University Press, 2005.

K. K. Charles and M. Stephens. Abortion legalization and adolescent substance use. The Journal of Law and Economics, 49(2):481{505, 2006.

Correia, S., Guimarães, P., & Zylkin, T. (2020). Fast Poisson estimation with high-dimensional fixed effects. *The Stata Journal*, *20*(1), 95-115.

J. J. Donohue III and S. D. Levitt. The impact of legalized abortion on crime. The Quarterly Journal of Economics, 116(2):379{420, 2001.

D. M. Drukker. Testing for serial correlation in linear panel-data models. The Stata journal, 3(2):168{177, 2003.

Freyaldenhoven, S., Hansen, C., & Shapiro, J. M. (2019). Pre-event trends in the panel event-study design. *American Economic Review*, *109*(9), 3307-38.

C. L. Foote and C. F. Goetz. The impact of legalized abortion on crime: Comment. The Quarterly Journal of Economics, 123(1):407{423, 2008.

B. J. George. Current abortion laws: Proposals and movements for reform. W. Res. L. Rev., 17:371, 1965.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, *225*(2), 254-277.

T. Joyce and M. Grossman. The dynamic relationship between low birthweight and induced abortion in new york city: An aggregate time-series analysis. Journal of Health Economics, 9(3):273 { 288, 1990. ISSN 0167-6296. doi: https://doi.org/10.1016/0167-6296(90)90047-7

T. Joyce, R. Tan, and Y. Zhang. Abortion before & after roe. Journal of health economics, 32(5):804{815, 2013.

P. B. Levine, D. Staiger, T. J. Kane, and D. J. Zimmerman. Roe v Wade and american fertility. American Journal of Public Health, 89(2):199{203, 1999.

B. Marianne, D. Esther, and M. Sendhil. How much should we trust dierences-in-dierences estimates?. The Quarterly Journal of Economics, 119(1):249, 2004. ISSN 00335533.

M. F. J. McKernan. Recent abortion litigation. Catholic Lawyer, 17:1{10, 1971. URL https://heinonline.org/HOL/P?h=hein.journals/cathl17&i=11.

T. G. Moyers. Abortion laws: A study in social change. San Diego Law Review, 7:237{243, 1970. URL https://heinonline.org/HOL/P?h=hein.journals/sanlr7&i=249.

B. Pendleton. The California therapeutic abortion act: An analysis. Hastings LJ, 19:242, 1967.

Rambachan, A., & Roth, J. (2019). An honest approach to parallel trends. Unpublished manuscript, Harvard University.

A. A. Sorensen. Black Americans and the medical profession, 1930-1970. The Journal of Negro Education, 41(4):337{342, 1972. ISSN 00222984, 21676437. URL http://www.jstor.org/stable/2966980.

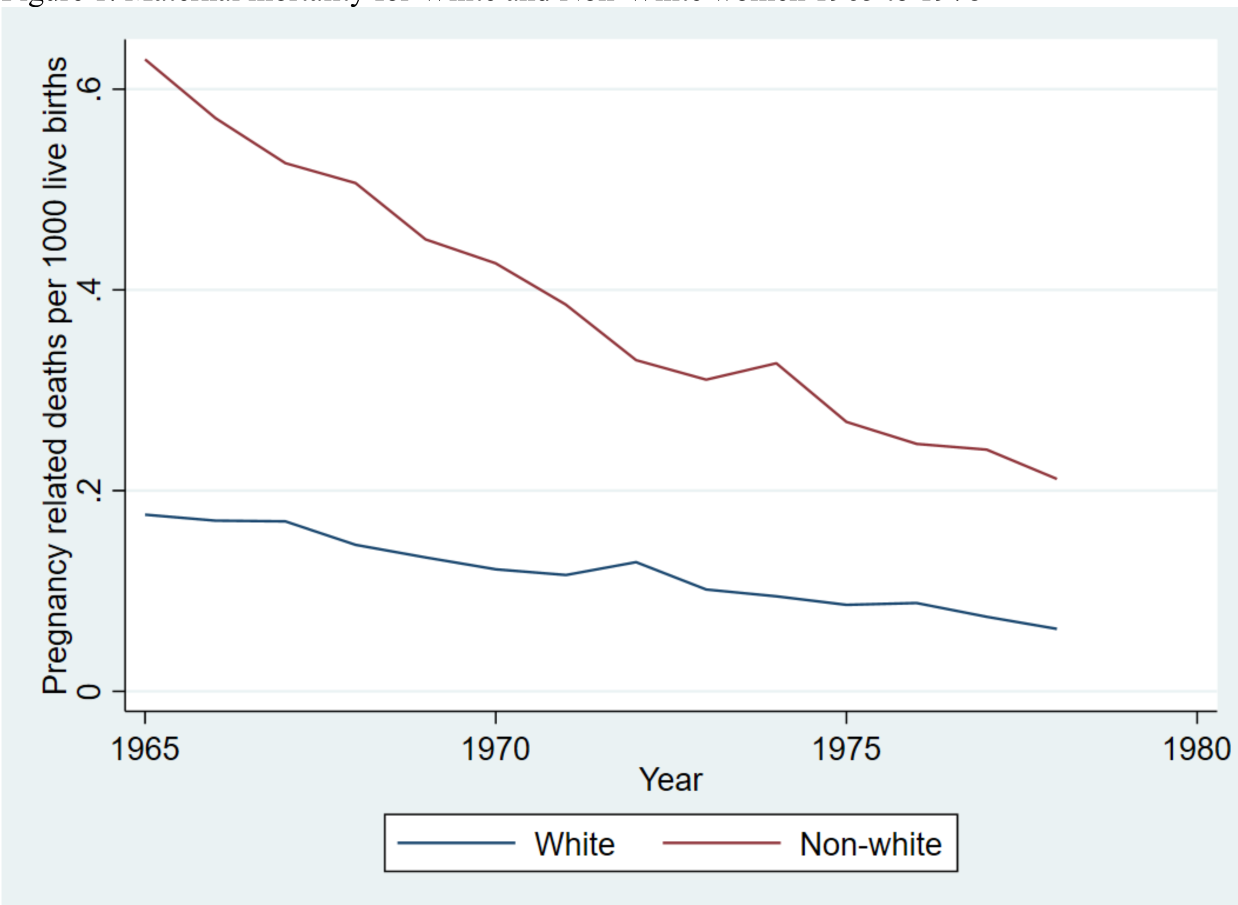
Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175-199.

United states v. Vuitch, 402 u.s. 62, 91 s. ct. 1294, 28 l. ed. 2d 601, 1971 u.s. LEXIS 50 (Supreme Court of the United States April 21, 1971, decided).

J. M. Wooldridge. Econometric analysis of cross section and panel data. MIT press, 2010.

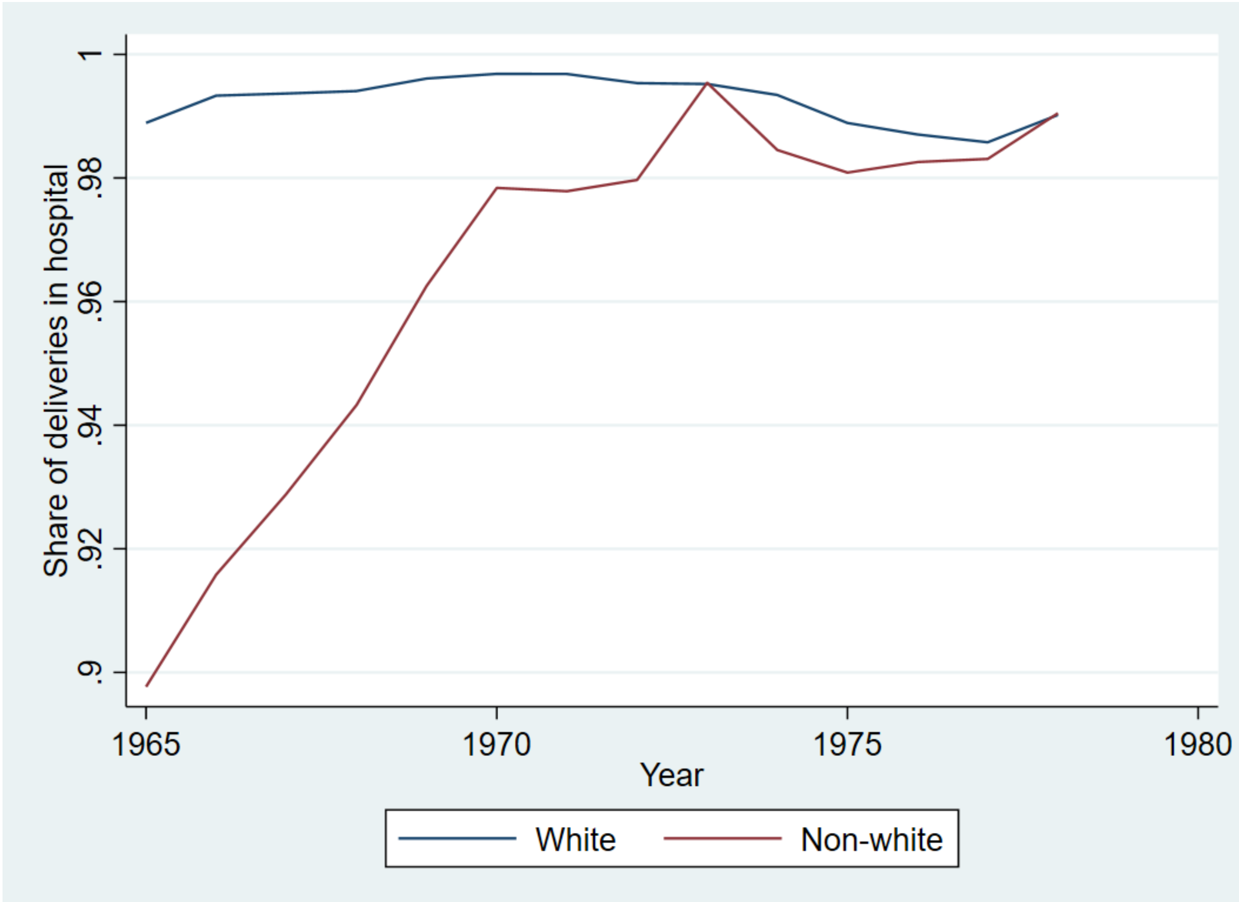
Figures

Figure 1. Maternal mortality for White and Non-White women 1965 to 1978



Source: Author's calculations from Center for Disease Control Vital statistics natality and mortality data.

Figure 2. Share of deliveries in hospital for White and Nonwhite women (1965 – 1978)



Source: Author's calculations from Center for Disease Control Vital statistics natality mortality data.

Figure 3. Event study graph of abortion reforms on pregnancy death for Non-white women

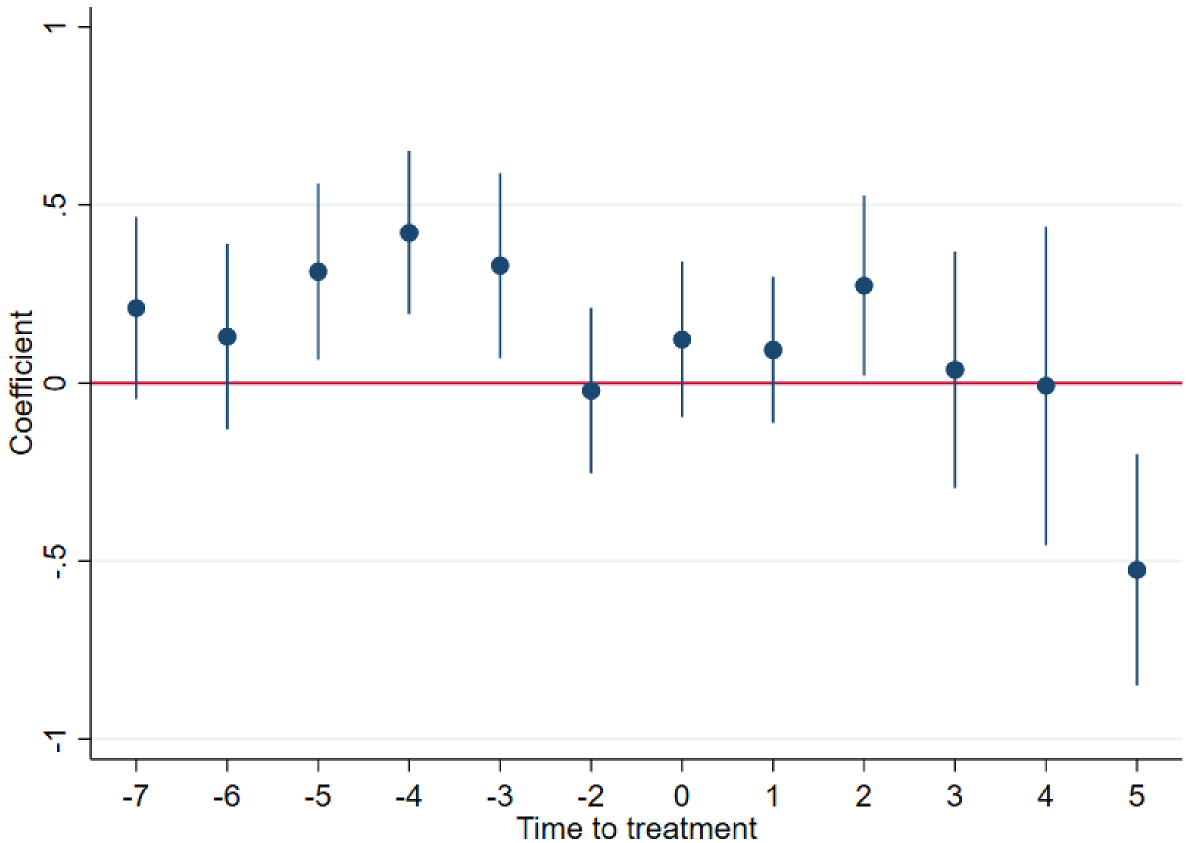


Figure 4. Event study graph of abortion reforms on pregnancy death for White women

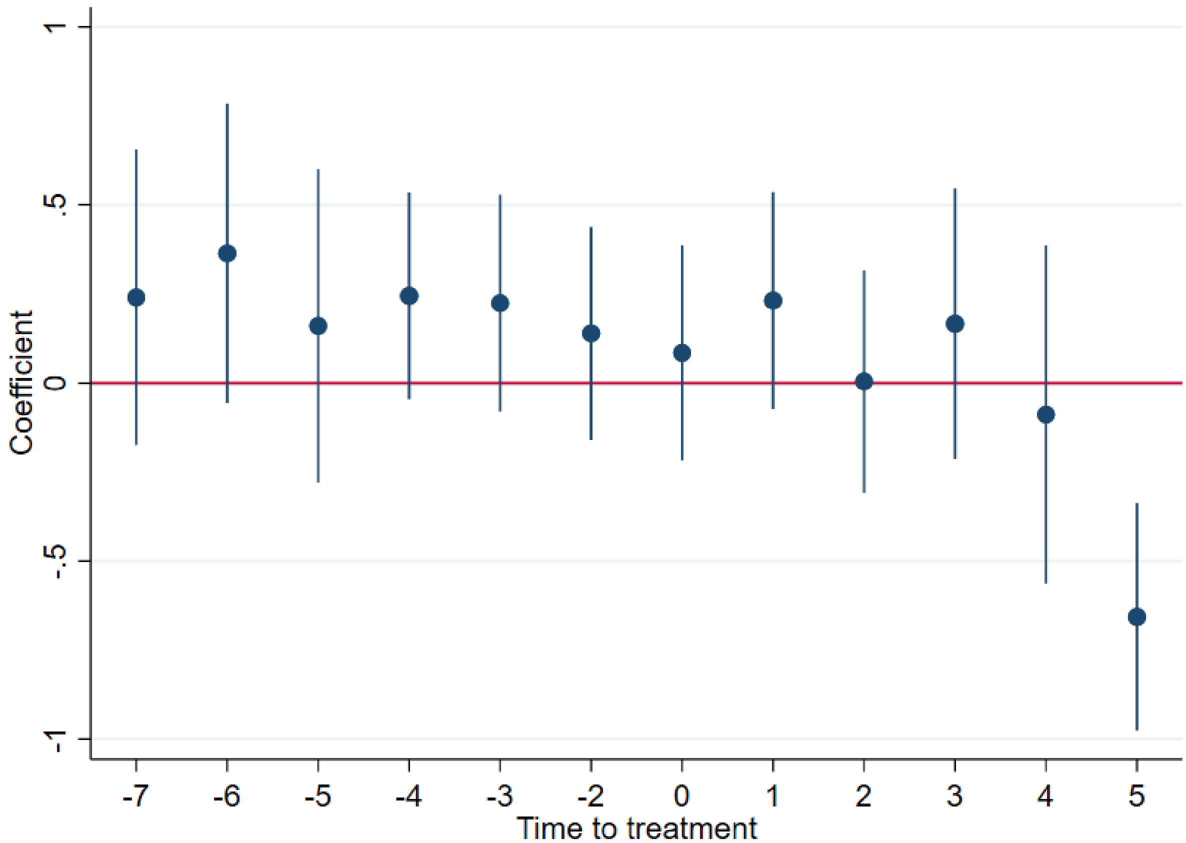


Figure 5. Event study graph of abortion decriminalization on pregnancy death for Non-white women

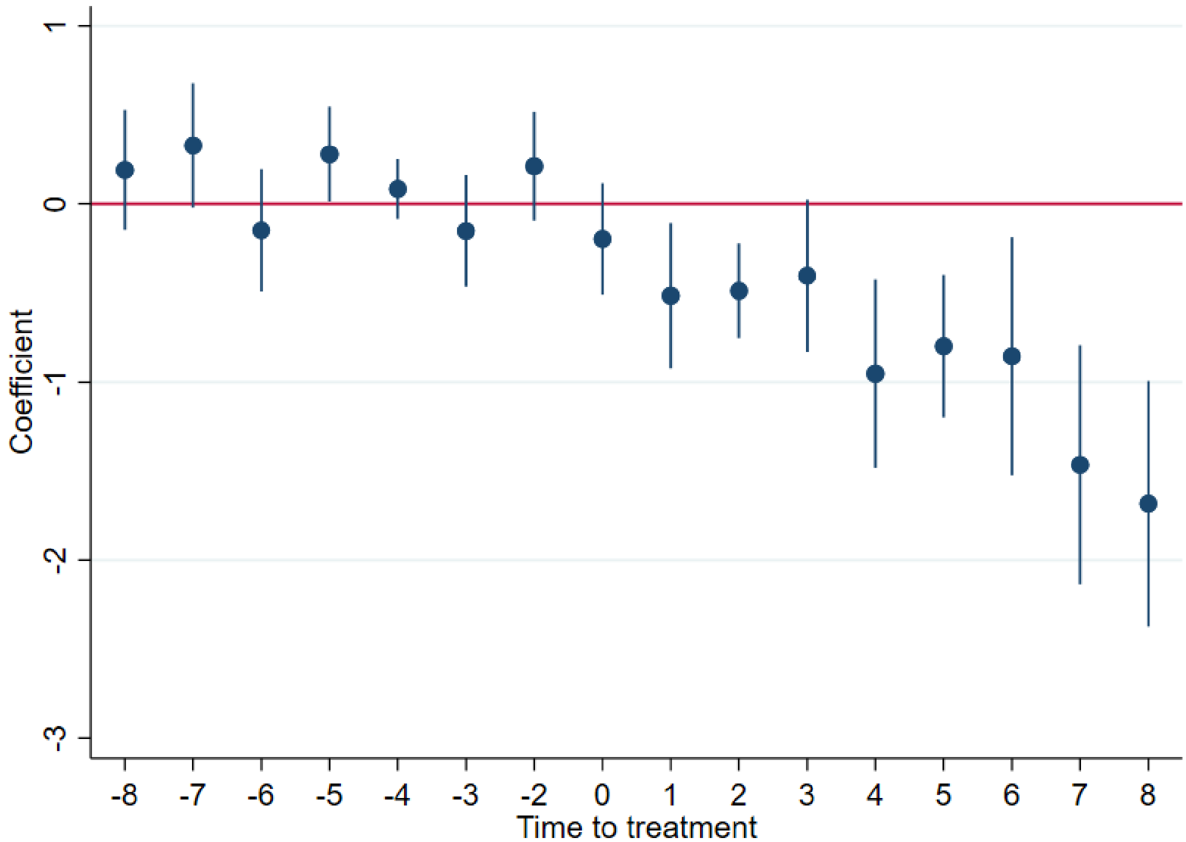


Figure 6. Event study graph of effect of abortion decriminalization on pregnancy deaths for White women

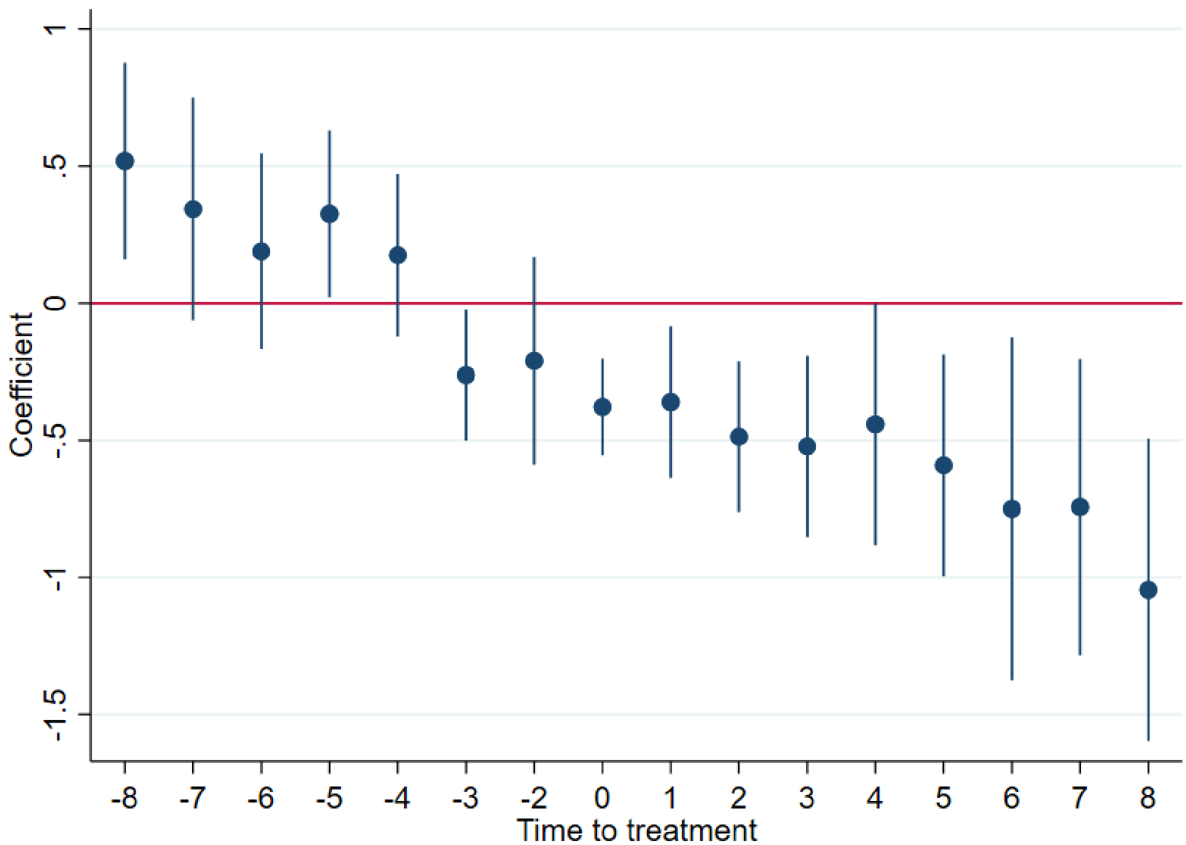
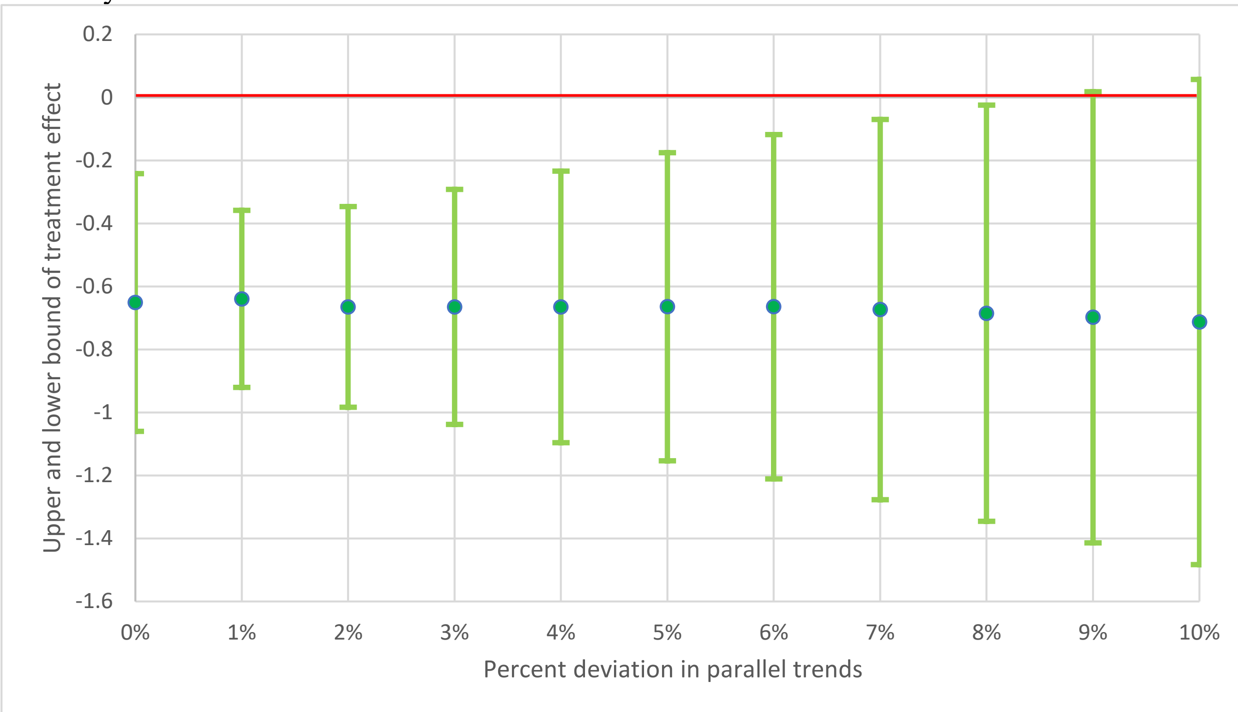


Figure 7. Robustness of the estimated treatment effect of abortion decriminalization on maternal mortality for non-white women



Tables

Table 1. Year of change in legal access to abortion by state

	Year MPC reform law adopted	Year of decriminalization
Alaska		1970
Arkansas	1969	
California	1967	1969
Colorado	1967	
Delaware	1969	
District of Columbia		1971
Florida	1972	
Georgia	1969	
Hawaii		1970
Kansas	1970	
Maryland	1968	
New Mexico	1969	
New York		1970
North Carolina	1967	
North Dakota		1974
Oregon	1969	
South Carolina	1970	
Virginia	1970	
Washington		1970
All other states		1973

Source: The coding of legal changes by state relies on the extensive work of Myers 2022.

Table 2. Variable definitions

Variables	Description
Reform	States that adopted MPC (= 1 post adoption)
Decriminalization	States that decriminalized (= 1 post decriminalization)
Hospital delivery (percent)	Share of births that took place in hospital
Health spending ($1,000)	Total state spending on the health sector
Income ($1,000)	Average household income
States neighboring New York	States that border New York (= 1 post decriminalization)
States neighboring District of Columbia	States that border District of Columbia (= 1 post decriminalization)
States neighboring California	States that border California (= 1 post decriminalization)

Table 3. Summary statistics

Variable	Obs	Mean	Std. dev.	Min	Max
Pregnancy Death	700	11.4	13.8	0	97
Number of livebirths	700	66,668	70,535.7	5,617	362,756
Hospital delivery (percent)	700	98.7	2.8	55.8	100
Income ($1,000)	700	4.5	1.5	1.7	12.8
Health spending ($ millions)	700	0.8	1.2	0.01	10.9
States neighboring New York	700	0.04	0.2	0	1
States neighboring District of Columbia	700	0.02	0.1	0	1
States neighboring California	700	0.03	0.2	0	1

Table 4. Estimated effect of Abortion Reforms on pregnancy related deaths using a Poisson pseudo maximum likelihood estimator.

VARIABLES	(1) All Pregnancy related deaths	(2) Non-white Pregnancy related deaths	(3) White Pregnancy related deaths
Share of deliveries in hospital (percent)	-0.00836*	0.00330	-0.0711*
	(0.00454)	(0.00678)	(0.0397)
Health spending ($1,000)	0.00319	0.0254	-0.00595
	(0.128)	(0.223)	(0.163)
Income ($1,000)	0.0103	0.00548	0.0250
	(0.0266)	(0.0360)	(0.0376)
Constant	-7.485***	-7.694***	-1.698
	(0.449)	(0.636)	(3.909)
Observations	352	312	352
Average pre-treatment trend	0.245***	0.231**	0.229*
	0.0698	0.116	0.121
Average post-treatment trend	-0.0293	-0.000862	-0.0427
	0.0781	0.0997	0.124

Notes: Robust standard errors in parentheses. All specifications include state and year fixed effects. Levels of significance indicated as *** p<0.01, ** p<0.05, * p<0.1

Table 5. Estimated effect of Abortion Reforms on pregnancy related deaths using Poisson pseudo maximum likelihood estimator

	Non-White women			White women		
	(1)	(2)	(3)	(1)	(2)	(3)
VARIABLES	Pregnancy deaths	Pregnancy deaths	Pregnancy deaths	Pregnancy deaths	Pregnancy deaths	Pregnancy deaths
Income	0.0446*	0.0451*	0.0553**	0.0235	0.0230	0.0464*
	(0.0258)	(0.0259)	(0.0247)	(0.0246)	(0.0218)	(0.0247)
Health spending ($ millions)	0.0318	0.0396	-0.0217	-0.0155	-0.0167	-0.0170
	(0.0477)	(0.0478)	(0.0578)	(0.0398)	(0.0386)	(0.0461)
Share of deliveries in hospital	-0.0102**	-0.00957**	-0.0167***	-0.0913**	-0.0934**	-0.0736
	(0.00437)	(0.00454)	(0.00456)	(0.0454)	(0.0434)	(0.0529)
States neighboring New York post treatment		0.000385	0.150		-0.0292	-0.271
		(0.128)	(0.265)		(0.0896)	(0.194)
States neighboring California post treatment		0.380	1.438***		-0.176	-0.213
		(0.368)	(0.408)		(0.254)	(0.266)
Constant	-6.741***	-6.821***	-6.302***	-0.142	0.0800	-1.459
	(0.447)	(0.473)	(0.691)	(4.465)	(4.309)	(5.257)
Observations	448	448	448	504	504	504
Average pre-treatment trend	0.114	0.118	0.204	0.154	0.149	-0.231
	(0.108)	(0.109)	(0.250)	(0.159)	(0.257)	(0.289)
Average pre-treatment trend	-0.817***	-0.833***	-0.495**	-0.591***	-0.591	-0.750
	(0.192)	(0.193)	(0.206)	0.161	0.556	0.540
State and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region by year fixed effects	No	No	Yes	No	No	Yes

Notes: Robust standard errors in parentheses. All specifications include state and year by region fixed effects. Levels of significance indicated as *** p<0.01, ** p<0.05, * p<0.1