ESSAYS IN THE ECONOMICS OF REPRODUCTIVE HEALTH

By

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ABSTRACT

This dissertation is composed of three chapters detailing the health and fertility effects of restricted access to abortion in the United States.

Chapter 1: The Maternal and Infant Health Consequences of Restricted Access to Abortion in the United States

Since the recent US Supreme Court decision in *Dobbs v. Jackson Women's Health Organization*, people across the country have experienced large sudden changes in their access to abortion care. In this paper, I look to the history of abortion access in the United States to inform predictions for this new future. I study the effects of targeted regulations on abortion providers (TRAP laws) on a variety of maternal and infant health outcomes, using variation in the timing of policy adoption across states and a direct measure of the distance to an abortion provider. I implement difference-in-differences techniques across outcomes from restricted-use microdata on the universe of US births and national survey data from the Behavioral Risk Factor Surveillance System. I find that TRAP laws lead to 11-16% increased rates of hypertensive disorders of pregnancy, and I provide suggestive evidence that these health effects may not be isolated to the period of pregnancy and birth. Additionally, I find evidence that TRAP laws widen existing disparities in adverse infant health outcomes across parental race and education. These results demonstrate the potentially wide-ranging health effects of restricting access to abortion.

Chapter 2: Notification vs Consent: The Differential Effects of Parental Involvement Laws on Teen Abortion

US state legislation requiring parental involvement in the abortion decision of a minor has grown in prevalence since its origin in the 1970s. Today, 36 states impose a parental involvement requirement on their residents below the age of 18. These laws come in two primary categories: parental notification and parental consent. Though much research estimates the effects of these policies, limited evidence exists regarding any differential impact between parental notification and parental consent. This paper uses the synthetic control method to determine if the increased marginal cost of an abortion imposed by a parental consent statute affects the abortion rate and birth

rate for minors relative to parental notification. Results indicate no evidence of a marginal effect of parental consent laws on the abortion/birth rate of minors overall, suggesting that the additional cost of a parental consent law may be small.

Chapter 3: The Effects of Restricted Abortion Access on IUDs, Contraceptive Implants, and Vasectomies: Evidence from Texas (with Cara Haughey and Brad Crowe)

Abortion and contraception are often considered to be substitutes, such that an increase in the cost of abortion will increase the demand for contraception. Although the effects of restricted abortion access are wide-reaching and often studied, we know little about the influence of abortion access on the take-up of contraception. In this paper, we exploit the timing of passage of House Bill 2 (HB2) in Texas, a regulation on abortion providers that shut down over half of all abortion clinics in the state. Using administrative outpatient records from Texas, we identify the effects of HB2 on the timing and take-up of intrauterine devices (IUDs), contraceptive implants, and vasectomies using difference-in-differences methods. We find suggestive evidence that expectations of limited abortion access significantly increase the take-up of IUDs, with no substantial evidence of an effect for the incidence of implants or vasectomies. These early findings support the hypothesis that abortion and contraception are substitutes, but the lack of evidence to indicate an effect of HB2 on the incidence of vasectomies suggests that partners may not internalize the cost of abortion in their contraceptive choices.

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CHAPTER 1

THE MATERNAL AND INFANT HEALTH CONSEQUENCES OF RESTRICTED ACCESS TO ABORTION IN THE UNITED STATES

1.1 Introduction

On June 24, 2022 the abortion landscape in the United States changed dramatically. The Supreme Court of the United States issued their ruling on *Dobbs v. Jackson Women's Health Organization*, holding that the Constitution does not confer a right to abortion and reversing the existing precedents set by *Roe* and *Casey*. Thirteen[1] states now restrict abortion in all or almost-all circumstances. Georgia restricts abortion after six weeks gestation, effectively prohibiting nearly all abortions. Indiana, Iowa, North Dakota, Montana, Ohio, South Carolina, Utah, and Wyoming currently have abortion bans that are temporarily blocked by state courts (Times, 2022) As a result of these recent policy changes, people all over the country with the capacity to become pregnant experience large and sudden increases to their travel distance to an abortion provider.

In this paper, I look to the history of restrictive abortion legislation in the United States to inform predictions for the new world post-*Dobbs*. I estimate the effects of state-level targeted regulations on abortion providers (TRAP laws) on maternal and infant health outcomes using restricted-use Vital Statistics Natality data. The adoption of TRAP laws serves as a relevant natural experiment for understanding the effect of *Dobbs* because these supply-side regulations often burden clinics to the point of closure and substantially increase the travel distance to a provider. In this way, they can be considered a microcosm of the current abortion environment.

In a restrictive abortion environment, people with the capacity to become pregnant may change their contraceptive and sexual behavior to avoid pregnancy, and this incentive may be particularly strong if they expect the pregnancy to be at a high risk for complications. At the same time, conditional on pregnancy, the additional cost of an abortion may prevent people who are pregnant from accessing the procedure, resulting in a greater number of pregnancies carried to term. Those who would otherwise seek an abortion but are prevented from accessing the procedure may have a

[1] At the time of writing, these states are: Alabama, Arkansas, Idaho, Kentucky, Louisiana, Mississippi, Missouri, Oklahoma, South Dakota, Tennessee, Texas, West Virginia, and Wisconsin.

higher risk of pregnancy/birth complications due to the selection into abortion. So, abortion access impacts health outcomes through a compositional change in the population of people carrying a pregnancy to term, and the theoretical predictions of their effects are ambiguous. Health outcomes may improve on average if a large number of high-risk pregnancies are avoided. However, if the cost of abortion results in more high-risk pregnancies carried to term, health outcomes will worsen on average. Then, the average effect of TRAP laws on maternal and infant health outcomes is largely an empirical question.

I exploit the timing of TRAP laws at the state level and use the Borusyak et al. (2021) difference-in-differences estimator to identify causal effects of restrictive abortion legislation on average rates of adverse health outcomes among birthing people[2] and infants that are robust to heterogeneity across treated units and time. I find that TRAP laws increase state-level rates of hypertensive disorders of pregnancy by 11-16%. These effects are stable across alternative TRAP policy codings from Austin and Harper (2019) and Jones and Pineda-Torres (2021), and robust to controlling for a variety of reproductive health policy indicators and including region-year fixed effects.

I complement this analysis relying on policy variation in abortion laws by directly measuring the effect of increasing travel distance to a provider. I use a panel of abortion provider distance at the county-month level compiled by Myers (2021b) and a fixed effects design including county fixed effects, time fixed effects, and a state time trend to measure the effect of increasing travel distance to a provider on county-level rates of adverse health outcomes. I find that increasing the distance to the nearest abortion provider by 100 miles increases county-level rates of pregnancy-associated hypertension and chronic hypertension by 8.7% and 16% respectively. And, this larger travel distance increases rates of diabetes and gestational diabetes by 10.3% and 8.6%.

Maternal and infant health effects are particularly relevant in the US context. Age-adjusted rates of hypertensive disorders of pregnancy nearly doubled in the US between 2007 and 2019, and significant disparities exist across racial/ethnic groups and region. These conditions are a leading cause of pregnancy-associated mortality, and a major contributor to the current maternal health

[2]Throughout the paper, “birthing people" refers to people with the capacity to become pregnant.

crisis in the United States (Cameron et al., 2022; Declercq and Zephyrin, 2020; MacDorman et al., 2021). Although rates of infant low birthweight and preterm birth are relatively stable over time, disparities between racial groups persist, with Black infants experiencing substantially higher rates of premature birth and low birthweight relative to white infants (Pollock et al., 2021; Gupta and Froeb, 2020).

I implement a triple-difference procedure to explore how these laws affect the disparities in adverse outcomes across demographic groups demonstrated to be more impacted by family planning access. I find that TRAP laws increase the gap in premature birth and low birthweight between Black and white infants by 3-6%, and these laws increase the gap in premature birth between infants born to people with a high school diploma or less and those born to college-goers by 19.5%.

This is the first study to describe the causal effects of any modern restrictive abortion policies in the United States on the health status of birthing people who carry to term and infants using administrative Vital Statistics Natality data. We know that restricted abortion access tends to decrease abortion rates and increase birth rates (Jones and Pineda-Torres, 2021; Myers, 2021a,b; Myers and Ladd, 2020; Lindo et al., 2019) but relatively little about how abortion access affects other outcomes. I contribute foremost to the literature surrounding effects of abortion access on outcomes for birthing people beyond abortion and birth rates. Most of this evidence is dedicated to socioeconomic outcomes (Jones and Pineda-Torres, 2021; Brooks and Zohar, 2022; González et al., 2020; Mølland, 2016; Bloom et al., 2009) and the limited evidence on health outcomes focuses almost exclusively on maternal mortality. Vilda et al. (2021) use a pooled cross-section of data on maternal mortality and state abortion policies to estimate that states with a greater number of abortion restrictions have higher rates of maternal mortality. However, their estimates do not have a direct causal interpretation. Hawkins et al. (2020) use standard difference-in-differences to assess the effect of a large panel of state-level policy decision on maternal mortality, finding that gestational limit laws increase the risk of maternal mortality by 38%, a surprisingly large estimate given the fact that these laws apply only to people seeking abortion after twenty weeks gestation when relatively few abortion occur. Hawkins et al. (2020) also study the passage of two TRAP laws, but find null effects. Notably, the timing of gestational limit laws may be correlated with other TRAP or demand-side abortion policies (mandatory waiting periods, parental involvement laws, etc.) in a way that is not accounted for in their research design. In a current working paper Farin et al. (2021) use difference-in-differences to estimate the effect of legalized abortion leading up to and at the time of *Roe v. Wade* on maternal mortality, finding a significant reduction in non-white mortality of 30-40%. The limited causal evidence on abortion and mortality outside of the US is consistent with this finding. Clarke and Mülrad (2021) estimate significant declines in maternal morbidity and abortion-related morbidity following abortion legalization in Mexico.

The closest existing work to this paper comes from The Turnaway Study, an analysis of being denied a wanted abortion by seeking it after the 20 week gestational limit. In this study of over 1,000 women, Ralph et al. (2019) find that women who are denied a wanted abortion are more likely to report chronic pain and lower overall health within five years relative to those who receive their abortion in the second trimester. The authors find no significant results in the five year rates of gestational diabetes, gestational hypertension, or non-gestational hypertension between these two groups in this small sample of individuals who seek an abortion around 20 weeks gestation. I make my primary contribution here, by estimating effects on maternal health beyond mortality using national data on the universe of US births. In addition, I analyze a natural experiment that is closely tied to the current state of abortion access, and I move beyond policy variation by directly measuring the effect of increasing provider distance.

Another closely connected literature studies the effects of abortion access on infants. A sizeable portion of this literature considers the effects of expanded abortion access around the time of *Roe* on infant mortality and infant health at birth, finding that abortion access is correlated with improvements in infant low birthweight and mortality (Gruber et al., 1999; Joyce and Grossman, 1990; Joyce, 1987; Corman and Grossman, 1985; Grossman and Jacobowitz, 1981). Two recent papers measure the association between modern abortion restrictions and adverse infant health outcomes. Redd et al. (2022) use a state-level abortion restrictiveness index and a multivariate logistic regression model to measure associations between restrictive environments and infant preterm birth and low birthweight. They find that national associations between abortion laws and these outcomes are not statistically significant, but there is some heterogeneity in effects across regions. Pabayo et al. (2020) also use a multivariate logistic model and a panel of state-level abortion laws including several demand-side policies and Medicaid funding restrictions, finding that infants born in states with more restrictions have higher odds of mortality. I provide the first causal evidence on the effects of modern abortion restrictions on infant health at birth in the United States.

The paper proceeds as follows: in Section 2, I describe the policy environment and categorize TRAP laws using two possible policy codings. In Section 3, I present a conceptual framework that describes the selection into abortion and potential pathways for treatment effects. In Section 4, I describe the data, estimation, and results for measuring the effects of TRAP laws on Vital Statistics Natality outcomes. In Section 5, I provide suggestive evidence regarding the potential for these effects to persist beyond the time surrounding pregnancy and birth. In Section 6, I summarize and conclude.

1.2 TRAP Laws

TRAP laws are a catch-all term to describe supply-side interventions in the market for abortion. These laws restrict where an abortion can be performed, under what conditions, and who can perform them. The treatment effects of TRAP laws come from the closure of clinics that cannot meet the requirements, either by shutting their doors or ceasing to provide abortion care.

Several recent papers study the effects of TRAP laws in a national or state-specific setting. In Texas and Pennsylvania, studies find that these laws increase the travel distance to a provider, reduce abortion rates, and increase birth rates (Lindo and Pineda-Torres, 2021; Kelly, 2020; Fischer et al., 2018; Quast et al., 2017). The only national evidence regarding the effects of TRAP laws comes from Jones and Pineda-Torres (2021). The authors use a difference-in-differences methodology, exploiting state-level policy variation in TRAP laws over time, to study the effects of being exposed to a TRAP law as a teenager on fertility and future socioeconomic outcomes. The find that birth rates increase for Black teens and that Black women exposed to TRAP laws as a teenager are less likely to attend and complete college.

Because TRAP laws are a broad category of legislation with variation in their nature and stringency, classifying a state as "treated" by a TRAP law is a complicated endeavor. To meet this challenge, I consider two possible TRAP law codings from the literature. I begin with the first published longitudinal database on TRAP laws published by Austin and Harper (2019). In this paper, the authors catalog supply-side regulations on abortion providers from 1973 to 2017, dividing them into three broad categories:

Ambulatory Surgical Center (ASC) Requirements

ASC laws require that abortion facilities in the state adhere to the regulations placed on ambulatory surgical centers. These often involve building codes and personnel guidelines. Some of these burdens include regulations on the width of doorways and hallways, access to medical equipment appropriate for an ASC that may not apply to abortion care, and staffing requirements. Meeting these requirements is often expensive, forcing providers to either purchase equipment and make renovations to the facility or shut down their abortion services.

Admitting Privileges

Some TRAP laws require a facility providing abortion services to have a clearly defined relationship with a nearby hospital. One type of these is an admitting privilege requirement. These laws specify that one or all physicians providing abortion care must have admitting privileges at a hospital that often must be within a certain radius of the abortion facility. This burden may be difficult for rural abortion clinics without a hospital in the proximity radius defined by the TRAP law. Admitting privilege requirements were declared unconstitutional by the Supreme Court in 2016 in *Whole Women's Health v. Hellerstedt*, but the laws were enforced for many years leading up to that decision. And, the *Whole Women's Health* decision was recently superseded by *Dobbs*, meaning these laws are back on the table for state legislatures.

Transfer Agreements

Transfer agreement laws are another example of legislation that requires an explicit clinic-hospital relationship. These laws specify that facilities providing abortion services must have a written agreement in place at a nearby hospital to transfer patients in the event of complications or an emergency. Transfer agreements are commonly a component of ASC requirements but can be part of separate legislation. Although transfer agreements are generally easier to acquire than admitting privileges, the burdens of the two laws are similar when there are proximity issues or public relations complications with the nearest hospital.

In addition, I use the TRAP law coding from Jones and Pineda-Torres (2021). This coding is similar to Austin and Harper (2019) with a few notable differences. First, Jones and Pineda-Torres define slightly different TRAP law categories: transfer agreements, admitting privileges, building regulations, and distance requirements. Essentially, this coding more closely identifies features of the TRAP law by considering building regulations separately from ASC requirements and distance to the nearest hospital regulations that are not a part of transfer agreements and admitting privilege requirements. Also, the authors implement a stringency requirement for TRAP treatment. In some cases, TRAP laws that may fall into one of these four categories are not considered strong enough to classify a state as "treated." A primary example is laws that apply only to providers of second trimester abortions. Since only ten percent of abortions take place in the second trimester, these restrictions likely do not have large effects on abortion access. Table 1.1 summarizes the treatment timing for various TRAP laws by Austin and Harper (2019) and Jones and Pineda-Torres (2021).

1.3 Conceptual Framework

To describe behaviors and outcomes under restrictive abortion environments, I expand on the predictions from a model of abortion and selection by Ananat et al. (2009). In their model, the authors consider decisions around pregnancy, abortion, and birth in the context of increased access to abortion care and make theoretical predictions about the effects of abortion access on infant health outcomes. I extend their logic by considering the effect of restricted access to abortion on maternal health outcomes.

In this model, a person makes decisions about pregnancy and abortion sequentially. The decision to become pregnant depends on the expected benefits and costs of childbirth, and people choose to

Table 1.1 TRAP Law Treatment Timing

	Austin and Harper (2019)			Jones and Pineda-Torres (2021)			
State	ASC	Transfer	Admit	Building Reg	Distance Req	Transfer	Admit
AL				1997			
AK	Pre-1990	Pre-1990				Pre-1990	
AZ			2000	2000	2012		2000
AR				1999			
CT				Pre-1990			
FL		2016	2016				
GA	Pre-1990	Pre-1990	Pre-1990				
IL	Pre-1990	Pre-1990	Pre-1990				
IN	Pre-1990	Pre-1990	2011	2006		2006	
KY		1998				1998	
LA			2014	2015			2014
MD	2012			2012			
MI	1999	1999			2012	2012	
MS	2005	2013					
MO	2007	2007	Pre-1990	Pre-1990	2005		Pre-1990
NC				1994			
ND			2014		2013		2013
NE				2001		2001	2001
OH	1999	1999			2015	2006	
PA	2012	2012	2012	2012	Pre-1990	Pre-1990	
RI	Pre-1990			2002			
SC	1996	1996	1996	1996			1996
SD				2006		2016	
TN	2015	2015	2015	2015		2015	2012
TX	2004		2013	2009			2013
UT		1998	1998	2011	2011	2011	2011
VA	2012	2012		2013			
WI		Pre-1990			Pre-1990	Pre-1990	

Notes: A description of the timing for each state treated under the policy codings from Austin and Harper (2019) and Jones and Pineda-Torres (2021).

get pregnant[3] as long as the marginal benefit outweighs the marginal cost. Once pregnant, a person may receive new information regarding the benefits and costs to birth and can use this information in their decision to receive an abortion. The choice to receive an abortion depends again on the marginal benefits and marginal cost for the procedure. I assume (as in the original model) that children's outcomes are directly related to the benefits of giving birth, where births that result from wanted pregnancies have better outcomes than births from unwanted pregnancies. Based on the evidence that people seeking abortion report that a concern for their health is a component of their reasoning, I make an additional assumption not explicitly specified in Ananat et al. (2009) that the health of the pregnant person is directly linked to the payoff from giving birth (Foster et al., 2018).

Then, abortion access potentially affects maternal and infant health outcomes by entering the decision both to become pregnant and to receive an abortion conditional on pregnancy. In a restrictive abortion environment, fewer people become pregnant because the risk of receiving negative information following the pregnancy is more costly given the reduced access to abortion. By assumption, those on this margin expect with higher probability that the birth will involve some risk to their individual health status or the health of the infant. By preventing these at-risk births through the channel of reduced pregnancy, abortion restrictions will improve average maternal and infant outcomes of births, all else equal.

In addition, restricted access to abortion affects the abortion decision among people who become pregnant by increasing the marginal cost of the procedure. This additional cost increases the number of births, and I follow the logic of the original model and refer to these new births that result from restricted abortion access as "marginal births." Because of the assumed direct relationship between health expectations and the payoff of birth, the marginal births have lower-than-average outcomes. So, the inclusion of these marginal births will decrease the average maternal and infant outcomes of births, all else equal.

Consider the potential effect of restricted abortion access on pregnancy-associated hypertension

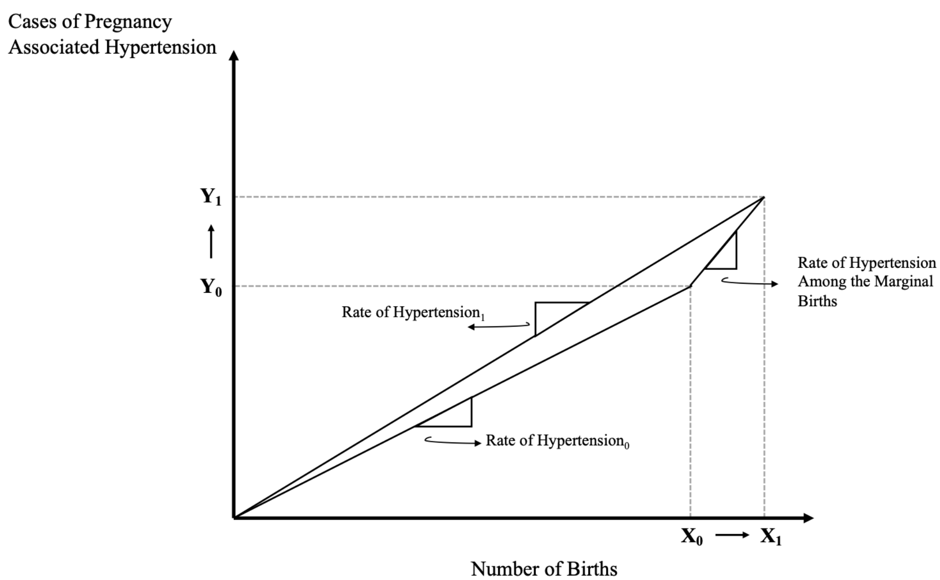
[3]Given evidence that nearly half of all pregnancies in the US are unintended, it may seem unusual to consider the first stage in this model to be the decision to become pregnant. It is worth noting that the logic and conclusions of the theoretical model are identical if the first stage is instead modeled as a decision around contraceptive and sexual behavior with various probabilities of pregnancy.

presented in Figure 1.1a. Let X_0 and Y_0 be the number of births and the number of cases of pregnancy-associated hypertension respectively, assuming no change in abortion access. Then, the counterfactual rate of hypertension (denoted Rate of Hypertension$_0$) is equivalent to $\frac{Y_0}{X_0}$. Suppose that an abortion restriction is passed, and further assume that people do not include the extra cost to abortion in their pregnancy decision. So, in this scenario, there are no pregnancies avoided due to the increased marginal cost of an abortion, and the presence of marginal births drives maternal health effects entirely. Following the restriction, the number of births increases to X_1 and the number of hypertension cases to Y_1. The new rate of hypertension under restricted abortion access is measured $\frac{Y_1}{X_1}$ and depends on the rate of hypertension among marginal births, $\frac{Y_1 - Y_0}{X_1 - X_0}$.

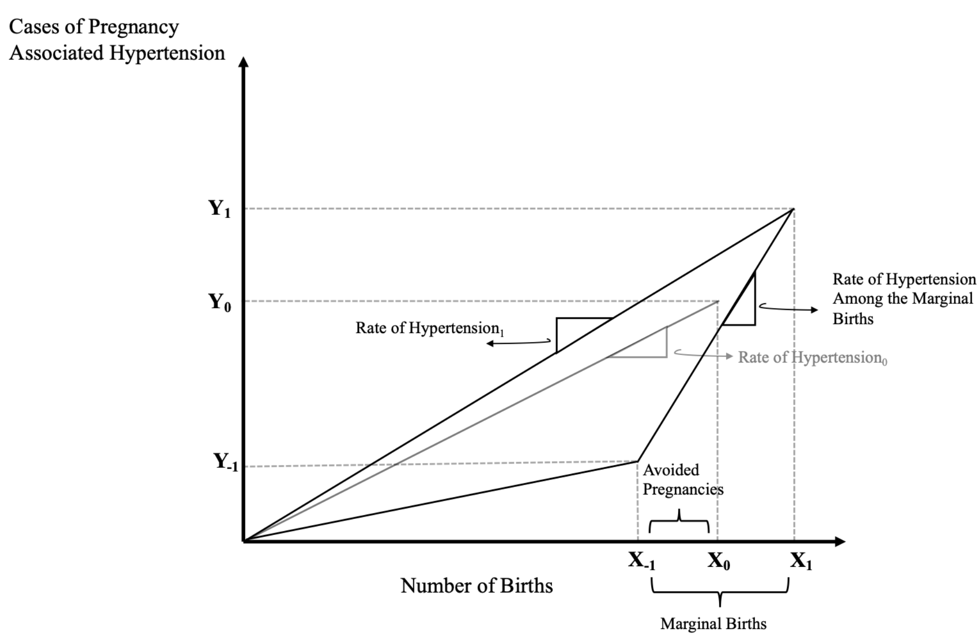
In Figure 1.1b, consider the same scenario but allow people to avoid pregnancy following a restrictive abortion law. Then, two competing effects on the number of births are included in the model. First, the number of births decreases from X_0 to X_{-1} and the cases of hypertension decreases from Y_0 to Y_{-1} as higher-risk individuals avoid pregnancy due to the high cost of abortion. Then, conditional on pregnancy, more people give birth as a result of the abortion restriction, increasing the number of births from X_{-1} to X_1 and the number of hypertension cases from Y_{-1} to Y_1. The rate of hypertension among the marginal births is $\frac{Y_1 - Y_{-1}}{X_1 - X_{-1}}$.

In this paper, I identify the changes in the average rates of adverse maternal and infant health outcomes following a restrictive abortion policy. So, coefficients that I estimate represent (Rate of Hypertension$_1$ - Rate of Hypertension$_0$). While I do not estimate the rates of adverse health outcomes among the marginal births, my estimates are informative of their direction and magnitude. Observing an increase in the rate of hypertension following a restrictive abortion law implies that the rate of hypertension among marginal births is higher than Rate of Hypertension$_0$. Outside of this observation, I do not make further comments on the rate of adverse health outcomes among marginal births. Using only birth records, this rate can not be calculated or bounded in any way that is informative. Note that the scenarios pictures in Figure 1.1a and Figure 1.1b involve the same average treatment effect of the policy on the rate of hypertension while having very different rates among the marginal births.

Figure 1.1 Visual Model of Treatment Effects



(a) A Model of Hypertension and Marginal Births



(b) A Model of Hypertension, Marginal Births, and Avoided Pregnancies

The most notable feature of this model of abortion and selection is that health effects from abortion access are not dependent on observing a change in the number of births. Because of the competing responses of pregnancy avoidance and the increased probability of birth conditional on pregnancy, health effects may be explained by the changing composition of people giving birth in states with restricted access to abortion with or without evidence that the number of births changes in response to an abortion policy.

1.4 TRAP Laws and Pregnancy/Birth Outcomes

1.4.1 Data

To identify the effect of these abortion policies on state-level rates of adverse health outcomes among people giving birth and infants, I use restricted All-County Natality files provided by the National Center for Health Statistics (NCHS, 2022). The files contain the universe of birth records in the United States from 1990 to 2017. Birth records include a rich set of demographic characteristics, indicators for the health status of the birthing person, indicators for adverse health outcomes associated with pregnancy, and various characteristics of the health of the infant at birth. Table 1.2 presents summary statistics for these data. Over the time period, the average birthing person is 27.41 years old. Half of all birthing people are white, and 80% have at least a high school diploma. Average gestational age for infants at birth is 38.95 weeks, and average birthweight is almost 3300 grams. Eight percent of infants born are low birthweight and twelve percent are born premature.

Beginning in 2003, US states adopted the revised standard birth certificate differentially over time. To address any potential confounding associated with this rollout adoption, I consider only health outcomes measures for birthing people and infants that are reported consistently across both the revised and unrevised certificate. The outcomes are: pregnancy-associated hypertension, chronic hypertension, diabetes[4], infant birthweight, gestational age at birth, and five-minute APGAR score. The selected maternal health outcomes are relatively rare: five percent of births involve

[4] The 2003 revised certificate makes a distinction between diabetes and gestational diabetes. Even though gestational diabetes info is only available in the revised certificate, I include that health outcome in my analysis for comparison.

Table 1.2 Summary Statistics - NCHS

Variable	Mean	S.D.	Number of Observations
Mother's Age (years)	27.41	6.09	112,863,754
Mother's Race			111,674,714
Non-Hispanic White	0.50		
Non-Hispanic Black	0.16		
Hispanic	0.28		
Other	0.05		
Mother's Education			81,749,166
0-8 years	0.06		
9-11 years	0.15		
12 years	0.32		
13-15 years	0.23		
16+ years	0.25		
Gestational Age (weeks)	38.95	4.07	112,148,648
Premature Birth (<37 weeks)	0.12	0.32	112,148,648
Birthweight (grams)	3297.66	618.70	112,803,275
Low Birthweight (<2500 grams)	0.08	0.27	112,803,275
Five Minute Apgar Score	8.87	0.80	97,742,540
Number of Prenatal Visits	11.14	2.07	109,214,623
Chronic Hypertension	0.01	0.10	111,676,723
Pregnancy-Associated Hypertension	0.04	0.20	111,676,723
Diabetes	0.04	0.20	111,167,704
Gestational Diabetes	0.05	0.22	41,005,843

gestational diabetes, four percent involve pregnancy-associated hypertension and diabetes, and only one percent involve chronic hypertension.

Pregnancy-associated and chronic hypertension are differentiated by the timing of diagnosis. Hypertension diagnosed prior to 20 weeks gestation is denoted chronic hypertension, while hypertension diagnosed after 20 weeks gestation is pregnancy-associated hypertension. An APGAR score is a quick summary measure of infant health after birth. Infant health is ranked in five categories (**A**ppearance **P**ulse **G**rimace **A**ctivity and **R**espiration) on a scale from 0 to 2. So, these scores range from 0 to 10, with higher scores generally indicating healthier infants.

Figure 1.2 provides a summary of the data over time by comparing trends in states that never receive treatment and states that pass at least one TRAP law over the study period. If TRAP laws are associated with higher rates of adverse health outcomes, then I expect to observe a widening gap between eventually-treated and never-treated states over time as more TRAP laws are passed. This trend is present in the rates of hypertensive disorders of pregnancy. The gap in the rate of chronic hypertension between treated and untreated states begins to widen in the early 2000s, and widens considerably for the rest of the study period — rates were nearly indistinguishable in 2000, but by 2017 treated states have a 33% higher rate of chronic hypertension. For pregnancy-associated hypertension, the gap between treated and untreated states widens in the mid-2000s but narrows toward the end of the period. Infant health outcomes premature birth and low birthweight have a significant gap throughout, but the gap widens by the end of the period. Treated states have a 10% higher rate of premature birth in 1990 and a 20% higher rate in 2017. A similar pattern exists for the rates of infant low birthweight. For maternal metabolic outcomes diabetes and gestational diabetes, the raw trends do not indicate a strong association with TRAP laws.

1.4.2 Estimation

To measure effects from abortion access on outcomes related to pregnancy and birth, I exploit the variation in state-level policies over time. So, I estimate the average treatment effect on the treated (ATT) using difference-in-differences methods.

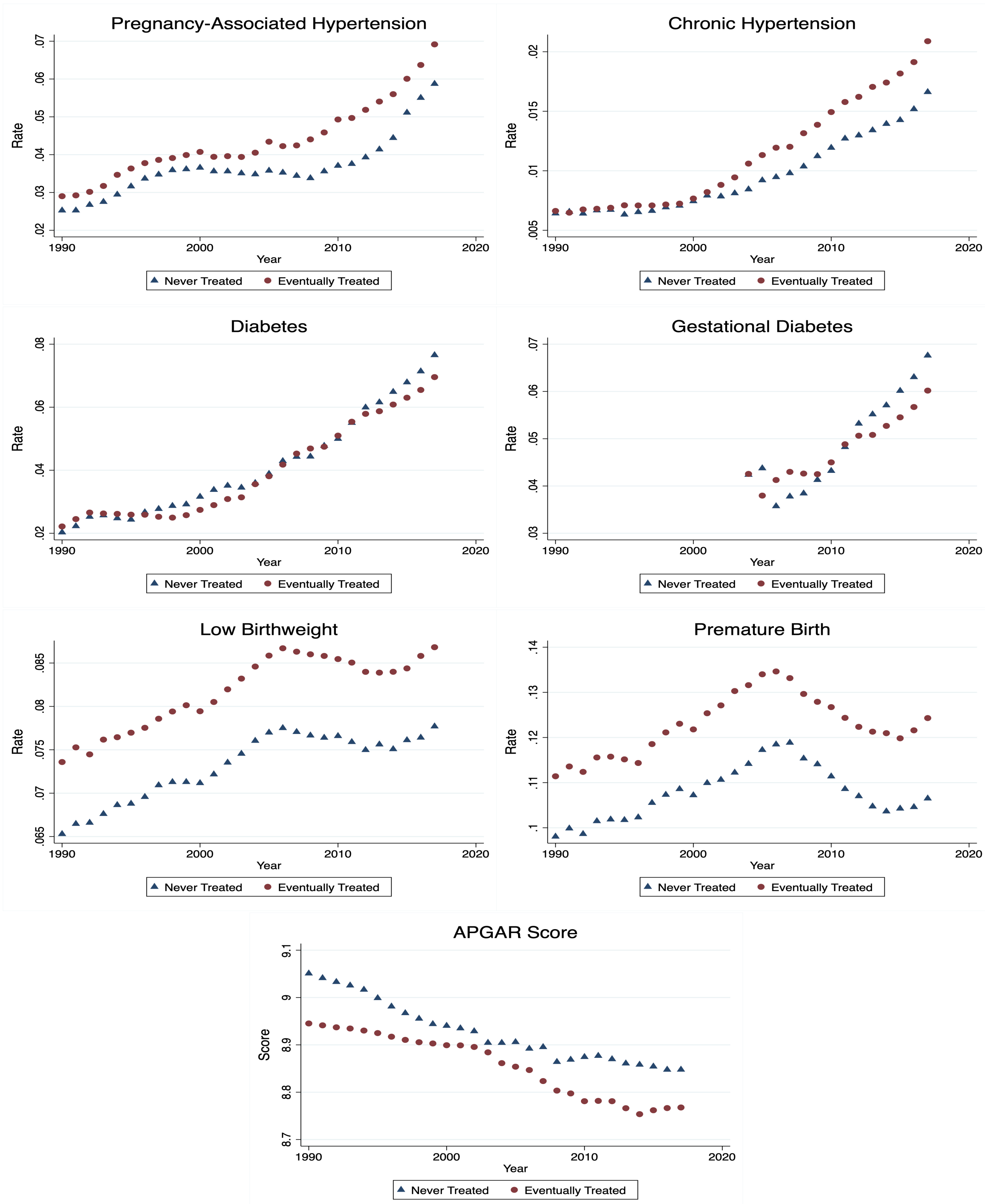
I begin with the standard two-way fixed effects (TWFE) specification for analysis:

$$Y_{ist} = \alpha_s + \delta_t + \beta p_{st} + \epsilon_{ist} \quad (1)$$

where Y_{ist} is the outcome of interest, α_s and δ_t are state and time fixed effects respectively, and p_{st} is a simple policy indicator taking value 1 if a state s has the policy being considered in year t and 0 otherwise. In an ideal setting, coefficient β identifies the ATT. TWFE under staggered intervention timing imposes a homogeneity assumption. This assumption requires that treatment effects are homogeneous across units/time, otherwise the estimate of the ATT is biased by the “forbidden comparison" between newly treated units and previously treated units (Goodman-Bacon, 2021). The heavily staggered nature of treatment in Table 1.1 demonstrates the importance of considering the bias introduced by a violated homogeneity assumption.

States likely experience heterogeneous responses to restrictive abortion legislation, and treatment effects are likely larger closer to the time of the policy change, when the “shock" occurs.

Figure 1.2 Abortion Restrictions and Birth Outcomes, 1990-2017



If this is the case, TWFE estimates for average treatment effects are attenuated. For this reason, the preferred specification is the Borusyak, Jaravel, and Spiess (2021) imputation estimator (BJS), which relaxes the homogeneity assumption.

The BJS estimation of the ATT is computed in a three-step process. In the first step, fixed effects are estimated according to equation (1) using only the set of untreated observations to impute potential outcomes $Y_{ist}(0) = \hat{\alpha}_s + \hat{\delta}_t$. I delay treatment timing by a year from the policy change, because these likely include the birth records of those who first responded to the TRAP law. Next, treatment effect τ_{ist} is defined to be the difference between observed and potential outcomes in a treated state s at time t. Finally, treatment effects are aggregated together according to weights w_{ist}. In my context, all treatment effects are weighted equally such that τ_w is the simple average.

$$\tau_{ist} = E[Y_{ist} - Y_{ist}(0)] \quad (2)$$

$$\tau_w = \sum_{ist} w_{ist}\tau_{ist} \quad (3)$$

Although the BJS estimator is robust to arbitrary heterogeneity across treated units and time, there are still a number of potential challenges to the identification of true treatment effects. The first is that while state fixed effects allow for static differences across states, there may be a concern that states in the treatment and control group differ in time varying ways that affect their trends in adverse birth outcomes and chronic conditions. To address this, I estimate and test for parallel pre-trends using the method outlined in Borusyak, Jaravel, and Spiess (2021). Here, a separate OLS regression similar to a traditional event study is performed using untreated observations only:

$$Y_{ist} = \alpha_s + \delta_t + \sum_{k=1}^{5} \gamma_k 1(timing_s - t = k) + \epsilon_{ist} \quad (4)$$

where $timing_s$ indicates the year that state s was treated by a policy change. Coefficients from this regression can be plotted alongside the previously estimated set of treatment effects in order to present a picture that can be interpreted in a similar manner to an event study. The parallel trends asumption is evaluated by estimating $\hat{\gamma}_k$ and testing $\gamma = 0$ using an F test.

Table 1.3 BJS Parallel Trends Assumption F Test

	F-stat	p-value	df
PA Hypertension	1.258	0.299	43
Chronic Hypertension	0.956	0.455	43
Diabetes	5.269	0.001	43
Gestational Diabetes	3.394	0.013	37
Low Birthweight	1.398	0.244	43
Premature Birth	1.678	0.160	43
APGAR Score	1.384	0.249	43

Notes: Results from testing $\gamma = 0$ from equation (4) by an F test.

Figure 1.3 and Table 1.3 demonstrate that for most pregnancy and birth outcomes, the parallel trends assumption for TRAP laws is satisfied. Exceptions are the metabolic outcomes, diabetes and gestational diabetes. These outcomes are only differentiated after the 2003 revision to the standard birth certificate, and the parallel trends violation could be a product of the staggered adoption of the revised certificate. I present results for these outcomes in the next section, but I consider the treatment effect estimates uninformative because of this parallel trend violation.

A second identification challenge is the passage of concurrent reproductive health policies in treatment and control states. I check to see if results are robust to the inclusion of controls for various reproductive health and family planning state-level policies compiled by Myers and Ladd (2020) and Myers (2021b). I augment equation (1) to include controls for the following indicators: access to over-the-counter emergency contraception, Medicaid expansions for pregnant people, an insurance mandate for private providers to cover prescription contraception, and a one-trip and two-trip mandatory waiting period for abortion services. Results, presented in the next section, indicate that effects are robust to the inclusion of these policies in the specification.

Because TRAP laws are heavily sorted into states in the South and Midwest, there may be a concern that effects are confounded by concurrent regional differences in maternal and infant health trends. To assuage this concern, I repeat the difference-in-differences analysis with the inclusion of region-year fixed effects. Results, presented in the appendix, suggest that estimates are robust to the inclusion of these regional effects.

Figure 1.3 BJS Event Studies - TRAP Laws (Vital Stats)

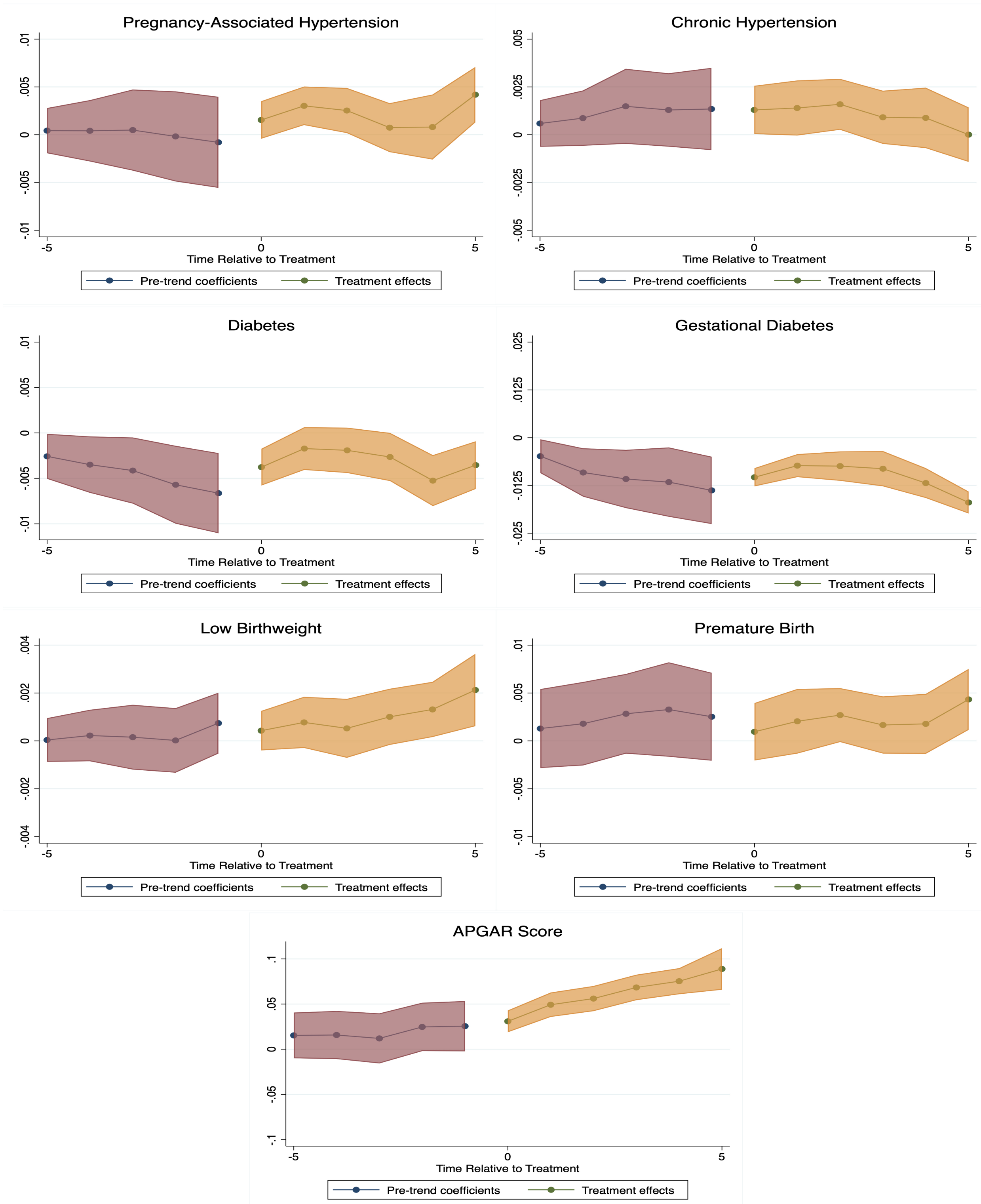


Table 1.4 Difference-in-Differences Results (Vital Statistics)

	TWFE	BJS		
	A&H (2019)	A&H (2019)	A&H (2019) w/policy controls	J&P (2021)
	(1)	(2)	(3)	(4)
PA Hypertension	0.0021	0.0046^{***}	0.0050^{***}	0.0033^{***}
(mean = 0.0403)	[0.002]	[0.001]	[0.001]	[0.001]
Chronic Hypertension	0.0010	0.0016^{**}	0.0010	0.0023^{***}
(mean = 0.0105)	[0.001]	[0.001]	[0.001]	[0.001]
Diabetes	-0.0032^{*}	-0.0018	-0.0007	-0.0008
(mean = 0.0403)	[0.002]	[0.002]	[0.002]	[0.001]
Gestational Diabetes	-0.0040^{*}	-0.0146^{***}	-0.0094^{***}	-0.0050^{***}
(mean = 0.0504)	[0.002]	[0.002]	[0.0003]	[0.001]
Low Birthweight	0.0013	0.0004	0.0006	0.0010^{*}
(mean = 0.0778)	[0.001]	[0.001]	[0.001]	[0.0005]
Premature Birth	0.0019	0.0014	0.0024^{*}	0.0043^{**}
(mean = 0.1159)	[0.002]	[0.002]	[0.001]	[0.002]
5-Minute APGAR Score	0.0085	0.0316^{**}	0.0649^{***}	0.0697^{**}
(mean = 8.87)	[0.016]	[0.013]	[0.017]	[0.009]

Notes: Results from TWFE and BJS difference-in-differences analysis. Column (2) uses the TRAP policy coding from Austin and Harper (2019), Column (3) uses the Austin and Harper (2019) coding along with a set of reproductive health policy controls, and Column (4) uses the alternative policy coding from Jones and Pineda-Torres (2021). In each specification, standard erros are clustered at the state level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

1.4.3 Results

Difference-in-Differences

Table 1.4 presents results from the difference-in-differences analysis with various specifications. Column 1 presents the TWFE results for comparison, and columns 2-4 present the BJS results for the Austin and Harper (2019) coding, the Jones and Pineda-Torres (2021) coding, and the inclusion of reproductive health policy controls. Treatment effect estimates are meaningfully different between TWFE and BJS methods, suggesting that treatment is likely not homogeneous across units/time. The primary specification the BJS method using the Austin and Harper (2019) TRAP treatment designation presented in column (2) of Table 1.4. I use this policy coding as the primary specification because it defines TRAP treatment more broadly without the stringency requirement of Jones and Pineda-Torres (2021), and therefore it should produce more conservative estimates of the average treatment effects.

With the exception of the APGAR score, outcome variables are binary indicators such that coefficients can be interpreted as percentage point changes in the rate of adverse health outcomes in a state following TRAP policy implementation. Coefficients on the APGAR score represent raw changes in the five-minute APGAR score ranging from zero to ten. For reference, I provide the sample mean of the health outcomes under their label on the left side of the table. So, the coefficient of pregnancy-associated hypertension in column (2) of 0.0046 means that the rate of pregnancy-associated hypertension among birthing people in states that passed a TRAP law increased by 0.46 percentage points on average following the policy change, and this is a 11.5% increase from the sample mean of 0.04.

Results from Table 1.4 indicate that TRAP laws increase state-level rates of hypertensive disorders of pregnancy, increasing the rate of pregnancy-associated hypertension by 11.5% and the rate of chronic hypertension by 16% and establishing a causal link between abortion access and the maternal health crisis in the United States. These results are robust to the inclusion of reproductive health policy controls in column (3) and an alternative TRAP policy coding from column (4). There is not enough evidence to suggest that TRAP laws increase the risk of premature birth and low birthweight among infants — coefficients are positive but small and not statistically significant in the primary specification. Effects on premature birth are only meaningfully larger and statistically significant using the policy coding from Jones and Pineda-Torres (2021) in column (4).

The counterintuitive negative effect of TRAP laws on metabolic outcomes is likely a product of the violated parallel trend assumption. In Figure 1.3, it appears that treatment effects for diabetes and gestational diabetes increase following a TRAP law, but the differential trends in the pre-treatment period result in coefficients that are negative. I argue that this parallel trend violation is a result of the staggered adoption of the revised birth certificate. If the timing of adoption of the revised certificate is correlated with lower rates of adverse maternal health outcomes, this may explain the differential trend leading up to the passage of a TRAP law. Since all other outcomes are reported consistently across the revised and unrevised birth certificate, the issue is isolated and the violation of the parallel trends assumption for metabolic outcomes does not limit the credibility of the research design for other results. In addition, I address this issue later by measuring the effect of travel distance to an abortion provider at the county level using a research design that is not confounded by the adoption of the revised certificate.

The coefficients in Table 1.4 also suggest that infant APGAR scores rise as a result of TRAP laws, implying that the laws result in healthier infants being born on average. While this result is theoretically possible, it stands in contrast to the maternal health results. It would be unusual to observe a policy decrease the average maternal health while increasing average infant health because maternal and infant health at birth are intricately connected. One possible explanation for the positive coefficients on the APGAR score is the limited variance of the scores within the data. While scores are reported on a 0-10 scale, 82% of infants in the sample have an APGAR score of 9. This low variance contributes to a low standard error of my estimate, leading to a coefficient that is statistically significant but not economically significant — a 0.0316 increase in APGAR score is 0.36 percent increase from the sample mean.

Heterogeneity and Health Disparities

Much of the literature surrounding abortion access establishes that the effects of abortion laws are often heterogeneous across race/socioeconomic status (Jones and Pineda-Torres, 2021; Myers, 2021a; Kelly, 2020; Clarke and Mülrad, 2021; Farin et al., 2021). To determine if there exists significant heterogeneity in the burdens of TRAP laws, I estimate effects by the birthing person's race and education in Table 1.5.

For nearly[5] all outcomes, treatment effects are larger for Black birthing people at equivalent levels of education. This indicates that Black birthing people likely experience a larger burden from the passage of a TRAP law, consistent with existing evidence in the literature. For people with a high school diploma, TRAP laws increase the rate of chronic hypertension for Black birthing

[5]The singular exception is the effect of TRAP laws on pregnancy associated hypertension among those with some college education. Here the treatment effect is slightly larger for white birthing people.

Table 1.5 Diff-in-Diff by Subgroup

	PA Hypertension	Chronic Hypertension	Premature Birth	Low Birthwt
White, college	0.0050***	0.0019**	0.0039**	0.0027**
(n = 36,328,911)	[0.0012]	[0.0010]	[0.0017]	[0.0011]
White, HS	0.0036**	0.0018*	0.0036	0.0014
(n = 12,078,283)	[0.0017]	[0.0009]	[0.0022]	[0.0014]
White, <HS	0.0010	0.0005	-0.0019	−0.0028*
(n = 4,551,301)	[0.0015]	[0.0007]	[0.0025]	[0.0016]
Black, college	0.0042**	0.0032**	0.0078***	0.0070***
(n = 7,162,788)	[0.0020]	[0.0015]	[0.0019]	[0.0015]
Black, HS	0.0037	0.0036**	0.0053**	0.0052*
(n = 3,906,562)	[0.0034]	[0.0017]	[0.0025]	[0.0029]
Black, <HS	0.0024	0.0027*	0.0054**	0.0029
(n = 2,390,145)	[0.0033]	[0.0016]	[0.0027]	[0.0035]
Full Sample	0.0046***	0.0016**	0.0014	0.0004
	[0.001]	[0.001]	[0.002]	[0.001]

Notes: Difference-in-Differences results by race and education using the specification in column (2) of Table 1.4. Standard errors are clustered at the state level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

people by 0.36 percentage points, double the 0.18 percentage point increase among white birthing people. The difference in the magnitudes of the treatment effects is often even larger for infants. TRAP laws increase rates of low birthweight among Black infants born to those with a high school diploma by 0.52 percentage points, 3.7 times the treatment effect among infants born to white birthing people with the same level of education.

I expect that TRAP laws are more burdensome among people with lower levels of income. The increased distance to a provider following the policy imposes a larger relative cost to people who may not have the financial means to travel to receive an abortion. In this subgroup analysis, I include measures of education to serve as a proxy for socioeconomic status. So, it is surprising to observe that within racial groups treatment effects tend to be larger among those with more education. This may be due to differences in the age distribution across education levels. Birthing people with higher levels of education tend to be older, and older births have higher rates of health complications, which could explain the result.

I implement a triple-difference specification to measure the differential effects of TRAP laws across demographic groups. I augment the imputation step of the BJS procedure to include group-state, group-time, and state-time fixed effects and include individual-level controls for age:

$$Y_{ist}(0) = \hat{\alpha}_{g*s} + \hat{\delta}_{g*t} + \hat{\beta} x_{ist} + \hat{\lambda}_{s*t}. \quad (5)$$

After imputing potential outcomes in this manner, calculating average treatment effects follows the same procedure outlined in equation (2) and (3). I estimate differential effects across two groups: race (Black vs white) and education (HS or less vs college-goers). Treatment effects from the triple-difference represent the average change in the gap between racial/education groups within a treated state after a TRAP law. The point estimates then describe the effect of TRAP laws on health disparities across race and education.

Figure 1.4 presents the results of the triple-difference specification. Point estimates for statistically significant coefficients are labeled along with the percent change from the average gap between groups across the entire sample presented in parentheses. While there does not appear to be evidence that TRAP laws significantly affect existing maternal health disparities, results indicate that infants born to Black birthing people and to those with a high school education or less experience disproportionately worse outcomes following a TRAP law. The rate of premature birth among Black infants increases by 0.28 percentage points more than the rate among white infants following a TRAP law. This effect is a 3.7% increase from the average gap in premature birth between Black and white infants in the entire sample. Similarly, there is a 0.4 percentage point larger increase in the rate of low birthweight among Black infants, a 5.9% increase in the average gap. This evidence is unsurprising, given that TRAP laws have a much larger effect on the rates of premature birth and low birthweight among Black infants in Table 1.5.

The triple-difference design allows me to measure differential effects by education and account for differences in the age distribution by including an individual level control for maternal age. I find that TRAP laws disproportionately affect infants born to birthing people with a high school education or less relative to infants born to college-goers. The rate of premature birth among infants born to people with a lower level of education increases by 0.34 percentage points more than the rate among infants born to college-goers. This differential effect is a 19.5% increase from the average gap between infants born to higher and lower educated parents across the entire sample.

Distance to an Abortion Provider

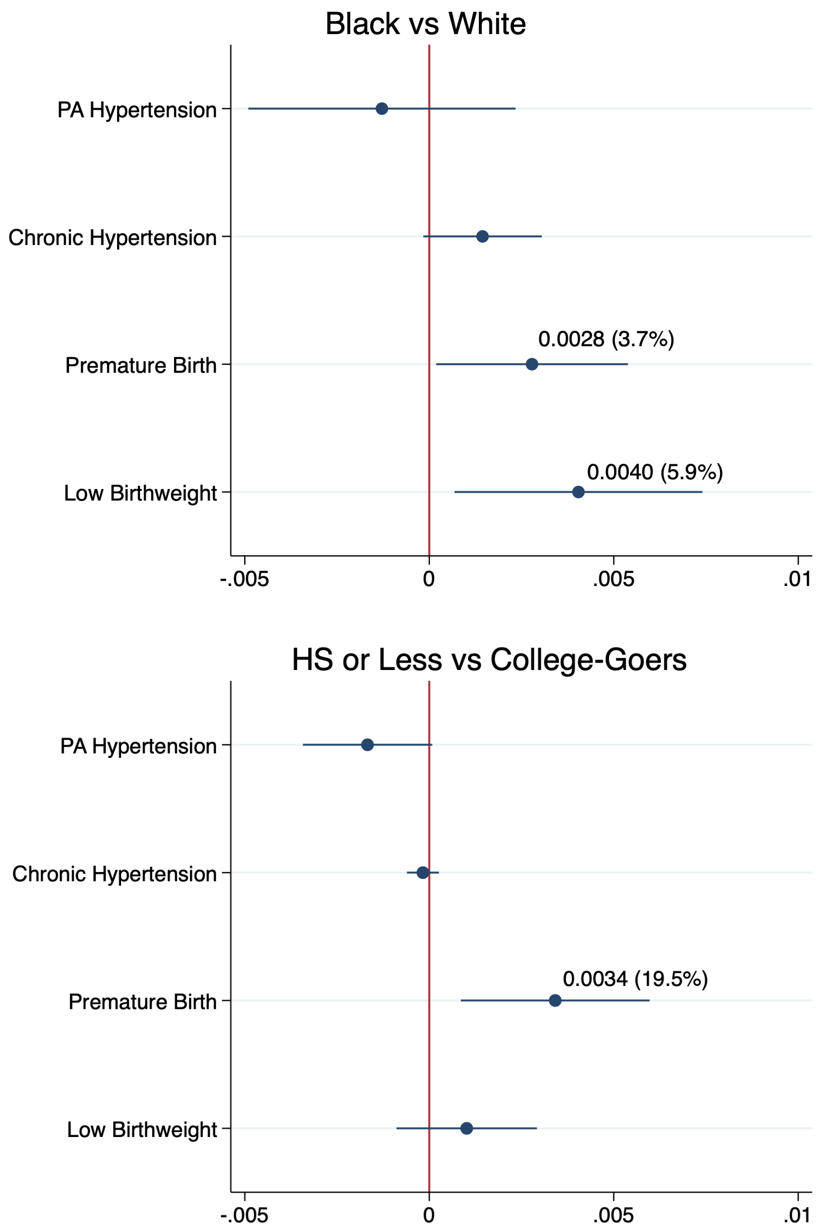
It is possible that defining treatment from TRAP laws with a binary policy indicator results in imprecise treatment effect estimates due to the wide variation in the nature of TRAP laws. To assuage this concern, I move away from the binary policy indicator for treatment, using a panel of travel distance to an abortion provider at the county-month level from 2009 to 2017 compiled by Myers (2021b). Travel distance is measured linearly from the population centroid. I use a fixed-effects design exploiting variation in the distance to an abortion provider at the county level over time to identify the average effect of increasing travel distance. I employ the specification:

$$Y_{ict} = \alpha_c + \delta_t + \beta distance_{100} + \lambda_s * t + \epsilon_{ict} \quad (6)$$

where Y_{ict} is the outcome of interest for an individual i residing in county c at time t, α_c and δ_t are county and year fixed effects, $distance_{100}$ measures the distance to an abortion provider in 100s of miles, and $\lambda_s * t$ is a state time trend.

The identifying assumption of this specification is that counties that experience an increase in their travel distance to an abortion provider would have experienced trends in their rates of adverse maternal and infant health outcomes similar to those counties that experience no change in travel distance, accounting for time-varying differences across states. I find similar effects to the difference-in-differences design on hypertensive disorders of pregnancy and infant health outcomes. This method has the added benefit of solving the parallel trends issue for maternal metabolic outcomes outlined in Table 1.3. The state time trend accounts for differential trends in the rates of diabetes and gestational diabetes across states, and this analysis is not confounded by the staggered adoption of the revised birth certificate. So, I can credibly estimate the effect of travel distance on maternal metabolic outcomes.

Figure 1.4 TRAP Laws and Health Disparities



Notes: Figure describes results from the triple-difference design, measuring the change in the gap in adverse health outcomes between demographic groups after TRAP treatment. All specifications include controls for maternal age and standard errors are clustered at the state level. Point estimates are indicated on the graph, with the percent change from the mean in parentheses.

Table 1.6 Travel Distance and Pregnancy/Birth Outcomes, 2009-2017

	PA Hypertension	Chronic Hypertension	Diabetes	Gestational Diabetes	Premature Birth	Low Birthweight	APGAR Score
Distance	0.0035	0.0016**	0.0041*	0.0043***	-0.0002	-0.0006	0.0465***
(100s miles)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.0003)	(0.013)
N	35378433	35378433	35378433	31688150	35464801	35464801	35307992

Notes: Data on travel distance comes from Myers (2021b). Results for the effect of increasing travel distance to an abortion provider on adverse health outcomes for birthing people and infants. Coefficients from a fixed-effects design specified in equation (6). Standard errors are clustered at the state level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Results in Table 1.6 indicate that restricted access to abortion increases rates of hypertensive disorders of pregnancy among birthing people. Increasing the distance to an abortion provider by 100 miles increases county-level rates of chronic hypertension by 16%. This increased distance also increases rates of pregnancy-associated hypertension by 8.75%, but the coefficient is not statistically different from 0 in this context. In addition, the fixed effects design in Table 1.6 is provides the only credible evidence of effects on metabolic outcomes among birthing people. Results indicate that increasing the travel distance to an abortion provider by 100 miles increases the county-level rate of diabetes and gestational diabetes by 10.25% and 8.6% respectively.

This analysis complements my difference-in-differences finding by providing further evidence that restricted abortion access increases rates of adverse health outcomes among birthing people but no evidence that restricted access results in significant health effects among infants on average. Overall, this evidence taken together tells a consistent story that restricted access to abortion causes poorer maternal health outcomes on average.

1.4.4 Discussion

The Composition of Births

In the Conceptual Framework in Section 3, I rationalize the effects of abortion access on the average health status of birthing people and infants through a compositional change in the population of people carrying a pregnancy to term. In this discussion, I turn to this question of composition. Do TRAP laws change the composition of people giving birth?

I hypothesize that people responsive to the cost of an abortion may differ in observable and

Table 1.7 The Effect of TRAP Laws on the Composition of Births, 1990-2017

	Black	Hispanic	Age	Number of Prenatal Visits	HS Educ or Less
TRAP Law	0.0038 (0.004)	-0.0066 (0.007)	-0.0651 (0.065)	-0.1042 (0.072)	-0.0026 (0.005)
N	96122838	96122838	97215229	97215229	69388926

Notes: Coefficients measure the effect of TRAP laws on the features of birthing people using the BJS procedure and the Austin and Harper (2019) policy coding. Includes effects on binary indicators for race/ethnicity (Black and Hispanic), age in years, the number of prenatal visits, and an indicator for a high school education or less. Standard errors are clustered at the state level.

unobservable ways from those who would carry to term regardless. To measure the effects of TRAP laws on the composition of people giving birth, I repeat the BJS difference-in-differences analysis using demographic features of the sample as the outcome variables.

In Table 1.7, I estimate the ATT of TRAP laws on the following characteristics among birthing people: simple indicators for race/ethnicity (Black and Hispanic), age measured in years, the number of prenatal visits, and an indicator for receiving a high school education or less. Coefficients suggest that TRAP laws may result in more births to Black people, fewer births to Hispanic people, slightly younger birthing people on average, fewer prenatal visits, and fewer birthing people with a high school education or less. But, none of these estimates are statistically different from zero. So, there is not enough evidence to suggest that TRAP laws substantially change the composition of births over these observable characteristics. Instead, the health effects from abortion access may be driven by unobservable changes in the composition of people carrying a pregnancy to term.

The Marginal Birth

While average effects of abortion access on state-level rates of adverse health outcomes are meaningful, a key coefficient of interest is the rate of adverse outcomes among the marginal births. Figure 1.1b sheds light on the fact that, when there are competing effects from avoided pregnancies, the rate of conditions among the marginal births cannot be calculated or informatively bounded. We may expect, however, that the downward effect of pregnancy avoidance on the number of births following a restrictive abortion law is theoretically small. This effect comes from the presence of people who would have given birth when abortion was accessible but now avoid pregnancy due

Table 1.8 The Effect of TRAP Laws on the Number of Births, 1990-2017

	Coefficient	S.D.	p	95% CI
# of Births	4432.34	2213.58	0.036	[289.79, 8574.88]

Notes: Coefficients measure the effect of TRAP laws on the number of births using the BJS procedure and the Austin and Harper (2019) policy coding. Standard errors are clustered at the state level.

to the restrictive abortion environment. This population is likely very small — the more likely scenario is that avoided pregnancies come from people who would have received an abortion in the counterfactual unrestricted environment. If this is the case, avoiding pregnancy should have little to no effect on the number of births following an abortion restriction.

So, I perform back-of-the-envelope calculations to describe the rate of adverse health outcomes among the marginal births in the setting depicted in Figure 1.1a where avoided pregnancy has no influence on the number of births. I first use the BJS procedure to estimate the change in the number of births following a TRAP law under the assumption that this effect is entirely driven by marginal births.

Table 1.8 presents the results from the BJS procedure using the number of births in each state-year as the outcome variable. This analysis indicates that implementation of a TRAP law increases the number of births by roughly 4,400 annually, a 5.6% increase from the sample mean. I assume this value represents the number of marginal births. To calculate the rate of adverse outcomes among these marginal births, I use the coefficients in column (2) of Table 1.4 and the average number of annual births in treated states (93,146) to back out the number of additional cases of pregnancy-associated hypertension and chronic hypertension in states following a TRAP law. I calculate that TRAP laws result in 428.47 additional cases of pregnancy-associated hypertension and 149.03 additional cases of chronic hypertension. If I assume that all of these additional cases come from the set of marginal births, then the rate of pregnancy-associated hypertension among marginal births is 9.67% and the rate of chronic hypertension is 3.36%. So, marginal births are significantly less healthy — they have a rate of pregnancy-associated hypertension about 2.5 times the mean rate and a rate of chronic hypertension about 3.36 times the mean rate.

“Real" Health Effects

In this section, I measure the causal effects of TRAP laws on average rates of adverse health outcomes among birthing people and infants in treated states. And although understanding the births on the margin of abortion policy is an important question, using only information from birth records it is impossible to determine if any observed health effects are “real" in the sense that the abortion restriction induces the presence of chronic conditions at the individual level. It could be the case that each birthing person on the margin has lower fundamental health status. For example, increased rates of hypertension among birthing people following a TRAP law could be due to the presence of people who already had or were prone to high blood pressure and now appear in the data because they carry to term. To get a sense regarding how abortion access affects the presence of chronic conditions, I look to individual survey data from the general population.

1.5 TRAP Laws and Individual Health Effects

When assessing health effects at the individual level, the quasi-experimental context is quite different. In an ideal setting, I would compare the rates of chronic conditions between people who are denied and people who receive an intended abortion. Because I do not observe these populations, I estimate instead the effects of TRAP laws on the rates of chronic conditions among reproductive age women.

Restricted access to abortion may affect individual health through a variety of factors. For those who carry to term as a result of the policy, the pregnancy, labor, and delivery have a relatively high potential for complications in the United States. Using insurance claims data, Blue Cross Blue Shield Association (2020) report that 19.6% of pregnancies had complications in 2016 — a 16.4% increase from the complication rate in 2014. Additionally, a small but increasing number of pregnancies involve complications during childbirth. The rate of childbirth complications was 1.69% in 2018, up 14.2% from 2014 rates. For comparison, the rate of complications from abortion procedures is 0.19% (Rolnick and Vorhies, 2012).

It is possible that health effects from pregnancy complications are not isolated to the period of pregnancy and immediately surrounding childbirth. There is a measured association between pregnancy complications and future risk of cardiovascular disease and metabolic conditions, but it is unclear if the complicated pregnancy is *causing* the increased risk of cardiovascular disease, hypertension, stroke, and diabetes, or if the complicated pregnancy and poor health outcomes are both results of some underlying cause (Neiger, 2017).

In addition to potential health complications, carrying a pregnancy and childbirth are both expensive endeavors. Among women with employer-based health insurance, average out-of-pocket costs for care during pregnancy, delivery, and three months after birth was $4,500 in 2015, up from $3,000 in 2008 (Moniz et al., 2020). Compared to peers without children, mothers are more likely to experience wage penalties, time lost in the workforce, and often substitute to lower-paying careers (Gangl and Ziefle, 2009). Causal effects of income on health status are difficult to estimate due to endogeneity concerns. Ettner (1996) uses 2SLS with a variety of instruments for income and finds that individuals with higher incomes report better health and fewer health-related work limitations. Lazar and Davenport (2018) provide a systematic literature review detailing the reduced access to healthcare among low-income individuals stemming from the increasing cost of care, proximity to providers, limitations of insurance coverage, and more. So, the potential lasting financial burdens of abortion restrictions may result in reduced access to healthcare and therefore a higher risk of some chronic health conditions.

1.5.1 Data

To identify effects of TRAP laws on individual health outcomes, I use data from the Behavioral Risk Factor Surveillance System (BRFSS). BRFSS is a large telephone survey of the US population administered annually. The survey collects data about individual demographics, health behaviors/risk, and health outcomes. I use information from 1993 to 2017, after the survey became a national sample. In addition, I restrict my sample to only include respondents who report being female and are in their reproductive lifetime[6] (18-44). The data contain demographic information on age group, employment, income, race, marital status, and health insurance coverage. I select cardiovascular and metabolic outcomes in BRFSS that are roughly equivalent to the maternal health

[6] Age range in the sample begins at 18, although the reproductive lifetime is generally defined to be 15-44, because BRFSS only samples adults

characteristics and outcomes I observe in the vital statistics data — hypertension, high cholesterol, and diabetes.

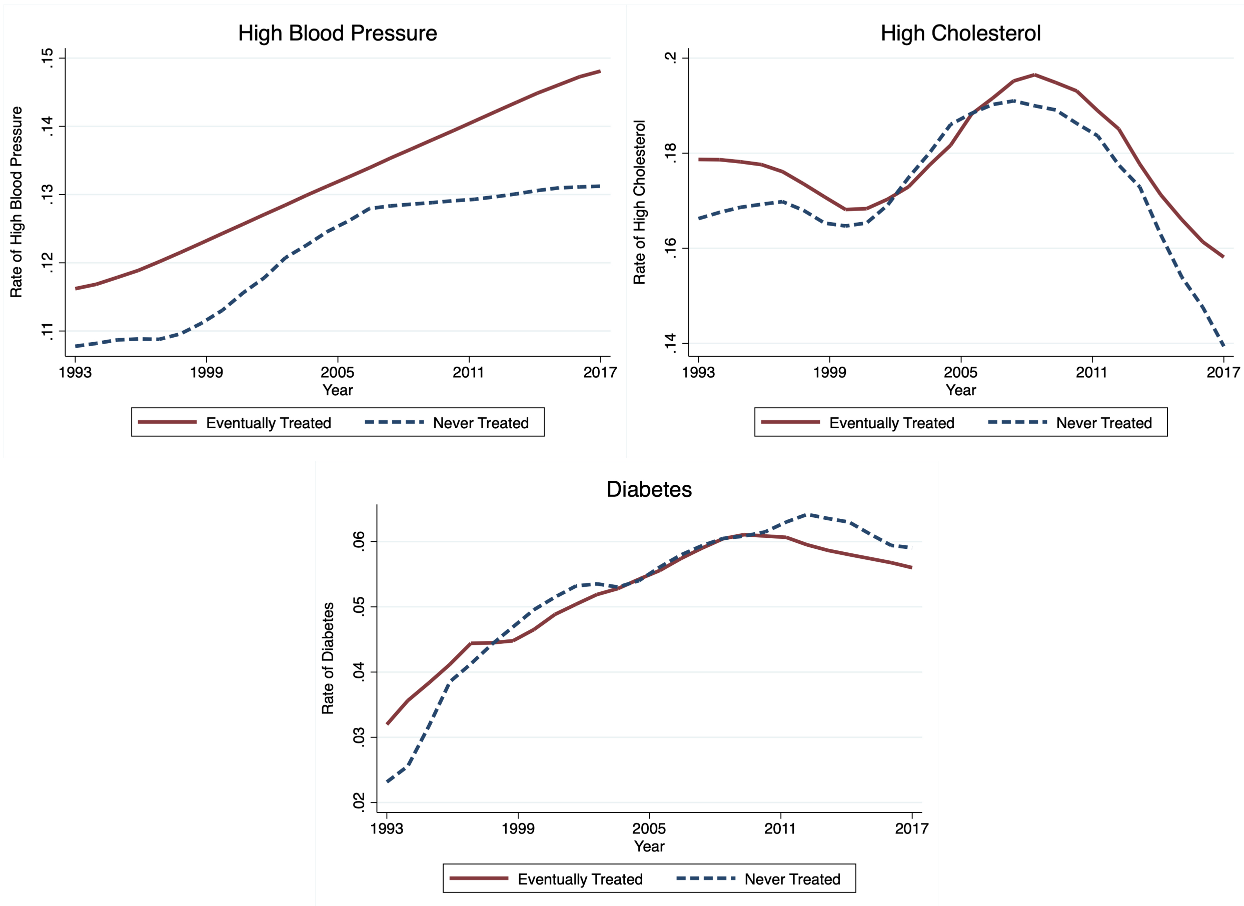
BRFSS is the largest continuously conducted health survey system in the world, and the sample of reproductive age women over the time period contains roughly 1.5 million observations. In addition, analysis using data in BRFSS is not encumbered by the selection mechanism inherent in data on births, potentially allowing for the observation of “real" effects of abortion access. However, information from BRFSS has a few key limitations. Aside from the inherent response bias in survey data, over my study period the modality of the survey changed to include a large proportion of data collected via cell phone, which potentially creates compositional changes in the respondent population. In addition, information on cardiovascular health is only collected biannually in most states, limiting the years of analysis. Most importantly, I am unable to identify a more precise treated population than reproductive-age women. I cannot observe people who were restricted from accessing abortion, or even individual pregnancy history. For these reasons, I interpret estimates with caution and consider effects to be suggestive rather than true causal effects.

Figure 1.5 describes the rate of cardiovascular and metabolic chronic conditions in the general population of reproductive-age women in eventually-treated and never-treated states over time. Overall, these trends do not suggest a strong association between TRAP treatment and these conditions. While the slope of the rate of high blood pressure is slightly larger among eventually-treated states, trends between groups are similar in their rates of diabetes and high cholesterol.

1.5.2 Estimation

I use an estimating procedure very similar to the one described in Section 4.2 to identify the effect of TRAP laws on the rate of chronic conditions among reproductive-age women in treated states. I use two-way fixed effects and the BJS (2021) method to estimate the ATT, exploiting variation in state TRAP policies over time. In this setting, I make a minor change to the imputation step to include individual-level controls, such that potential outcomes are imputed using $Y_{ist}(0) = \hat{\alpha}_s + \hat{\delta}_t + \hat{\beta} x_{ist}$, where x includes controls for marital status and race. In addition, I consider only short-term effects of TRAP laws to limit confounding from differential trends in

Figure 1.5 TRAP Laws and Chronic Conditions, 1993-2017



Notes: Plots compare trends in the rates of chronic conditions between states that are never treated by a TRAP law and states that are treated eventually throughout the study period. Always-Treated states are excluded. Graphs include the data only on odd years of info from BRFSS, because a majority of states as questions about cardiovascular health status biannually.

general health between states. I estimate the average effects of TRAP laws within five years of the policy change.

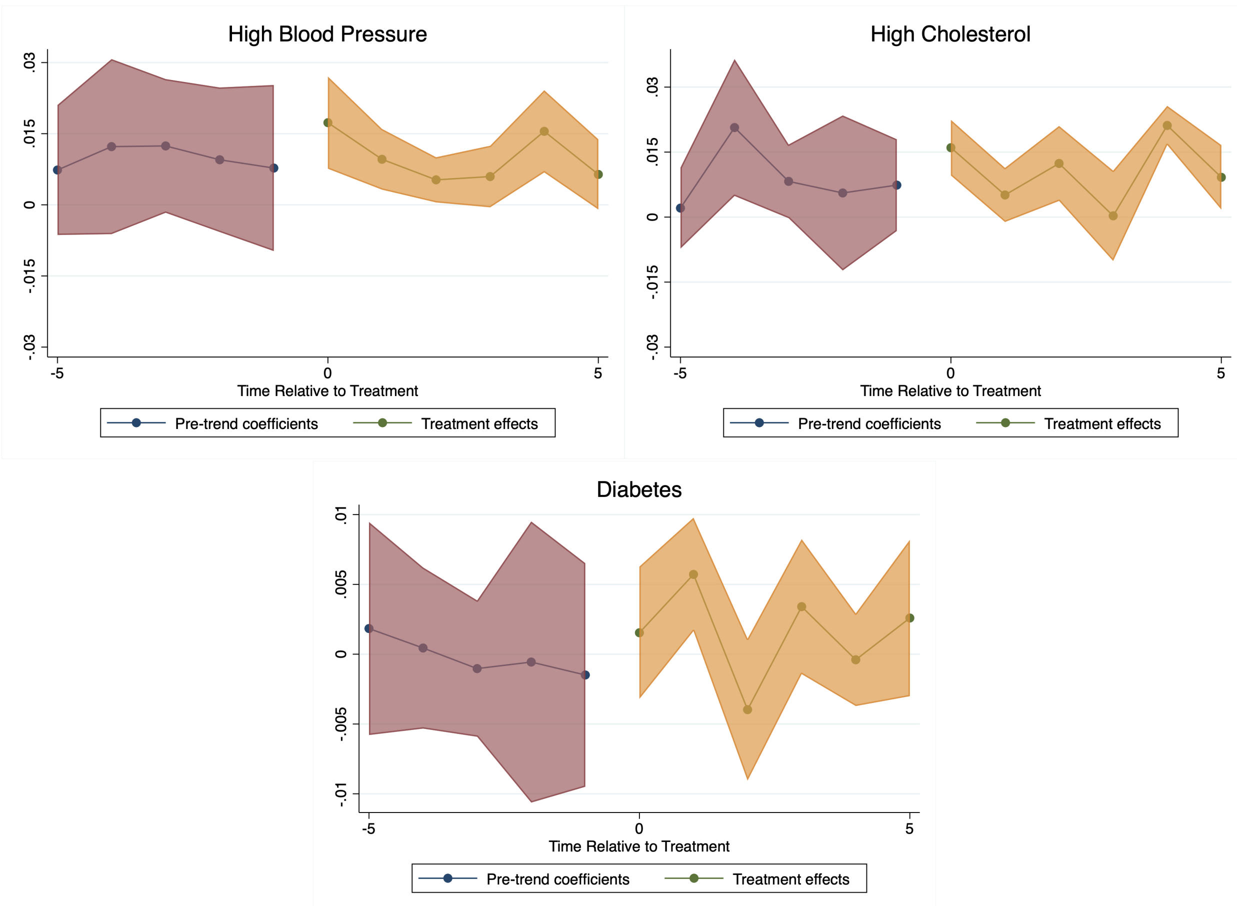
To determine if states that pass TRAP laws experience differential trends in their rates of chronic conditions leading up to the policy change, I use a specification equivalent to that in equation (4) and perform an F test similar to the one presented in Table 1.3. Results from Figure 1.6 and Table 1.9 indicate that the parallel trends assumption does not hold for the rate of high cholesterol. If this violation is due to differential trends in overall health among states treated by TRAP laws, then estimates for all chronic conditions lack credibility. To overcome this challenge, I compare these difference-in-differences results with a triple difference specification comparing women in their

Table 1.9 Parallel Trend Assumption F Test (BRFSS)

	F-stat	p-value	df
High Blood Pressure	0.974	0.444	43
High Cholesterol	2.616	0.038	43
Diabetes	0.273	0.925	43

Notes: Results from an F test of $\gamma = 0$ in equation (4) using outcome data from BRFSS.

Figure 1.6 BJS Event Study — TRAP Laws (BRFSS)



Notes: Plots describing the pre-trend coefficients along with treatment effects of TRAP laws on outcomes from BRFSS using the method in Borusyak, Jaravel, and Spiess (2021). Pre-trends and treatment effects are disjoint and colored differently to indicate that they are estimates from separate methods rather than the result of a dynamic specification found in traditional event studies.

reproductive lifetime to women 45-59, under the assumption that women aged 45-59 are not treated by a TRAP law and that this difference between age groups across states over time will account for trends in the health of the overall population in treated states.

Table 1.10 Difference-in-Differences Results (BRFSS)

	TWFE	BJS Diff-in-Diff	BJS Triple-Diff
	(1)	(2)	(3)
High Blood Pressure	0.009^{**}	0.008^{***}	-0.003
(mean = 0.138)	[0.004]	[0.003]	[0.006]
High Cholesterol	0.008^{**}	0.006^{***}	0.004
(mean = 0.180)	[0.004]	[0.002]	[0.007]
Diabetes	0.002	0.001	-0.007^{***}
(mean = 0.054)	[0.002]	[0.003]	[0.002]

Notes: Results from the difference-in-differences and triple-difference design measuring the effect of TRAP laws on chronic conditions in the general population of reproductive-age women in treated states within five years of the policy change. Outcome data from BRFSS. All specifications include controls for marital status and race. Standard errors are clustered at the state level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

1.5.3 Results

Table 1.10 presents the results for the TWFE specification, the BJS difference-in-differences, and the triple-difference design. Outcome variables are 0-1 indicators such that coefficients may be interpreted as percentage point changes in rates of chronic conditions among adult women of reproductive age in states treated by TRAP laws. All specifications include controls for marital status and race, and the TRAP policy coding from Austin and Harper (2019) is used.

Results from the BJS difference-in-differences design in column (2) indicate that TRAP laws increase rates of high blood pressure among women 18-44 by 5.8%, with no evidence supporting an increase in rates of diabetes. These results are consistent with biological evidence regarding the short-term persistence of hypertension and diabetes following pregnancy. Following a diagnosis of preeclampsia (pregnancy-associated hypertension alongside some form of maternal organ failure, affects about 3.4% of pregnancies), 41.5% of patients are diagnosed with high blood pressure within a year after delivery (Benschop et al., 2018), while only 10% of birthing people with a gestational diabetes diagnosis experience Type 2 diabetes within five years (Kim et al., 2002). By ten years after pregnancy, 50% of those who experience gestational diabetes are diagnosed with Type 2 diabetes, but this is unlikely to contribute to my estimates when measuring effects within the first five years of a TRAP policy.

However, these results do not hold under a triple-difference design comparing women in their reproductive lifetime (18-44) with women who are just beyond (45-59). Coefficients from the triple-difference specification in column (3) indicate that, compared to within-state trends among women just beyond reproductive age, women in their reproductive lifetime do not experience significantly increased rates of hypertension following a TRAP law. And, women of reproductive age experience declines in their rates of diabetes following a TRAP law relative to slightly older women. Therefore, it may be the case that states treated by TRAP laws in this sample experience declines in overall health for reasons unrelated to abortion policy. It is also true that women aged 45-59 may not be a reasonable control group for women of reproductive age. Comparing women who are 25 to women who are 55 may provide estimates that are confounded by issues affecting older adults, particularly when measuring rates of chronic conditions that are strongly associated with age.

Subgroup Analysis

I repeat the triple-difference procedure for population subgroups who are more likely to be affected by abortion legislation to explore the potential for individual health effects to be heterogeneous across demographic groups. I estimate effects within Black women, women with a high school education or less, and women in households making less than $35,000 per year. Each specification includes controls for marital status, and the specifications based on education and income also include controls for race.

Results from Table 1.11 indicate that triple-difference estimates for subpopulations based on education and household income are not meaningfully different from the results measured in the general population. For Black women, it appears that women of reproductive age may have higher rates of chronic conditions in treated states, but these estimates are noisy and not statistically significant.

Ultimately, these results present mixed evidence regarding the “real" health effects of TRAP laws at the individual level. Difference-in-Differences results suggest that TRAP laws are associated with

Table 1.11 Triple-Difference by Subgroup (BRFSS)

	Black	HS Education or Less	HH Income <35K
High Blood Pressure	0.0055	-0.0028	-0.0115
	[0.013]	[0.006]	[0.008]
High Cholesterol	0.0143	0.0037	0.0036
	[0.014]	[0.007]	[0.009]
Diabetes	0.0003	-0.0070^{***}	-0.0091^{**}
	[0.007]	[0.002]	[0.004]
N	228,945	2,478,891	1,223,910

Notes: Results from a triple-difference BJS specification comparing women of reproductive age and women beyond reproductive age with states by population subgroup. Standard errors are clustered at the state level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

higher rates of hypertension among reproductive-age women in treated states, but these estimates are specification sensitive. With data that more precisely identifies individuals who are treated or plausibly treated by abortion laws, future research may shed more light on the division between individual health effects from abortion policy and effects driven by the selection into abortion and birth.

1.6 Conclusion

Abortion restrictions in the United States have implications for maternal and infant health outcomes. TRAP laws increase rates of adverse cardiovascular health outcomes among birthing people in treated states by 11-16%, and it is possible that these effects persist beyond pregnancy. These policies also increase health disparities in infant health outcomes at birth across parental race and education — increasing gaps in premature birth and low birthweight between Black and white infants by 3-6%, and gaps in premature birth between infants born to parents with a high school diploma or less and those born to college-goers by 19.5%. In addition, increasing the travel distance to an abortion provider by 100 miles increases rates of hypertensive disorders of pregnancy by 8-16% and rates of adverse maternal metabolic conditions by 8-10%.

This demonstrates the importance of considering how access to reproductive healthcare like abortion affects maternal and infant health, and how the growing hostility toward abortion access in US legislatures may contribute to the current maternal health crisis. When envisioning what the reproductive health environment looks like following the *Dobbs* decision, these results indicate that significant public health consequences could occur as more restrictive abortion legislation is passed in state legislatures. Abortion laws may increase observed adverse maternal health outcomes — adding to a crisis that is already concerning to public health professionals. And, these laws may exacerbate existing health disparities. These additional cases of adverse pregnancy outcomes have implications for clinical practice, as doctors practicing pregnancy and birth-related care in states with abortion restrictions are likely to see more complications. Finally, the results have implications for public finance. Medicaid is the largest payer of births in the United States, and adverse conditions during pregnancy may impose extra financial costs on the social insurance sector through additional monitoring and treatment.

CHAPTER 2

NOTIFICATION VS CONSENT: THE DIFFERENTIAL EFFECTS OF PARENTAL INVOLVEMENT LAWS ON TEEN ABORTION

2.1 Introduction

In the United States, parental involvement (PI) laws are state-level policies that require the participation of a parent in the abortion decision of an unemancipated, unmarried minor (aged < 18). These laws come in two broad categories: notification and consent. Parental notification laws mandate that the abortion provider make a satisfactory effort to contact and notify the parent(s) or guardian(s) of an unemancipated, unmarried minor prior to performing an abortion. Under a consent law, providers are required to collect various forms of parental consent, from simple verbal consent to notarized written consent.

In any single period, pregnant teens will make the decision to have an abortion based upon their marginal benefits and marginal cost. Parental involvement laws and other forms of restricted abortion legislation increase the marginal cost of an abortion. Because a parental consent law requires parental notification by necessity, a policy change from notification to consent will (weakly) increase the marginal cost of an abortion. Using these state-level policy changes, I test the hypothesis that, relative to parental notification, a parental consent law will decrease the abortion rate for minors (15-17). Additionally, evidence suggests that a proportion of minors who are restricted from accessing abortion carry their pregnancy to term and give birth as a result (Myers and Ladd, 2020). So, I also test the hypothesis that the policy change to parental consent will increase the birth rate for minors.

The theoretical foundation of the literature on restricted access to abortion considers abortion to be an insurance policy against negative information realized after pregnancy. Forms of restricted access (include PI laws) increase the marginal cost of the insurance policy (Kane and Staiger, 1996). Some research suggests that restricted access to abortion has long term negative consequences for women. In a current working paper, Miller, Wherry, and Foster (2020) survey women just before and just after the gestational limit and find that seeking but being denied an abortion results in large increases in measures of financial distress, and that this distress persists for six years after the intended abortion.

Using sibling fixed effects, Johansen, Nielson, and Verner (2019) show that those in Denmark who give birth under the age of 21 obtain fewer years of schooling, experience a lower employment rate, and have lower earnings at age 35. The authors make special note that these effects exist within a robust welfare state in Denmark, and would likely be exacerbated in nations with fewer support programs for young parents, such as the United States.

For teens specifically, Maynard and Hoffman (2008) show that motherhood is associated with negative educational, financial, and health outcomes for both the mother and child compared to peers who are not parents. In their book *Kids having kids: Economic costs and social consequences of teenage pregnancy*, Maynard and Hoffman also report that teen births cost taxpayers between \$9.4 and \$28 billion every year through public assistance, foster care, and criminal justice services. The potential consequences for the teen mother, the child, and the state motivate a discussion surrounding any policies that could be exacerbating these issues by restricting teen access to abortion.

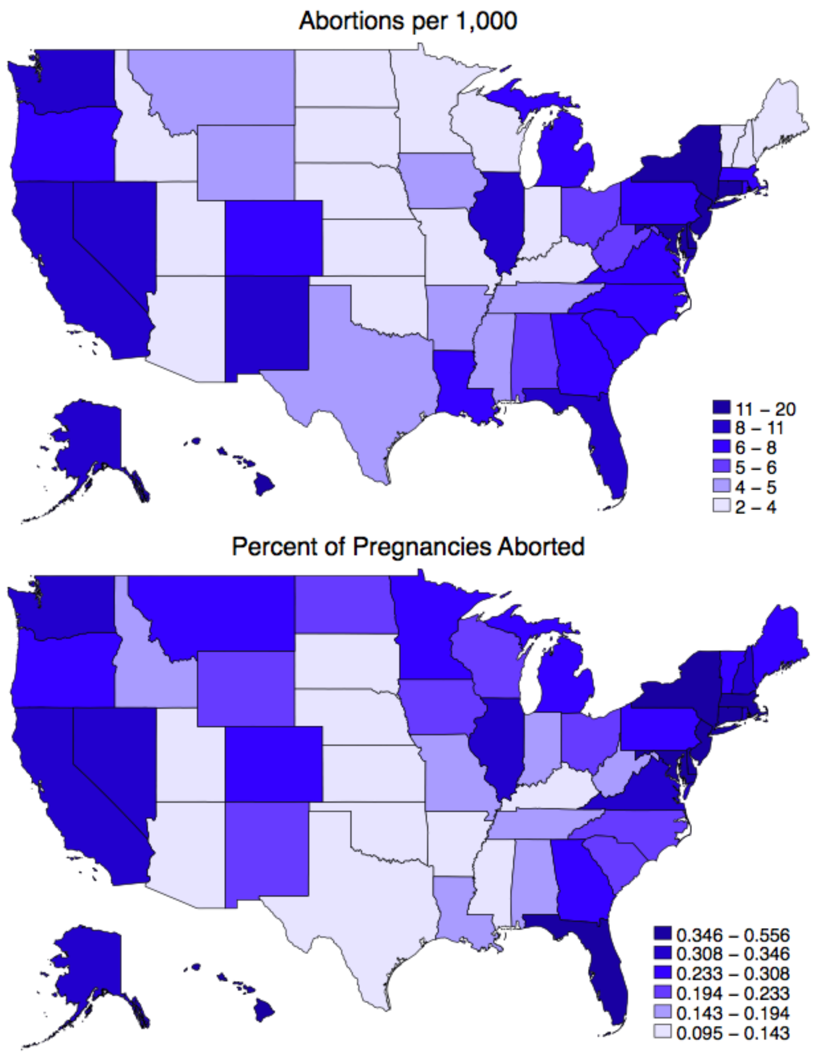
2.2 Background

2.2.1 Trends in Teen Abortion

Non-trivial variation across states in their teen abortion rates provides another motivation for studying topics related to teenage abortion. Figure 2.1 uses data from a Guttmacher Institute report detailing the pregnancy rate and abortion rate for 15-17 year-olds in all 50 states in 2013.

These graphs show significant variation in the teen abortion rate (per 1000 residents assigned female at birth) and the percent of teen pregnancies aborted. Maryland has a 15-17 abortion rate of 10, five times the abortion rate of Nebraska (teen abortion rate of 2). Minors in Maine abort their pregnancies roughly 35 percent of the time, which is nearly three times the percent of pregnancies aborted in West Virginia (12.5 percent). The variation in the percent of pregnancies aborted means that the observed variation in the teen abortion rate cannot be solely attributed to differences in pregnancy rates. This paper considers whether the type of parental involvement law contributes to

Figure 2.1 Abortion Rate and Percent of Pregnancies Aborted, 2013



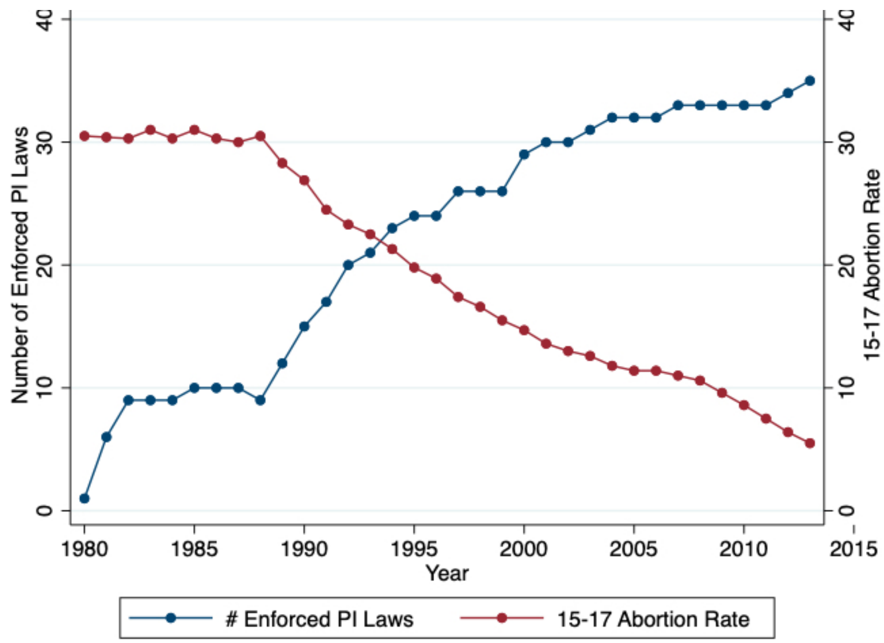
Source: Kost et al. (2017)

the variation in the teen abortion rate.

2.2.2 Parental Involvement Laws

Utah passed the first parental involvement law in 1974. Since then, their prevalence has grown tremendously. As of March 2023, 36 states have a PI law in place (though many of these states have recently passed complete abortion bans following *Dobbs*). Of these states, 21 require only parental consent, 9 require only parental notification, and 6 require both notification and consent. The policies are still up for consideration in state legislatures. As recently as 2020, Florida passed a bill that changed their parental notification law to a parental consent law. Illinois had efforts to

Figure 2.2 PI Laws and the 15-17 Abortion Rate Over Time (1980-2013)



Source: Kost et al. (2017); Myers and Ladd (2020)

eliminate their parental notification statute appear for legislative consideration in 2019 and 2020.

Figure 2.2 demonstrates the strong correlation between the number of enforced PI laws and the declining abortion rate among minors, and a broad literature estimates the causal effects of these policies. Generally, studies fall into two categories: a national approach to determine the effects of PI laws across the entire country (or a large part of it), and a state-specific approach analyzing a policy change in one single state.

Among national studies, Ohsfeldt and Gohmann (1994) compare states with and without a PI law over a pooled sample from 1984, 1985, and 1988. Their outcome of interest is the ratio between the abortion rate of minors (15-17) and the abortion rate of older teens (18-19). They use the abortion rate for older teens to account for overall trends in the abortion rate within a state. Their analysis implicitly assumes that the abortion rate for older teens is independent of the abortion rate for minors, and the abortion rate for older teens acts as a control for overall statewide trends in the abortion rate. Using a linear regression with controls for abortion price proxies and abortion attitude proxies, they find that parental involvement laws reduce the adolescent (15-17) abortion rate within a state by roughly 18 percent. In a similar study controlling for state-level characteristics such as abortion attitudes, Haas-Wilson (1996) reports a similarly sized effect of these laws on the abortion rate for teens: a reduction of 13-25 percent among 15-19 year-olds. In a later work, Levine (2003) uses difference-in-differences and triple-difference designs and reports findings consistent with the earlier papers. Both Levine (2003) and Ohsfeldt and Gohmann (1994) also consider the effect of PI laws on birth rates for minors. Studying this outcome helps distinguish between two possible adolescent behavioral responses: increased use of contraception and abstinence resulting in fewer overall pregnancies, and the restricted access to abortive care resulting in a greater number of births. These two early papers, however, are not in agreement about the effects of PI laws on teen birth rates. While Levine's results indicate a reduction in the abortion rate for minors without a corresponding increase in the teen birth rate, Ohsfeldt and Gohmann find that PI laws increase adolescent fertility by 10 percent.

A significant drawback to these papers using early data from the 1980s and 1990s is the inability to identify teens that travel out of state to have an abortion. The data often come from national sources and surveys such as the CDC Abortion Surveillance Summaries, which did not report abortion by the state of *residence* until the mid-2000s. This limitation is particularly important in light of evidence that teens do travel out-of-state to have an abortion when they are facing parental involvement law restrictions (Cartoof and Klerman, 1986; Joyce and Kaestner, 1996).

In more recent work, Myers and Ladd (2020) exploit better county-level data and a measure of distance that a minor would have to travel to avoid a PI law to determine the effect of parental involvement laws on the teen birth rate. The authors confirm Levine's earlier result that PI laws in the 1980s and 1990s were not associated with higher teen birth rates. In more recent years, however, they find that these laws result in an increase in teen births of around 3 percent. This difference likely arose from the increased spread of PI laws making it more difficult for a teen to travel out of state to escape the law. They write that they are unable to provide a credible estimate of any effect of PI laws on the teen abortion rate because nationally reported data from the CDC and the Guttmacher Institute is too limited.

Joyce et al. (2020) use a synthetic control method over a group of 14 states to assess the impact of parental involvement laws on the abortion rate for minors. The authors estimate separate effects for the PI law in each state. Their results indicate that some states experience a statistically significant reduction in the abortion rate of minors and other states see no meaningful effect.

State-level policy analysis is fairly consistent with the national studies. Two studies consider the implementation of a parental notification law in Texas, reporting results of a 16 percent and 25 percent decrease in the abortion rate for minors (Joyce et al., 2006; Colman et al., 2008). In 2015, MacAfee et al. (2015) studied the New Hampshire notification law and reported a 47 percent decrease in the number of abortions performed on minors in New Hampshire, with 62 percent of this change being driven by a decrease in minors from Massachusetts traveling to New Hampshire to avoid the parental consent law there. The authors determine that the New Hampshire law resulted in a 19.3 percent decrease in the abortion rate among resident minors. Two papers also consider the parental notification law in Illinois, with one finding no apparent decrease in the abortion rate for minors compared to that of older teens, and the other reporting a small decrease in the portion of abortions performed on women under 20 (Ralph et al., 2018; Ramesh et al., 2016).

2.2.3 Notification and Consent

A much smaller literature considers any differential effects of parental notification and parental consent laws. The basic theory underlying our understanding of parental involvement laws suggests that parental consent laws should (at least weakly) reduce the abortion rate of minors relative to parental notification laws, since a parental consent law represents a greater marginal cost of an abortion. The findings in the literature, however, are quite mixed. An early study on this topic finds a counterintuitive result – parental notification laws reduce the abortion rate for minors more than parental consent laws (Tomal, 1999). This paper has a few limitations, including a small sample of states and the inability to account for interstate travel mentioned earlier. Using data from nearly all 50 states, New (2008) determines that parental consent laws reduce the abortion rate for minors by 18.7 percent, while notification laws reduce the abortion rate by only 5 percent. Two papers also determined no significant differential effect between parental consent and parental notification. Using a 2SLS estimation of abortion demand, Medoff (2007) reports no significant difference in the effects of parental consent laws and parental notification laws. Joyce (2010) exploits a natural experiment – the policy change from parental notification to parental consent in Arkansas. Using a difference-in-differences design between age groups within the state, Joyce reports no significant reduction in the abortion rate for minors compared to older teens following the policy change.

I contribute to this literature by providing an extension to the analysis of parental consent in Arkansas by Joyce (2010). This paper considers the effect of a policy change from parental notification to consent in seven states spanning the US South, Midwest, and West. Therefore this work contributes to the question of external validity of the natural experiment in Arkansas. In addition, this paper uses a different empirical method, the synthetic control, to estimate treatment effects. Because there is no general consensus on the marginal effects of parental consent laws, a variety of methodologies is useful to get closer to understanding any true effects. The use of synthetic control is particularly important in this context, as the dynamic nature of fertility choice implies that older teens may effectively be treated by PI laws in years following any policy change, and this limits their credibility as a control group.

2.3 Data

To determine the legislative history of a state, I use the legal coding developed by Myers and Ladd (2020). I divide states into a treatment and control group based upon their legislative history. States that change their law from parental notification to parental consent make up the treatment group, while states that maintain a consistent parental involvement law serve as the control. Table 2.1 provides a description of the treatment and control group.

Data on state-level abortion rates comes largely from the CDC abortion surveillance summaries. I supplement CDC data with state-level induced termination of pregnancy (ITOP) reports when ITOP data reports the age categories (15-17) necessary for my analysis. CDC and ITOP data are normally reported with raw numbers for abortions rather than abortion rates. Therefore, I use population estimates from the SEER database in order to impute an abortion rate (per 1,000 residents assigned female at birth in age category).

Abortion data from the CDC surveillance has limitations. Abortion counts from the CDC

Table 2.1 List of Treatment and Control States

Treatment		Control	
State	Law	State	Law
Arkansas	Notification: 1995-2004	Alabama	Consent: 1995-2016
	Consent: 2005-2016	Arizona	Consent: 2003-2016
Kansas	Notification: 1995-2010	Colorado	Notification: 2003-2016
	Consent: 2011-2016	Georgia	Notification: 1995-2016
Nebraska	Notification: 1995-2010	Iowa	Notification: 1995-2016
	Consent: 2011-2016	Illinois	Notification: 1995-2016
Ohio	Notification: 1995-2005	Indiana	Notification: 1995-2016
	Consent: 2006-2016	Kentucky	Consent: 1995-2016
Texas	Notification: 2000-2004	Massachusetts	Consent: 1995-2016
	Consent: 2005-2016	Maine	No Law: 1995-2016
Utah	Notification: 1995-2005	Michigan	Consent: 1995-2016
	Consent: 2006-2016	Minnesota	Notification: 1995-2016
Virginia	Notification: 1995-2002	Missouri	Consent: 1995-2016
	Consent: 2003-2016	Mississippi	Consent: 1995-2016
		Montana	No Law: 1995-2016
		North Carolina	Consent: 1996-2016
		New Jersey	No Law: 1995-2016
		New Mexico	No Law: 1995-2016
		Nevada	No Law: 1995-2016
		New York	No Law: 1995-2016
		Oregon	No Law: 1995-2016
		Pennsylvania	Consent: 1995-2016
		South Carolina	Consent: 1995-2016
		South Dakota	Notification: 1995-2016
		Tennessee	Consent: 2000-2016
		Vermont	No Law: 1995-2016
		Washington	No Law: 1995-2016
		Wisconsin	Consent: 1995-2016
		West Virginia	Notification: 1995-2016

Source: Myers and Ladd (2020)

come from voluntary reports from state health departments, and there have been demonstrated inconsistencies between the abortion surveillance summaries and clinic survey counts of abortion incidence from the Guttmacher Institute (Joyce et al., 2020). In particular, CDC counts are often underreported relative to Guttmacher surveys. To compensate for the limitations of abortion count data available, I also estimate effects of the policy change on birth rates among minors. The birth data from the National Vital Statistics System Natality Reports contain more credible reports of birth counts by age, and therefore may be better suited to measuring the fertility effects of a parental consent law.

2.4 Methods

2.4.1 The Synthetic Control

The synthetic control method (SCM) is an empirical strategy that is often used in comparative case study frameworks with a potentially small sample of data. Synthetic control allows researchers to identify the effects of policy interventions at the state/regional level when a control group for the area is not obvious. Instead of comparing one treated unit to one untreated control unit, the treated state is compared to a weighted average of several potential control states.

Following Abadie et al. (2010), the method an be thought of as a generalization of the difference-in-differences method commonly used in linear panel data settings. Define $\alpha_{it} = Y_{it}^I - Y_{it}^N$ to be the treatment effect for unit i at time t. Y_{it}^I is the outcome of interest in the presence of intervention, and Y_{it}^N is the outcome of interest absent intervention – the counterfactual. Then, the observed outcome for unit i at time t may be written as

$$Y_{it} = Y_{it}^N + \alpha_{it} D_{it}$$

where D_{it} is an indicator for the policy intervention. Since the counterfactual outcome Y_{it}^N is never observed when $D_{it} = 1$, suppose that it can be represented by a factor model

$$Y_{it}^N = \delta_t + \theta_t Z_i + \lambda_t \mu_i + \epsilon_{it}.$$

Here, δ_t is an unknown common factor, Z_i is an observed set of covariates, θ_t is a vector of unknown parameters, λ_t is a set of unobserved common factors, and μ_i is an unknown vector of factor loadings. The $\lambda_t \mu_i$ term separates synthetic control from the usual difference-in-differences. While difference-in-differences assumes that unobserved confounders are constant across time, this method does not. So, synthetic control allows for unobserved *time-varying* confounders to exist.

Since Y_{it}^N is not observed, it is estimated through a pre-treatment period matching process. I select a relevant set of matching characteristics and outcomes for both the treated unit and the set of controls. Then, a set of weights W is generated such that any differences between the treated unit and the weighted controls are minimized, only considering the pre-intervention period. Following the work of Klößner and Pfeifer (2018), I use only lagged dependent variables in order to construct the weights,

$$W_1 = \text{argmin}_{w_j^1 \epsilon [0,1]} \sum_{t=t_0-5}^{t_o-1} (Y_{1t} - \sum_{j=2}^{J+1} w_j^1 Y_{jt})^2,$$

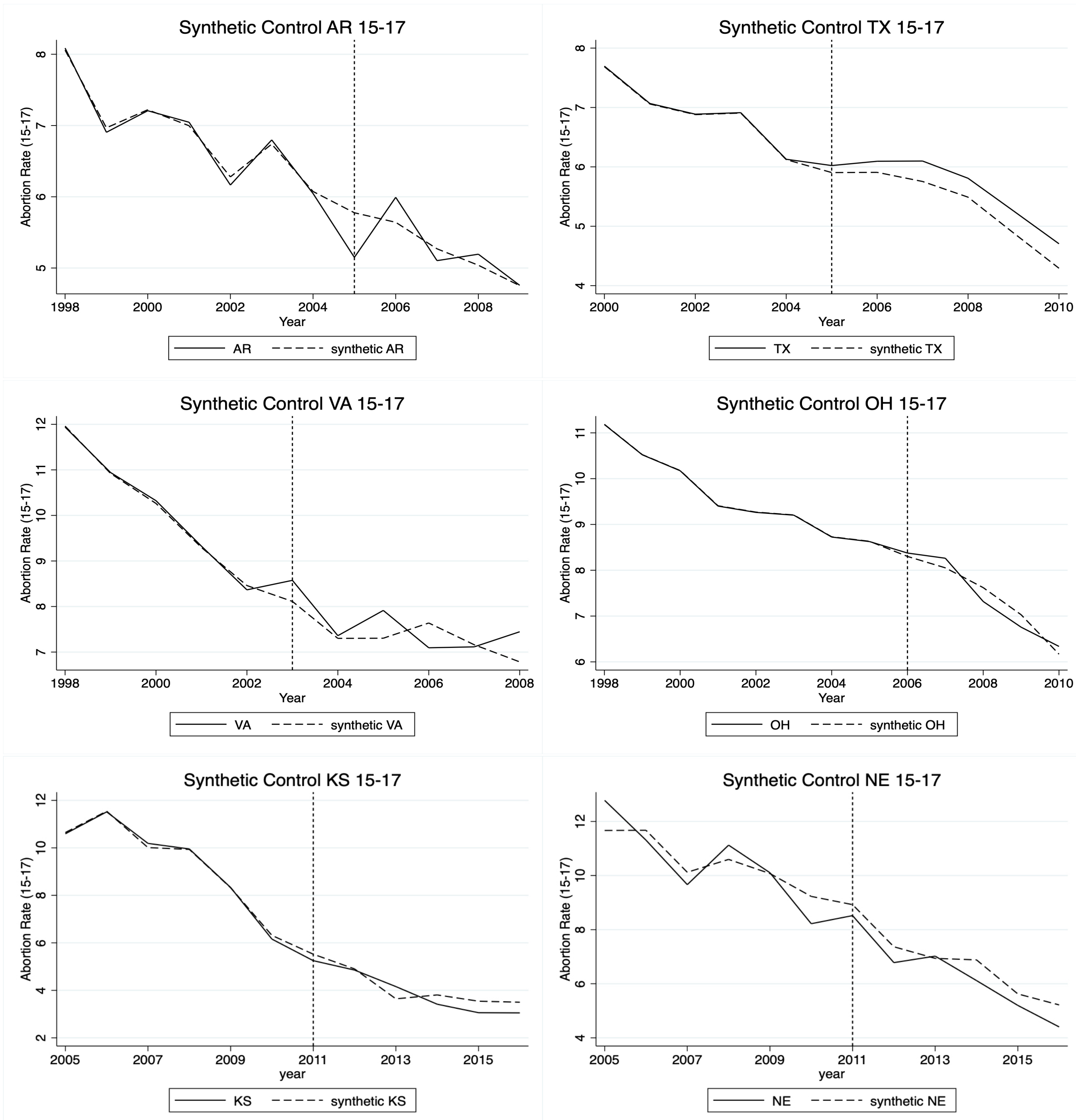
where unit 1 is the treated unit and five pre-treatment time periods are used. The central idea is that this weighted average of the control states is close to identical to the treated unit. Therefore, it will serve as a good estimate of the counterfactual. This leads to the treatment effect estimator presented in Abadie et al. (2010)

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}.$$

Figures 2.3a and 2.3b show the visual results from the synthetic control for the six treated states for both the 15-17 abortion rate and birth rate.

Figures 2.3a and 2.3b provide information about the quality of the synthetic control match and the general direction of the treatment effects. In the pre-period, the abortion/birth rate trends for the treated states and their synthetic control group appear similar, and this supports the assumption that the synthetic control group estimates a counterfactual in the post-period. Post-period differences in the abortion rates for the treated states and their synthetic control group represent treatment effects $\hat{\alpha}_{it}$. Post-period trends in the abortion rate for minors in Figure 2.3a generally do not indicate that there are substantial differences between a treated state and its synthetic control group. The largest treatment effect, $\alpha = 0.29$, of a parental consent law on the abortion rate for minors occurs in Texas, and represents a small 3% increase from the pre-period rate. Generally effect sizes range from 0.2% to 3% changes from the pre-period, and the direction of the treatment effect is heterogeneous across states. A similar pattern exists for effects of the parental consent law on the birth rate for minors - effect sizes range from a 1% to 4% change from the pre-period average with no consistent direction. The results for the abortion and birth rate of minors taken together suggests that the marginal effect

Figure 2.3 Synthetic Control for the Abortion and Birth Rate of Minors

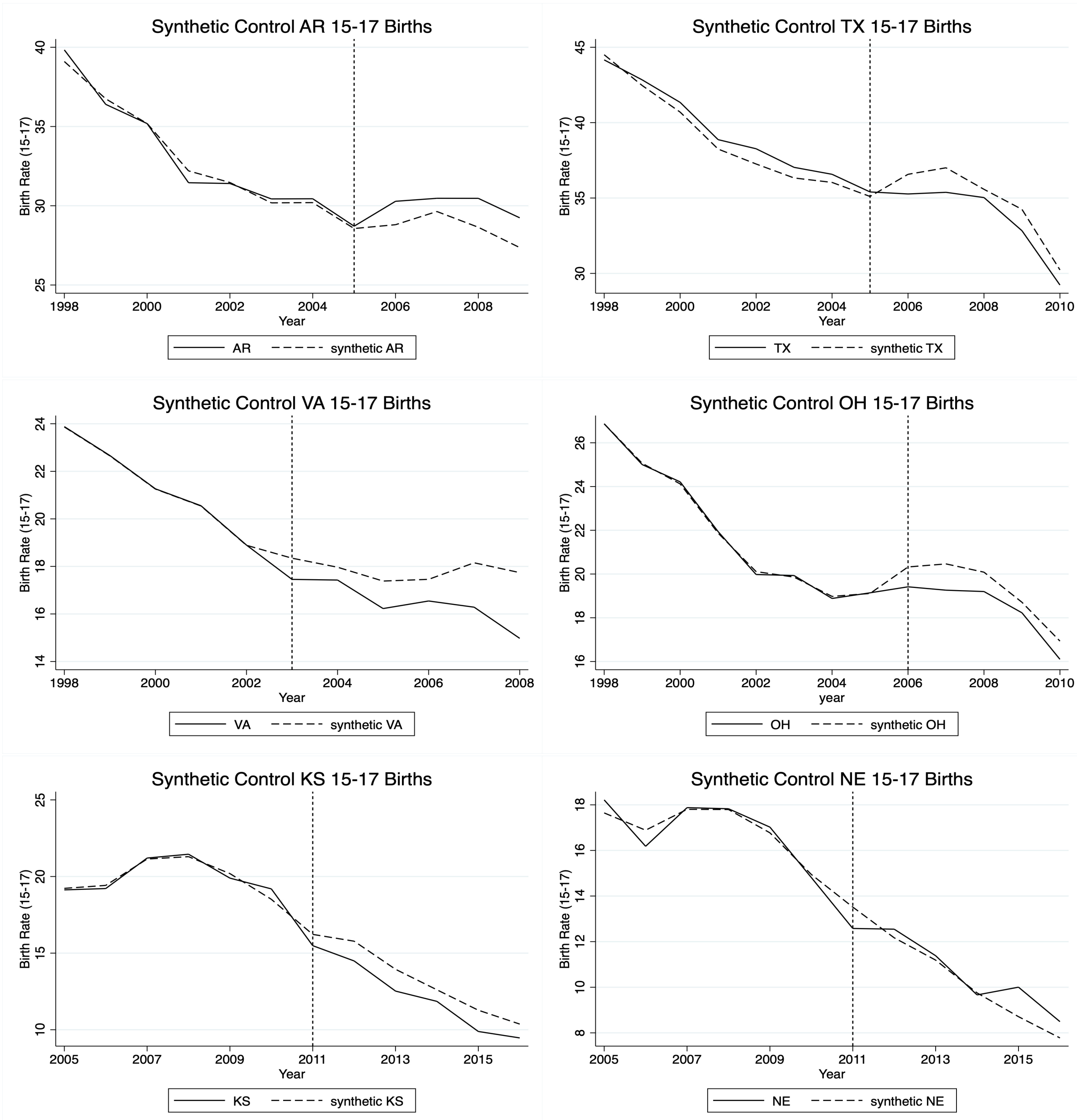


(a) Abortion Rate (15-17)

of a parental consent law is limited. A full description of the make up of the synthetic control group for each outcome and treated state is presented in the appendix.

A notable requirement for developing a synthetic control group is that the outcomes in the treated state that are used in the matching process must lie in the convex hull of the control state outcomes. In other words, the trends in the donor pool of control states must contain values that

Figure 2.3 (cont'd)



(b) Birth Rate (15-17)

are above and below the trend in the treated state. If this condition is not met, a good synthetic control match using the standard method cannot be attained. Although the state of Utah qualifies as a treated state, because they changed their parental notification law to a parental consent law in 2006, the abortion rate for minors in the pre-period (the characteristics used to match) does not sit in the convex hull of the abortion rate for minors in the control states. For this reason, I exclude

Utah from the analysis.

2.4.2 Inference

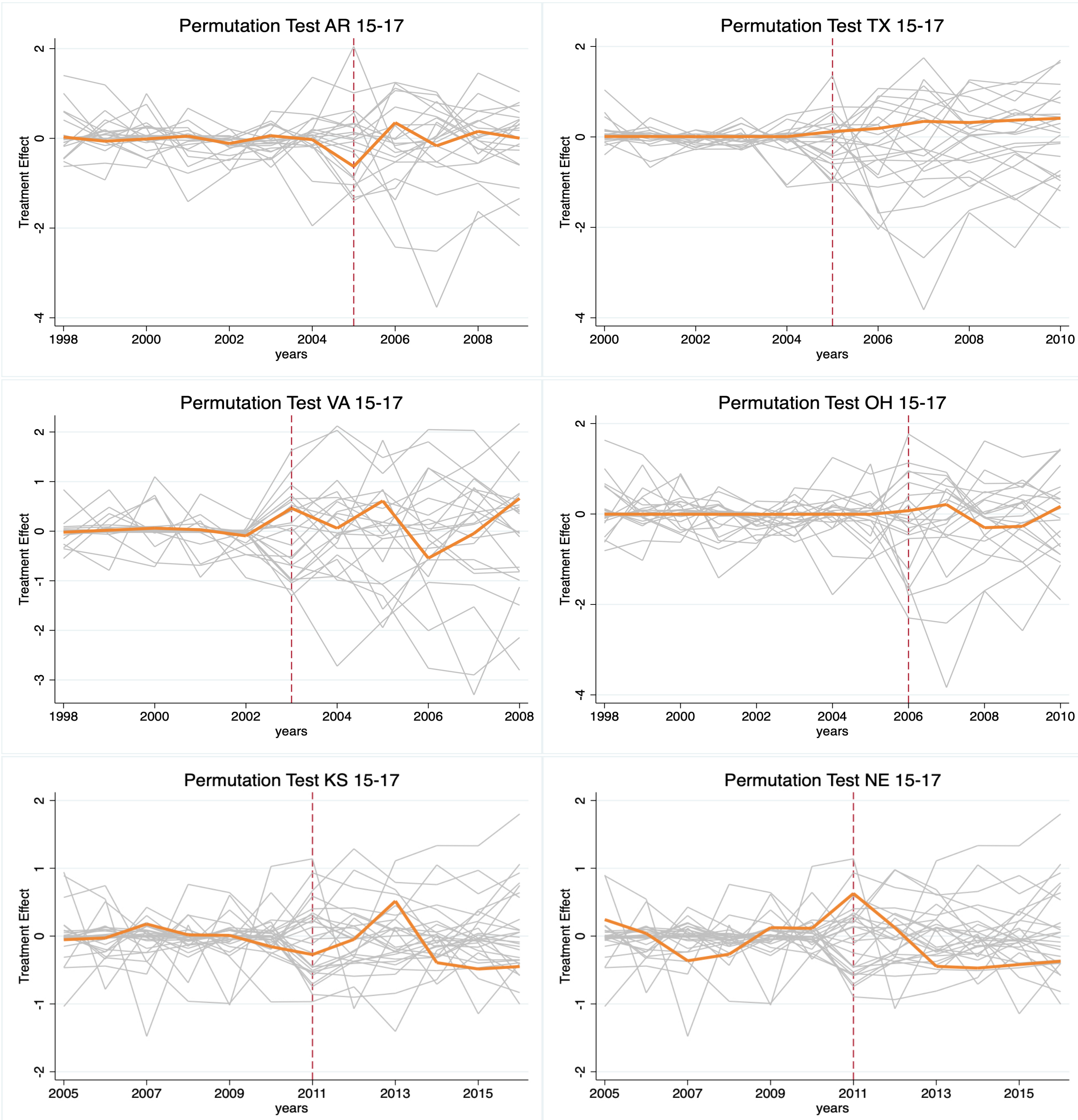
Standard in the synthetic control method, I use placebo tests for permutation inference. For each treated state, I generate a set of placebo effects by repeating the SCM procedure on the pool of control states as if they were treated at the time of the policy change. From this permutation inference, I can view the effect size of the policy in the treated state relative to a state chosen at random. Figures 2.4a and 2.4b present the placebo tests for the abortion/birth rate of minors. These graphs present the difference between the abortion or birth rate in a given state and its synthetic control group. When the synthetic control match in the pre-period is poor for one of my placebo states, it is eliminated from the graph and analysis. If the synthetic control match for a control state is poor in the pre-period, its trend in the post period (the placebo effect) is not very informative.

To determine the statistical significance of any effect, it is common to use a percentile rank statistic that has a similar interpretation to the parametric p-value used in regression analysis. I calculate the percentile rank statistic based upon the average treatment effect in the post-period $\bar{\alpha}_1 = \frac{1}{s}\sum_{t=t_0}^{t_0+s}\alpha_{1t}$. The percentile rank statistic will be $p_1 = \hat{F}(\bar{\alpha}_1)$, where $\hat{F}$ is the empirical CDF of the average placebo effects $\bar{\alpha}_j$ from the control group[1]. Percentile rank statistics around 0.5 indicate that the treatment effect lies near the middle of the distribution of placebo effects, as is the case for the permutation test for the abortion rate of minors in Ohio pictured in Figure 2.4a ($p = 0.5$). This may be evidence that whatever treatment effect we observe in that state could be due to random variation in the abortion rate. Small percentile rank statistics indicate that the treatment effect lies toward the extreme values of the placebo distribution. This is the case in the permutation test for the birth rate of minors in Arkansas pictured in Figure 2.4b ($p = 0.16$). A full summary of treatment effects and percentile rank statistics is presented in the Results section in Tables 2.3 and 2.4.

To aggregate information from multiple treated units, I use the pooling method presented by Dube and Zipperer (2015). The pooling method first requires that permutation tests be performed and the percentile rank statistics of each treated state be calculated. Under the null hypothesis

[1]Following the method described by Dube and Zipperer, I also use the Weibull-Grumbel rule: $p_1 = \frac{r_1}{N+1}$, where r_1 describes the rank of the treatment effect, and N is the number of control states.

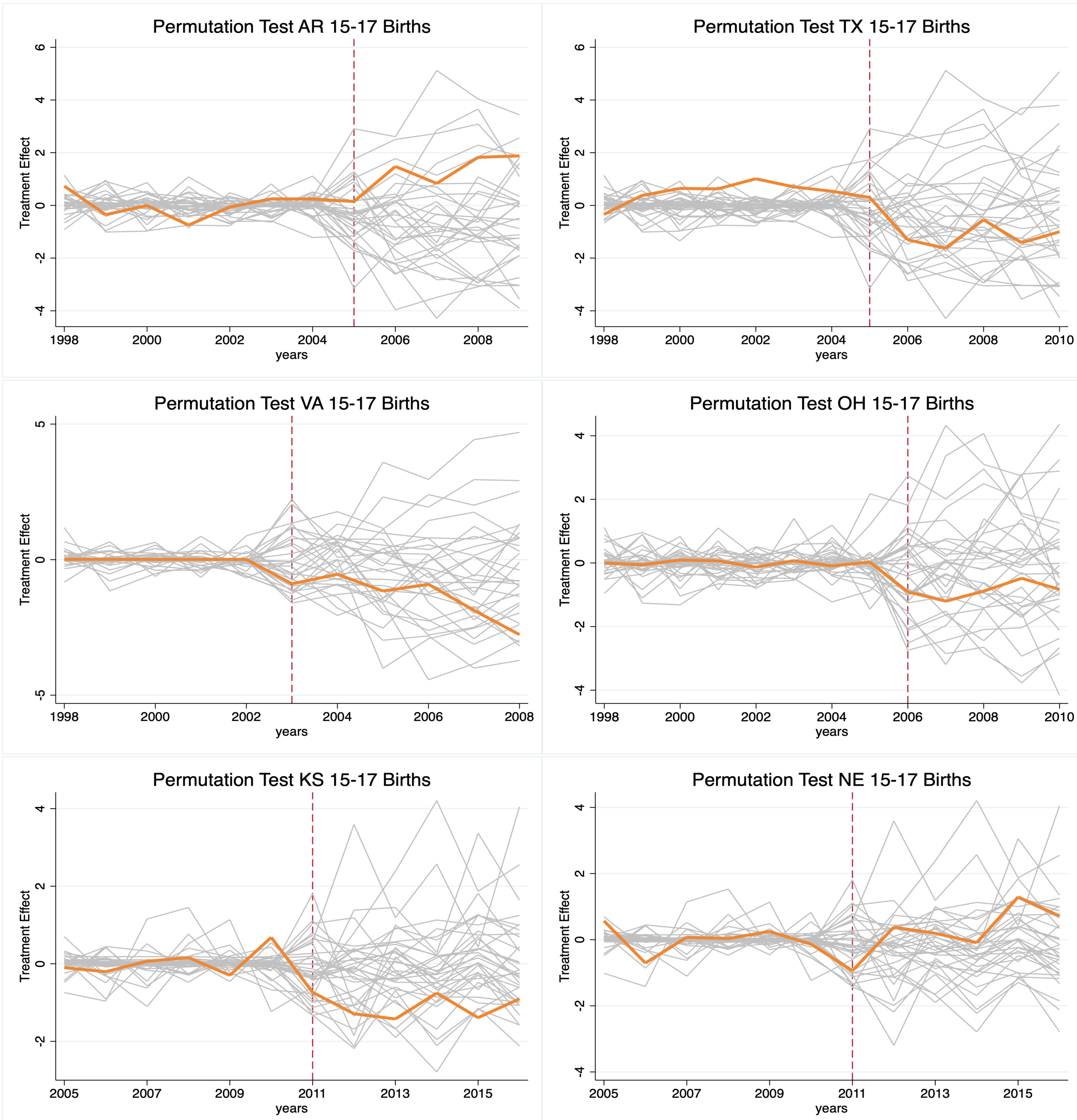
Figure 2.4 Permutation Tests for the Abortion and Birth Rate of Minors



(a) Abortion Rate (15-17)

that the policy intervention has no effect, these percentile ranks should be random draws from the Uniform[0,1] distribution. So, while the null hypothesis may not be rejected in any treated state individually, we could consider whether or not these percentile ranks from several treated units reasonably represent consecutive random draws from the uniform distribution. To do this, the percentile rank statistics from the treated units are pooled together into a simple average $\bar{p}$. Then,

Figure 2.4 (cont'd)



(b) Birth Rate (15-17)

I use the Irwin-Hall distribution of the sum of independent uniform random variables to test the hypothesis that $\bar{p}$ is distributed with mean 0.5.

2.5 Results

I select two possible groupings for pooling analysis. In one grouping, I pool all of the treated states together to get an overall sense of the effect of the policy intervention. Following the

Table 2.2 Treatment Effect for Abortion Rate of Minors (15-17)

	Treatment Effect	Pre-Period Average	*p*
Early States:			
Arkansas	-0.06	7.46	0.41
Texas	0.29	9.36	0.63
Virginia	0.20	11.83	0.67
Ohio	-0.02	10.09	0.50
Late States:			
Kansas	-0.19	13.21	0.39
Nebraska	-0.16	6.89	0.37

Table 2.3 Treatment Effect for Birth Rate of Minors (15-17)

	Treatment Effect	Pre-Period Average	*p*
Early States:			
Arkansas	1.50	36.70	0.16
Texas	-0.93	42.31	0.64
Virginia	-1.45	23.81	0.74
Ohio	-0.86	24.24	0.66
Late States:			
Kansas	-1.08	22.53	0.91
Nebraska	0.26	18.75	0.31

observations in Joyce (2020), my second grouping is based on the timing of the policy. Joyce observes that states that pass their PI law earlier see a larger effect size. So, I divide my states into early treatment (2003-2006) and late treatment (2011) to see if my results are also consistent with this observation.

Tables 2.2 and 2.3 report the average treatment effect and percentile ranks from the placebo tests for minors and older teens. The treatment effect is the simple average of the difference between the abortion rate in the state and its synthetic control group in the post-treatment period. The percentile rank corresponds to the alternate hypotheses for the group. The rank for a state when considering the abortion rate for minors describes the proportion of placebo effects that are at or below the treatment effect (because the alternate hypothesis is that the treatment *reduces* the abortion rate for minors), while the rank considering the birth rate for minors describes the proportion of placebo effects that are at or above the treatment effect (because the alternate hypothesis is that the treatment *increases* the birth rate for minors).

Simply from the treatment effects and percentile ranks, it does not appear that the implementation of a consent law has a very large effect on the abortion rate for minors. The treatment effects also do not operate in a consistent direction across states. Arkansas, Ohio, Kansas, and Nebraska have negative treatment effects, indicating that the policy change may reduce the abortion rate for minors. But, there is not evidence that any of these effects are statistically different from zero. While Texas and Virginia have surprising positive treatment effects, the effect sizes are small (3.1% and 1.7% change from the pre-period average respectively) and still lie toward the center of the distribution of placebo effects.

The effects of the policy change to parental consent on the birth rate of minors exhibit a similar pattern. Treatment effects are generally small, not statistically significant from placebo inference, and do not operate in any consistent direction. It is interesting to note that the direction of the treatment effects on birth rates do not directly correspond to the direction of the effects on abortion rate. We may expect a policy that decreases the abortion rate for minors will increase the birth rate and vice-versa, but this is not the case in the analysis presented. This could be further evidence that post-period differences in the abortion rate between the treated states and their synthetic control group are due to random variation unrelated to the policy change. Overall, results from the synthetic control on individual states do not support a conclusion that the marginal cost of a parental consent law has large fertility effects for minors. To observe average effects across all treated units, I use the pooling analysis described in the previous section.

2.5.1 Pooling Inference

Tables 2.4 and 2.5 describe the results from pooling. The average treatment effect here is the simple average of effects for the group in question – a kind of average of averages. The value for $\bar{p}$ comes from the simple average of percentile ranks within the group. The "p-value" comes from testing the hypothesis that the values for p within the group are n independent random draws from U[0,1] using the Irwin-Hall statistic.

Results of the pooling analysis are consistent with the observations made from the state-level treatment effects and percentile rank statistics. There is no evidence of a significant negative effect

Table 2.4 Pooling Results for the Abortion Rate of Minors (15-17)

	Average Treatment Effect	$\bar{p}$	p-value
Early States (n=4)	0.103	0.553	0.637
Late States (n=2)	-0.175	0.380	0.259
All States (n=6)	0.010	0.495	0.483

Table 2.5 Pooling Results for the Birth Rate of Minors (15-17)

	Average Treatment Effect	$\bar{p}$	p-value
Early States (n=4)	-0.435	0.550	0.631
Late States (n=2)	-0.410	0.610	0.696
All States (n=6)	-0.427	0.570	0.719

of the policy change among minors. Effects on the abortion rate for minors in Table 2.4 are different across early and late adopting states. For early adopting states, average differences between the treated unit and its synthetic control group is equivalent to 0.103 additional abortions per 1,000 AFAB[2] residents per year. For later states, average differences are 0.175 fewer abortions per 1,000 AFAB residents per year. Neither of these treatment effects, however, are statistically different from zero. For the birth rates in Table 2.5, effects across early and late states are nearly indistinguishable.

2.6 Discussion

A straightforward interpretation of the results would suppose that there is no marginal effect of a parental consent law on fertility outcomes for minors because the additional cost of parental consent is small. In this sense, the barriers to abortion access are driven by broad parental involvement and not dependent on the specific nature of the PI law. I propose an additional potential mechanism behind these null effects where an institutional feature of parental involvement, the judicial bypass option, mitigates barriers to abortion access.

2.6.1 The Judicial Bypass

The judicial bypass option allows minors to petition the court at no financial cost for access to an abortion without meeting the parental involvement requirement. Joyce (2010) describes the relative importance of the juidical bypass option for minors seeking abortion care. In Arkansas,

[2] AFAB = assigned female at birth

roughly 10% of minors who received an abortion did so using the judicial bypass. The statutory standards for a judicial bypass are fairly consistent across states. A judge may grant a minor access to an abortion without parental involvement if one of the following criteria are met:

1. The judge determines that the minor is mature enough to make their own reproductive choices.
2. The judge determines that the minor may be in immediate danger by seeking to satisfy the parental involvement requirement.
3. The judge determines that the abortion would be in the best interest of the minor.

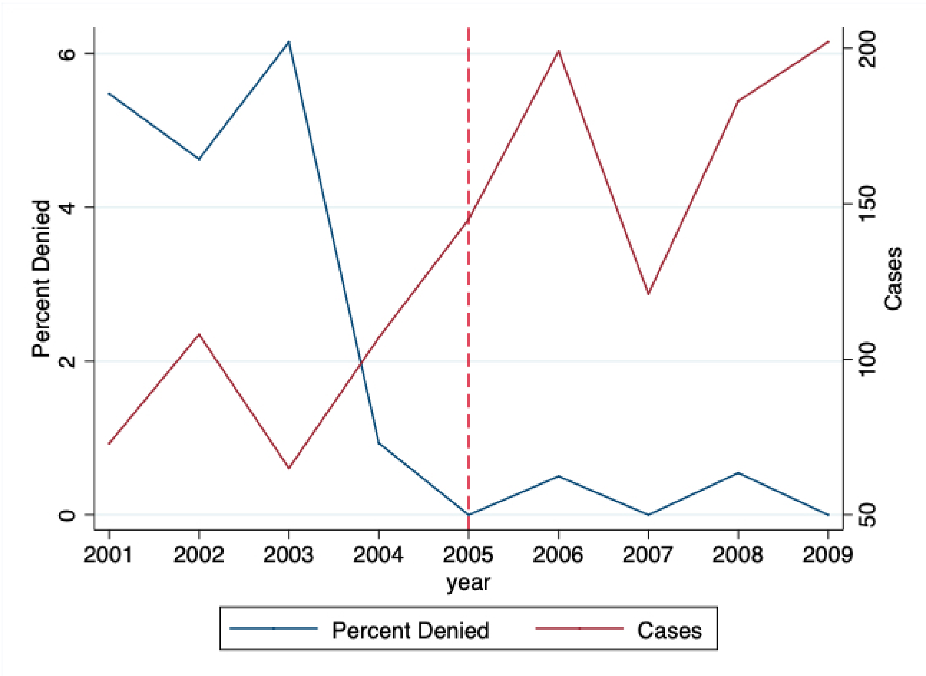
Note that this set of criteria is quite subjective. Particularly the first and third item, which require the judge presiding to use their personal judgment to assess the case. The subjective nature of the judicial criteria, however, implies that the generosity of the judicial bypass may change in response to a more restrictive parental involvement law. Judges who believe that a law is too restrictive have the ability to grant additional judicial bypass waivers.

Data regarding the judicial bypass is difficult to come by. Generally, the records for such court proceedings are sealed by law. The best evidence to describe the generosity of the judicial bypass comes from state-level non-profit organizations that assist minors in seeking the option. One such organization is Jane's Due Process (JDP). Based in Texas, JDP collects their own data on the number of cases judicial bypass cases that they refer to an attorney, and how many of these cases result in a judicial bypass waiver.

Though this is just observational data, a much smaller percentage of JDP judicial bypass cases were denied following the change from parental notification to parental consent in Texas in 2005. Additionally, the JDP was sending a larger number of judicial bypass cases to the courts after 2005. This evidence, though limited, demonstrates the plausibility that the judicial bypass option became more generous in Texas in response to the parental consent law.

If this trend in the generosity of the judicial bypass procedure exists broadly following parental consent legislation, it may mitigate additional barriers to abortion access imposed by the more stringent PI requirement. While some minors may be prevented from accessing an abortion due

Figure 2.5 JDP Cases and Denials 2001-2009



Source: Stevenson et al. (2020)

to the new parental consent law, other minors may benefit from the additional generosity of the judicial bypass. These effects together may help explain the null effects of the policy change on the abortion and birth rate for minors. Further research into the generosity of the judicial bypass across states and the nature of judicial bypass recipients is needed to confirm the presence of this treatment mechanism.

2.7 Conclusion

Overall, this research suggests that there is not evidence to support a differential effect between parental notification and parental consent laws on the abortion rate (and birth rate) for minors (15-17). The evidence supports a conclusion that legislative shifts from parental notification to parental consent are unlikely to be a primary driving force behind the the wide variation in the abortion rate for minors across the United States.

This study also provides information regarding the external validity of the effect of parental consent in Arkansas presented in Joyce (2010). In this paper, I study the effect of a policy change from notification to consent in six states across the US South and Midwest, and I observe results consistent with Joyce's finding that there is no evidence of a substantial marginal effect of a parental consent law on the abortion rate for minors. I use an empirical methodology that does not rely on comparisons between minors and older teens (18-19), limiting potential bias introduced due to the dynamic nature of fertility choice. In addition, I provide some descriptive evidence that the generosity of the judicial bypass procedure may be affected by strict parental involvement requirements. This may be an important mitigating factor in explaining the null effect of a parental consent law.

The primary limitation of this study is the quality of the abortion count data. Systematic changes in reporting behavior across states could potentially mask real effects of the policy change, resulting in a false null effect. To address this limitation, I provide complementary analysis of the effects of a shift from notification to consent on the birth rate for minors. I find consistent results that demonstrate a lack of evidence to support the conclusion that the policy change has strong effects for birth rates as well as abortion rates, but understanding birth effects does not entirely compensate for the limitations in estimating abortion effects. True effects on the abortion rate may be too small to cause significant birth effects, and changing contraceptive and sexual behavior among minors following the policy change may diminish upward pressure on the birth rate driven by restricted abortion access. Multiple initiatives currently exist to collect regular high-quality data on abortion counts across the US, including the "#WeCount Project" from the Society of Family Planning ("SFP", 2022). As more of this information becomes available, higher quality estimates of the effects of public policy on abortion rates become possible.

CHAPTER 3

THE EFFECTS OF RESTRICTED ABORTION ACCESS ON IUDS, CONTRACEPTIVE IMPLANTS, AND VASECTOMIES: EVIDENCE FROM TEXAS

3.1 Introduction

In 2022, the United States Supreme Court transferred the power to regulate abortion access to individual states in the landmark case *Dobbs v. Jackson Women's Health Organization*. This decision ultimately ruled that the U.S. Constitution does not guarantee the right to abortion, overturning *Roe V. Wade* (1973) and *Planned Parenthood v. Casey* (1992). In anticipation of the ruling, thirteen states enacted "trigger" laws to ban or limit abortion immediately after the repeal of *Roe*. In the months that followed the *Dobbs* decision, many states created laws to restrict abortion access or prohibit abortion entirely. In the one hundred days following *Dobbs*, 66 abortion clinics closed across 15 states (Kirstein et al., 2022), creating substantial barriers to abortion access across the United States.

Limited access to abortion could affect fertility and sexual behavior through multiple channels. For instance, reduced access to abortions may lead to increases in births, particularly for those who would have chosen abortion if the option were available. Alternatively, restricted abortion access may lead to fewer births through either decreases in sexual activity or increases in contraceptive use. Prior research finds that restricted abortion access affects birth rates (Kane and Staiger, 1996; Myers and Ladd, 2020; Myers, 2021b; Jones and Pineda-Torres, 2021; Fletcher and Venator, 2019; Guldi, 2008) and sexual behavior (Colman et al., 2013; Klick et al., 2012). However, research on how changes in abortion access affects contraceptive use is limited.

In this paper, we study how restricting access to abortion affects the demand for long-acting contraceptives. In 2013, Texas implemented House Bill 2 (HB2), a regulation on abortion providers that shuttered over half of all abortion clinics in the state. This resulted in a significant change in the distance to an abortion provider for many Texas residents. We pair this variation in the abortion facility distance with administrative outpatient records from hospitals, hospital-owned facilities, and ambulatory surgical centers (ASCs) to compare trends in the incidence of intrauterine devices

(IUDs), contraceptive implants, and vasectomies.

We find that overall hospital-based long-acting reversible birth control (LARC[1]) incidence rose in Texas between 2011 and 2015, and trends peak around the introduction and passage of HB2. In counties that experience a greater-than-30 mile increase in their distance to an abortion provider, LARC incidence increases at a significantly higher rate around the time of the policy change. The increases in LARC within the treated counties is primarily driven by IUD insertions, the most popular long-acting contraception method in our sample.

This paper contributes to the literature on the effects of restrictive abortion policies. In addition to the aforementioned effects on fertility and sexual behavior, researchers identify detrimental socioeconomic and health outcomes resulting from decreased access to abortion. Previous studies find poor pregnancy-related health outcomes, decreases in college-going behavior, and decreases in future family income (Farin et al., 2021; Jones and Pineda-Torres, 2021; Miller et al., 2020).

We also add to the literature on contraceptive use and family planning. Much of the previous literature uses increased access to contraceptives to examine how contraceptives decreased family size and improved educational and labor market outcomes for women by giving them more control over the timing and frequency of births (Bailey, 2010; Goldin and Katz, 2002; Bailey, 2006). Further, researchers find that access to contraceptives lead to a decrease in the share of children born to economically disadvantaged households and a decrease in the number of children with a low birthweight (Ananat and Hungerman, 2012). Bailey (2013) finds long-run changes in educational attainment and labor supply for children whose parents had access to contraceptives. However, much of this literature focuses specifically on one type of contraceptive: the oral contraceptive pill. Our analysis adds to this work by studying the take-up of LARC, which entered the market in the 2000s and provided more effective protection against pregnancy. Lindo and Packham (2017) also examine LARC take-up, but their research uses expanded access to LARC rather than contracted access to abortion as their main source of variation.

Two papers measure the effect of abortion access on contraceptive choices. Miller and Valente

[1]Long-acting reversible birth control includes hormonal and non-hormonal IUDs and subdermal contraceptive implants

(2016) consider the rapid expansion of legal abortion access in Nepal, finding evidence that abortion and contraception are substitutes. Felkey and Lybecker (2017), however, find no effect of state abortion restrictions in the United States on the reported contraceptive method of choice among reproductive-age women. Our research therefore contributes to the understanding of the take-up of new contraceptive technologies resulting from restricted access to abortion services.

Finally, our research adds to the literature evaluating the effects of HB2 and similar legislation in Texas. Prior research shows that changes in the distance to the nearest abortion provider reduced abortion rates and increased birth rates within the state (Lindo et al., 2019). Additionally, two studies examine the relationship between Texas legislation and contraceptive use. The first find a correlation between Texas legislation that excluded Planned Parenthood from Medicaid reimbursement and decreases in LARC and injectable contraceptive utilization among Medicaid recipients (Stevenson et al., 2016). Additionally, Fischer et al. (2018) find no change in emergency contraceptive purchases or condom sales due to HB2.

The remainder of this paper proceeds as follows. Section 2 explains the history and background including a description of LARC and the policy environment that generates our source of variation. Section 3 describes the data and Section 4 presents our methods and results. Section 5 concludes.

3.2 History and Background

3.2.1 Long-Acting Reversible Contraception

IUDs are flexible, T-shaped devices placed in the uterus by a physician. There are two main categories including hormonal and non-hormonal IUDs. Both types prevent fertilization by decreasing sperm motility. The non-hormonal IUDs, such as Paragard, are wrapped in copper and can protect from pregnancy up to 12 years. Hormonal IUDs include products such as Mirena and Kyleena and can protect from pregnancy between 3 and 8 years, depending on the product. Similarly, subdermal inplants are small, flexible rods that are inserted into the upper arm. The implants, such as Nexplanon and Implanon, are also hormonal products and are effective for up to 5 years.

Although IUDs were introduced shortly after the oral contraceptive pill, they received significant negative press following a series of studies in 1974 that linked IUDs to pelvic inflammatory disease (Sonfield, 2007). Modern IUDs were introduced in 1988 (Paragard) and 2001 (Mirena). Although take-up increased in recent years, it remains lower than the pill at 12.9 percent compared to 21.4 percent of contraceptive users (Guttmacher, 2022). The first contraceptive implant, Norplant, was introduced in the US in 1991 and was met with considerable demand. However, women began experiencing unpleasant side-effects such as irregular menstrual bleeding which led to discontinued use (Fraser et al., 1998) and sales were suspended in 2002 due to manufacturing concerns. Implanon was introduced to the US market in 2006 and is used by 3.1 percent of contraceptive users.

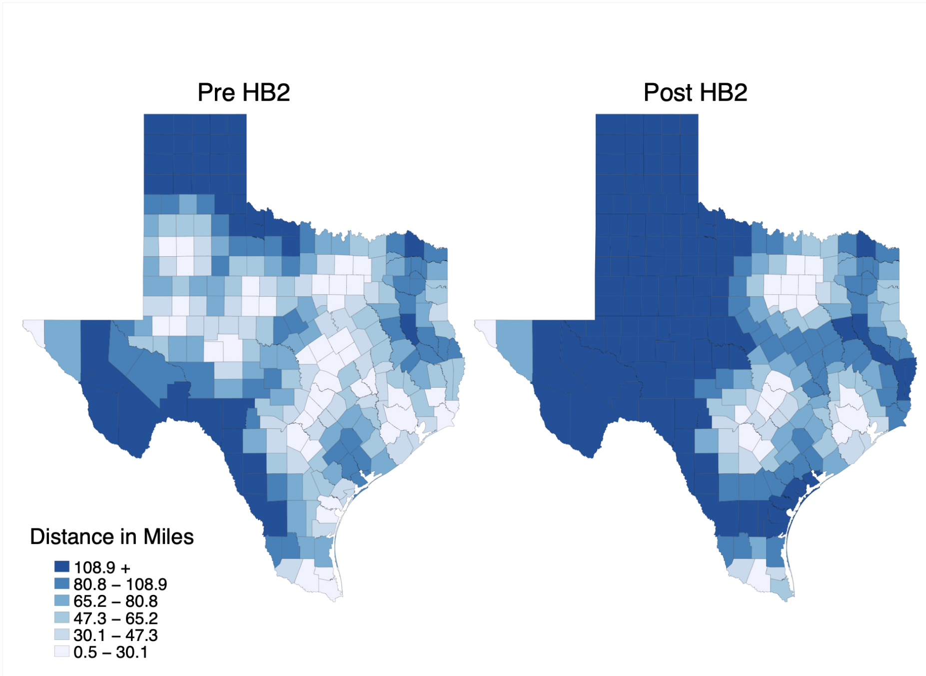
The ease of use and effectiveness of both types of LARCs may make them particulalry attractive to individuals facing increased barriers to abortion access. LARCs are highly effective at preventing pregnancy and less than 1 in 1000 using an IUD or implant become pregnant with typical use. In contrast, condoms are 87 percent effective and the oral contraceptive pill is 93 percent effective at preventing pregnancy with typical use. Further, while oral contraceptives and condoms are subject to user error, LARCs are both inserted and removed in a physician's office and therefore require very little effort on the part of the user. However, LARC insertion and removal can be painful, and users may experience side effects including irregular bleeding, spotting, and mood changes (Lindo and Packham, 2017).

3.2.2 Legislative Framework

In July 2013, the Texas legislature passed House Bill 2 (HB2), which significantly limited abortion access within the state. The bill was aimed at abortion providers and required that clinics meet the standards of an ambulatory surgical center, that doctors performing abortions have admitting privileges at a hospital within 30 miles of the clinic, and that individuals taking abortion-inducing medication have medical oversight. In addition, HB2 prohibited abortions 20 weeks post-fertilization.

HB2 was the final product of a highly-contentious debate regarding abortion access during the eighty-third Texas legislature. After multiple abortion bills were introduced and failed during the regular legislative session, Governor Rick Perry ordered a first special session to begin in late May

Figure 3.1 Travel Distance to an Abortion Provider in Texas



Source: Myers (2021b)

2013. During this special session, the Texas Senate introduced Senate Bill 5 (SB5), a bill containing many details of the abortion bills that failed to pass in the regular session. When SB5 failed to pass, Governor Perry instituted a second special session to begin in July. SB5 was introduced again as HB2, which passed both houses and was signed into law on July 18, 2013. Because it is essentially identical to SB5, we consider June 2013 to be the original introduction date of HB2, and this is the date of treatment for our analysis.

HB2 involved multiple targeted regulations on abortion providers, or TRAP laws. TRAP laws often burden clinics to the point of closure, and after HB2 went into effect, the number of abortion clinics in Texas dropped significantly. In 2011, Texas had 42 operating abortion facilities. By the end of 2014, only 17 remained. The clinic closures increased the burden of abortion access by inducing large increases in the distance that residents of Texas must travel to meet an abortion provider, as shown in Figure 3.1. Before HB2, a majority of Texas residents lived within 100 miles of an abortion provider, and many lived closer. After the implementation of HB2, clinic access was concentrated in the metropolitan areas, and residents of the western portion of the state were required to travel over 100 miles to access abortion care.

3.3 Data

To measure changes in the incidence of LARC and vasectomies in Texas, we use data from the 2011-2015 Texas Health Care Information Collective Outpatient Public Use Data Files (Texas Department of State Health Services, 2023). The files contain outpatient discharge data from nearly all[2] licensed hospitals, hospital-owned outpatient facilities, and ambulatory surgical centers in the state, representing 782-956 facilities in each quarter-year. We identify cases of IUD insertion, IUD removal with reinsertion, contraceptive implant insertion, and vasectomy procedures using relevant CPT procedure codes. Ultimately, we identify 8,379 IUD insertions/reinsertions, 3,325 contraceptive implant insertions, and 6,314 vasectomies. Table 3.1 summarizes the data for LARC and vasectomy recipients.

Descriptive statistics in Table 3.1 indicate that IUD implantations make up the majority (about 72%) of LARC cases, which aligns with the national share of IUD users. Between 2011 and 2013, 10% of women who used contraception reported using an IUD and only 1% reported using a contraceptive implant (Kaiser Family Foundation, 2019). Further, Table 3.1 shows stark differences in some demographic features of LARC and vasectomy recipients in our sample. LARC recipients are generally younger, with a mean age in the range of 25 to 29, while vasectomy recipients average between 35 and 39 years old. The primary payer for LARC is overwhelmingly Medicaid, which pays for less than 1% of all vasectomy procedures. Instead, commercial insurance pays for the bulk of vasectomies in the data. These differences suggest that Texas residents seeking LARC and vasectomies are quite different beyond the expected differences in gender composition. Therefore, we may expect heterogeneous responses to public policy decisions related to abortion access between these groups.

Figure 3.2 demonstrates how the incidence of LARC and vasectomies evolved over the study period. The raw trends in IUD and contraceptive implant cases experience a sharp increase in incidence that coincides with the original introduction of HB2 in June 2013. In the time leading

[2]Exceptions include: facilities in a county with less than 35,000 residents, facilities in a non-urban county with less than 100 hospital beds, facilities that do not seek insurance payments or government reimbursement

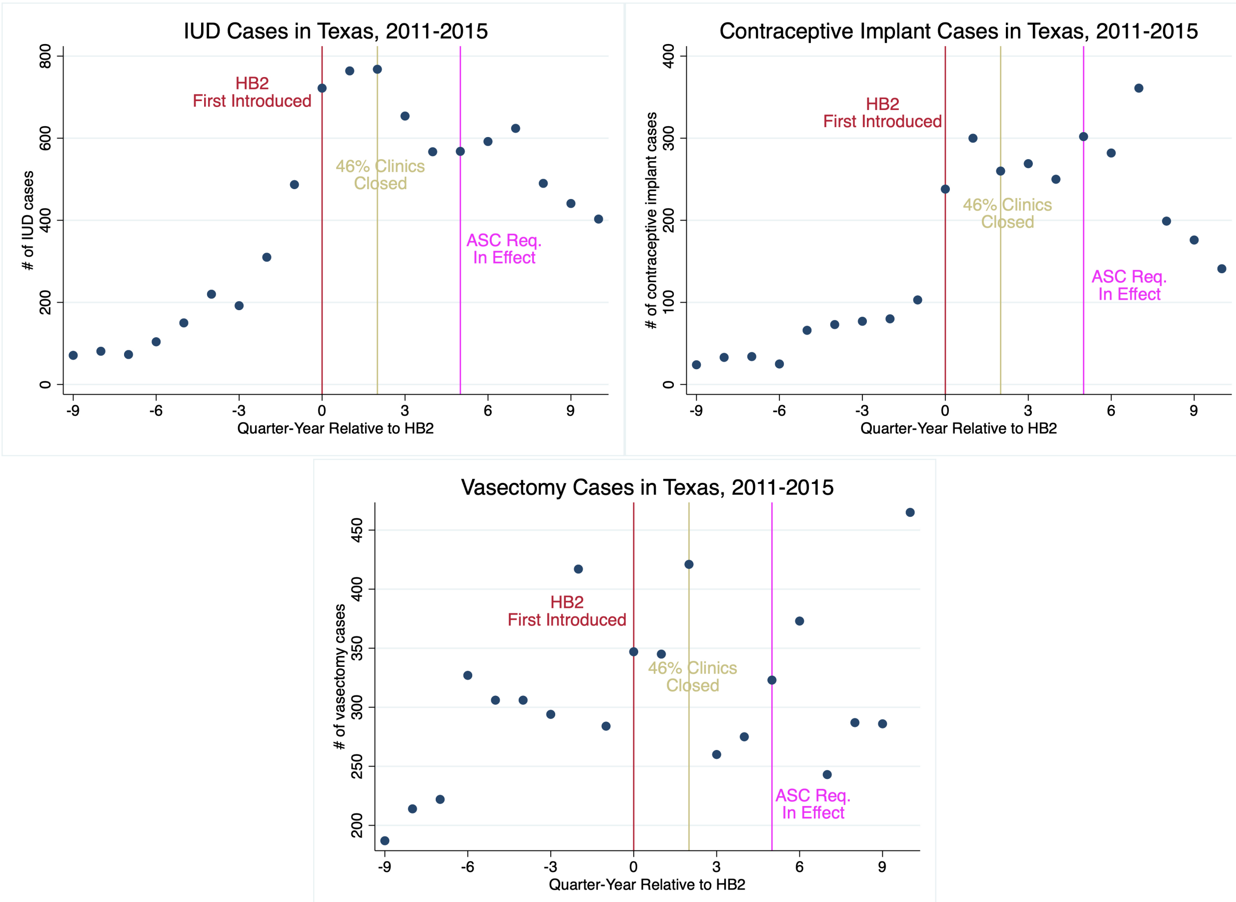
Table 3.1 Summary Statistics for LARC and Vasectomy Cases, 2011-2015

	LARC		Vasectomy	
Variable	Mean	N	Mean	N
Age (year category)	25-29	11696	35-39	6314
Race (%)		11696		6309
White	50.44		57.30	
Black	15.02		4.99	
AAPI	2.59		0.87	
American Indian	0.17		0.17	
Other	31.78		36.63	
Ethnicity (%)		11690		6290
Hispanic	39.85		17.74	
Non-Hispanic	60.15		82.26	
Length of Service (days)	1.31	11015	1.37	6243
First Payment Source (%)		11690		6290
Medicaid	33.24		0.52	
Charity, Indigent, Unknown	22.70		5.78	
Health Maintenance Org	11.95		8.82	
Commercial Insurance	9.49		37.86	
Blue Cross/Blue Shield	8.57		17.54	
PPO	7.09		23.13	
Other	6.96		6.35	
Gender (%)		11659		6304
Female	99.98		0.11	
Male	0.02		99.89	
LARC Type		11696		
IUD	71.64			
Implant	28.43			
IUD + Implant	0.07			

up to the introduction of HB2, the number of IUD insertions increased from around 200 to nearly 800 per quarter and contraceptive implant insertions increase from around 100 to 300 per quarter. A more modest increase in IUDs and implants occurred following enforcement of the ambulatory surgical center requirement in September 2014.

After the policy changes, the incidence of IUDs and implants declines back toward pre-treatment levels. This is consistent with the hypothesis that people respond to new information about the availability of abortion when making contraceptive choices. Given the durability of LARC, we would not necessarily expect the changes in the incidence of LARCs to persist continuously over time.

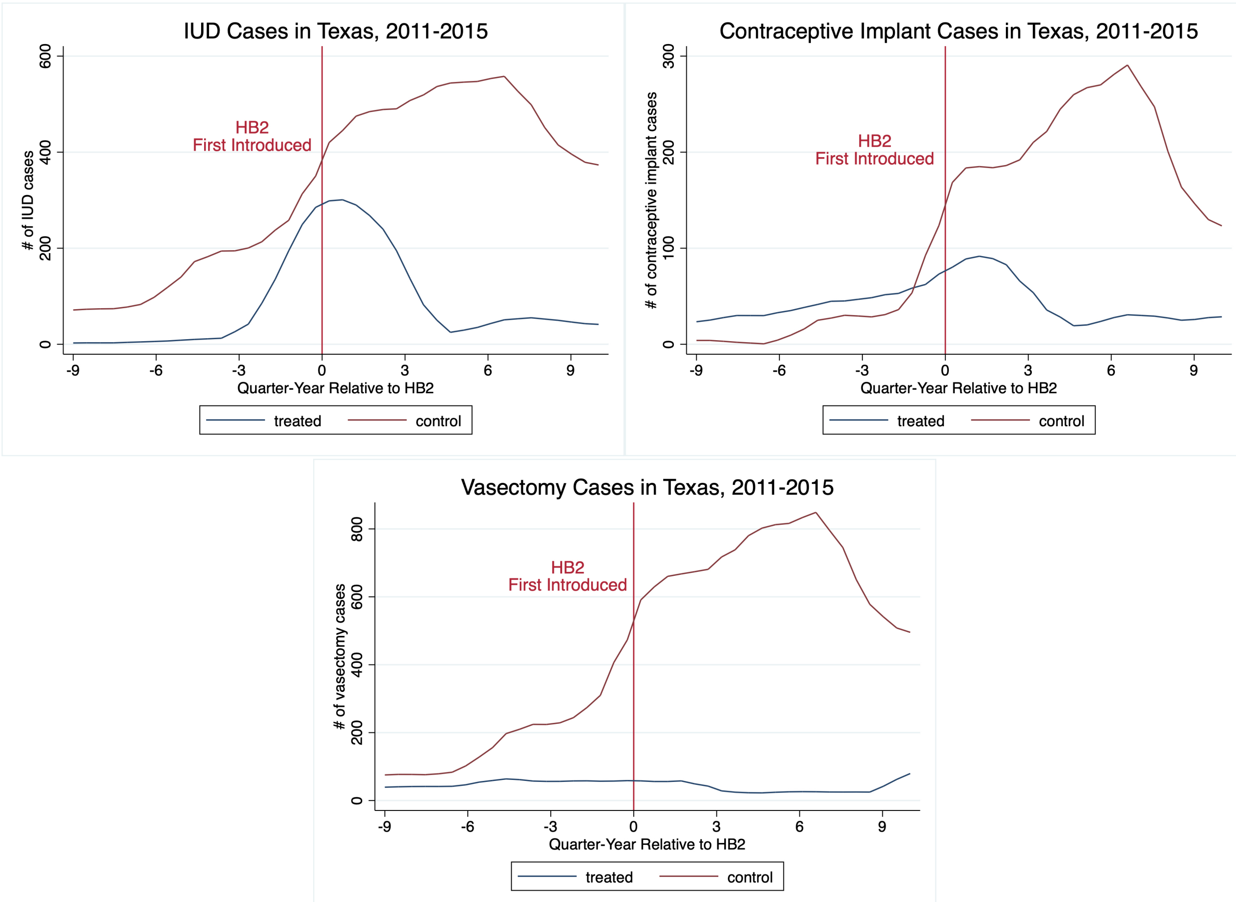
Figure 3.2 Trends in LARC and Vasectomies



Trends in the incidence of vasectomy are relatively noisy, which appears to be driven by strong seasonal effects. The number of vasectomies is much higher in the final quarter of each year. Seasonality in vasectomy procedures is well-known and is likely because vasectomy procedures require recovery time away from work. For example Ostrowski et al. (2018) finds peaks in vasectomy procedures at the end of the year when patients have met their insurance deductibles and may have time away from work. Even outside of the seasonal effects, it does not appear that these trends are strongly correlated with the timing of abortion legislation.

To determine a measure of treatment from HB2, we use a panel describing the distance to the nearest abortion provider from Myers (2021b). Myers calculates the travel distance (in miles) from the county population to the centroid to the nearest abortion providing facility in each month from 2009 to 2021. We average these distances across quarter-years and then match the aggregated

Figure 3.3 Trends in LARC and Vasectomies by Treatment Status



distance measures to our outcome data for Texas counties from 2011 Q1 to 2015 Q4.

Figure 3.3 plots raw trends in IUDs, implants, and vasectomies over time by treatment status. We define a county as treated when the distance to an abortion provider increases by >30 miles following HB2. This definition of treatment results in 117 treated counties and 137 control counties.

Figure 3.3 indicates that the incidence of LARC and vasectomies rose substantially during the study period in control counties. This may be indicative of general trends in contraceptive behavior within Texas and could also be associated with the policy change. If people respond to *expectations* of limited abortion access, they may substitute toward long-acting forms of contraception even if they do not experience the loss of an abortion provider as a result of the TRAP law. We do not consider this potential bias overly concerning, as it attenuates our treatment effects and results in plausibly conservative estimates of the effect of HB2 on contraceptive behavior in treated counties.

Treated counties experience a sharper increase in the number of IUD insertions leading up to the time of the policy change. The trend peaks in the first few months following the introduction and passage of HB2 and then declines back to pre-treatment levels roughly one year after. Contraceptive implant insertions also increase in treated counties around the time of the policy change, but at a lower rate than in the control group. This may suggest that individuals responding to the policy change are more likely to seek an IUD rather than a contraceptive implant. Interestingly, the incidence of vasectomy is stable and near zero in treated counties across the study period even though the rate of vasectomies steadily increases in control counties. Because vasectomies do not appear to be a common method of contraception in treated counties, the policy change may not be effective at influencing behavior on this margin. In the next section, we formalize analysis of the descriptive trends between the treatment and control group using an event-study design.

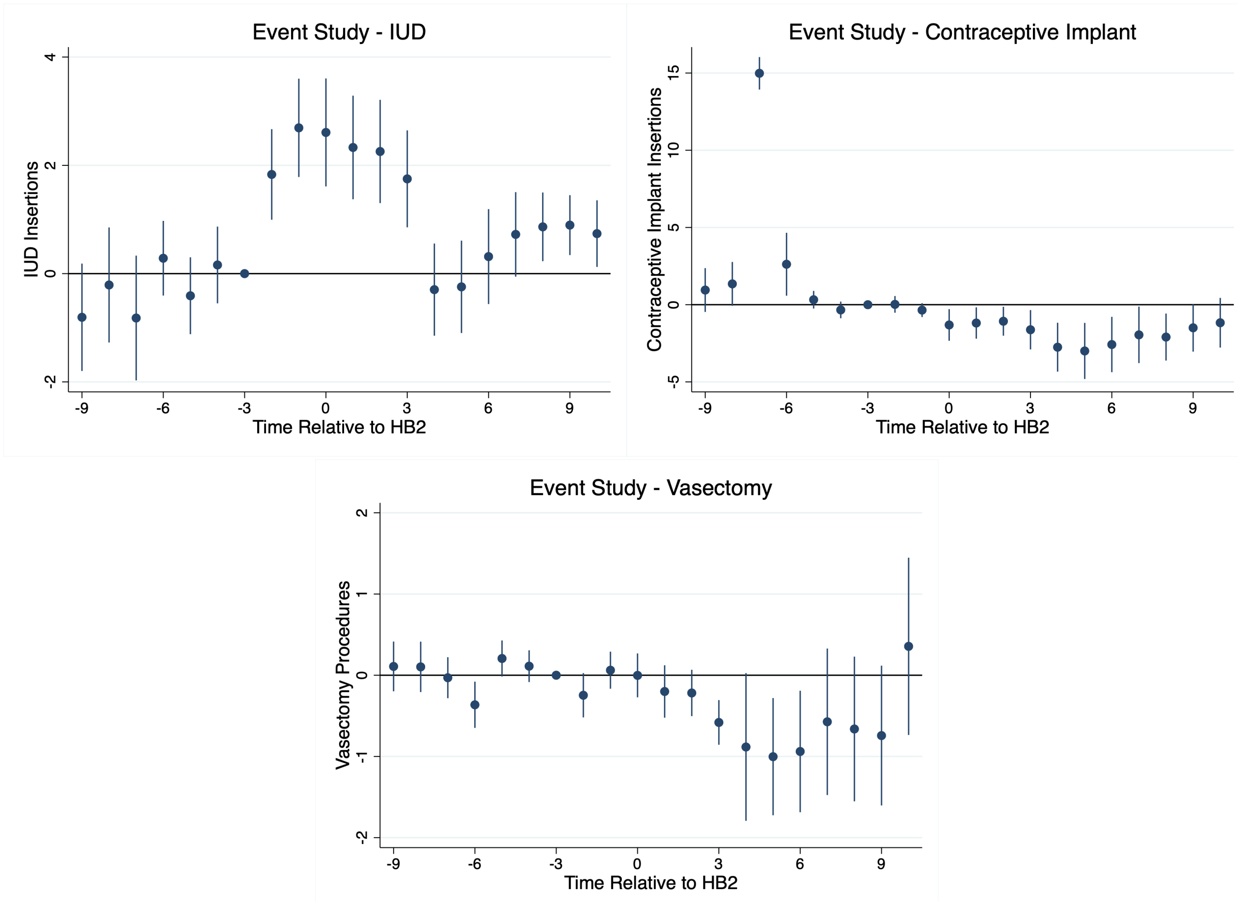
3.4 Methods and Results

We rely on an event-study design to estimate the effects of restrictive abortion policies on LARC and vasectomy take-up. Contentious policy proposals, such as those governing access to abortion, often receive media attention before formal bills are debated in legislative houses. Forward-looking agents may use their perception of expected future abortion access in their contraceptive decisions. The dynamic specification in Equation (1) permits the observation of these anticipation effects. Because the data contain discrete counts of LARC and vasectomy incidence occasionally equal to zero, we employ a Poisson model that takes the form:

$$E[Y_{ct}|\alpha_c, \delta_t, \beta x_{ct}, \Sigma_{k=-9}^{9}\lambda_k 1(t=k)] = exp(\alpha_c + \delta_t + \beta x_{ct} + \Sigma_{k=-9}^{9}\lambda_k 1(t=k)) \qquad (1)$$

where Y_{ct} is the number of IUDs, contraceptive implants, or vasectomies in county c at quarter-year t, α_c and δ_t are county and quarter-year fixed effects respectively. Here, k indexes the coefficients λ according to the time relative to the original introduction of HB2 as Senate Bill 5 in 2013 quarter 2. We include controls x_{ct} for the total number of outpatient discharge records for county c in quarter-year t to account for changing overall healthcare utilization across counties over

Figure 3.4 Poisson Event Studies



time.

Figure 3.4 presents the Poisson event-study plots of λ_k for each contraceptive method along with the 90% confidence intervals. For IUD insertions, the event plot reveals that trends are parallel between treatment and control counties leading up to three quarters prior to the introduction of HB2. In the two quarters leading up to HB2, IUD insertions increased significantly in treated counties. Notably, this time of anticipation effects occurs immediately following the 2012 state elections in Texas and during the eight-third legislative session, where multiple restrictive abortion bills are introduced that ultimately fail. At this time, news articles circulated around Texas detailing the strong anti-abortion stance of the legislature and the governor, which may contribute to the increases in IUDs prior to the introduction of HB2. Treated counties have a higher rate of IUD insertion until one year following the policy change, at which point trends return close to pre-treatment levels. Ultimately, IUD insertions increase by an average of 0.616[3] cases per county in quarters immediately surrounding the introduction of the policy change. This value is small in magnitude but meaningful. The mean number of IUD insertions in the entire outpatient data is 1.59 per county-quarter. Among counties with any IUD insertion, the mean is 9.62 insertions per quarter.

Results in Figure 3.4 for contraceptive implants and vasectomies do not indicate the same changes in contraception behavior. For contraceptive implants, the parallel trend assumption may be in question due to the large outlier in the Poisson coefficient λ in late 2011, and the coefficients are roughly zero otherwise throughout the study period. In the event plot for vasectomies, trends are parallel leading up to the time of the policy, but there is not strong evidence of an effect for treated counties after the introduction of HB2. The results do indicate a small negative effect in 2014, and this may be explained by the stable trend in vasectomies in treated counties compared to the general increase among the control group in Figure 3.3.

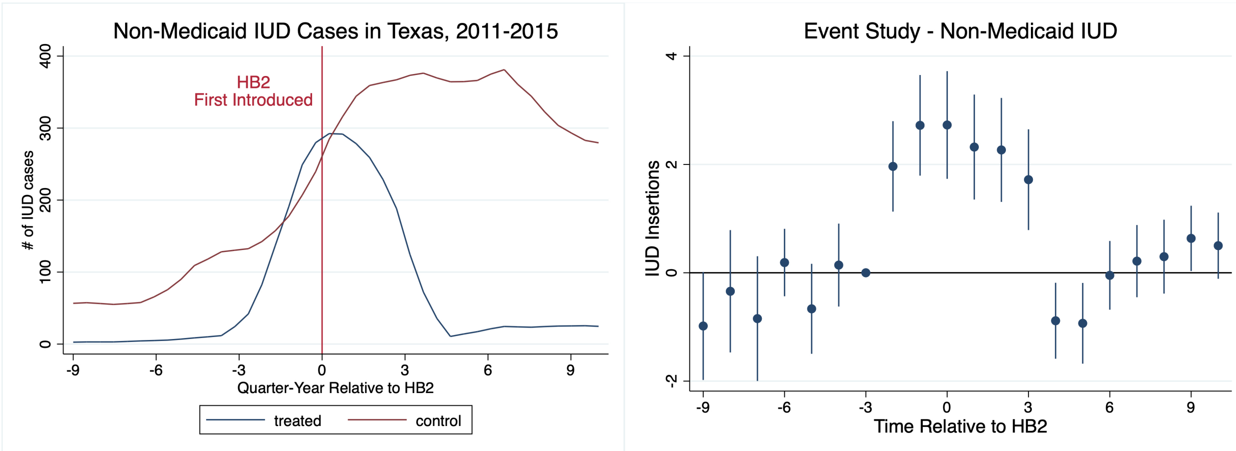
One limitation of our data is that we can only see LARC insertions and vasectomies in hospital-based outpatient and ASC settings. Therefore, we are concerned that the observed trends are mechanically driven by changes in reproductive healthcare in Texas during this time period. In 2012, Texas made changes to its Women's Health Program (WHP) to exclude sexual health clinics that are affiliated with or make referrals to abortion providers from receiving Medicaid reimbursement. In addition, many abortion clinics that also provide family planning services may have been closed due to the implementation of HB2 in mid-2013. Both policies could cause people to switch from care in independent clinics toward hospital-owned facilities, resulting in a false conclusion that take-up of these contraceptive methods increased in the state overall. We make two efforts to explore the possibility that these mechanisms confound our estimates.

3.4.1 Medicaid Recipients

The decision to exclude family planning clinics from receiving reimbursement from the WHP could drive Medicaid recipients from independent family planning clinics (such as Planned Par-

[3]This value comes from transformation of the Poisson model coefficients where log(Yk | policy) - log(Yk | no policy) = λ_k.

Figure 3.5 Trends and Event Study, Non-Medicaid IUD Insertions



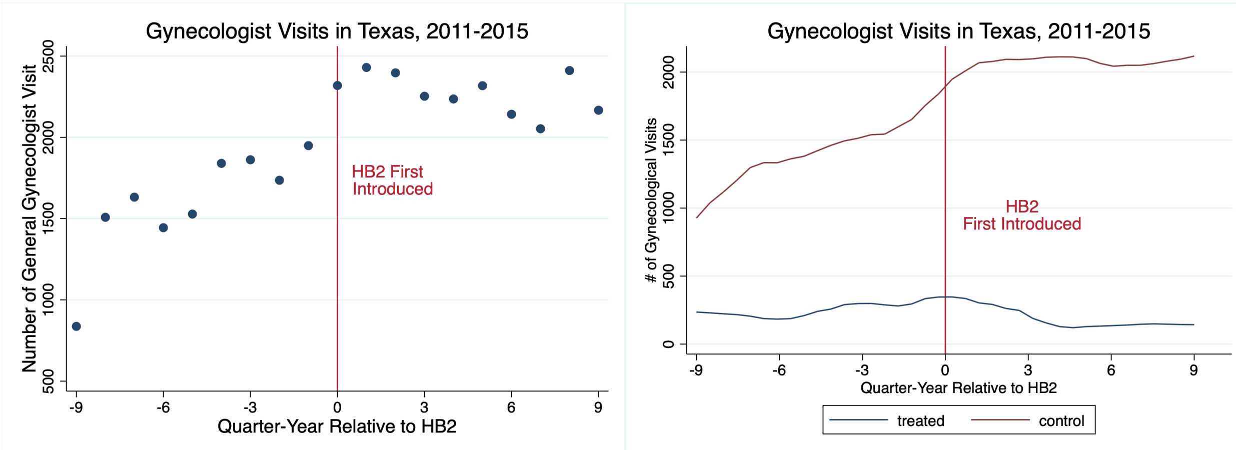
enthood affiliates) toward hospital-based outpatient facilities. If this facility shift occurs primarily in our group of counties treated by HB2, it would result in increases in LARC in our data that we falsely attribute to restricted abortion access. Fischer et al. (2018) show that there is little correlation between increased distance to an abortion provider following HB2 and restricted access to publicly funded family planning clinics after the change to the WHP. So, we do not expect that the effects we observe in Figure 3.4 are heavily driven by funding restrictions. To be sure, we repeat the event study analysis for IUD insertions in Equation (1) excluding Medicaid recipients from our sample.

Figure 3.5 shows that excluding Medicaid recipients does not fundamentally shift the trends between treatment and control counties or the event plot, therefore providing evidence that Medicaid recipients are not driving changes in IUD insertion around the timing of HB2. This supports a conclusion that the changes to the Texas Women's Health Program are unlikely to cause the increases in IUD insertions we observe for counties that experience increased distance to an abortion provider following the TRAP law.

3.4.2 Other Services

Policies like HB2 that target abortion clinics may lead to consequences for other family planning services. When abortion clinics close, access to all other services provided by the clinic also decreases. In the Myers (2021b) data describing the distance to an abortion provider, "closures"

Figure 3.6 Trends in General Gynecologist Visits in Texas



indicate facilities that shut down or simply stopped providing abortion care. Facilities that continue to operate without providing abortions would not influence the access to contraceptive care for residents of that county. On the other hand, complete facility closures may result in a mechanical increase in LARC incidence when the population served by that facility seeks contraceptive care elsewhere. While this response is a consequence of the TRAP law, it is not necessarily related to abortion access. Therefore, this behavior may be confounding our central research question.

We explore the possibility that people in areas experiencing facility closures shift their care to a hospital-owned facility or ASC in our data by observing trends in other reproductive health services unrelated to contraception. In Figure 3.6, we plot the average trend and the trends by treatment status for general gynecologist visits over the study period. If people are making a fundamental shift in their facility for care, we would expect to see corresponding increases in visits to the gynecologist after clinic closures due to HB2, concentrated in treated counties. Trends demonstrate that the overall incidence of general gynecologist visits did increase over the study period, but this increase occurs almost exclusively in the control group. Among treated counties, the incidence of gynecologist visits is stable. So, it does not appear that there is evidence to suggest that residents of counties affected by HB2 systematically shift to receive healthcare in hospital-owned an ASC facilities.

3.5 Conclusion

In this paper, we measure the effect of restricted abortion access in Texas on the incidence of hospital-based long-acting reversible contraception and vasectomies. For identification, we exploit the within-state geographic variation in the distance to a nearest abortion provider following the 2013 passage of House Bill 2, a TRAP law that shut down over half of all abortion clinics in Texas. We find that counties with an increase in their travel distance to an abortion provider greater than 30 miles experience an average increase of 0.616 IUD insertions per quarter around the time of the policy change. Overall, this amounts to roughly 432 additional IUD insertions during our sample period, representing 5.15% of total hospital-based IUD cases between 2011 and 2015. We do not find evidence that counties affected by HB2 experience increases in the take-up of contraceptive implants or vasectomies.

Our data on outpatient procedures includes only discharge records for LARC and vasectomies that occurred in a hospital-owned facility or ambulatory surgical center. As such, our results may be biased by changes in the location of care resulting from these policies. However, we do not believe this is the case and we make efforts to argue that the public policy environment in Texas did not significantly shift the location of care in the state from independent clinics toward hospital-owned facilities. Our results are robust to the exclusion of Medicaid recipients from the sample — a population that experienced reduced access to contraceptive care in publicly funded clinics after changes to the reimbursement structure for the state Women's Health Program in 2012. In addition, we do not find evidence that counties affected by the abortion clinic closures following HB2 increased the number of hospital-based general gynecologist visits, supporting a conclusion that these counties did not make large changes in their location of reproductive healthcare during the study period.

Ultimately, we find that increasing the cost of abortion through travel distance increases the demand for IUD insertions. To our knowledge, our study is the first in the United States to provide empirical evidence supporting the hypothesis that abortion and IUDs are substitutes. We also explore the potential for this substitutability to extend to vasectomies, a long-acting contraception method used by people without the capacity to become pregnant themselves. We find that the incidence of vasectomies does not significantly increase in counties treated by the policy change, suggesting that the additional cost of abortion may not pass through to partners.

Our study has a few key limitations. When presented with new information, people may respond to changes in their expectation of abortion access in the future, regardless of realized restrictions in access. In this way, people across the entire state of Texas may be influenced by the media surrounding abortion access during the eighty-third legislative session, and therefore defining a treatment group to only include counties affected by abortion clinic closures may be too narrow. So, we consider our treatment effects to be conservative estimates of changing contraceptive behavior. Additionally, we only measure the effects of abortion access on a subset of contraceptive options. While 65.3% of US reproductive-age women report using contraception, only 10.4% report using long-acting reversible contraception (Daniels and Abma, 2020). LARC is the third most common contraceptive method among women, behind sterilization (18.1%) and the pill (14%). More research is necessary to determine the influence of abortion access on the take-up of contraception, broadly. Finally, our study relies on variation in abortion access in a single US state. Although Texas is populous, it contains only 8.9% of the total US population, and results may not be generalizable to the entire country.

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APPENDIX A

CHAPTER 1 APPENDIX

Supplemental Analysis

Diff-in-Diff with Region-Year Fixed Effects

To ensure that treatment effects are not driven by concurrent regional changes in rates of adverse maternal and infant health outcomes, I separate US states into four regions (Northeast, South, Midwest, West) according to the Census Bureau regions and divisions of the United States, and I repeat the BJS difference-in-differences analysis described in Table 1.4 with the inclusion of region-year fixed effects. So, the imputation step is now:

$$Y_{ist}(0) = \hat{\alpha}_s + \hat{\delta}_t + \hat{\gamma}_{r*t}$$

where $\hat{\gamma}_{r*t}$ represents the region-year fixed effects. Average treatment effects are then calculated according to equation (2) and (3).

There are no material changes to the difference-in-difference estimates and interpretations after including these additional fixed effects. Figure A1 and Table A1 present the BJS event study graphs and results from the F test described in Section 4. Table A2 presents the ATT estimates from the BJS difference-in-differences specification using the Austin and Harper (2019) policy coding.

Table A.1 BJS Parallel Trends Assumption F Test (Regional FEs Included)

	F-stat	p-value	df
PA Hypertension	1.780	0.138	42
Chronic Hypertension	2.000	0.098	42
Diabetes	1.514	0.206	42
Gestational Diabetes	2.911	0.026	36
Low Birthweight	0.847	0.524	42
Premature Birth	1.225	0.314	42
APGAR Score	1.251	0.303	42

Figure A.1 BJS Event Studies - TRAP Laws (Region FEs Included)

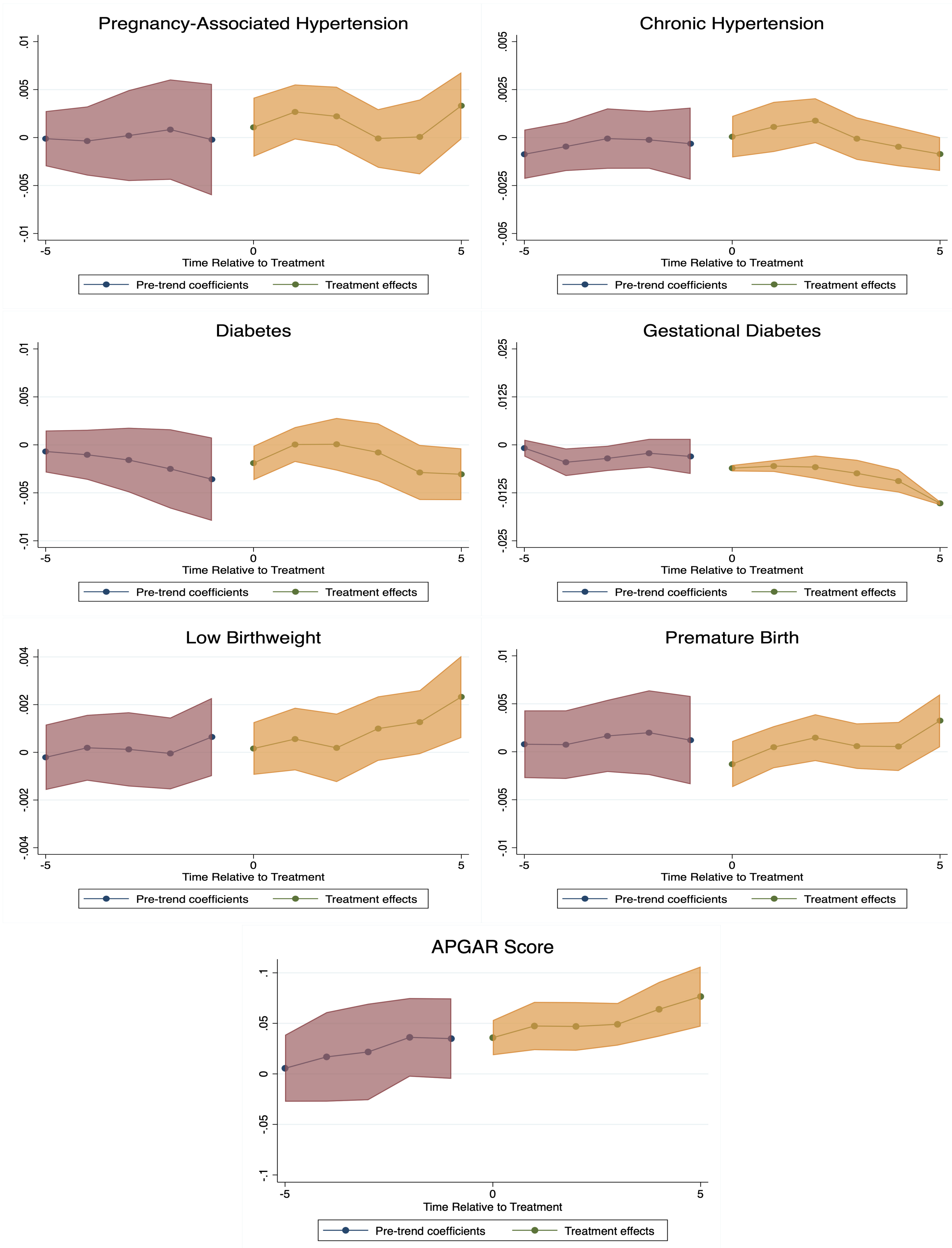

Table A.2 BJS Difference-in-Differences Results (Regional FEs Included)

	PA Hypertension	Chronic Hypertension	Diabetes	Gestational Diabetes	Premature Birth	Low Birthweight	APGAR Score
TRAP Law	0.0042***	0.0010	-0.0008	−0.0097***	0.0013	0.0013*	0.0487***
	[0.001]	[0.001]	[0.002]	[0.0005]	[0.001]	[0.001]	[0.012]
N	95654017	95654017	95654017	30995668	96695485	96695485	81645928

APPENDIX B

CHAPTER 2 APPENDIX

Data Sources

Demographics

Surveillance, Epidemiology, and End Results (SEER) Program Populations (1969-2018), National Cancer Institute, DCCPS, Surveillance Research Program, released December 2019.

CDC Abortion Data

Koonin LM, Smith JC, Strauss MRLT Abortion Surveillance – United States, 1995. MMWR Surveillance Summ 1998;47(SS-2):31-68.

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Koonin LM, Strauss LT, Chrisman CE et al. Abortion Surveillance – United States, 1997. MMWR Surveillance Summ 2000;49(SS11):1-44

Herndon J, Strauss LT, Whitehead S et al. Abortion Surveillance – United States, 1998. MMWR Surveillance Summ 2002;51(SS03):1-32

Elam-Evans LD, Strauss LT, Herndon J et al. Abortion Surveillance – United States, 1999. MMWR Surveillance Summ 2002;51(SS09):1-28

Elam-Evans LD, Strauss LT, Herndon J et al. Abortion Surveillance – United States 2000. MMWR Surveillance Summ 2003;52(SS12):1-32

Strauss LT, Herndon J, Chang J et al. Abortion Surveillance – United States 2001. MMWR Surveillance Summ 2004;53(SS09):1-32

Strauss LT, Herndon J, Chang J et al. Abortion Surveillance – United States 2002. MMWR Surveillance Summ 2005;54(SS07):1-31

Strauss LT, Gamble SB, Parker WY et al. Abortion Surveillance – United States 2003. MMWR Surveillance Summ 2006;55(SS11):1-32

Strauss LT, Gamble SB, Parker WY et al. Abortion Surveillance – United States 2004. MMWR

Surveillance Summ 2007;56(SS09):1-33

Gamble SB, Strauss LT, Parker WY et al. Abortion Surveillance – United States 2005. MMWR Surveillance Summ 2008;57(SS13):1-32

Pazol K, Gamble SB, Parker WY et al. Abortion Surveillance – United States 2006. MMWR Surveillance Summ 2009;58(SS08):1-35

Pazol K, Zane SB, Parker WY et al. Abortion Surveillance – United States 2007. MMWR Surveillance Summ 2011;60(SS01):1-39

Pazol K, Zane SB, Parker WY et al. Abortion Surveillance – United States 2008. MMWR Surveillance Summ 2011;60(SS15):1-41

Pazol K, Creanga AA, Zane SB et al. Abortion Surveillance – United States 2009. MMWR Surveillance Summ 2012;61(SS08):1-44

Pazol K, Creanga AA, Burley KD et al. Abortion Surveillance – United States 2010. MMWR Surveillance Summ 2013;62(SS08):1-44

Pazol K, Creanga AA, Burley KD et al. Abortion Surveillance – United States 2011. MMWR Surveillance Summ 2014;63(SS11):1-41

Pazol K, Creanga AA, Jamieson DJ Abortion Surveillance – United States 2012. MMWR Surveillance Summ 2015;64(SS10):1-40

Jatlaoui TC, Ewing A, Mandel MG et al. Abortion Surveillance – United States 2013. MMWR Surveillance Summ 2016;65(SS12):1-44

Jatlaoui TC, Shah J, Mandel MG et al. Abortion Surveillance – United States 2014. MMWR Surveillance Summ 2017;66(SS25):1-48

Jatlaoui TC, Boutot ME, Mandel MG et al. Abortion Surveillance – United States 2015. MMWR Surveillance Summ 2018;67(SS13):1-45

Jatlaoui TC, Eckhaus L, Mandel MG et al. Abortion Surveillance – United States 2016. MMWR Surveillance Summ 2019;68(SS11):1-41

ITOP Data

Arkansas Department of Health Statistics. (2000-2016) Induced Abortions.

Georgia Department of Public Health Online Analytical Statistical Information System. (1995-2016). Induced Termination of Pregnancy. https://oasis.state.ga.us/oasis/webquery/qryITOP.aspx

Iowa Department of Health. (2005-2016). Vital Statistics: Termination of Pregnancy Data. https://idph.iowa.gov/health-statistics/data

Minnesota Department of Health. (2009-2016). Reports to the Legislature: Induced Abortions in Minnesota. https://www.health.state.mn.us/data/mchs/pubs/abrpt/abrpt.html South Dakota Department of Health. (2008-2016). Vital Statistics: Induced Abortion. https://doh.sd.gov/statistics/

Utah Office of Vital Records and Statistics. (1998-2016). Utah's Vital statistics: Abortions. https://digitallibrary.utah.gov/awweb/main.jsp

Synthetic Control Details

Arkansas

Figure B.1 Synthetic Control for the Abortion Rate of Minors - Arkansas

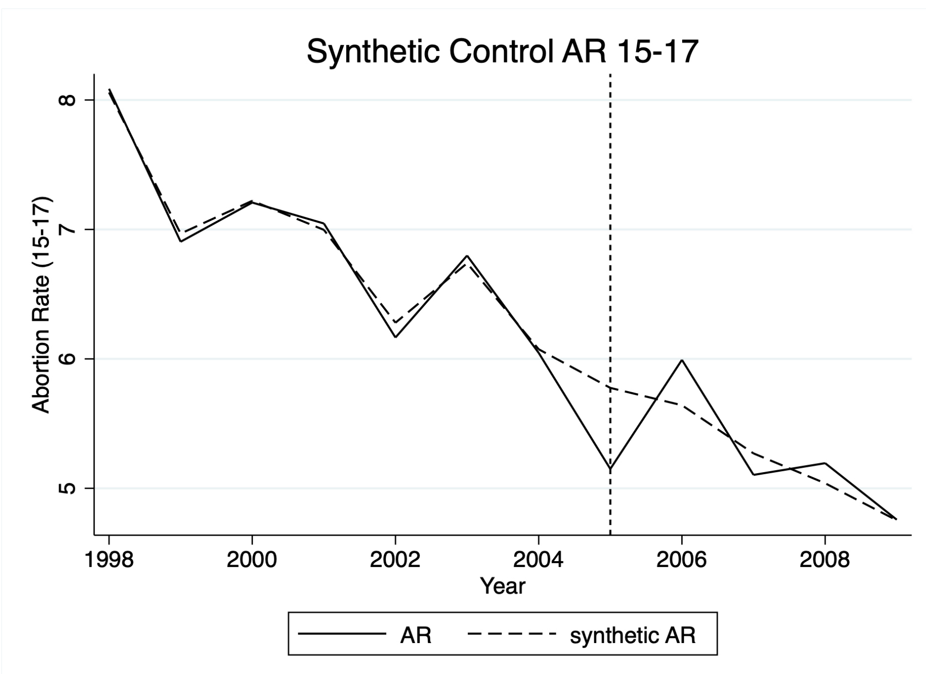

Table B.1 Arkansas - Synthetic Control Group for Abortion Rate of Minors

State	Weight
MI	0.146
NE	0.028
NM	0.085
OR	0.038
WI	0.704

Figure B.2 Synthetic Control for the Birth Rate of Minors - Arkansas

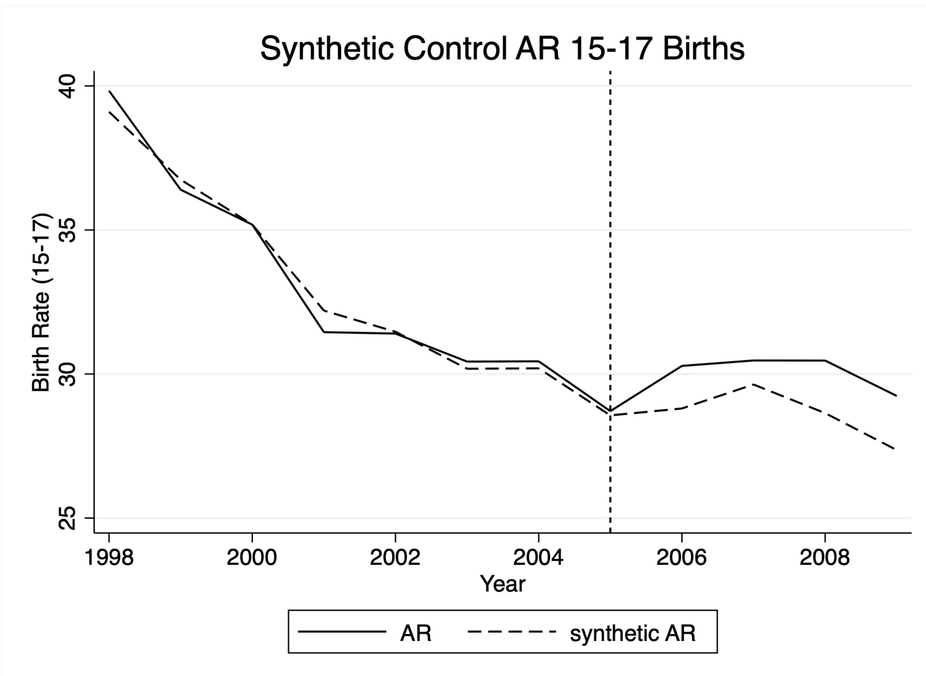

Table B.2 Arkansas - Synthetic Control Group for Birth Rate of Minors

State	Weight
AL	0.478
CA	0.116
NM	0.352
WY	0.054

Texas

Figure B.3 Synthetic Control for the Abortion Rate of Minors - Texas

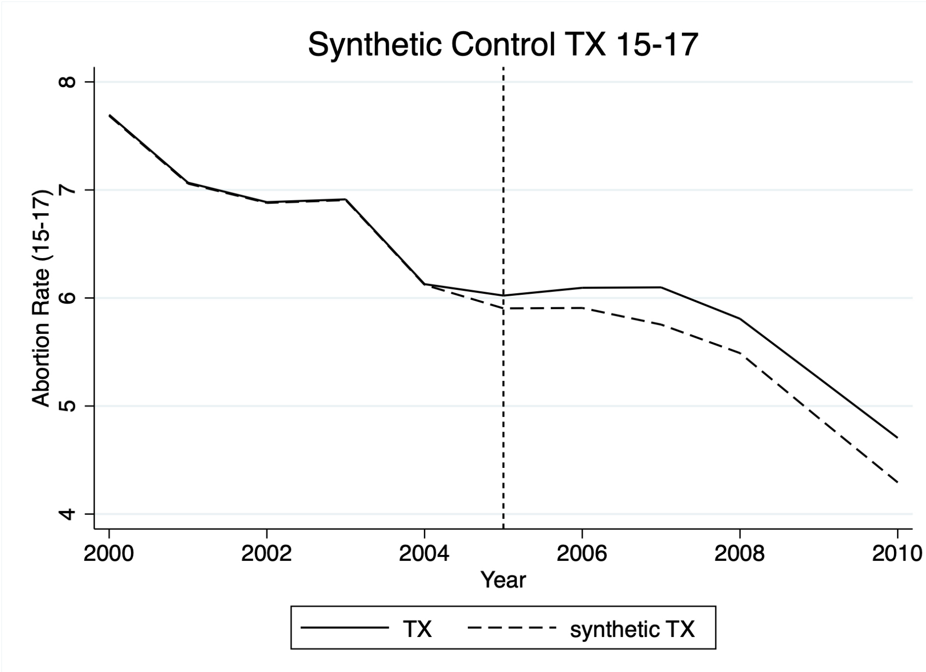

Table B.3 Texas - Synthetic Control Group for Abortion Rate of Minors

State	Weight	State	Weight
AL	0.02	NC	0.019
GA	0.033	NE	0.028
IA	0.027	NJ	0.031
IL	0.018	NM	0.024
IN	0.025	NV	0.014
KS	0.018	NY	0.01
MA	0.088	OR	0.04
ME	0.019	SC	0.025
MI	0.037	SD	0.037
MN	0.025	TN	0.024
MO	0.051	WA	0.018
MS	0.32	WI	0.025
MT	0.023		

Figure B.4 Synthetic Control for the Birth Rate of Minors - Texas

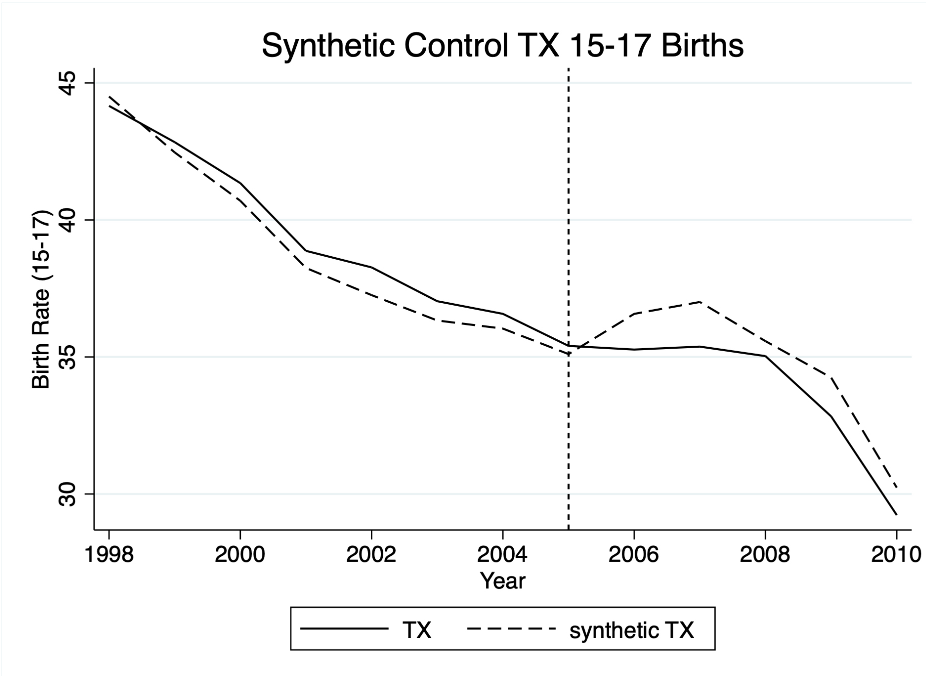

Table B.4 Texas - Synthetic Control Group for Birth Rate of Minors

State	Weight
MS	0.319
NM	0.681

Virginia

Figure B.5 Synthetic Control for the Abortion Rate of Minors - Virginia

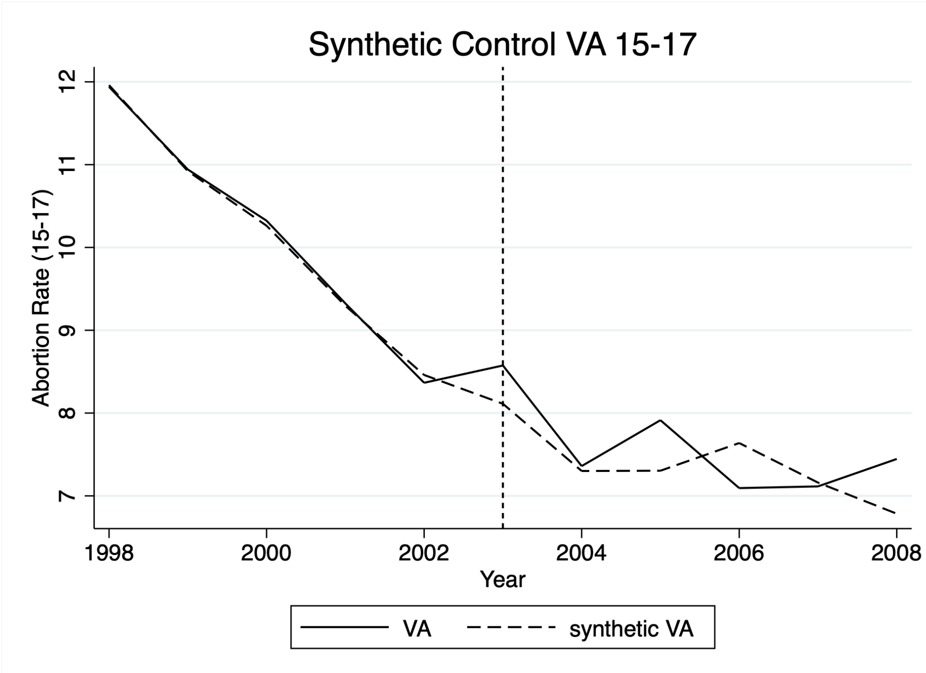

Table B.5 Virginia - Synthetic Control Group for Abortion Rate of Minors

State	Weight
AL	0.484
MS	0.078
NE	0.029
OR	0.33
WI	0.079

Figure B.6 Synthetic Control for the Birth Rate of Minors - Virginia

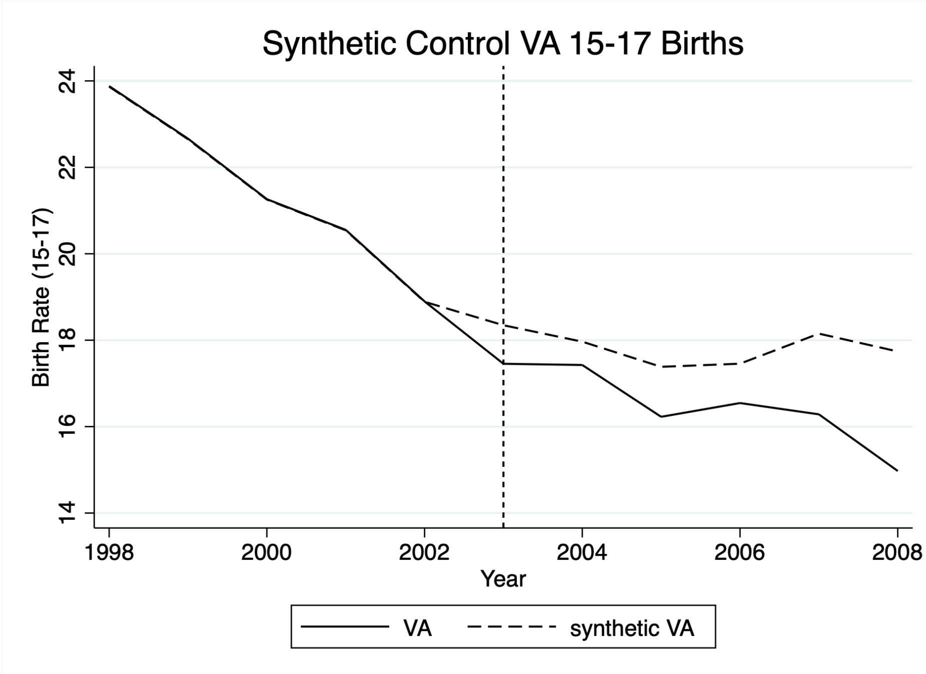

Table B.6 Virginia - Synthetic Control Group for Birth Rate of Minors

State	Weight	State	Weight
AL	0.005	MT	0.005
CA	0.01	NC	0.007
CO	0.005	ND	0.021
DE	0.111	NE	0.154
GA	0.007	NJ	0.016
IL	0.01	NM	0.007
IN	0.008	NV	0.007
KS	0.06	NY	0.016
KY	0.007	OR	0.011
LA	0.007	PA	0.011
MA	0.019	RI	0.236
MD	0.009	SC	0.006
ME	0.013	SD	0.009
MI	0.011	VT	0.066
MN	0.011	WA	0.009
MO	0.007	WI	0.046
MS	0.004	WY	0.068

Ohio

Figure B.7 Synthetic Control for the Abortion Rate of Minors - Ohio

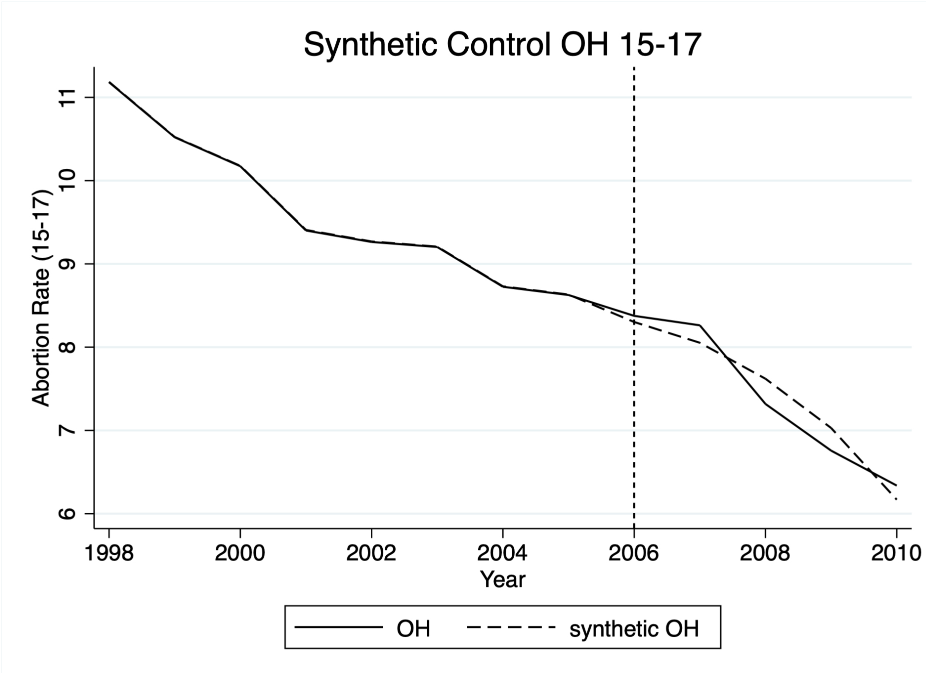

Table B.7 Ohio - Synthetic Control Group for Abortion Rate of Minors

State	Weight	State	Weight
AL	0.007	NC	0.006
GA	0.065	NE	0.005
IL	0.007	NJ	0.022
IN	0.007	NM	0.189
KS	0.011	NV	0.017
MA	0.01	NY	0.118
ME	0.008	OR	0.005
MI	0.023	SC	0.006
MN	0.012	SD	0.037
MO	0.007	WA	0.009
MS	0.275	WI	0.015
MT	0.14		

Figure B.8 Synthetic Control for the Birth Rate of Minors - Ohio

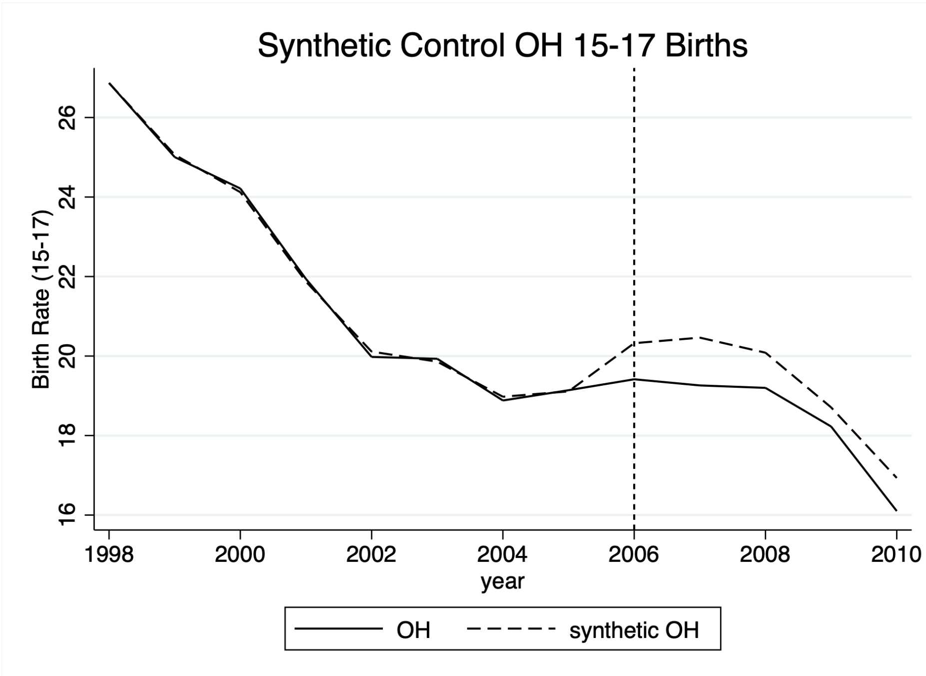

Table B.8 Ohio - Synthetic Control Group for Birth Rate of Minors

State	Weight
ME	0.078
MI	0.112
MS	0.125
ND	0.204
OR	0.187
RI	0.04
SC	0.158
SD	0.096

Kansas

Figure B.9 Synthetic Control for the Birth Rate of Minors - Kansas

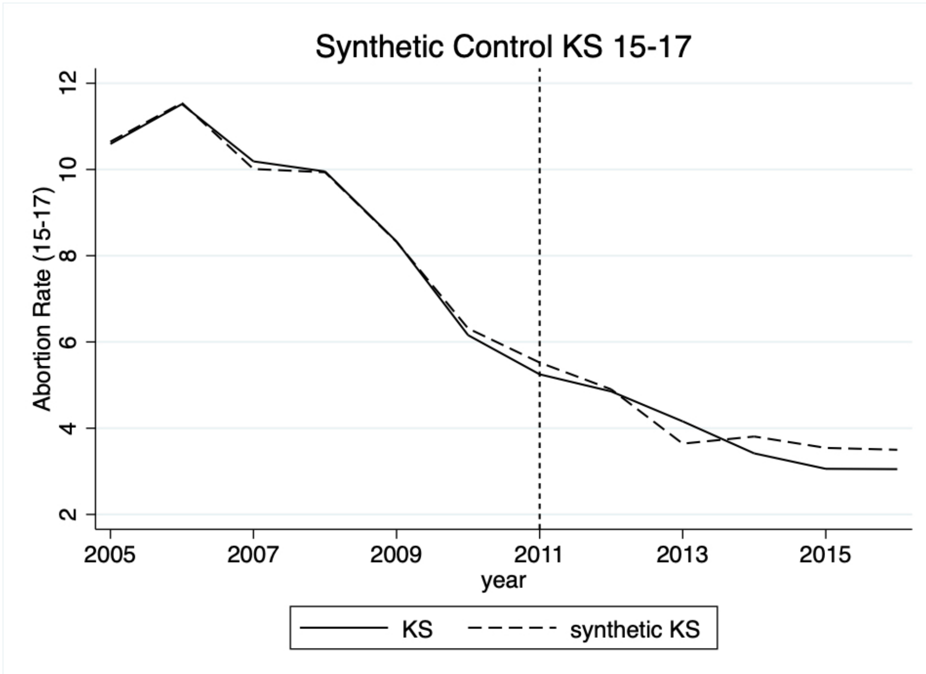

Table B.9 Kansas - Synthetic Control Group for Abortion Rate of Minors

State	Weight
MN	0.249
NV	0.606
SC	0.013
WA	0.132

Figure B.10 Synthetic Control for the Birth Rate of Minors - Kansas

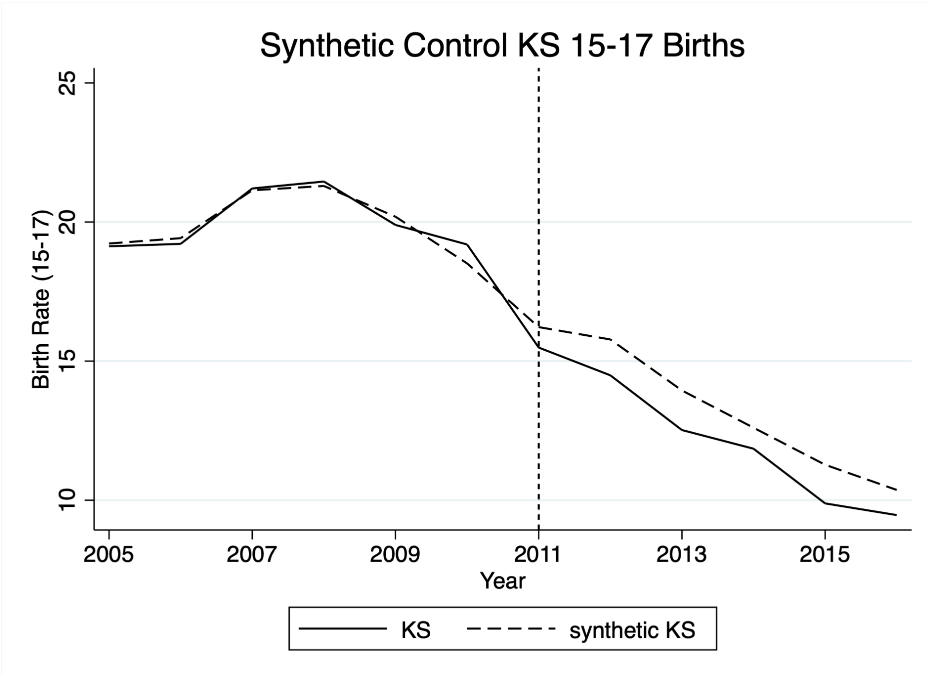

Table B.10 Kansas - Synthetic Control Group for Birth Rate of Minors

State	Weight
MS	0.073
ND	0.333
NM	0.054
WV	0.243
WY	0.296

Nebraska

Figure B.11 Synthetic Control for the Abortion Rate of Minors - Nebraska

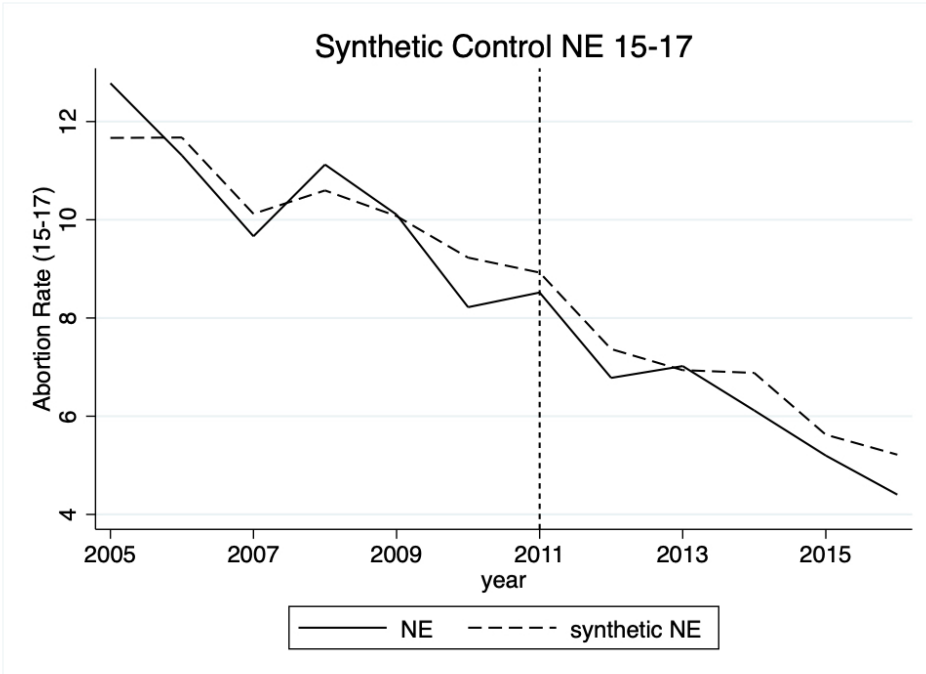

Table B.11 Nebraska - Synthetic Control Group for Abortion Rate of Minors

State	Weight
KY	0.025
MS	0.157
MT	0.128
WI	0.427
WV	0.263

Figure B.12 Synthetic Control for the Birth Rate of Minors - Nebraska

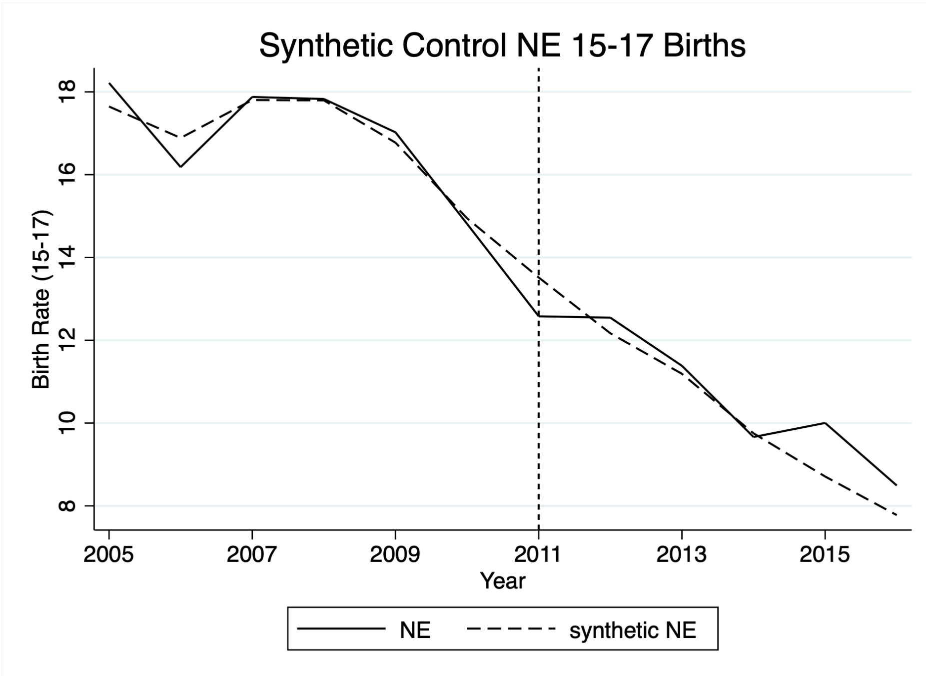

Table B.12 Nebraska - Synthetic Control Group for Birth Rate of Minors

State	Weight
KY	0.025
MS	0.157
MT	0.128
WI	0.427
WV	0.263