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

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Abstract

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.

Keywords: abortion, TRAP laws, maternal health, infant health, health disparities
JEL Codes: J13, I10, J18, I18

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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. Arizona, Indiana, Iowa, North Dakota, Michigan, Montana, Ohio, South Carolina, Utah, and Wyoming currently have abortion bans that are temporarily blocked by state courts (The New York 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 marginal cost of an abortion may be binding, 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 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 effect of additional pregnancies carried to term dominates, 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

[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.

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

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 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 who have been 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 quite a lot about how restricted access to abortion affects fertility outcomes such as abortion rates and 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, these are the only abortion policies under consideration, and 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, but the effects are localized to a 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.

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 thse 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 legal 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 a minority of abortions take place in the second trimester, these restrictions likely to not have large effects on abortion access. Table 1 summarizes the treatment timing for various TRAP laws by Austin and Harper (2019) and Jones and Pineda-Torres (2021).

Table 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).

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 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 presented in Figure 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

[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.

rate of hypertension among marginal births, $\frac{Y_1 - Y_0}{X_1 - X_0}$.

Figure 1a: A Model of Hypertension and Marginal Births

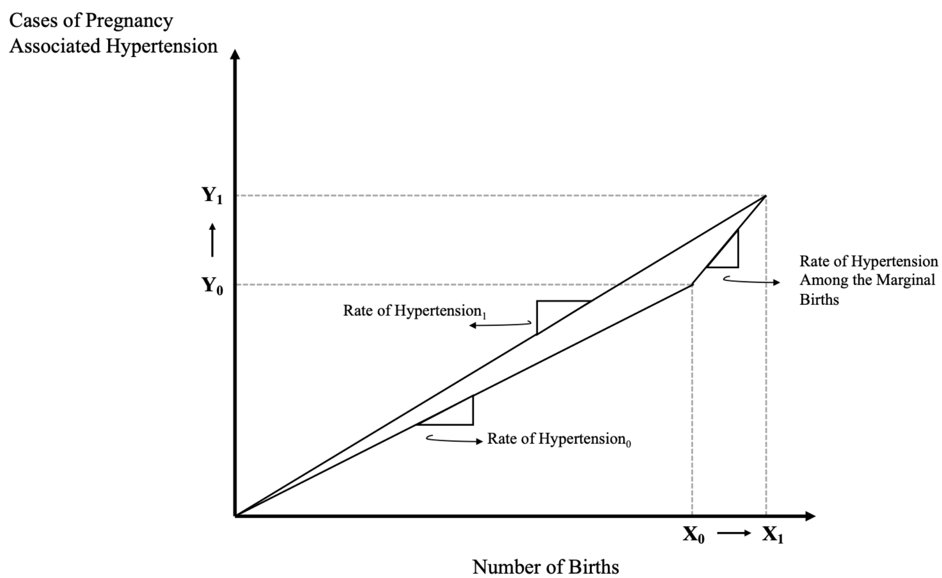
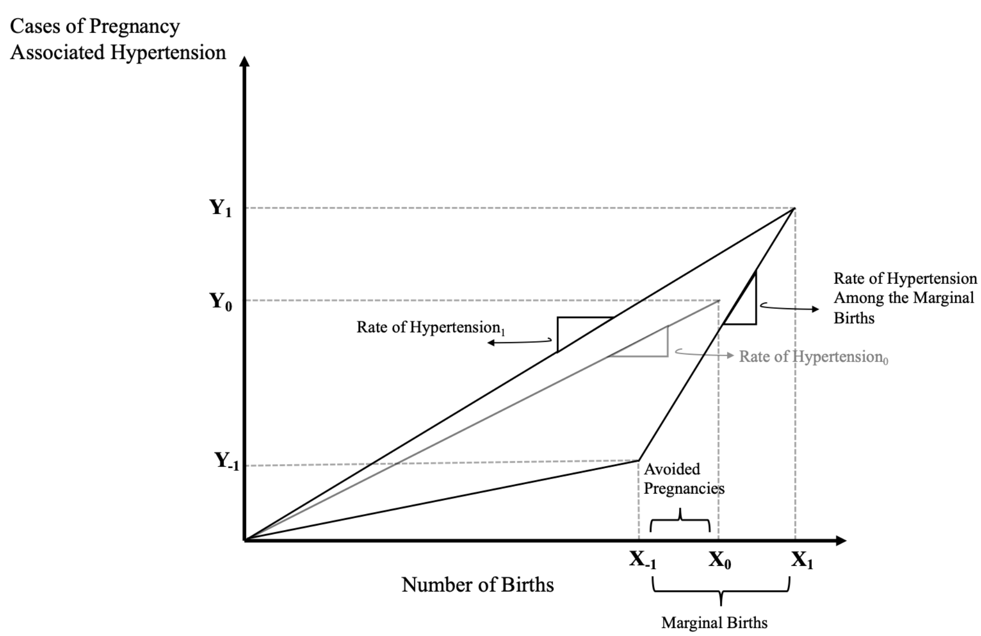


Figure 1b: A Model of Hypertension, Marginal Births, and Avoided Pregnancies



In Figure 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 1a and Figure 1b involve the same average treatment effect of the policy on the rate of hypertension while having very different rates among the marginal births.

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.

4 TRAP Laws and Pregnancy/Birth Outcomes

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

Table 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

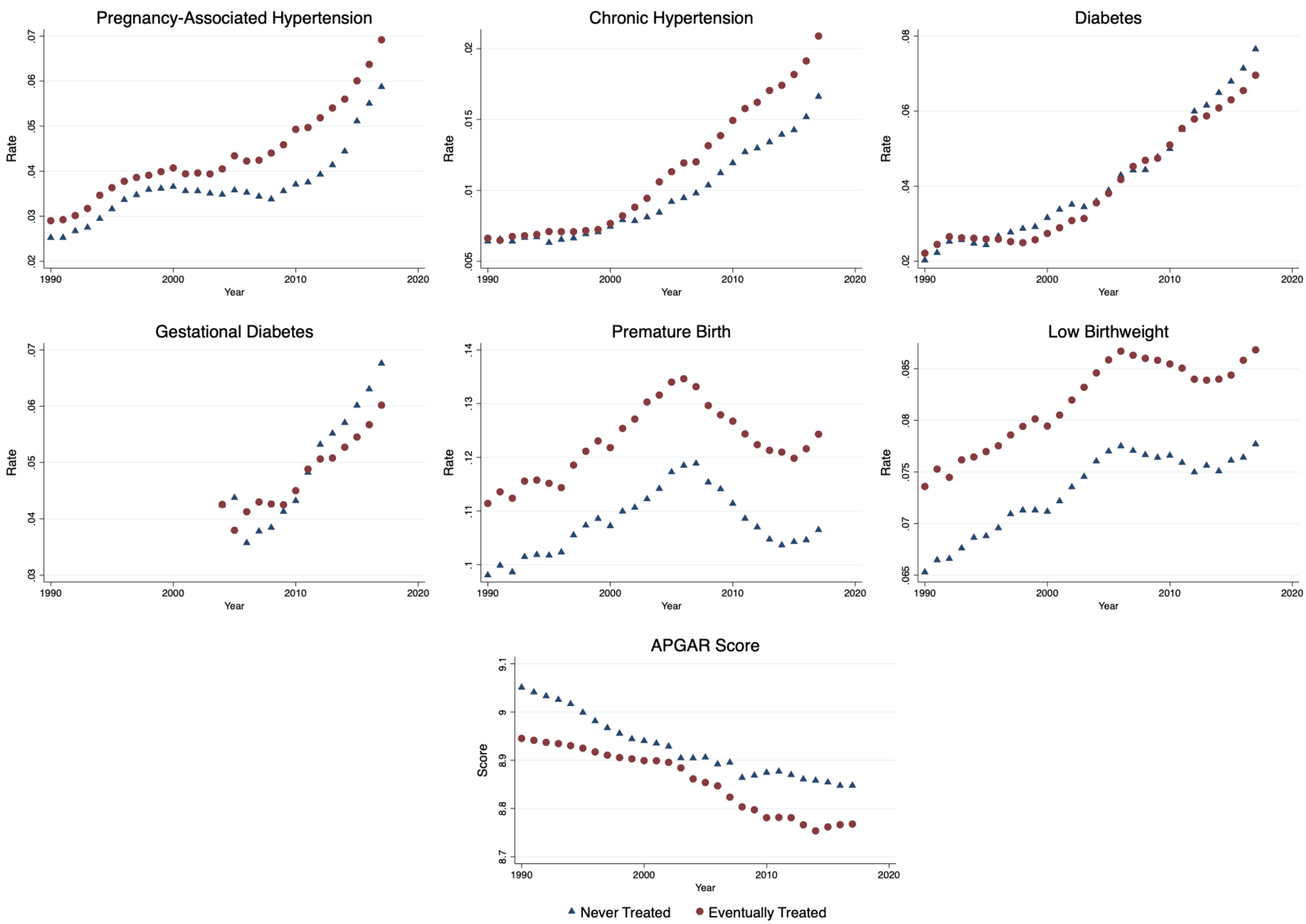
Notes: Data from NCHS (2022). Summary statistics describing the universe of births in the US, 1990-2017.

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

[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.

Grimace **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 2: Abortion Restrictions and Birth Outcomes, 1990-2017



Notes: Figure describes the rates of average adverse health outcomes over time separately by treatment status. "Eventually Treated" refers to states that pass a TRAP law at some point between 1990 and 2017.

Figure 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.

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. In addition to the parallel trends assumption, 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 demonstrates the importance of the homogeneity assumption.

States likely experience heterogeneous reponses to restrictive abortion legislation, and treatment effects are likely larger closer to the time of the policy change, where the "shock" occurs. Under this condition, 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.

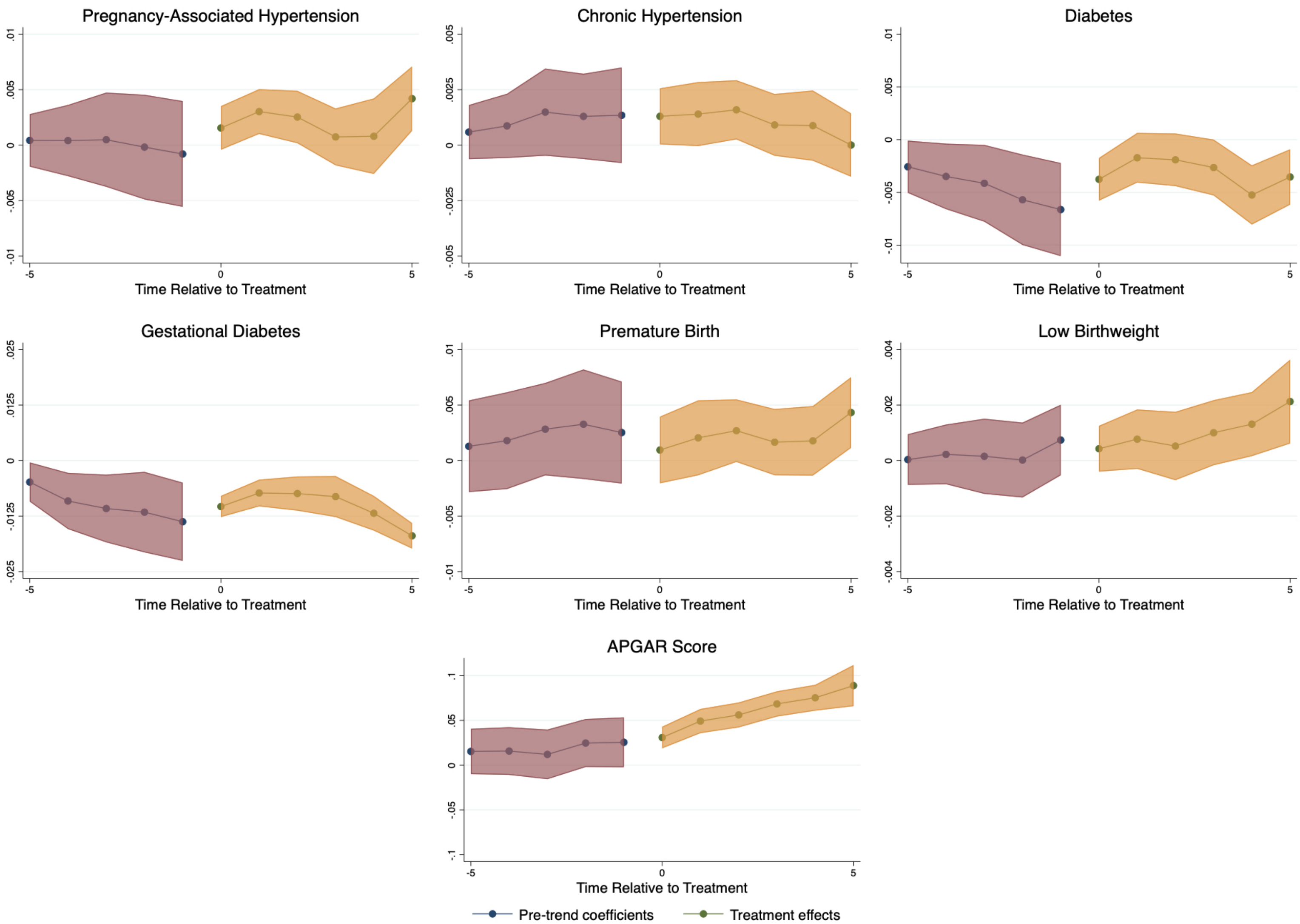
Figure 3 and Table 3 demonstrate that for most pregnancy and birth outcomes, the parallel trends assumption for TRAP laws is satisfied. Exceptions are the metabolic outcomes, diabetes and gestational diabetes. These outcomes are only differentiated after the 2003 revision to the standard birth certificate, 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.

Table 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 3: BJS Event Studies — TRAP Laws (Vital Stats)



Notes: Plots describing the pre-trend coefficients along with treatment effects of TRAP laws on vital statistics outcomes from 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 dynamic specification commonly found in traditional event studies.

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.

4.3 Results

Difference-in-Differences

Table 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 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 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 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 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, I argue that 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 solve 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, and there I find consistent results that restricted abortion access results in higher rates of all adverse maternal health outcomes.

The coefficients in Table 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 5.

Table 5: Diff-in-Diff by Subgroup

	PA Hypertension	Chronic Hypertension	Premature Birth	Low Birthweight
White, college	0.0050^{***}	0.0019^{**}	0.0039^{**}	0.0027^{**}
	[0.0012]	[0.0010]	[0.0017]	[0.0011]
White, HS	0.0036^{**}	0.0018^{*}	0.0036	0.0014
	[0.0017]	[0.0009]	[0.0022]	[0.0014]
White, <HS	0.0010	0.0005	-0.0019	-0.0028^{*}
	[0.0015]	[0.0007]	[0.0025]	[0.0016]
Black, college	0.0042^{**}	0.0032^{**}	0.0078^{***}	0.0070^{***}
	[0.0020]	[0.0015]	[0.0019]	[0.0015]
Black, HS	0.0037	0.0036^{**}	0.0053^{**}	0.0052^{*}
	[0.0034]	[0.0017]	[0.0025]	[0.0029]
Black, <HS	0.0024	0.0027^{*}	0.0054^{**}	0.0029
	[0.0033]	[0.0016]	[0.0027]	[0.0035]

Notes: Difference-in-Differences results by race and education using the specification in column (2) of Table 4. Standard errors are clustered at the state level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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 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. I suspect that this is 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:

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

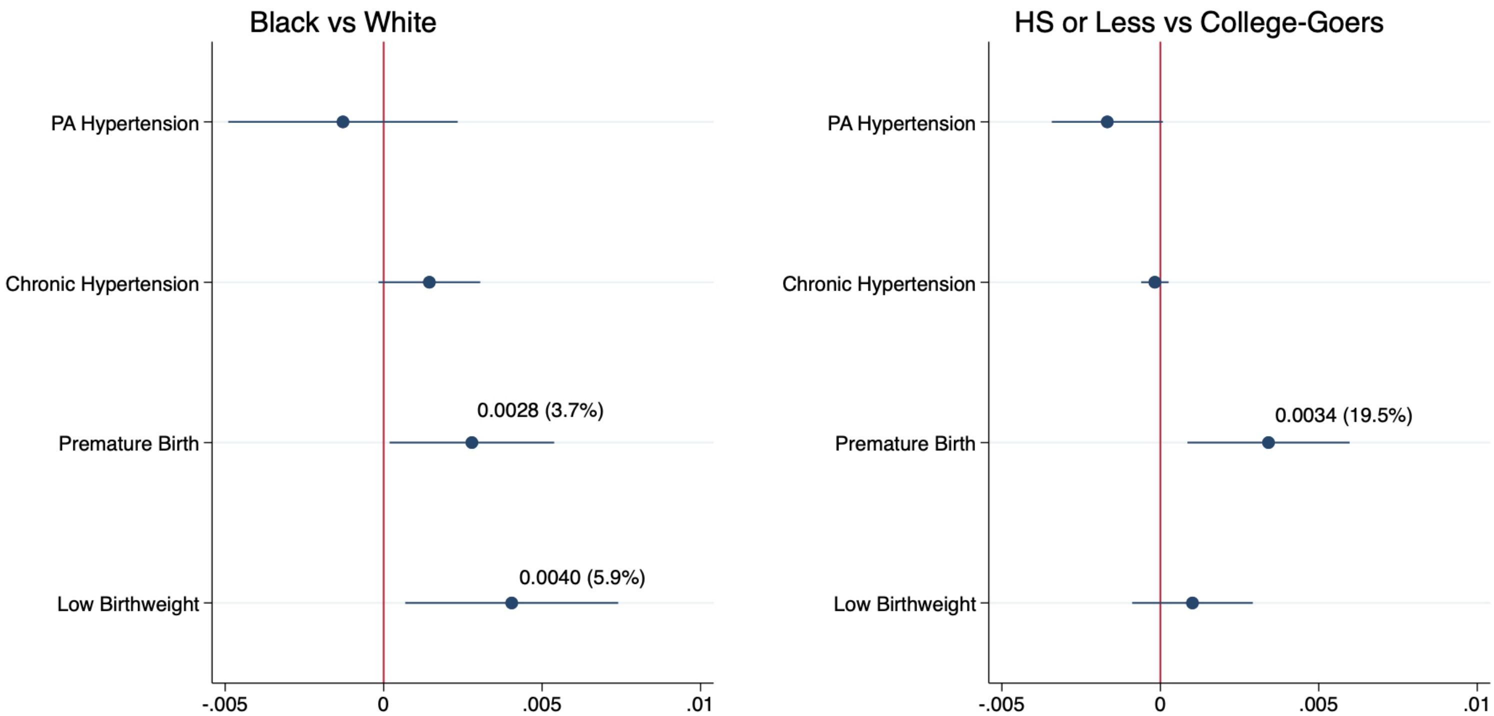
After imputing potential outcomes in this manner, calculating average treatment effects follows the same procedure outlined in equation (2) and (3). I estimate differential effects across two groups: race (Black vs white) and education (HS or less vs college-goers). Treatment effects from the triple-difference 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 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,

[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.

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 5.

Figure 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.

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). 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.

This specification allows me to measure the effect of increased travel distance to an abortion provider, rather than relying on a TRAP policy indicator. 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 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.

Table 6: Travel Distance and Pregnancy/Birth Outcomes, 2009-2017

	PA Hypertension	Chronic Hypertension	Diabetes	Gestational Diabetes	Premature Birth	Low Birthweight	APGAR Score
Distance (100s miles)	0.0035	0.0016**	0.0041*	0.0043***	-0.0002	-0.0006	0.0465***
	(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 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 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.

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 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.

Table 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.

In Table 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 Black births, fewer Hispanic births, 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 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 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 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 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.

Table 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 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 2.5x the mean rate and a rate of chronic hypertension about 3.36x 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.

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 specifically 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.

5.1 Data

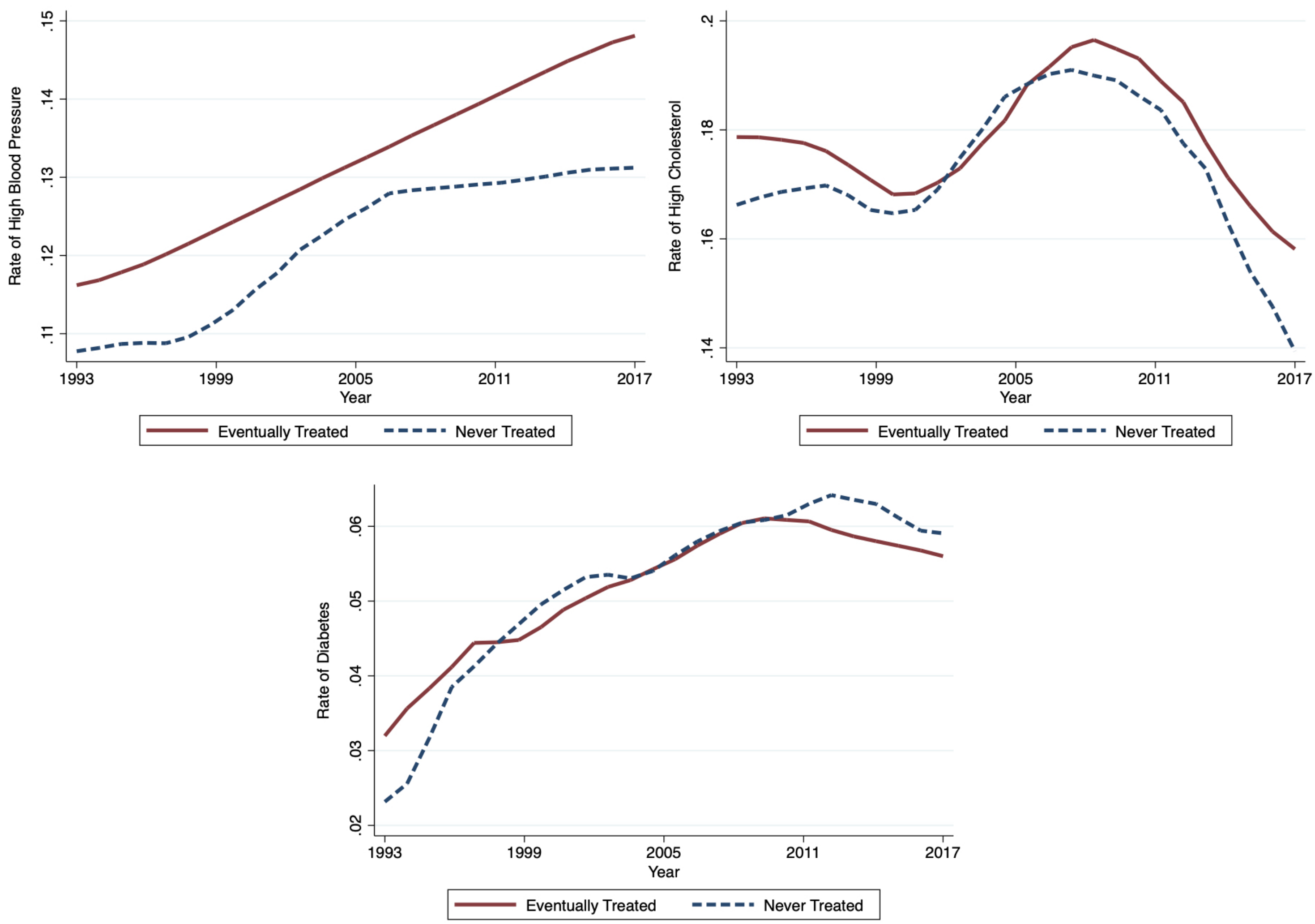
To identify effects of TRAP laws on individual health outcomes, I use data from the Behavioral Risk Factor Surveillance System (BRFSS). BRFSS is a large telephone survey of the US population administered annually at the state level. The survey collects data about individual demographics, health behaviors/risk, 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 characteristics and outcomes I observe in the vital statistics data — hypertension, high cholesterol, and diabetes.

The data in BRFSS have both strengths and weaknesses for determining the individual health effects of abortion access. One strength is the large sample size. 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. If abortion restrictions do induce the presence of chronic conditions at the individual level, I am more likely to observe effects in the general population using a large-scale survey. 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

[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

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 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.

Figure 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.

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

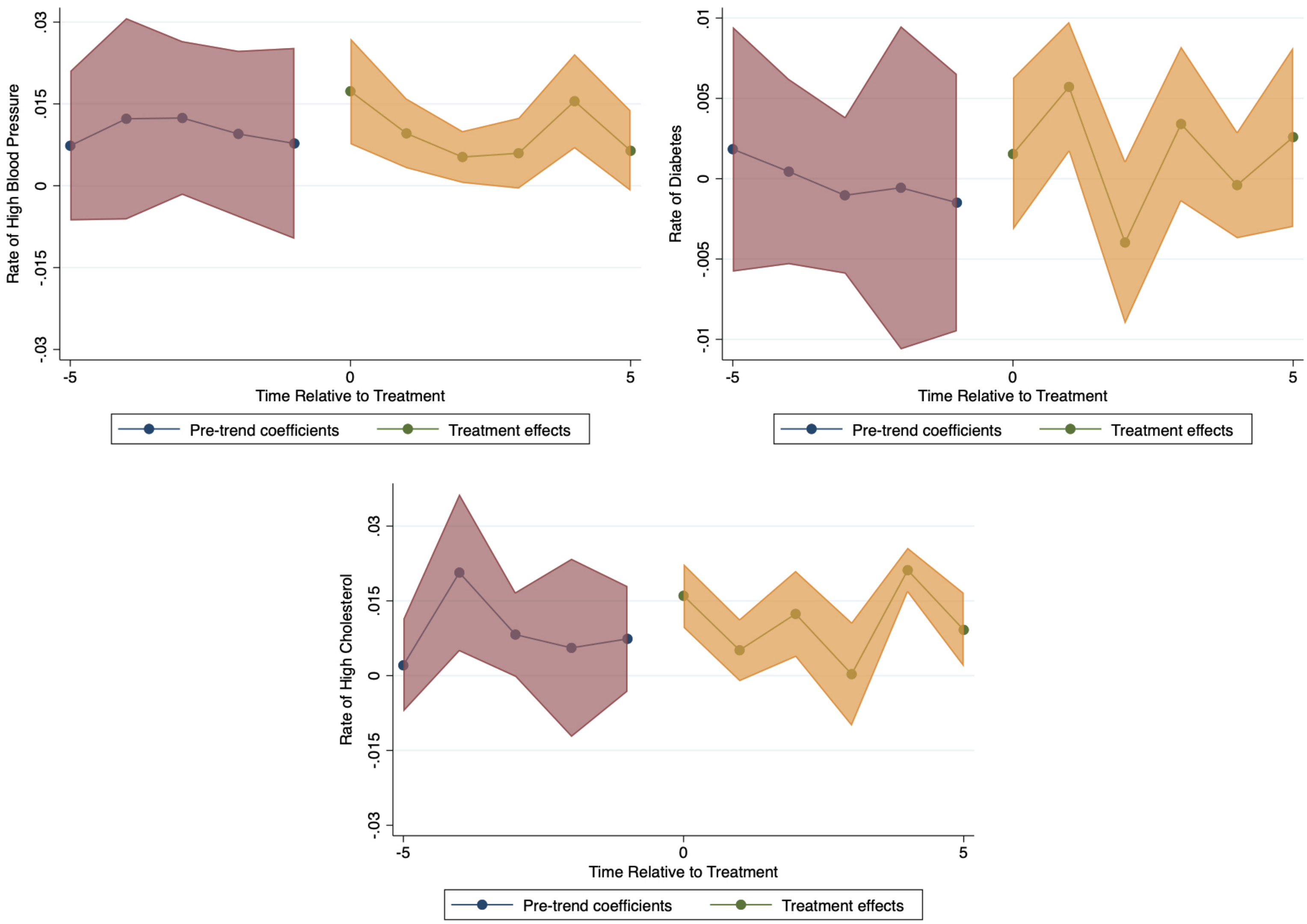
To determine if states that pass TRAP laws experience differential trends in their rates of chronic conditions leading up to the policy change, I use a specification equivalent to that in equation (4) and perform an F test similar to the one presented in Table 3. Results from Figure 6 and Table 9 indicate 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 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 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 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.

5.3 Results

Table 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.

Table 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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.

Table 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Results from Table 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 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.

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.

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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 4 with the inclusion of region-year fixed effects. So, the imputation step is now:

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

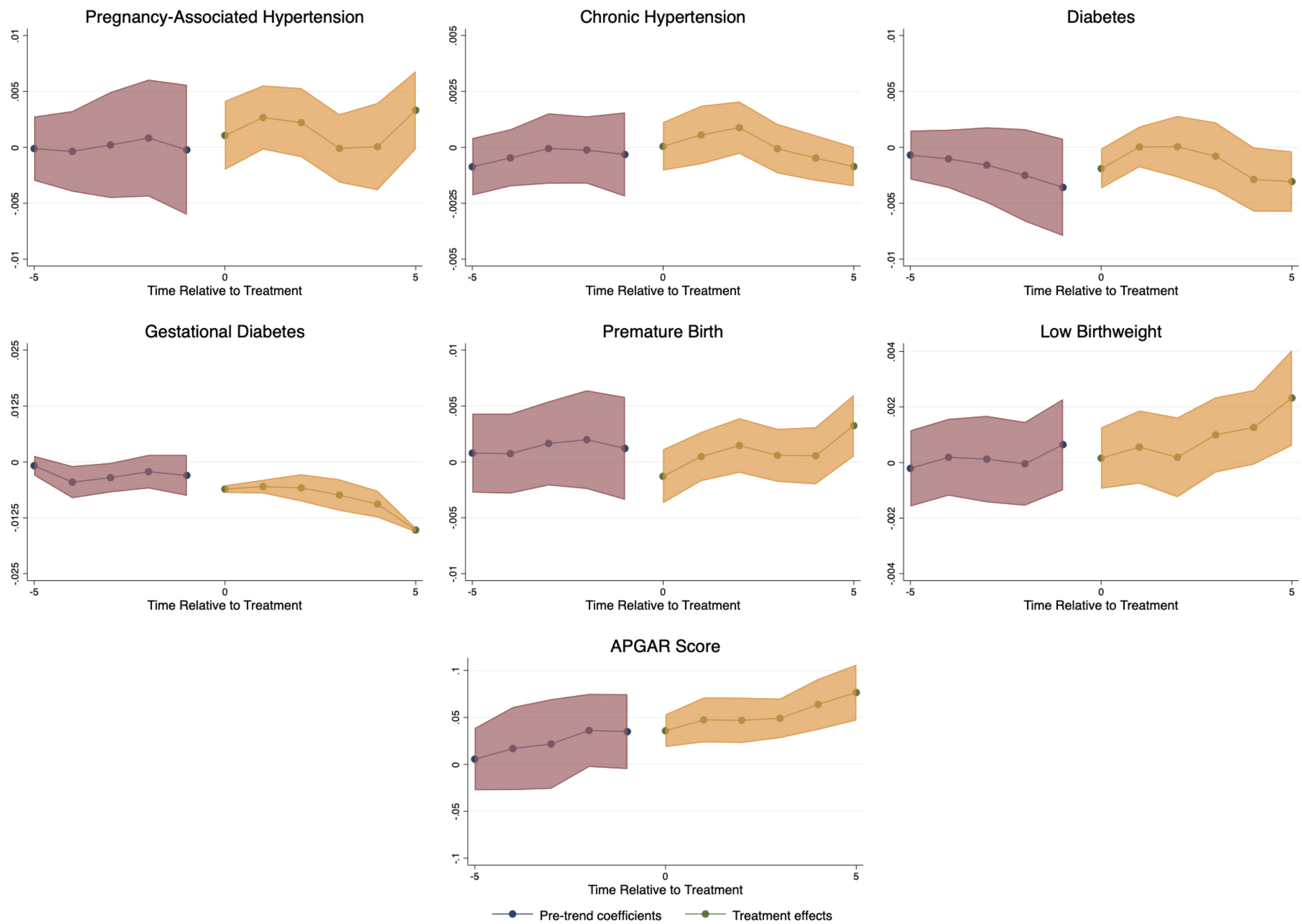
where $\hat{\gamma}_{r*t}$ represents the region-year fixed effects. Average treatment effects are then calculated according to equation (2) and (3).

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 A1: 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 A1: BJS Event Study — TRAP Laws (Region FEs Included)



Notes: Plots describing the pre-trend coefficients along with treatment effects of TRAP laws on outcomes from NCHS (2022) using the method in Borusyak, Jaravel, and Spiess (2021) and a specification that includes region-year fixed effects. 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.

Table A2: 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