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. I find that TRAP laws lead to 11-16% increased rates of hypertensive disorders of pregnancy. Additionally, I find evidence that TRAP laws widen existing disparities in adverse infant health outcomes across parental race. 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*. Fourteen[1] states now restrict abortion in all or almost-all circumstances. Florida, Georgia, and South Carolina restrict abortion after six weeks gestation, effectively prohibiting nearly all abortions. Iowa, Montana, and Wyoming currently have abortion bans that are temporarily blocked by state courts (The New York Times, 2024). As a result of these recent policy changes, people all over the country experience large and sudden increases in their travel distance to an abortion provider.

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 (Ananat et al., 2009). Abortion restrictions may result in avoided pregnancies through changes in contraceptive/sexual behavior, but they also result in a higher probability that a pregnancy is carried to term. Health outcomes may improve if a large number of high-risk pregnancies are avoided. However, if a greater number of high-risk pregnancies are carried to term, health outcomes will worsen. Then, the average effect of restricted abortion access on maternal and infant health outcomes is largely an empirical question.

In this paper, I estimate the effects of state-level targeted regulations on abortion providers (TRAP laws) on rates of adverse maternal and infant health outcomes using restricted-use Vital Statistics Natality data. The adoption of TRAP laws serves as a relevant natural

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

experiment for understanding some elements of the potential consequences of *Dobbs* because these supply-side regulations often burden abortion 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. Although much is changing in the post-*Dobbs* world, the estimated effects of TRAP laws permit the understanding of one major consequence of abortion bans.

I exploit the timing of TRAP laws at the state level and use the Borusyak, Jaravel, and Spiess (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 Pineda-Torres (2021) and robust to controlling for a variety of alternative 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 (2023) and a fixed effects design including county fixed effects, time fixed effects, and state-time fixed effects to measure the effects 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 from 0 to 100 miles increases county-level rates of pregnancy-associated hypertension and chronic hypertension by 8.7% and 16% respectively.

[2] Throughout the paper, "birthing people" refers to people of any gender who give birth.

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[3] in the United States (Cameron, et al., 2022; Declercq & Zephyrin, 2020; MacDorman, Thoma, Declerq, & Howell, 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, Gennuso, Givens, & Kindig, 2021; Gupta & Froeb, 2020). I implement a triple-difference procedure to explore how abortion laws affect the disparities in adverse outcomes across race, and I find that TRAP laws increase the gap in premature birth and low birthweight between Black and white infants by 3-6%.

This is the first study to describe the causal effects of any modern restrictive abortion policies in the US on the health status of people who carry to term and infants using administrative Vital Statistics Natality data. We know that restricted abortion access decreases abortion rates and increases birth rates (Jones & Pineda-Torres, 2024; Myers, 2021; Myers, 2023; Myers & Ladd, 2020; Lindo et al., 2017; Caraher, 2023; Dench et al., 2023), but relatively little about how abortion access affects other outcomes. I contribute foremost to the literature surrounding the effects of abortion access on outcomes beyond abortion and birth rates. Most of this evidence is dedicated to socioeconomic outcomes (Jones & Pineda-Torres, 2024; Brooks & Zohar, 2022; González et al., 2020; Mølland, 2016; Bloom et al., 2009; Lindo et al., 2020; Bahn

[3] The "maternal health crisis" refers to the increasing trends in adverse pregnancy outcomes/maternal mortality in the United States as well as the large persistent racial disparities in maternal health in recent decades. Though this term is common in the public health space, there is some evidence to suggest that the increasing rates of maternal mortality may be due to changes in measurement (Dattani, 2024)

et al., 2019), and the limited evidence on health outcomes focuses almost exclusively on maternal mortality. The mortality studies find that abortion restrictions are associated with higher rates of maternal mortality (Vilda et al., 2021; Hawkins et al., 2020) while expanded access to abortion results in lower maternal mortality/morbidity (Farin et al., 2024; Clarke and Mühlrad, 2021).

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, Levine, & Staiger, 1999; Joyce & Grossman, 1990; Joyce, 1987; Corman & Grossman, 1985; Grossman & 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 statistically insignificant, 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 summarize the vital statistics data. In Section 4, I estimate the effects of TRAP laws on state-level pregnancy and birth outcomes. In Section 5, I explore the heterogeneity of treatment effects by race. In Section 6, I estimate county-level effects from increasing travel distance to an abortion provider. Section 7 provides a discussion of the results, and Section 8 concludes.

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 et al., 2020; Kelly, 2020; Fischer, Royer, & White, 2018; Quast, Gonzalez, & Ziemba, 2017). Among national studies, Jones and Pineda-Torres (2024) 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. They find that teen birth rates increase by roughly 3% and that Black women exposed to TRAP laws as a teenager are less likely to attend and complete college. Arnold (2022) finds that TRAP laws decrease the abortion rate by 11-14% and increase birth rates by 2-3% in years following their passage. Caraher (2023) finds that TRAP laws reduce the abortion rate by 6% in affected states, largely driven by counties with large Black and Hispanic populations. In addition, Bahn et al., (2020) find that US TRAP laws increase rates of "job lock" among women in affected areas.

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 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.

The number of TRAP laws in the United States grows over the study period. Figure 1 plots the number of total TRAP laws in effect between 1990 and 2017. Over the course of 28 years, the number of TRAP policies grows from 7 laws in 1990 to 21 laws in 2017. In addition, I consider the TRAP legal coding from Jones and Pineda-Torres (2024). 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 the 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 the 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 do not have large effects on abortion access. Table A1 in the appendix summarizes the treatment timing for various TRAP laws by Austin and Harper (2019) and Jones and Pineda-Torres (2024).

3. 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). These files contain the universe of birth records in the United States between 1990 and 2017[4]. 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 the pregnancy, and various characteristics of the health of the infant at birth. Table 1 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.

I measure the effects of TRAP laws on maternal hypertensive disorders of pregnancy (pregnancy-associated hypertension, chronic hypertension) and common measures of infant health at birth (low birthweight, premature birth). Hypertensive disorders are cited as key risk factors during pregnancy by the CDC and NIH ([CDC], 2023; [NIH], 2018). 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. These outcomes are relatively rare: four percent of pregnancies involve pregnancy-associated hypertension and only one percent involve chronic hypertension.

Figure 2 provides a summary of the data over time by comparing the 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 predominantly in the rates of hypertensive disorders of

[4] The data on birth outcomes stop in 2017 so that they match the end date of the Austin and Harper (2019) TRAP law policy coding, which describes the presence of TRAP laws at the state level through 2017.

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 have a significant gap throughout, but the gap does widen 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. This evidence shows that states with TRAP laws have higher rates of some adverse health outcomes, but it does not support a claim that TRAP laws *caused* the difference in rates. To answer the question of causality, I turn to a variety of difference-in-differences methods.

4. State-Level Abortion Policy

To measure effects from abortion access on outcomes related to pregnancy and birth, I first exploit the variation in state-level TRAP policies over time. 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. I do not include individual-level demographic controls (such as for maternal age or race) in the specification because I hypothesize that TRAP laws may affect birth outcomes by changing the *composition* of people giving birth. So, in my setting, changing the average age or the racial composition of births is a possible pathway to the treatment effect, and not a potential confounder.

Recent evidence indicates that the TWFE procedure under staggered intervention timing may produce biased estimates of the ATT through the "forbidden comparison" between newly treated units and previously treated units when treatment effects are heterogeneous across units/time (Goodman-Bacon, 2021). For this reason, the preferred specification is the Borusyak, Jaravel, and Spiess (2021) imputation estimator (BJS).

The BJS estimation of the ATT is computed in a three-step process. In the first step, outcomes are regressed against the fixed effects using only never treated and not-yet treated state-time observations to impute potential outcomes for the treated units $Y_{ist}(0) = \hat{\alpha}_s + \hat{\delta}_t$. 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[5].

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

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

The underlying logic of this procedure is that it completely avoids the "forbidden comparison" of newly treated units to previously treated units. This imputation process ensures

[5] Standard error calculations in the BJS method are based on the sum-squared of residuals. The state-clustered variance estimator can be written as $\hat{\sigma}_w^2 = \sum_s (\sum_{it} v_{ist} \tilde{\epsilon}_{ist})^2$, where $\tilde{\epsilon}_{ist} = Y_{ist} - A'_{ist}\hat{\alpha}_s^* - X'_{ist}\hat{\delta}_t^* - D_{ist}\tilde{\tau}_{ist}$. The estimated treatment effects τ_{ist} are already imputed as a kind of residual ($Y_{ist} - Y_{ist}(0)$), so this variance estimator uses a new set of "treatment effects" $\tilde{\tau}_{ist}$, estimated using an auxiliary model that imposes the equality of treatment effects within state-year groups of observations. Borusyak et al. (2021) show that this procedure results in conservative standard error estimates.

that treated observations are only ever compared to untreated observations at the time period of analysis. 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:

$$Y_{ist} = \alpha_s + \delta_t + \sum_{k=-5}^{0} \gamma_k 1(timing_s - t = k) + \epsilon_{ist} \quad (4)$$

where $timing_s$ indicates the year that state s was treated by a policy change. This specification to determine pre-trends will use only the set of untreated observations (both never-treated and not-yet-treated units). 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. A pre-trends test to provide suggestive evidence regarding the likelihood of parallel trends is performed by estimating $\hat{\gamma}_k$ and testing $\gamma = 0$ jointly using an F test. Figure 3 and Table 2 demonstrate that there is not evidence of differential trends in the treatment/control group before the policy change.

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 (2021). So, 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 onetrip and two-trip mandatory waiting period for abortion services. Results in Table 3 indicate that effects are robust to the inclusion of these policies in the specification.

Table 3 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 (2024) 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 is the BJS method using the Austin and Harper (2019) TRAP treatment designation presented in column (2) of Table 3. 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 (2024), and therefore it should produce more conservative estimates of the average treatment effects.

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. Results from Table 3 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), alternative TRAP policy coding in column (4), and a standard errors adjustment for multiple hypothesis correction (shown in the appendix). There is not enough evidence to suggest that TRAP laws increase the risk of premature birth and low birthweight among infants – coefficients are generally small and not statistically significant.

Effects on premature birth are only meaningfully larger and statistically significant using the policy coding from Jones and Pineda-Torres (2024) in column (4).

Because TRAP laws are more common 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.

5. Health Disparities

Much of the literature establishes that the effects of abortion laws are often heterogenous across race (Jones & Pineda-Torres, 2024; Myers, 2021; Kelly, 2020; Clarke and Mühlrad, 2021; Farin et al., 2024). To determine if there exists significant racial disparities in the burdens of TRAP laws on maternal and infant health outcomes, I first present estimates of treatment effects by the birthing person's race. Here, I include maternal age fixed effects in the imputation step to account for differences in the age distribution across race:

$$Y_{ist}(0) = \hat{\alpha}_s + \hat{\delta}_t + \hat{\lambda}_a. \qquad (5)$$

where λ_a is a set of maternal age fixed effects. Treatment effects are then calculated according to equation (2) and aggregated separately by racial group according to equation (3).

Figure 4 shows that TRAP laws are associated with larger increases in adverse maternal/infant health outcomes for Black birthing people. This indicates that Black birthing people likely experience a larger burden from the passage of a TRAP law, consistent with the existing evidence in the literature. TRAP laws are associated with a 0.73 percentage point increase in the rate of pregnancy-associated hypertension among Black birthing people, nearly double the

0.39 percentage point increase among white birthing people. The difference in the magnitudes of the treatment effects is often larger for infants. TRAP laws are associated with a 5 percentage point increase in the rate of premature birth and low birthweight among Black infants, while the laws appear to be associated with small improvements[6] in average health at birth for white infants.

I implement a triple-difference specification to measure the differential effects of TRAP laws across race (Black vs white). I augment the imputation step of the BJS procedure to include group-state, group-time, and state-time fixed effects along with age fixed effects:

$$Y_{ist}(0) = \hat{\alpha}_{s*g} + \hat{\delta}_{t*g} + \hat{\lambda}_a + \hat{\lambda}_{s*t}. \quad (6)$$

After imputing potential outcomes in this manner, calculating average treatment effects follows the same procedure outlined in equation (2) and (3). Treatment effects from the triple-difference represent the average change in the gap between racial groups within a treated state after a TRAP law. The point estimates then describe the effect of TRAP laws on health disparities across race.

Figure 5 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 Black infants 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

[6] The difference in the direction of the treatment effect across Black and white infants may be explained in part by differential access to contraception. In the US, Black women are more likely to live in a “contraception dessert,” with reduced access to highly effective contraceptive options (Barber, et al., 2019). Additionally, 24% of Black women report using no contraceptive method, compared to 16% of white women (Dehlendorf, et al., 2014). So, the ability to avoid pregnancy when faced with restricted abortion access may be differential across race.

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 Figure 4.

6. Distance to an Abortion Provider

In this section, I move away from the binary policy indicator for treatment, using a panel of the travel distance to an abortion provider at the county-month level from 2009 to 2017 compiled by Myers (2023). Figure 6 describes the relationship between TRAP laws and distance to an abortion provider. On average, counties in states that pass a TRAP law between 2009 and 2017 experience a 17-mile increase in travel distance. For comparison, counties in states that do not pass TRAP laws experience an average increase in travel distance of less than one mile. Within states that pass TRAP laws during this period, there is large variation in the changes in travel distance – many counties see little to no change in their distance to an abortion provider, but others see increases over 300 miles. Identifying the county-level effects of changing travel distance, rather than state-level average effects of a policy change, may more precisely estimate the causal relationship between abortion access and pregnancy/birth outcomes.

I use a fixed-effects design exploiting variation in the distance to an abortion provider at the county level within a state over time to identify the average effect of increasing travel distance. I employ the specification:

$$Y_{ict} = \alpha_c + \delta_t + \beta distance_{100} + \lambda_{s*t} + \epsilon_{ict} \quad (7)$$

where Y_{ict} is the outcome of interest for an individual 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 λ_{s*t} is a state-time fixed effect.

The identifying assumption of this specification is that, within a given state, counties that experience an increase in their travel distance to an abortion provider would have experienced trends in their rates of adverse maternal/infant health outcomes similar to counties in the same state that experience no change in travel distance. To evaluate the plausibility of this assumption, I compare pre-trends across 1,332 counties that experience increases in travel distance and 1,340 counties that experience no changes in distance over the study period. I replace the distance measure in equation (7) with a set of indicators for years prior to the county-level distance change:

$$Y_{ict} = \alpha_c + \delta_t + \sum_{k=-5}^{0} \gamma_k 1[t - T_{change} = k] + \lambda_{s*t} + \epsilon_{ict} \quad (8)$$

where T_{change} is equal to the year that the distance increases in treated counties. In Figure 7, I plot the γ_k coefficients and find no evidence of differential pre-trends between counties that experience increases in distance and counties that see no distance changes.

Results from equation (7) shown in Table 4 indicate that larger travel distances increase rates of hypertensive disorders of pregnancy. Recent evidence from the growing literature on difference-in-differences with continuous treatment suggests that the precise interpretation of these estimates depends on the strength of the parallel trend assumption (Callaway, Goodman-Bacon, & Sant'Anna, 2024). Under the weaker parallel trend assumption described Figure 7, treatment effects can be interpreted as an "average level treatment effect" – the effect of increasing travel distance from 0 to 100 miles. So, increasing the distance to an abortion provider from 0 to 100 miles increases county-level rates of chronic hypertension by 16%. This increased distance also increases rates of pregnancy-associated hypertension by 9%, but the coefficient is not statistically different from 0 in this context. In the appendix, I show that these effects are linear, where larger travel distances correspond to larger effects on maternal hypertension.

In the special case where a stronger parallel trends assumption holds, coefficients may be interpreted more generally as an "average causal response." The strong parallel trend assumption requires that the evolution of outcomes in the treated group are equivalent to the evolution of outcomes if treatment was applied to the entire population. In other words, this assumption is equivalent to assuming that the average treatment effect on the treated (ATT) is equal to the average treatment effect (ATE). If this assumption is applied, an interpretation of the coefficients from Table 4 would be: "Increasing the travel distance to an abortion provider by 100 miles increases county-level rates of chronic hypertension by 16%." In my setting, this may be an implausibly strong assumption, as areas affected by the closure of abortion clinics likely differ in fundamental ways from areas that maintain consistent abortion access. For this reason, I interpret the results as an average level treatment effect.

Even under a weaker parallel trend assumption, there may be concerns about substantial treatment effect heterogeneity and the presence of counties that experience decreases in travel distance over the study period. I find that results are consistent after dropping counties with significant decreases in travel distance. To address the potential heterogeneity of treatment effects, I convert the continuous travel distance measure into a binary treatment variable and repeat the BJS procedure to produce estimates that are robust to heterogeneity across units/time. In this specification, coefficients are similar to those in Table 4. The results from both robustness checks are available in the appendix.

The distance analysis complements my state-level policy analysis by providing further evidence that restricted abortion access increases rates of adverse maternal health outcomes without evidence that restricted access significantly affects infants. Overall, the policy and distance analyses tell a consistent story that restricted access to abortion causes poorer maternal health outcomes on average.

7. Discussion

The Composition of Births

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.

In Table 5, 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, an indicator for attending fewer than 8 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, a higher incidence of insufficient prenatal care, 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[7]. So, I perform back-of-the-envelope calculations to describe the rate of adverse health outcomes among the marginal births by first using the BJS procedure to estimate the change in the number of births following a TRAP law.

Table 6 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 3 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.

[7] This term describes the additional births that are attributable to restricted abortion access.

This estimated effect on the average number of births following a TRAP law is larger than existing estimates in the literature. Arnold (2022) estimates the fertility effects of TRAP laws from 1995-2015 using a slightly different policy implementation coding and finds that birth rates increase by 3.2% following a TRAP law, which corresponds to 2 additional births per 1,000 reproductive-age females. Applying this estimate to my setting, where the population of reproductive-age females in treated states is 1,505,501 on average, implies that TRAP laws would increase the number of births by 3,011. Using this estimate to represent the number of marginal births, the rate of pregnancy associated hypertension among marginal births would be 14.2% (3.6x the mean rate) and the rate of chronic hypertension would be 4.9% (4.9x the mean rate).

8. 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%. These policies also increase racial disparities in infant health outcomes at birth – increasing gaps in premature birth and low birthweight between Black and white infants by 3-6%. In addition, increasing the travel distance to an abortion provider from 0 to 100 miles increases rates of chronic hypertension by 16%.

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 and exacerbate existing health disparities – adding to a crisis that is already concerning to public health professionals.

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Tables and Figures

Table 1: 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			108,840,640
0-8 years	0.05		
9-11 years	0.15		
12 years	0.30		
13-15 years	0.24		
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
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

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

Table 2: 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
Low Birthweight	1.398	0.244	43
Premature Birth	1.678	0.160	43

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

Table 3: 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 = 0.04)	[0.002]	[0.001]	[0.001]	[0.001]
Chronic Hypertension	0.0010	0.0016^{**}	0.0010	0.0023^{***}
(mean = 0.01)	[0.001]	[0.001]	[0.001]	[0.001]
Low Birthweight	0.0013	0.0004	0.0006	0.0010^{*}
(mean = 0.08)	[0.001]	[0.001]	[0.001]	[0.0005]
Premature Birth	0.0019	0.0014	0.0024^{*}	0.0043^{**}
(mean = 0.12)	[0.002]	[0.002]	[0.001]	[0.002]

Notes: Results from TWFE and BJS difference-in-differences analysis. Column (1) is estimated according to equation (1). Columns 2-4 use the BJS estimating procedure where potential outcomes are imputed first, and then treatment effects are calculated and aggregated according to equation (2) and (3). Column (2) uses the TRAP policy coding of Austin and Harper (2019), Column (3) uses the Austin and Harper coding along with a set of reproductive health policy controls included in the imputation step, and Column (4) uses the alternative policy coding from Jones and Pineda-Torres (2024). In each specification, standard errors are clustered at the state level. * *p<0.1*, ** *p<0.05*, *** *p<0.01*

Table 4: Travel Distance and Pregnancy/Birth Outcomes, 2009-2017

	PA Hypertension	Chronic Hypertension	Premature Birth	Low Birthweight
Distance (100s miles)	0.0036	0.0016***	-0.0006	-0.0005*
	(0.002)	(0.001)	(0.0005)	(0.0003)
Mean	0.05	0.02	0.12	0.08
N	35378433	35378433	35464801	35464801

Notes: Data on travel distance from Myers (2023). 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 (7). Standard errors are clustered at the state level. * *p<0.10,* ** *p<0.05,* *** *p<0.01*

Table 5: The Effect of TRAP Law on the Composition of Births, 1990-2017

	Black	Hispanic	Age	<8 Prenatal Visits	HS Educ Or Less
TRAP Law	0.0038	-0.0066	-0.0651	0.0199	-0.0026
	(0.004)	(0.007)	(0.065)	(0.012)	(0.005)
Mean	0.16	0.28	27.41	0.19	0.50
N	96122838	96122838	97215229	97215229	69388926

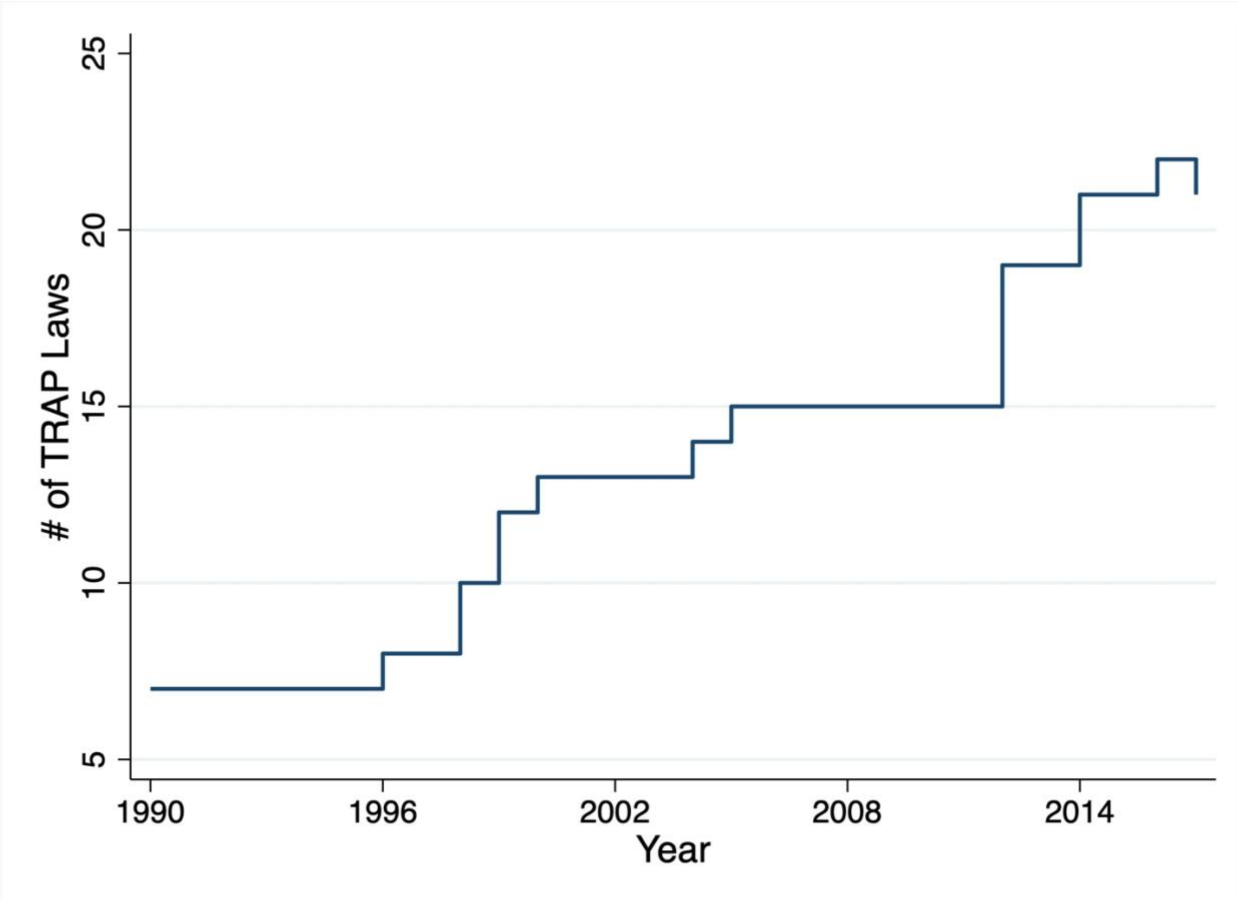
Notes: Coefficients measure the effect of TRAP laws on the features of birthing people using the BJS procedure (equation 2 and 3) and the Austin and Harper (2019) policy coding. Includes effects on binary indicators for race/ethnicity (Black and Hispanic), age in years, an indicator for receiving fewer than 8 prenatal visits, and an indicator for a high school education or less. Standard errors are clustered at the state level.

Table 6: The Effect of TRAP Laws on the Number of Births, 1990-2017

	Coefficient	Mean	S.D.	P	95% CI
# of Births	4432.34	80106.18	2213.58	0.036	[289.79, 8574.88]

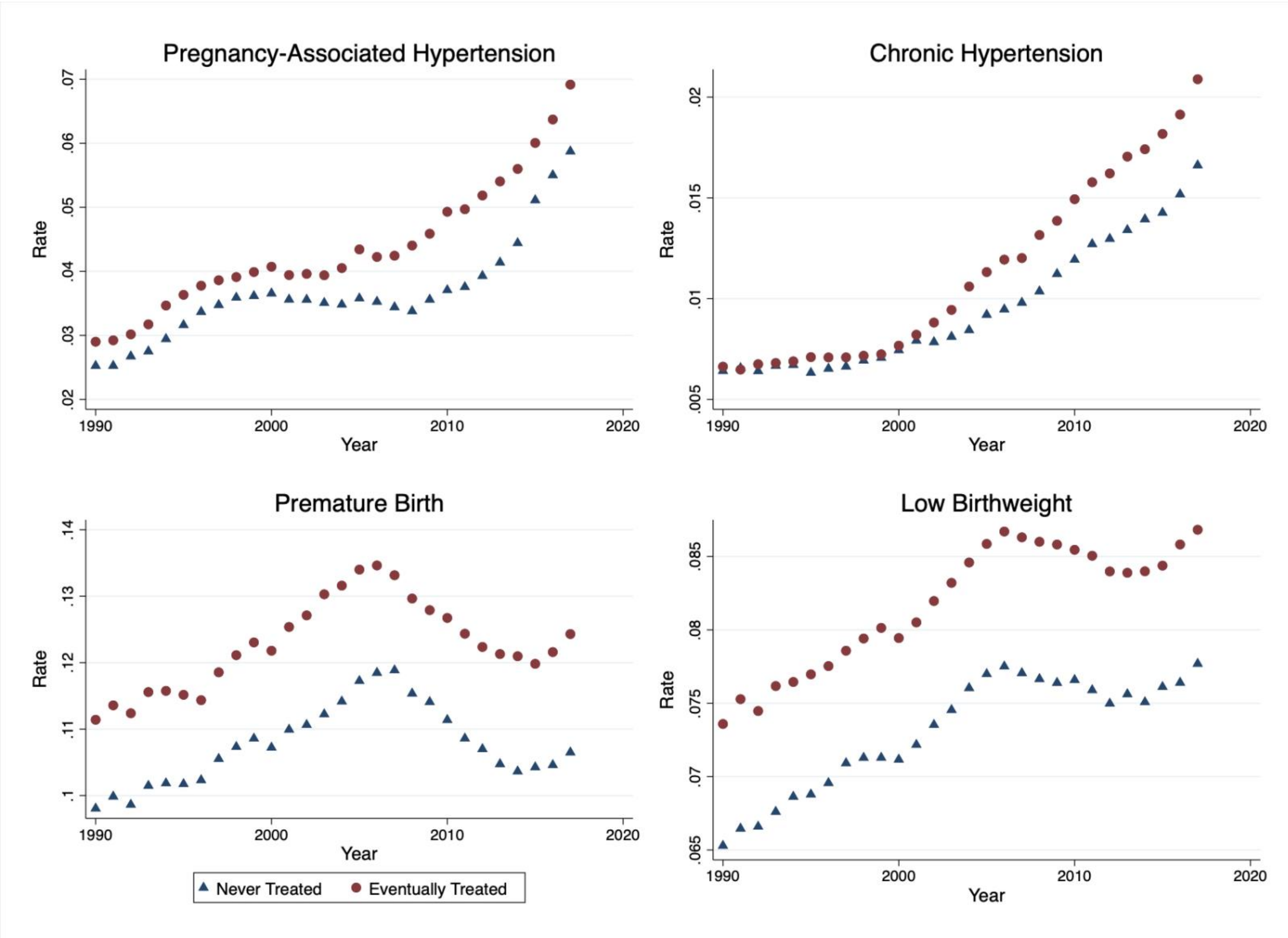
Notes: Coefficients measure the effect of TRAP laws on the number of births using the BJS procedure and the Austin and Harper (2019) policy coding. Standard errors are clustered at the state level.

Figure 1: Number of TRAP Laws Over Time (1990-2017)



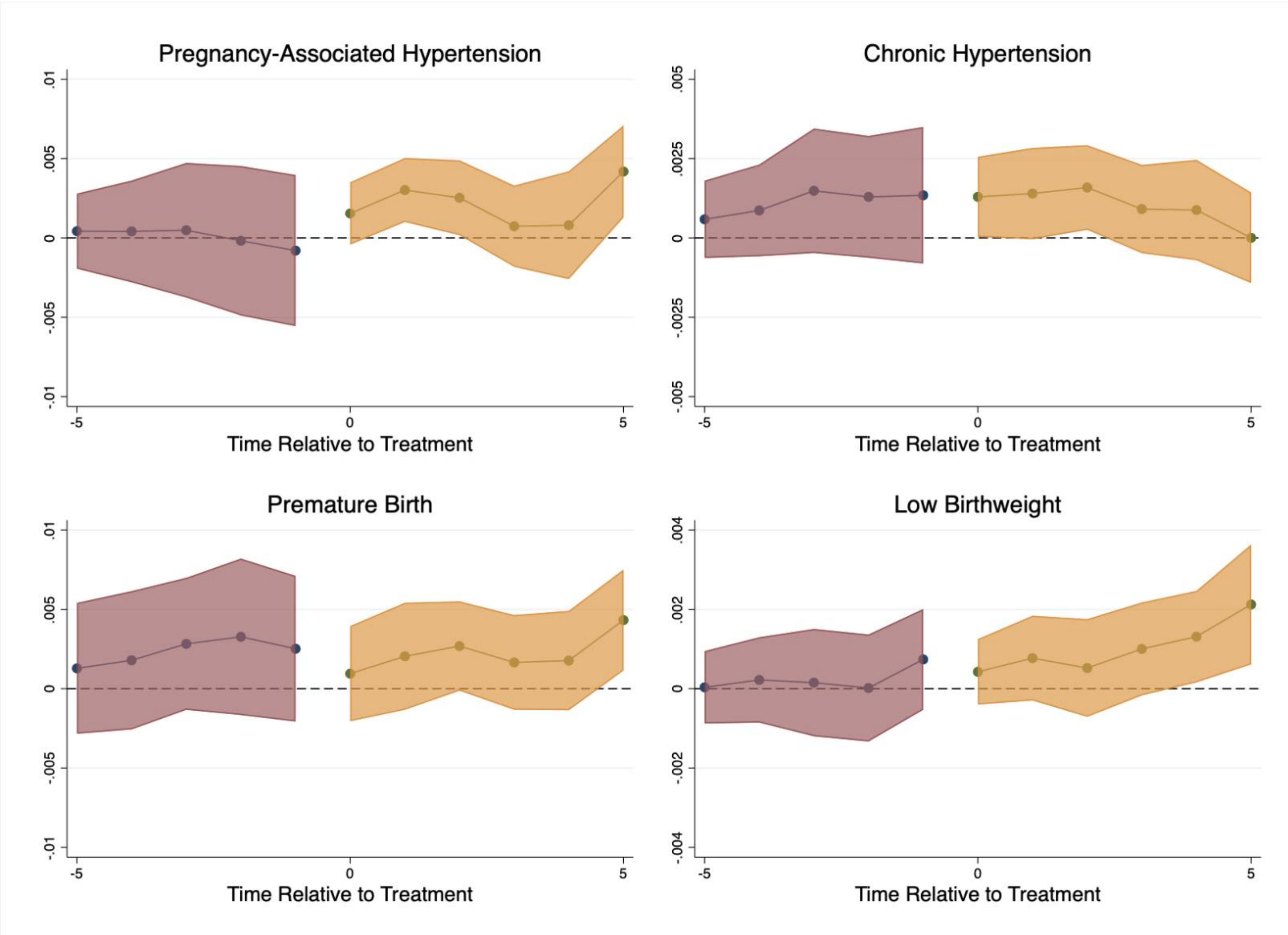
Notes: Figure describes the number of TRAP laws in the United States over time between 1990-2017. The number of TRAP laws in a given year is determined by the Austin and Harper (2019) policy coding. A single state may have multiple TRAP laws. So, the graph describes the number of total TRAP policies in effect, rather than the number of states with any TRAP law.

Figure 2: Abortion Restriction and Birth Outcomes, 1990-2017



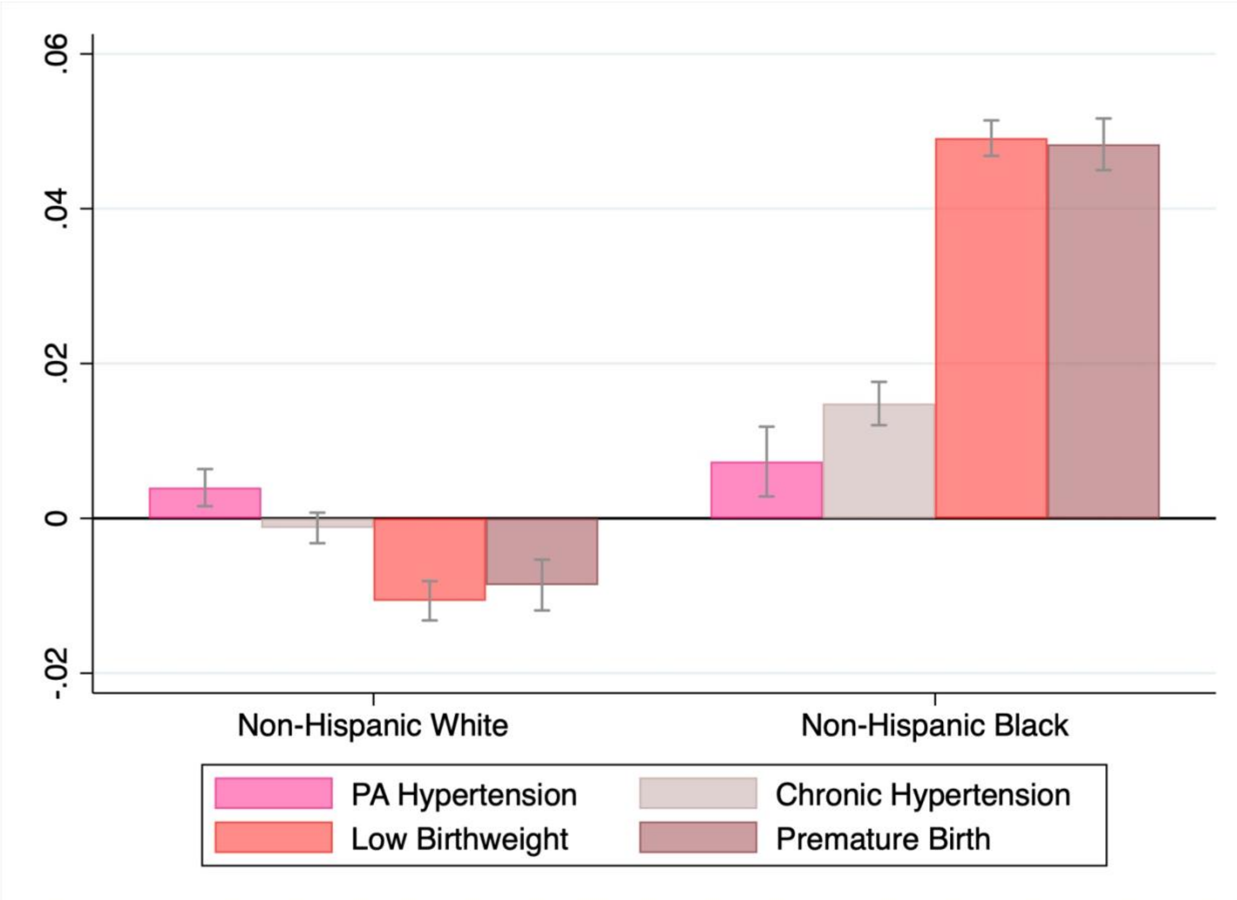
Notes: Figure describes the rates of average adverse health outcomes over time separately by treatment status. "Eventually Treated" refers to states that pass a TRAP law at some point between 1990 and 2017.

Figure 3: BJS Event Studies - TRAP Laws



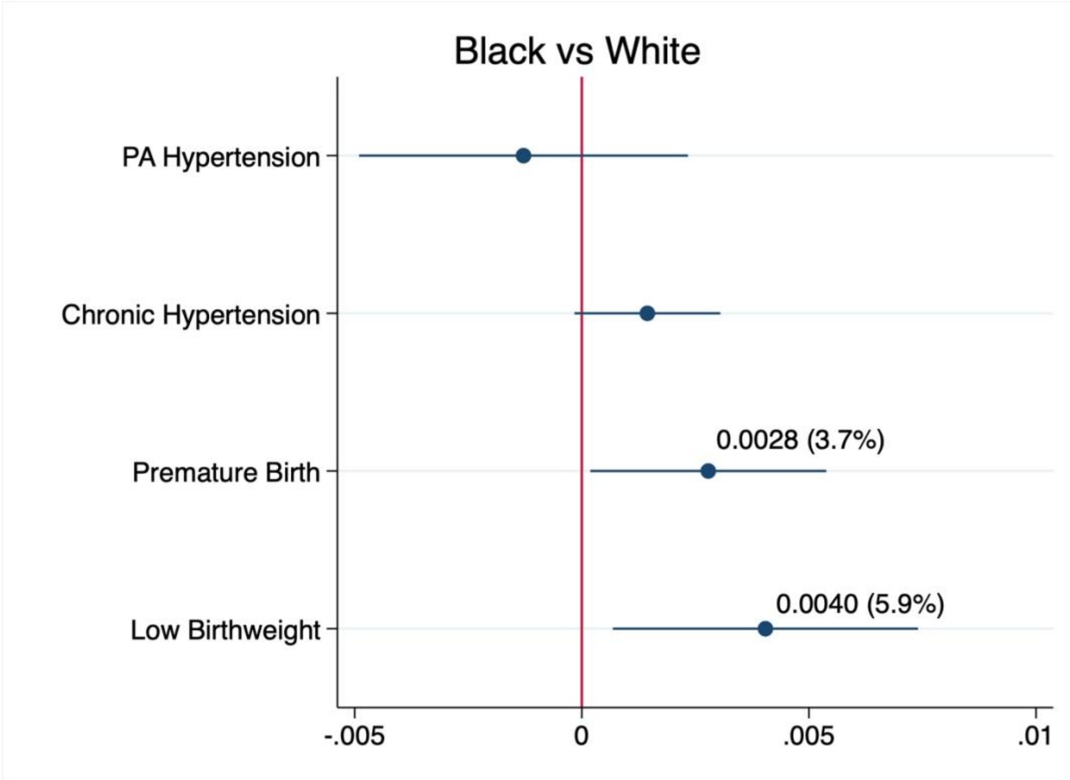
Notes: Plots describing the pre-trend coefficients along with treatment effects of TRAP laws on vital statistics outcomes from Borusyak, Jaravel, and Spiess (2021). Pre-trends and treatment effects are disjoint and colored differently to indicate that they are estimates from separate methods rather than the result of a dynamic specification commonly found in traditional event studies. Pre-trend coefficients are estimates of γ from equation (4). Treatment effects in the post period are weighted averages of the treatment effects imputed according to equation (2) and aggregated according to equation (3) by year.

Figure 4: BJS Difference-in-Differences Estimates by Race



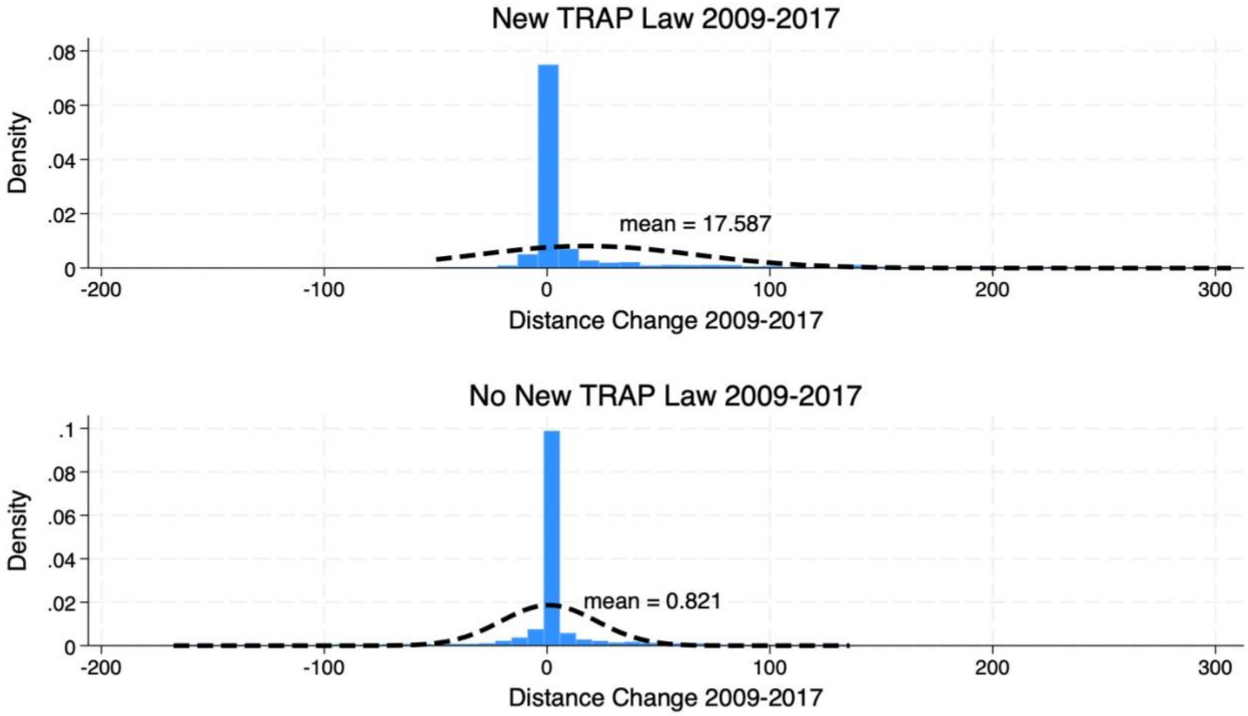
Notes: Figure describes the treatment effect estimates by maternal race from the BJS diff-in-diff procedure using the Austin and Harper (2019) policy coding. Here, individual treatment effect estimates are imputed according to equation (2) and aggregated according to equation (3) within the racial group.

Figure 5: TRAP Laws and Health Disparities



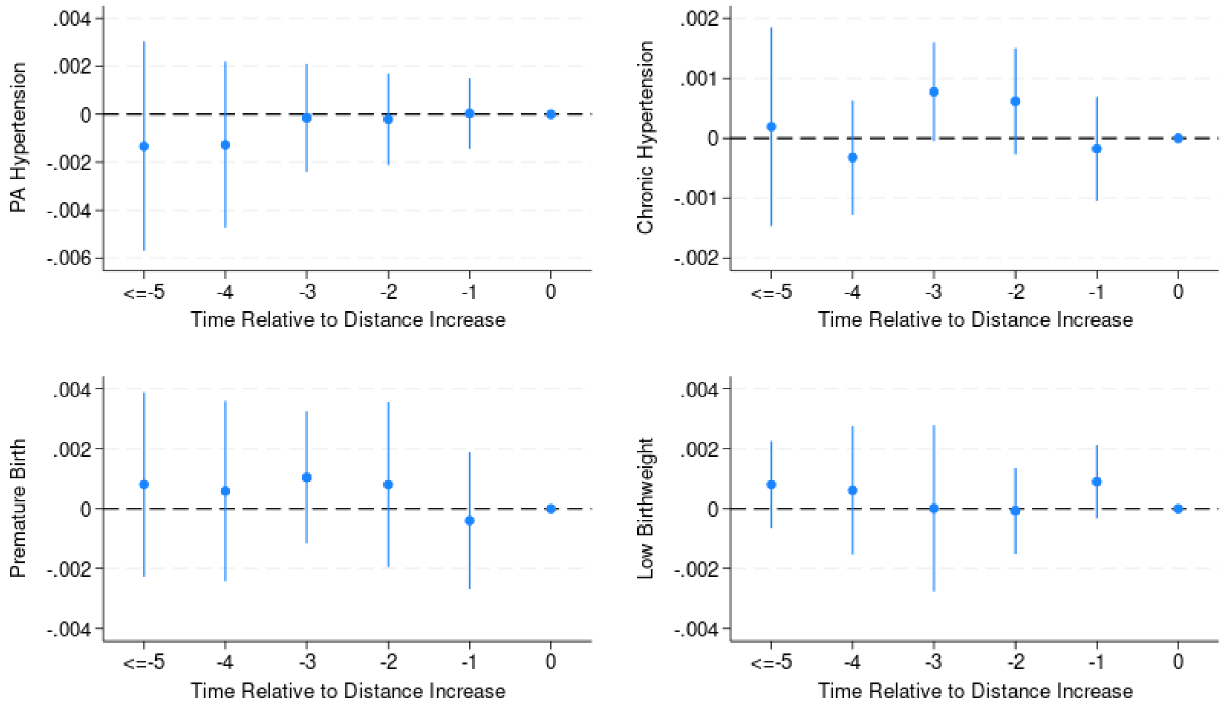
Notes: Figure describes results from the triple-difference design, measuring the change in the gap in adverse health outcomes across race after TRAP treatment. Potential outcomes are imputed according to equation (6), and treatment effects are calculated and aggregated according to equation (2) and (3). 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.

Figure 6: TRAP Laws and Travel Distance



Notes: Histograms describe the 2009-2017 change in travel distance by county by TRAP treatment status. The black dashed line indicates a normal distribution curve fit to the values, and average changes within each group are noted on the graphs.

Figure 7: Pre-Trends for Travel Distance Analysis



Notes: Graphs plot the γ_k coefficients from equation (8), describing the pre-trends across counties that experience increases in travel distance compared to counties that experience no distance changes over the study period.

Appendix – Supplemental Analysis

Summary of TRAP Laws

Table A1: TRAP Law Treatment Timing

	Austin and Harper (2019)			Jones and Pineda-Torres (2024)			
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 coding from Austin and Harper (2019) and Jones and Pineda-Torres (2024)

Table A1 describes in greater detail the timing of the passage of TRAP laws based on the two policy codings from Austin and Harper (2019) and Jones and Pineda-Torres (2024).

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

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

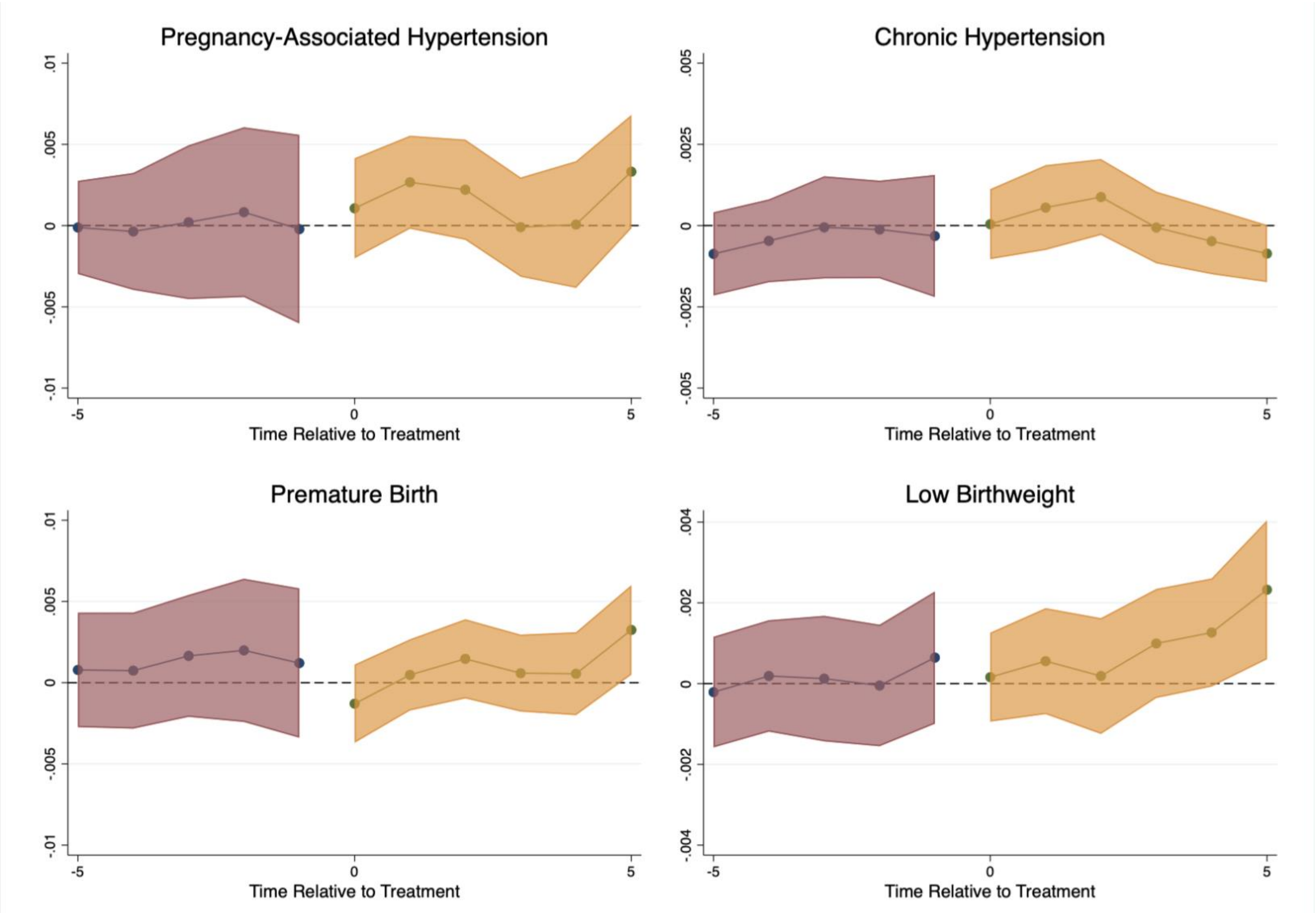
where $\hat{\gamma}_{r*t}$ represents the region-year fixed effects. Average treatment effects are then calculated according to equation (2) and (3).

There are no material changes to the difference-in-differences estimates and interpretations after including these additional fixed effects. Figure A1 and Table A2 present the BJS event study graphs and results from the F test described in Section 4. Table A3 presents the ATT estimates from the BJS difference-in-differences specification using the Austin and Harper (2019) policy coding.

Table A2: BJS Parallel Trends Assumption F Test (Region FEs Included)

	F-stat	p-value	df
PA Hypertension	1.780	0.138	42
Chronic Hypertension	2.000	0.098	42
Low Birthweight	0.847	0.524	42
Premature Birth	1.225	0.314	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 A3: BJS Difference-in-Differences Results (Region FEs Included)

	PA Hypertension	Chronic Hypertension	Premature Birth	Low Birthweight
TRAP Law	0.0042***	0.0010	0.0013	0.0013*
	[0.001]	[0.001]	[0.001]	[0.001]
N	95654017	95654017	96695485	96695485

Travel Distance and Heterogenous Treatment

The fixed effects design used to measure the effect of changing travel distance on maternal and infant health outcomes may be subject to a similar kind of bias as a TWFE specification due to the "forbidden comparison," as newly treated units are compared to counties that have previously experienced changes in travel distance. If there are heterogeneous treatment effects from travel distance across counties/time, this would introduce bias to the fixed effects estimates in Section 6.

In this section, I convert the continuous measure of distance to an abortion provider from Myers (2023) into a discrete treatment variable and repeat the BJS difference-in-differences procedure at the county level to provide estimates that are robust to treatment effect heterogeneity. I select a treatment threshold of 100 miles, such that counties with travel distances greater than (or equal to) 100 miles are treated, and counties with travel distances less than 100 miles are untreated. So, estimates describe the effects of travel distance moving beyond the 100 miles threshold on infant/maternal health outcomes.

The BJS difference-in-differences procedure is equivalent to the process described in Section 4, with one change to the imputation step. Potential outcomes are imputed according to equation (7) such that $Y_{ict}(0) = \hat{\alpha}_c + \hat{\delta}_t + \hat{\lambda}_{s*t}$. This allows me to exploit the more granular county-level information on provider distances, and the inclusion of the state-time fixed effect accounts for any variation in health outcomes driven by the staggered adoption of the revised birth certificate.

Results, presented in Table A4, demonstrate that estimates from this specification are similar to the fixed effects specification in Table 4. Estimated effects on rates of chronic hypertension are similar in direction and magnitude compared to the fixed effects specification with continuous treatment. Across the alternative specifications, the direction of the effect of travel distance on infant low birthweight varies but is not statistically different from zero in either case. The coefficient on pregnancy-associated hypertension from the BJS specification is larger compared to the fixed effects specification but operates in the same direction. The effect on premature birth among infants is quite different across specifications, with no statistically significant effect observed the fixed effects design while the BJS procedure indicates that crossing the 100-mile threshold increases the rate of premature birth by 3.5 percentage points.

Table A4: BJS Diff-in-Diff with Binary Travel Distance Treatment, 2009-2017

	PA Hypertension	Chronic Hypertension	Premature Birth	Low Birthweight
Distance > 100 miles	0.0078***	0.0017**	0.0035**	0.0006
	(0.002)	(0.001)	(0.001)	(0.001)
N	33901690	33901690	33986532	33986532

I do not anticipate that the effects of travel distance on health outcomes would be exactly equivalent across both alternative specifications. The fixed effects design measures the effect of increasing the distance to a provider from 0 to 100 miles, while the BJS specification measures the effects of changing travel distance from less than 100 miles to 100 miles or more. This definition of treatment in the BJS specification is less intuitive, as a county with a 101-mile distance to a provider would be compared to a county with a 99-mile distance – a limitation of converting a continuous variable into a binary treatment. The goal of this exercise is merely to demonstrate that results persist using methods robust to treatment effect heterogeneity.

Travel Distance Analysis with Decreasing Distance Counties Dropped

In Figure 6, I show that there is a wide variation in the county-level changes in travel distance from 2009-2017, with some counties experiencing large decreases in travel distance. Because the presence of counties with decreasing travel distance may complicate the interpretation of the treatment effect estimates in Table 4, I repeat the fixed-effects procedure described in Section 6 excluding counties with large (>20 mile) decreases in their travel distance to an abortion provider. This amounts to dropping roughly 2% of the birth data sample.

Table A5: Travel Distance and Pregnancy/Birth Outcomes (Decreasing Counties Dropped)

	PA Hypertension	Chronic Hypertension	Premature Birth	Low Birthweight
Distance (100s miles)	0.0047**	0.0018***	-0.0002	-0.0006***
	(0.002)	(0.001)	(0.0004)	(0.0002)
N	34582274	34582274	34667721	34667721

Notes: Results shown use the fixed effects specification in equation (7) after excluding the set of counties with greater than 20-mile decreases in their travel distance to an abortion provider between 2009 and 2017. Standard errors are clustered at the state level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Results in Table A5 indicate that coefficients are similar to the coefficients in Table 4 after dropping counties that experience decreases in their travel distance to an abortion provider.

Multiple Hypothesis Correction

In this paper, I study two empirical questions: the effect of a TRAP law and the effect of increasing the travel distance to an abortion provider. In each case, I study the effect of abortion access across four outcomes, corresponding to four individual hypotheses. Because of this, there may be a reasonable concern of an inflated rate of Type 1 error in statistical tests. To assuage this concern, I perform a multiple hypothesis correction using the Benjamini-Hochberg Method (Benjamini & Hochberg, 1995).

This method is directed at controlling the false discovery rate (FDR), the expected proportion of incorrectly rejected null hypotheses (type 1 errors). Here, hypotheses are ordered in ascending order of their p-values. Hypotheses are rejected using an alternative threshold from standard statistical significance level α. The new threshold for significance becomes $\frac{i}{m}\alpha$, where i is the rank of the p-value in the ordered list, and m is the total number of hypotheses.

In the BJS difference-in-difference specification reported in column (2) of Table 3, the coefficients for the four outcomes and their p-values are ranked as follows:

1. pregnancy-associated hypertension (p=0.002) 2. chronic hypertension (p=0.035) 3. low birthweight (p=0.519) 4. premature birth (p=0.533)

To determine, for example, if the estimated coefficient describing the effect of a TRAP law on pregnancy-associated hypertension is significant at the 1% level after correcting for multiple hypothesis testing, the p-value must be less than $\frac{i}{m}\alpha = \frac{1}{4}(0.01) = 0.0025$. In this case, $i = 1$ because the p-value of the effect of the policy change on pregnancy-associated hypertension is in the first position of the hypothesis ranking. Since $p = 0.002 < 0.0025$, the null hypothesis is rejected at the 1% level. Table A6 summarizes the rejection of the four hypotheses at different levels of significance.

Table A6: Rejection of the Null Hypothesis Following Multiple Hypothesis Correction (TRAP)

Null Hypothesis	Reject at $\alpha = 0.01$	Reject at $\alpha = 0.05$	Reject at $\alpha = 0.10$
Effect on PA Hypertension = 0 (p = 0.002)	X		
Effect on Chronic Hypertension = 0 (p = 0.035)			X
Effect on Low Birthweight = 0 (p = 0.519)			
Effect on Premature Birth = 0 (p = 0.533)			

Notes: Table describes the rejection of my four null hypotheses regarding the effects of TRAP laws following a multiple hypothesis correction using the Benjamini-Hochberg method for controlling the false discovery rate.

The hypotheses regarding the effect of a TRAP law on pregnancy-associated hypertension is still rejected at the 1% level after the correction. The null hypothesis that TRAP laws have no effect on rates of chronic hypertension is rejected at the 10% level after the correction, but this hypothesis is rejected at the 5% level without any multiple hypothesis correction.

I repeat the procedure above for my second empirical test measuring the effects of increasing the travel distance to an abortion provider. In this case, the hypotheses are ranked according to the p-value of the initial test as follows:

1. chronic hypertension (p = 0.010) 2. premature birth (p = 0.056) 3. pregnancy-associated hypertension (p = 0.135) 4. low birthweight (p = 0.253)

Table A7 describes rejection of the null hypotheses using the same procedure for determining new significance thresholds detailed earlier. After correction, the null hypothesis regarding the effect of increasing travel distance on chronic hypertension is rejected at the 5% level.

Table A7: Rejection of the Null Hypothesis Following Multiple Hypothesis Correction (Distance)

Null Hypothesis	Reject at $\alpha = 0.01$	Reject at $\alpha = 0.05$	Reject at $\alpha = 0.10$
Effect on PA Hypertension = 0 (p = 0.135)			
Effect on Chronic Hypertension = 0 (p = 0.010)		X	
Effect on Low Birthweight = 0 (p = 0.253)			
Effect on Premature Birth = 0 (p = 0.056)			

Notes: Table describes the rejection of my four null hypotheses regarding the effects of increasing travel distance to an abortion provider following a multiple hypothesis correction using the Benjamini-Hochberg method for controlling the false discovery rate.

Are the Effects of Distance Linear?

Using the fixed effects design in equation (7), I explore the linearity of the treatment effects of increasing the travel distance to a provider. I generate a set of indicators for travel distance and repeat the estimating procedure on these indicators. I use the following travel distance thresholds: 50-100, 100-150, 150-200, and 200+. Results in Table A8 indicate that for maternal health outcomes, treatment effects are increasing in distance. For these outcomes, treatment effects are generally increasing but not statistically different from zero until distance is greater than 200 miles. Treatment effects for infant health outcomes do not exhibit the same pattern. The direction of the treatment effect changes often across higher distance thresholds.

Table A8: Discrete Travel Distance and Pregnancy/Birth Outcomes, 2009-2017

	PA Hypertension	Chronic Hypertension	Premature Birth	Low Birthweight
$50 \leq$ dist. < 100	0.0011	-0.0004	0.0001	-0.00001
	(0.002)	(0.0003)	(0.002)	(0.0005)
$100 \leq$ dist. < 150	0.0011	0.00004	-0.0005	-0.0005
	(0.002)	(0.0007)	(0.001)	(0.0005)
$150 \leq$ dist. < 200	0.0011	0.0012	0.0013	0.0026***
	(0004)	(0.0008)	(0.003)	(0.0007)
$200 \leq$ dist.	0.0134***	0.0054***	-0.0017***	-0.0022***
	(0.004)	(0.0011)	(0.0006)	(0.0005)
N	35378433	35378433	35464801	35464801

Notes: Data on travel distance from Myers (2023). 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 (7) where the treatment is defined to be a series of indicators for travel distance at different thresholds. Standard errors are clustered at the state level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$