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Abstract

This paper analyzes the impact of supply-side abortion restrictions on aggregate abortion and birth rates in the United States. Specifically, I exploit state and time variation in the implementation of the first targeted regulation of abortion provider (TRAP) law in a state to identify the effects of the laws. I find that TRAP laws are associated with a reduction in the abortion rate of approximately 5% the year the first law is implemented, and an average reduction of 11-14% in subsequent years. There is also evidence that TRAP laws increased birth rates by 2-3%, which accounts for approximately 80-100% of the observed decline in abortion rates.

Keywords: Abortion, Abortion Providers, Economics of Gender, Public Policy
JEL Codes: I18, J13, J16, J18

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1 Introduction

On June 29, 2020, the U.S. Supreme Court struck down a Louisiana law requiring abortion providers to have admitting privileges at a nearby hospital. The ruling came just four years after *Whole Woman's Health v. Hellerstedt*; in that ruling, the Court struck down two similar requirements in Texas' House Bill 2 (HB2). These measures imposed strict regulations targeting abortion providers that led to over half the abortion-providing facilities closing in Texas. The regulations highlighted in these court cases are examples of the more restrictive forms of laws commonly known as "Targeted Regulation of Abortion Providers" or TRAP laws. According to the Alan Guttmacher Institute, as of July 1, 2020, 23 states had laws or policies categorized as TRAP laws. Despite the prevalence of these laws, few papers have studied the impact of these regulations on women's fertility outcomes. This lack of research is especially notable since the *Whole Woman's Health v. Hellerstedt* ruling stressed the importance of evidence-based research to weigh the benefits of a law against the burden it imposes.

Given the Court's stated emphasis on research, along with a large number of restrictions on abortion providers that remain today, it is crucial to empirically investigate the impact of these laws on abortion access and health outcomes in a variety of settings. I collect data on the first date a TRAP law was implemented in a state and exploit the state-time variation in implementation dates. This study uses data from the past two decades to examine the effect of the initial implementation of TRAP laws on state-level abortion and birth rates.

Previous work has studied the impact of Texas' HB2 on abortion access in Texas (Grossman et al., 2017; Lindo et al., 2019; Fischer et al., 2018). These studies examine the effects of increasing the distance to the nearest abortion provider on the abortion rate by using HB2 as a natural experiment for the closure of Texas' abortion clinics. Overall, they find evidence that HB2 shuttered abortion clinics, increasing driving distances to the nearest abortion provider, which led to fewer abortions. A study on TRAP laws passed in Wisconsin finds effects on abortions and births similar to those found in the Texas context (Venator and Fletcher, 2020). While these studies provide convincing evidence that distance to an abortion provider is an essential determinant of obtaining an abortion in Texas and Wisconsin, at least in the short run, there is a lack of research that analyzes the impact of supply-side abortion policies in a context outside of Texas, and whether these effects persist over time (Medoff, 2010).

The contribution of this paper is two-fold. First, I analyze whether the first TRAP law passed in a state has a measurable impact on aggregate in-state abortion and birth rates across the nation. Second, I identify whether the effects persist in the long run and if there are nonlinear impacts of these laws over time. I exploit the state and time variation in the implementation of TRAP laws to analyze the potential dynamic effects of the laws on abortion and birth rates over time.

I find an initial 5% reduction in the abortion rate the year a state first passes a TRAP law, and the effect grows over the next several years. The primary difference-in-differences estimate reveals that TRAP laws are associated with an approximate 14% decrease in the abortion rate, roughly 1.9 fewer abortions per 1,000 women of childbearing age.

While the reduction in abortion rate results is substantial, it should be interpreted with some caution because the abortion rate is measured in the state of occurrence. Thus, even though abortion rates in a state are declining, women may travel to neighboring states to obtain an abortion, which would lead to an overestimate of the total decline in abortion rates. An analysis using birth rates as the primary outcome variable may alleviate some of this potential bias because women are likely not systematically traveling from non-TRAP states to TRAP states to give birth. Consequently, by analyzing the impact of TRAP laws on birth rates, I can measure whether the observed decline in in-state abortions results in more births. Findings from this analysis reveal that post-implementation, birth rates rise by 2-3%. To put this finding in context, this is roughly 1.2-2 more births per 1,000 women of childbearing age. This estimate implies that approximately 80-100% of the observed decline in in-state abortions may result in pregnancies being carried to term.

Overall, the findings suggest that TRAP laws meaningfully reduce in-state abortion rates and that this effect is sustained for up to a decade after a state implements the law. Furthermore, there is evidence that birth rates increase, which accounts for much of the observed decline in abortion rates in a state. However, there are several important nuances to keep in mind when interpreting the findings from this study. First, in some specifications, the estimated increase in births does not 100% offset the observed decline in abortions; this could be due to increased inter-state travel or increased risk avoidance behavior by women.

Secondly, there are different types of TRAP laws (see Section 2.1 for more discussion), and even within one specific category, there may be wide variation between states in the written text of these laws. Thus, it is important to emphasize that the effects identified here are *average* effects; studying a specific TRAP statute in one state may lead to effect sizes that are significantly larger than those found in this paper, while other laws may have effects that are substantially smaller (or even negligible) than those identified in this paper.

Lastly, one should be careful when interpreting the long-run results because several mechanisms are difficult to disentangle, all of which may contribute to the observed increasing magnitude of the point estimates over time. One possibility is that these laws become more impactful over time, potentially due to delayed implementation of the TRAP law. Another possibility is that a state passes additional TRAP laws after passing its first TRAP law. States tend to "stack" TRAP laws, enacting more and more over time;

thus, the increasing effects observed could be due to greater burden placed on providers. Finally, states may pass other policies that impact abortion (birth) rates in the same direction as the TRAP law after they implement their initial TRAP law. While I control for some state policies that may impact both abortions and births, there may be other laws enacted after the initial TRAP law that may impact abortions and births in the same direction as my findings.[1]

The remainder of the paper is structured as follows: Section 2 describes TRAP laws and previous literature, Section 3 describes the data, Section 4 explains the empirical strategy, Section 5 documents the results and discusses potential mechanisms, and Section 6 concludes.

2 TRAP Laws and Background

2.1 TRAP Laws

States can regulate abortion providers by imposing burdensome legal requirements on the facilities or physicians that perform abortions; these laws are broadly referred to as "Targeted Regulation of Abortion Providers" or TRAP laws. TRAP laws vary in their specific regulations, but there are three broad ways to classify TRAP law requirements: abortion facility, ambulatory surgical center, and admitting privileges requirements.

The first two classifications, abortion facility and ambulatory surgical requirements, target the abortion facility's physical structure. Abortion facility requirements impose standards that facilities must meet to perform an abortion. These requirements may regulate various facility conditions such as the size of the procedure and recovery room, hallway and doorway width, emergency power systems in place, or ventilation requirements. The ambulatory surgical center regulation requires abortion facilities to meet the same standards of ambulatory surgical centers. The necessary renovations to meet these requirements can be costly, and if a facility is unable to afford the upgrades, then the abortion provider may shut down (Grossman et al., 2017) or increase the price of abortions (Medoff, 2008).

The third type of requirement, the admitting privilege requirement, can be a stand-alone law or can be paired with either the general abortion facility requirements or ambulatory surgical center requirement. The admitting privilege law requires that either the facility or physician have admitting privileges, or another transfer agreement, at a local hospital. In some instances, the law specifies an exact distance to the local hospital. For example, in Texas' HB2 bill, the abortion provider was required to have admitting privileges at a hospital within 30 miles of the abortion facility. This situation is not limited to Texas; before the

[1] Concerning demand-side abortion restrictions, I control for parental notification laws, Medicaid abortion restrictions, and mandatory waiting periods. I also control for Medicaid family planning expansions during this time frame. However, there may be other laws passed during this time for which I am unable to control.

Whole Woman's Health v. Hellerstedt, Wisconsin and Mississippi also had similar requirements, though court challenges enjoined the laws, and after the *Whole Woman's Health v. Hellerstedt* decision, the appeals were dropped. In South Carolina, the law states that any facility that performs abortions shall have one physician with admitting privileges at a local hospital (S.C. 61-12.205(C)(2)). Another statute requires *all* physicians at clinics performing abortions after 14 gestational weeks to have hospital admitting privileges (S.C. 61-12.305(B)). Neither subsection specifies a distance to the hospital where the physician has admitting privileges. Laws similar to South Carolina's remain in effect, and states continue to enact comparable requirements; for example, Indiana passed a similar bill in 2016.

2.2 Expected Impact of TRAP Laws

If TRAP laws are effective at increasing the cost of providing abortions, facilities may pass this cost along to patients (Medoff, 2008), or they may shut down if they are unable to meet the requirements (Grossman et al., 2017). However, the expected impact of this cost increase on abortion and birth rates is unclear. Below I detail five different scenarios for how the laws may (or may not) impact abortion and birth rates. While I discuss each scenario as mutually exclusive, there is always the possibility that different women may fall into each of the proposed scenarios. However, I do not observe individual-level decisions that women are making, so, ultimately, the aggregate effect that I measure is determined by the proportion of women that fall into each category.

1. *Women who previously would have terminated a pregnancy now cannot and instead carry to term.* This scenario would appear in the data as a decline in abortions and an increase in births.

2. *In response to the higher cost of obtaining an abortion, there is a reduction in risky behavior, either through increased use of effective contraception or abstinence.* Empirically, this situation would appear as a reduction in abortions but would have no impact on births.

3. *Women may travel to another state to obtain an abortion.* This would appear in the data as a decline in abortions in the TRAP state, an increase in abortions in the neighboring state, and no change in births in the TRAP state.

4. *Women may be able to travel to another abortion provider within the state.* As long as the remaining clinics can accommodate the increase in demand, this would not impact abortions or births at the state-level.

5. *Women who are unable to travel may self-induce abortions.* This would result in a decline in the reported (legally-induced) abortions and no change in births.

2.3 Related Literature

There is a substantial literature exploiting state-level variation in the implementation of demand-side policies intended to influence access to contraception and abortion on women's reproductive and labor outcomes (Goldin and Katz, 2002; Bailey, 2006; Kearney and Levine, 2015). One particular issue with studying the effect of these policies is the difficulty in obtaining information on when a specific law, such as access to birth control (or a TRAP law), becomes effective in a state or locality. This difficulty can lead to discrepancies in the legal coding of a particular law. As Myers (2017) shows, the legal coding led to overestimating the impact of access to birth control on marriage and childbearing and underestimating the impact of abortion policies on marriage and the onset of motherhood. In her recent work, Myers (2022) does an extensive deep-dive into the historical policy environment and provides suggested legal coding to resolve the discrepancies for many of the most studied fertility policies such as the legalization of abortion, confidential access to birth control for unmarried women, confidential access to legal abortion, and parental involvement laws.

A handful of recent studies have explored the impact of supply-side abortion restrictions on abortion access (Joyce, 2011; Grossman et al., 2017; Lindo et al., 2019; Lu and Slusky, 2019; Fischer et al., 2018; Venator and Fletcher, 2020; Kelly, 2020). Joyce (2011) compares the effect of a Texas policy that had provisions impacting both the demand and supply side. The demand side abortion policy stated that patients must receive specific information 24 hours before obtaining an abortion. The supply-side abortion policy required abortions performed after 16 gestational weeks to be in an ambulatory surgical center. If the demand-side policy reduced abortions, then there should be an observed reduction for all gestational weeks, while the supply-side regulation should only impact abortions after 16 gestational weeks. Joyce (2011) finds that the demand-side policies do not appear to influence the abortion rate, while the supply-side policy meaningfully reduces the number of abortions performed after 16 weeks of gestation.

Grossman et al. (2017) focus on the impact of Texas' HB2 bill on the number of abortion providers and abortion rates in Texas counties. Grossman et al. (2017) show that between 2012 and 2014, as the nearest abortion provider's distance increases, the number of abortions in a county decreased. Lindo et al. (2019) also study the impact of Texas' HB2 bill on abortions in Texas. They find that there are nonlinear effects regarding distance to the nearest abortion facility and abortion rate. Specifically, increasing distance to the nearest abortion provider from 0-50 miles to 50-100 miles leads to an approximate 16% reduction in the abortion rate; the effects are largest when the initial distance is 0-50 miles, and decrease as initial distance increases. Novelly, The authors also show that increased travel distance is not the only channel that could result in falling abortions, but also increased congestion at clinics due to fewer clinics serving the population contributes significantly (approximately 59%) to the observed decline in abortion rates.

Fischer et al. (2018) also examine the impact of the HB2 bill and two other legislative changes in Texas; one change restricts Medicaid reimbursements for abortion, the other change cuts funding to family planning services. Fischer et al. (2018) use these laws to study how these policies impact abortion rates, birth rates, and contraceptive purchases. They find that when there is no access to an abortion provider within 50 miles, abortions fall by 16.7%, and births rise by 1.3%. They observed minimal effects of these policies on the purchases of male contraceptives.

Like the previous studies mentioned, Venator and Fletcher (2020) also study the effect of TRAP laws in one state; however, their focus is on Wisconsin rather than Texas. Specifically, Venator and Fletcher (2020) study the impact of three TRAP laws passed in WI between 2010 and 2017, which ultimately resulted in the closure of two out of five abortion clinics in the state. The authors find that increasing the distance to the nearest clinic by 100-miles results in approximately 30.7 percent fewer abortions and 3.2 percent more births. Unlike Lindo et al. (2019), Venator and Fletcher (2020) find that these effects are solely due to increased distance and not increased congestion.

Kelly (2020) is similar to above in that she studies the impact of a TRAP law in one state. Specifically, she examines the effect of a 2011 Pennsylvania law that passed new abortion facility licensing requirements regulations which ultimately led to the closure of 9 out of 22 abortion providers. She finds a decrease in the abortion rate of approximately 14% and a potential increase in the birth rate of 3.4%. Unique to Kelly (2020) is the channel that is likely causing the observed effect. Whereas in Venator and Fletcher (2020), nearly all of the impact on abortions and births is due to increased travel distance, the findings from Kelly (2020) are entirely attributable to increased congestion. She can attribute this finding largely to increased congestion because the clinic closures primarily occurred in urban areas; so, while there are fewer clinics to serve the same population size, the average distance to the nearest clinic remained unchanged.

Lu and Slusky (2019) study the effect of reducing access to family planning clinics (which includes but is not limited to abortion providers) on Texas's fertility rates. Specifically, they examine how increasing the distance to the nearest family planning clinics impacts these outcomes. They find that increased driving distance to a family planning clinic increased fertility rates by approximately one to two percent.

Finally, there is a small but growing literature examining the impact of fertility policy on *second* generation fertility behavior. Gutierrez (2022) uses the 1966 Romanian abortion ban to study the effect on the next generation's demand for children and finds that individuals whose mothers were impacted by the ban had significantly lower demand for children. This finding highlights that today's fertility policy can impact population and demographics for generations.

Overall, the previous literature on supply-side abortion restrictions has generally focused on the immediate effects of responses to a state policy change. Together, these papers provide compelling evidence that bills in a specific state meaningfully impact abortion and birth rates in that state, and potentially the next generations demand for children as well. What remains unanswered is the persistence of these supply-side policies and whether the findings from specific examples may hold in other contexts.

3 Data

3.1 Abortion and Birth Data

There are two prominent sources for abortion data in the United States, the Centers for Disease Control (CDC) and the Alan Guttmacher Institute (AGI). Both datasets report the number of abortions and the abortion rate by state of occurrence. The CDC abortion data is generally viewed as a lower bound estimate because a state's central health agency or another source (such as hospitals) voluntarily submit information to the CDC, while the AGI dataset is a survey of all abortion providers in a state. The CDC publishes their statistics in their annual "Abortion Surveillance Report," from which I collect the abortion rates from these reports for 1995-2015.

States vary in how they collect and report abortion data to the CDC. For example, when a state reports to the CDC, they indicate the number of abortions in their state, referred to as "state of occurrence." Some states will additionally breakdown this data by a woman's state of residence. However, not all states report data by state of residence. Furthermore, those that report by a woman's state of residence do not necessarily publish this information every year. Since there are inconsistencies in the collection and reporting regarding a woman's state of residence, my analysis focuses on abortions by state of occurrence.

Due to the abortion surveillance system's voluntary nature, some states do not report abortion statistics to the CDC. For example, the CDC does not have abortion data for California after 1996 or New Hampshire after 1997. Other states fail to report this information for select years. For example, Louisiana does not report abortion data for the years 2005 and 2006. Specifically, between 1995 and 2015, the following states are missing at least one year of abortion data, with the number of years missing in parenthesis: Alaska (5 years), California (19 years), Delaware (1 year), Louisiana (2 years), Maryland (9 years), New Hampshire (18 years), Oklahoma (2 years), West Virginia (2 years), and Wyoming (8 years). For all analyses involving the CDC data, I present the results that include all 50 states and D.C. The appendix presents the results obtained when I exclude states with any missing data.

The other prominent source of abortion estimates by state of occurrence comes from the AGI. Similar to the CDC dataset, I use the abortion rate by "state of occurrence." An advantage to the AGI dataset is that the AGI collects information on the number of abortions performed in a state by conducting a direct survey of abortion providers for each state beginning in 1973.[2] I use data from the AGI for the years 1980 through 2014. One difference between the AGI and CDC dataset is that the AGI abortion count is typically higher-although the correlation between the two datasets is over 97%. The downside of the AGI dataset is that the institute does not conduct the survey every year. Between 1980 and 2014, the AGI did not collect abortion data for the following years: 1990, 1993, 1994, 1997, 1998, 2001, 2002, 2003, 2006, 2009, and 2012.

Using the data on the number of abortions in a state-year, I calculate the abortion rate, the number of abortions per 1,000 women age 15 through 44 (henceforth referred to as the "childbearing age"). Data on the number of women of childbearing age in a state-year is from the Surveillance, Epidemiology, and End Results (SEER) Program.

The data for the analysis of birth rates is from the CDC's Wonder database, which reports state-level birth counts beginning in 1995. The birth rate is calculated by dividing the number of births in a state-year by the female population of childbearing age a state-year.[3] Again, state-level population data for childbearing age women is from the SEER Program.

3.2 TRAP laws

The main source of identification of TRAP laws is from two datasets, the abortion facility licensing and ambulatory surgical center requirement, maintained by LawAtlas. The LawAtlas datasets were created by a team from the Policy Surveillance Program along with the researchers who conceptualized and designed the study, Jones et al. (2018).[4] Of particular interest for this study are the laws that require abortion facilities to enter into a transfer agreement with a local hospital and laws that require facilities to meet physical or structural standards to operate. LawAtlas has a series of questions, such as, "What type of relationship, if any, is the facility required to have related to patient hospital transfers?" and lists the corresponding state statute. For example, the law in Kentucky requires facilities to have a transfer agreement with a hospital. The corresponding state statute is Ky. Rev. Stat. §216B.0435. I then used WestLaw to identify the statute and its effective date.[5] Figure 1 shows the year each state first implemented a TRAP law as described above. The dates for this analysis range from 1983 through 2015.

[2] In 1973, the AGI did not collect data for Louisiana or North Dakota.

[3] I multiply this ratio by 1,000 to obtain the number of births per 1,000 women of childbearing age.

[4] The online appendix of Jones et al. (2018) contains detailed information about each TRAP law.

[5] Continuing the Kentucky example, I identified Ky. Rev. Stat. Ann. §216B.0435 (West) to be effective as of July 15th, 1998

Table 1: Dates of Enforcement of TRAP Laws by State

State	Year	State	Year
Alaska	1997	North Carolina	2013
Alabama	2013	North Dakota	2013
Arkansas	2012	Nebraska	2000
Arizona	2000	Connecticut	1996
Oklahoma	1998	Florida*	2005
Rhode Island*	2002	Pennsylvania	1983
Indiana*	2006	South Carolina*	1996
Kansas	2011	South Dakota	2006
Kentucky	1998	Tennessee	2012
Louisiana	2015	Texas‡	2003
Michigan	1978	Utah	1990
Missouri	2007	Virginia**	2009
Mississippi†	1991	Wisconsin	2013

TRAP laws are from LawAtlas and Jones et al. (2018).
*These states adopted additional TRAP laws that specifically targeted post-first trimester abortions.
†Mississippi adopted a TRAP law applying specifically to post-first trimester abortions in 1996.
‡Texas' law applied only to post-first trimester abortions until 2009, when Texas adopted a law applying to all stages.
**Virginia's law applied only to post-first trimester abortions until 2012, when Virginia adopted a law applying to all stages.

3.3 Demographic Data

All models will control for state-level time-varying economic and demographic characteristics. Data on demographic conditions for the years 1980 through 2015 are from the Integrated Public Use Microdata Series Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS). Demographic characteristics are averages for the female population of childbearing age. Specifically, all regressions control for the average age the female, childbearing age population and for the proportion of the childbearing age population that is: black, white, Hispanic, married, living at or below the federal poverty line (FPL), and receiving Medicaid. Data on annual state-level unemployment rates for 1980 through 2015 are from the Local Area Unemployment Statistics, U.S. Bureau of Labor Statistics (BLS).

Table 2 Column (1) presents the summary statistics for non-TRAP states only, Column (2) shows the statistics for TRAP states only, and Column (3) displays the statistics for all states. The demographic variables are mostly comparable between TRAP and non-TRAP states. A few characteristics that might stand out are the racial composition and marriage rates. In particular, TRAP states appear to have a slightly larger white (80% versus 78%) and black population (13% versus 11%), and a smaller Hispanic population (12% versus 13%) and appear to have a slightly larger proportion of married women.

Table 2: Summary Statistics

	Non-TRAP states (1)	TRAP states (2)	Total (3)
Abortion rate (CDC)	14.03	11.52	12.72
	(6.94)	(5.47)	(6.34)
Abortion ratio (CDC)	222.91	177.86	199.47
	(114.70)	(94.06)	(106.81)
Birth rate	63.66	66.67	65.23
	(6.54)	(7.82)	(7.38)
Average age	30.00	29.88	29.94
	(0.52)	(0.57)	(0.55)
Proportion white	0.78	0.80	0.79
	(0.18)	(0.10)	(0.14)
Proportion Black	0.11	0.13	0.12
	(0.13)	(0.11)	(0.12)
Proportion Hispanic	0.14	0.12	0.13
	(0.12)	(0.12)	(0.12)
Proportion married	0.46	0.48	0.47
	(0.06)	(0.05)	(0.06)
Proportion at or below FPL	0.15	0.16	0.15
	(0.04)	(0.04)	(0.04)
Unemployment rate	5.81	5.79	5.80
	(1.85)	(2.03)	(1.94)

Data on the abortion rate in a state-year is from the CDC's annual Abortion Surveillance Reports, data on number of births in a state-year is from the CDC's Wonder database, and data on the female population is from SEER. Data on demographic characteristics for women of childbearing age are from IPUMS CPS 1995-2015. FPL is the federal poverty level. The annual state-level unemployment rate is from the BLS.

4 Methodology

To study the effects of TRAP laws on abortion rates, I use an analysis that exploits the state and time variation in the adoption of TRAP laws and allows the impact of the laws to evolve non-linearly over time. In particular, I estimate the following state-level model using weighted least squares:

$$ln(Y_{s,t}) = \beta_0 + \sum_{y=-10}^{-2} \pi_y TRAP_s 1(t - T_s = y) + \sum_{y=0}^{10} \delta_y TRAP_s 1(t - T_s = y) \\ + \%Neigh\ TRAP_{s,t}\beta_1 + X_{s,t}\beta_2 + \gamma_s + \phi_t + \epsilon_{s,t}, \quad (1)$$

where the dependent variable, Y is the natural log of the abortion rate (or birth rate) in state s at time t. The coefficients of interest in equation 1 are π_y and δ_y. These are the coefficients on the interaction between $TRAP_s$, a dummy equal to one if the state has ever enacted a TRAP law, and $t - T_s$, a dummy equal to one if the observation occurs y years before (or after) the TRAP law is implemented.[6] All regressions omit the year before implementation of the TRAP law. The coefficient π_y describes the differences in abortion rates between TRAP and non-TRAP states relative to the year before the TRAP law, while δ_y describes the evolution in the abortion rate after implementation of the TRAP law.

It is important to note here that there are several mechanisms that could contribute to increasing effect sizes over time that are difficult to disentangle. First, TRAP laws vary widely from state to state, as do the implementation and enforcement of these laws. Texas' HB2 bill provided a unique opportunity to study the impact of clinic closures due to the nature of how quickly the law went into effect. Other states may pass a TRAP law, but some clinics may be "grandfathered in," essentially giving clinics a year or two to comply with the law. Because of this, we might not expect to see impacts on abortion or birth rates immediately after legislation.

A second mechanism is that once the state passes one bill, it may adopt more restrictive TRAP laws in the future. Adopting more TRAP laws is common practice and is often mentioned as a strategy to make it increasingly difficult to provide or access an abortion. For example, according to AGI, in 2019, two states enacted TRAP laws, Arkansas and Louisiana; in 2018, two states enacted TRAP laws, Indiana and Louisiana; and in 2017, Missouri, Arkansas, Indiana, and Texas all passed TRAP laws. All of the states mentioned had previously implemented a TRAP law. These new bills may adjust or add to the existing laws in place. Thus, we might expect effect sizes to become more prominent over time as states adopt more laws, thereby increasing the burden placed on providers.

Lastly, if TRAP states tend to adopt other policies that impact abortions (births) in the same direction as

[6] Values Y less than -9 years or greater than 9 are grouped together to ensure that the coefficients are well estimated.

the TRAP law, this could also cause an increase in effect sizes over time. While I control some demand-side abortion and family planning policies, states may have passed other policies during this time that I do not observe.

I summarize the main results in a difference-in-differences analysis where I replace the individual indicators in equation (1) with dummies for three year categories (abbreviated D_y in equation 2 for the period: -8 (8 or more years before a TRAP law is passed), -5 to -7, -2 to -4, Year 0, 1-3, 4-6, 7-9, and 10 or more years after the passage of TRAP law. The year prior to the TRAP law is omitted from the regression. Specifically, I estimate the following state-level model using weighted least squares:

$$Y_{s,t} = \beta_0 + \beta_y \sum_y TRAP_s D_y + \%Neigh\ TRAP_{s,t}\beta_1 + X_{s,t}\beta_2 + \gamma_s + \phi_t + \epsilon_{s,t} \tag{2}$$

Finally, I use a difference-in-difference (DiD) analysis, estimated by the following equation:

$$\begin{aligned} Y_{s,t} = \beta_0 + Year\ 0_t \times TRAP\_s\beta_1 + Post_t \times TRAP_s\beta_2 + \%Neigh\ TRAP_{s,t}\beta_3 \\ + X_{s,t}\beta_4 + \gamma_s + \phi_t + \epsilon_{s,t}, \end{aligned} \tag{3}$$

Where $Year\ 0$ is equal to one the year the TRAP law is implemented and $Post$ is an indicator variable equal to one for every year post-implementation. These two binary variables are interacted with $TRAP_s$, a binary equal to one if state s has implemented a TRAP law.

For the analysis described by Equation (1), I present two models. Model 1 controls for state and year fixed effects as well as time-varying demographic characteristics and other state-level reproductive health policies. Model 2 adds to Model 1 by including region-by-year fixed effects.

Time-varying demographic characteristics for the female population of childbearing age include the average age and the proportion of this population that is black, white, Hispanic, married, at or below the federal poverty line (FPL), receiving Medicaid. I also control for the state-level unemployment rate.

Because the unit of observation is the state of occurrence, and women may travel from more restrictive to less restrictive states to obtain an abortion, it is important to control for the TRAP status of neighboring states to avoid overstating the impact of the TRAP law on abortion rates (Medoff, 2008). All regressions will include $\%Neigh\ TRAP$, a continuous variable between 0-100 that reflects the percentage of neighboring states that have adopted a TRAP law.

I also control for other reproductive policies that may impact abortion or birth rates. One of those policies is whether a state has a TRAP law that explicitly targets post-first trimester abortions providers.[7] Other

[7] Georgia and New Jersey have TRAP laws that only apply to post-first trimester abortions. Florida, Indiana, Mississippi,

state-level reproductive health policies controlled for include: a mandatory waiting period for obtaining an abortion, restricted Medicaid funding for abortions, expanded access to family planning services with a Medicaid waiver based either on income or for postpartum women (Kearney and Levine, 2015), and parental notification law for minors (Myers and Ladd, 2020).

For the analyses described by Equation (2)-(3), I present a third model that uses the specification from Model 2, but excludes states that never-adopted a TRAP law. As described in Borusyak and Jaravel (2017), the estimates from this specification may be biased toward short-run effects because the long-run effects may be negatively weighted. Therefore, the results from this specification should be interpreted with some caution.

Recently, there has been a surge in the literature identifying potential problems with traditional two-way fixed effects (TWFE) when treatment adoption is staggered (Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Borusyak et al., 2021). In particular, Goodman-Bacon (2021) shows that under the "best" conditions, when variance-weighted parallel trends hold *and* treatment effects do not vary over time, two way fixed effects estimates a variance-weighted average treatment effect on the treated. When treatment effects vary over time, the estimator may be biased. In response to this work, many researchers have developed alternative methods for estimating the average treatment effect on the treated (ATT) (Borusyak et al., 2021; Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Gardner, 2021).

To address these concerns, I use two newer techniques as alternatives to the traditional TWFE specification, Callaway and Sant'Anna (2021) and Borusyak et al. (2021). In the first estimation method, two groups can be used as control groups. One control group is the never-treated units, which I refer to as "CS Never Treated (2020)." The second control group is never treated and not-yet-treated units, which I will refer to as "CS Not Yet Treated (2020)". The third estimation I will use is an alternative approach to the problem, which uses an imputation method, as developed by Borusyak et al. (2021), which I will refer to as BJS (2021).

Before presenting the results, there are four points regarding estimation and identification to make clear. First, the identifying assumption for the difference-in-differences analysis is: absent of treatment, the trend in the abortion rate in treatment states would have evolved similarly to control states. When controlling for region by year fixed effects, the estimates rely on comparison within each region. Though there is no formal way to test the parallel trend assumption, the inclusion of leads and lags in the primary analysis allows for examining pre-treatment trends between treatment and control groups, relative to the year before implementation.

Rhode Island, South Carolina, Virginia, and Texas have TRAP laws that apply to all abortions as well as separate TRAP laws that apply to only post-first trimester abortions.

Secondly, there are several limitations of the study design and data. One potential concern is that the state-level abortion rates may overestimate the impact of reductions in overall abortions obtained due to women traveling to neighboring states. While I control for the TRAP status of neighboring states to capture this effect, the variable is rough because of its aggregate nature. Another potential limitation is the presence and use of "omnibus" bills. These omnibus bills may package several measures relating to abortion providers, other reproductive policy, or even entirely unrelated subject matters. While ideally, every unique requirement would be identified and coded to disentangle the effects; this is well beyond the scope of this project.

Thirdly, it is important to emphasize that the estimates presented are reduced form estimates and can be thought of as the policy's impact on abortion and birth rates. It is important to keep in mind that due to the variety of TRAP laws (which are broadly discussed in Section 2.1), and the variation even within one specific category, there may be wide variation in effect sizes by the type of law passed. The exact text of each bill are not separately identified and coded, therefore the effects identified in this paper are *average* effects. Some TRAP laws (or particular aspects of the law) may lead to effect sizes that are significantly larger than those found in this paper, while others may have effects that are significantly smaller (or even negligible) than those identified in this paper.

Finally, to improve efficiency, the results from the analysis presented weight by the population of women aged 15-44 in state s at time t. All standard errors are clustered at the state level to allow for correlation within a state over time.[8]

5 Results

5.1 Effect of TRAP laws on Abortions

5.1.1 CDC Data

Figure 1 presents the estimates for π_y and δ_y as described in Equation 1 using the CDC data. Figure 1 plots the point estimates from each of the four models (and corresponding 95% confidence intervals) using all states.

Model 1, denoted with small "x" markers, includes state and year fixed effects, controls for state-level reproductive policies, and time-varying economic and demographic characteristics. Model 2, denoted with hollow, square markers, adds region by year fixed effects to Model 1. For more detailed descriptions of each

[8]The main analysis was conducted for both the abortion rate and birth rate specification, excluding population weights. The findings from these analyses are quantitatively similar to those presented in the primary analysis. A Poisson analysis using the count of abortions and births was also conducted. In the Poisson specification, regressions are not weighted. Instead, population is used as the exposure variable. The Poisson results for abortions and births are largely similar to the main findings.

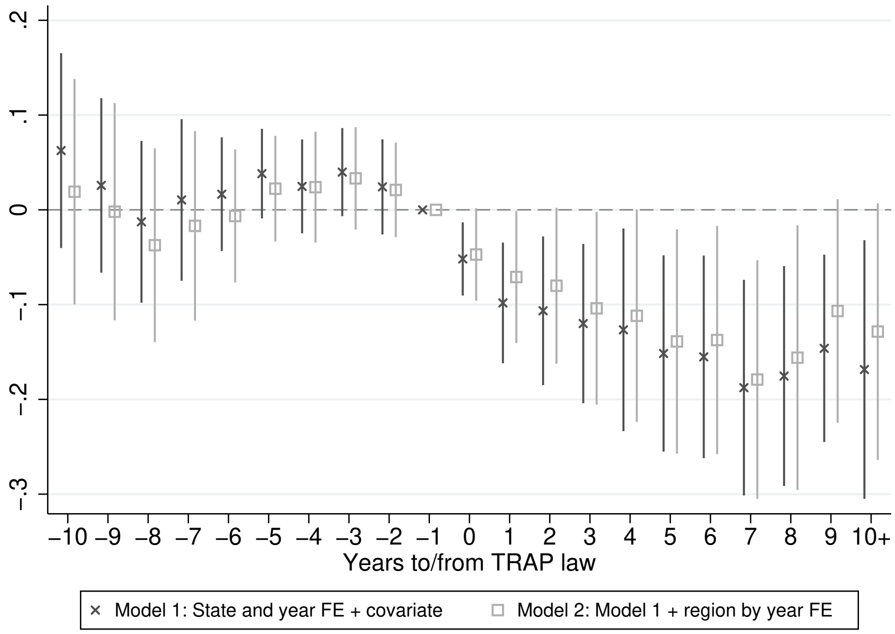

Figure 1: The Effect of TRAP Laws on Log of Abortion Rate using CDC Data

Notes: Model 1 controls for state and year fixed effects (FE), demographic characteristics, the unemployment rate, and policy controls. See Table 3 for details on the data sources including demographic and policy variables and the data sources. . Model 2 adds region by year fixed effects to Model 1. All regressions weight by the female population of childbearing age and cluster standard errors at the state level.

model and control variables, see Section 4.[9]

Overall, the results presented in Figure 1 shows that the differences in abortion rates between treatment and control states are generally statistically indistinguishable from zero and small in magnitude, relative to the year before adopting a TRAP law. After implementing the TRAP law, there is an initial reduction in the abortion rate. This decline grows within the first few years and stabilizes around year five. The point estimates from the two models are generally similar to one another and within each other's confidence intervals.

Table 3 presents the grouped event study and the difference-in-differences estimates. Column (1) presents Model 1, Column (2) presents Model 2, and Column (3) Model 3. The *pre-TRAP* results from all models in Panel A suggest no statistical differences between the trends in the abortion rate in the treatment and control states relative to one year before the TRAP law. I test whether the coefficients from the *pre-TRAP* variables are jointly significant for each of the models presented and show the two-sided p-value from this test in the row labeled "T-Test." The test fails to reject that the pre-treatment coefficients are jointly significantly different from zero in all specifications.

The post-treatment results from Panel A, Column (1) of Table 3 imply that there is an approximate 5%

[9]An alternative robustness check using the estimator proposed in de Chaisemartin et al. (2019) is presented in the appendix. The point estimates from this exercise are similar to those presented in 1

Table 3: CDC Data: Effect of TRAP Laws on Log of Abortion Rate

	(1)	(2)	(3)
Panel A: Grouped Event Study Estimates			
Pre-TRAP			
Year -8 or less	0.041	0.004	0.097
	(0.046)	(0.053)	(0.075)
Years -5 to -7	0.020	-0.002	0.024
	(0.029)	(0.035)	(0.035)
Years -4 to -2	0.027	0.023	0.029
	(0.022)	(0.025)	(0.022)
Year 0 × TRAP	-0.051**	-0.047*	-0.052*
	(0.019)	(0.025)	(0.028)
Post-TRAP			
Years 1 to 3	-0.107***	-0.082**	-0.116***
	(0.031)	(0.039)	(0.041)
Years 4 to 6	-0.142***	-0.127**	-0.173***
	(0.049)	(0.056)	(0.060)
Years 7 to 9	-0.166***	-0.144**	-0.225**
	(0.052)	(0.063)	(0.082)
Year 10+	-0.163**	-0.125*	-0.246***
	(0.066)	(0.065)	(0.086)
T-Test	0.584	0.453	0.341
Panel B: Difference-in-Difference Estimator			
% Neigh TRAP	0.001*	0.002**	0.001
	(0.001)	(0.001)	(0.001)
Year 0 × TRAP	-0.071***	-0.049**	-0.055**
	(0.022)	(0.020)	(0.026)
Post × TRAP	-0.147***	-0.113***	-0.142**
	(0.032)	(0.040)	(0.052)
Region by Year FE	N	Y	Y
TRAP States Only	N	N	Y
Panel C: Alternative approaches to TWFE			
ATT	-0.080*	-0.076*	-0.104***
	(0.046)	(0.046)	(0.040)
CS Never Treated (2020)	Y	N	N
CS Not Yet Treated (2020)	N	Y	N
BJS (2021)	N	N	Y

There are 997 observations in columns (1)-(2). All regressions control for state and year fixed effects. Demographic characteristics for the female- childbearing population include: the average age and the fraction that is white, black, Hispanic, married, on Medicaid, and at or below the FPL. Policy variables include: the fraction of neighboring states that have adopted a TRAP law (% Neigh TRAP) and whether the state has: a mandatory waiting period for abortion, Medicaid funding restrictions for abortion, expanded access to Medicaid family planning services based on income, and expanded Medicaid family planning service to postpartum women. "CS Never Treated (2020)" presents the simple ATT from using the doubly robust DiD methods, and the control group is never treated units. "CS Not Yet Treated (2020)" presents the simple ATT from the doubly robust DiD methods and uses never and not-yet-treated groups as a control. BJS (2021) uses the imputation method for finding the simple ATT as proposed in Borusyak et al. (2021). All regressions are weighted by the female population in each state-year. Standard errors clustered at the state level are reported in parentheses. * p<0.10; ** p<0.05; *** p<0.01.

decrease in the abortion rate in the year the TRAP law is adopted. This effect increases (in absolute value terms) over time and remains large 10-plus years after implementing the TRAP law. The effect diminishes slightly with the inclusion of region by year fixed effects in Column (2). Excluding non-TRAP states, as presented in Column (3), yield similar estimates to the first two columns.

Panel B of Table 3 presents the DiD results for each specification and the impact on abortion rates when a neighboring state adopts a TRAP law, $\%Neigh\ TRAP$. In general, abortion rates in a state appear to increase in response to neighboring states adopting a TRAP law, although this variable is roughly estimated. The coefficient on $\%Neigh\ TRAP$ from Column (1) implies that when 100% of neighboring states adopt a TRAP law, abortion rates rise by 1.1% or .154 more abortions per 1,000 women of childbearing age.[10] The coefficient on $Year\ 0 \times TRAP$ in Panel B of Column (1) implies that the year a state adopts a TRAP law, abortion rates fall by roughly 7%. Finally, the coefficient on $Post \times TRAP$ in Column (1) implies that in the subsequent years, there is an approximate 14.7% decrease in the abortion rate.

Results from Panel B, Column (2) of Table 3 show that the estimate on $Post \times TRAP$ diminishes slightly with the inclusion of region by year fixed effects, to an 11.3% reduction in the abortion rate.[11] When Model 3 is specified, the point estimate is between that of Model 1 and Model 2, 14.2%.

Panel C presents the findings from the alternative estimators. Column (1) of Panel C shows the estimates from the simple ATT Callaway and Sant'Anna (2021) using the never treated units as controls.[12] The findings imply that becoming a TRAP state leads to an approximate 8% reduction in the abortion rate. In Column (2), the control group is both never and not-yet-treated units, and the finding is similar to that in Column (1). Finally, Column (3) shows the estimate using the proposed estimator in Borusyak et al. (2021) and finds that adopting a TRAP law leads to an approximate 10.4% reduction in the abortion rate. These findings are a bit smaller than is suggested by the TWFE estimators.

Overall, the effect for $Post \times TRAP$ in Column (1) of Table 3 implies that going from a non-TRAP to TRAP state leads to a 14.7% decrease in the abortion rate. This effect translates to roughly 1.9 fewer abortions per 1,000 women of childbearing age. Using the more conservative DiD estimates from columns (2) implies around 1.5 fewer abortions per 1,000 women of childbearing age.[13]

[10] The average abortion rate before a neighboring state adopts a TRAP law was 14 abortions per 1,000 women of childbearing age.

[11] The results from an alternative specification that excludes states that ever fail to report abortion data to the CDC from all estimations are largely comparable both qualitatively and quantitatively to those presented in Table 3.

[12] It is important to note here that because this method estimates the group-time ATT and then aggregates, I am unable to include the covariates for all the typical characteristics and state policies. I cannot do this because I have many groups composed of just one treatment state. I can include these covariates with the BJS (2020) estimation method.

[13] The average abortion rate for TRAP states before implementing the TRAP law during this time was 13.1 abortions per 1,000 women of childbearing age

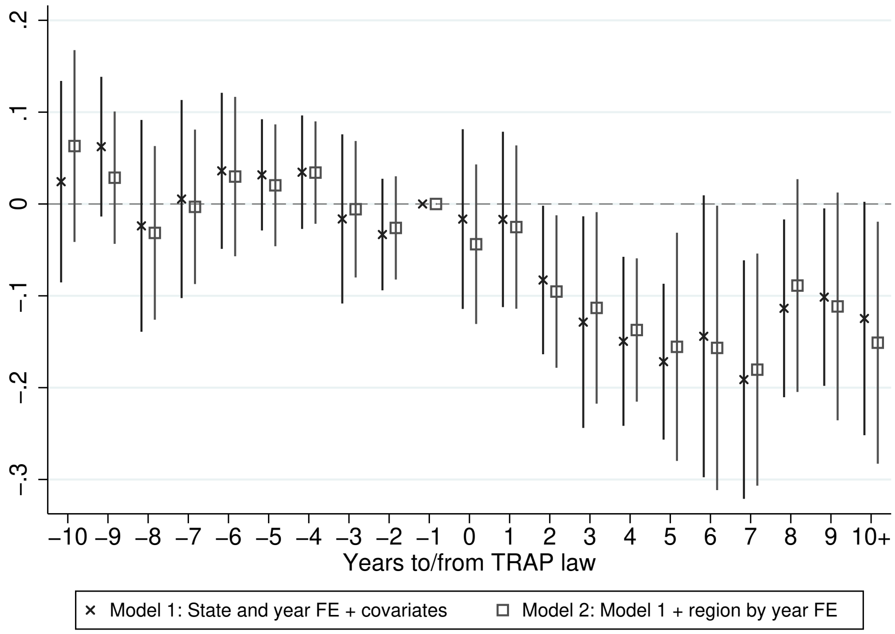

Figure 2: AGI Data: The Effect of TRAP Laws on Log of Abortion Rate

Notes: Model 1 controls for state and year fixed effects (FE) as well as state-level reproductive health policies (Kearney and Levine, 2015; Myers and Ladd, 2020) and demographic characteristics; see Table 3 for details. Model 2 adds region by year fixed effects to Model 1. The spikes represent the 95% confidence interval for the respective model. All regressions weight by the female population of childbearing age and cluster standard errors at the state level.

5.1.2 AGI Data

Figure 2 and Table 4 presents results using the abortion rate estimates from the Alan Guttmacher Institute. The models and structure of the tables and figures are the same as those previously presented in Section 5.1.1.

Figure 2 presents the results using the AGI dataset. Overall, the estimates are similar to those presented in Figure 1; however, the first two years post-TRAP law appear to be less impactful than the findings presented in 5.1.1.

Table 4 presents the grouped event study and difference-in-differences estimator. Once again, the results are similar to those presented in Table 3. The coefficient on $Post \times TRAP$ in Panel B Column (1) implies a roughly 11.5% reduction in the abortion rate. The inclusion of region by year fixed effects yields estimates similar to those presented in Column (1). When I restrict the analysis to only states that have adopted a TRAP law by 2014, the effect is relatively stable, though slightly larger, at around -12.1%.

As was done in Table 3, Panel C presents the findings from the alternative estimators. The results across the three estimation methods are quantitatively similar to one another. They suggest that adopting a TRAP law is associated with an approximate 9.5% decrease in the abortion rate. These estimates are slightly smaller than the TWFE estimation methods but still within their confidence intervals.

Table 4: AGI Data: Effect of TRAP Laws on Log of Abortion Rate

	(1)	(2)	(3)
Panel A: Grouped Event Study Estimates			
Pre-TRAP			
Year -8 or less	0.022	0.044	-0.025
	(0.049)	(0.042)	(0.046)
Years -5 to -7	0.026	0.015	-0.013
	(0.031)	(0.031)	(0.029)
Years -4 to -2	-0.010	-0.007	-0.021
	(0.029)	(0.027)	(0.023)
Year 0 × TRAP	-0.018	-0.047	-0.031
	(0.045)	(0.040)	(0.032)
Post-TRAP			
Years 1 to 3	-0.063	-0.071*	-0.075
	(0.042)	(0.040)	(0.044)
Years 4 to 6	-0.156***	-0.148***	-0.154***
	(0.038)	(0.046)	(0.041)
Years 7 to 9	-0.125***	-0.116**	-0.126**
	(0.045)	(0.054)	(0.052)
Year 10+	-0.121*	-0.142**	-0.204***
	(0.061)	(0.064)	(0.054)
T-Test	0.652	0.519	0.806
Panel B: Difference-in-Difference Estimator			
Neighbor	0.001	0.001	0.002
	(0.001)	(0.001)	(0.001)
Year 0 × TRAP	-0.029	-0.062	0.006
	(0.051)	(0.050)	(0.044)
Post × TRAP	-0.115***	-0.121***	-0.088*
	(0.039)	(0.045)	(0.044)
Region by Year FE	N	Y	Y
TRAP States Only	N	N	Y
Panel C: Alternative approaches to TWFE			
ATT	-0.099***	-0.094***	-0.095***
	(0.034)	(0.031)	(0.025)
CS Never Treated (2020)	Y	N	N
CS Not Yet Treated (2020)	N	Y	N
BJS (2021)	N	N	Y

There are 1071 observations in columns (1)-(2) and 525 observations in column (3). All regressions include state and year fixed effects as well as demographic and policy controls. See Table 3 for details on demographic and policy variables. "CS Never Treated (2020)" presents the simple ATT from using the doubly robust DiD methods, and the control group is never treated units. "CS Not Yet Treated (2020)" presents the simple ATT from the doubly robust DiD methods and uses never and not-yet-treated groups as a control. BJS (2021) uses the imputation method for finding the simple ATT as proposed in Borusyak et al. (2021). All regressions are weighted by the female population in each state-year. Standard errors clustered at the state level are reported in parentheses. * p¡0.10; ** p¡0.05; *** p¡0.01.

The average abortion rate in a TRAP state before passing a TRAP law using the AGI dataset during this period (1980-2014) was approximately 17.3 abortions per 1,000 women of childbearing age. The results from Panel B Columns (1) and (2) indicate that moving from non-TRAP to TRAP status leads to about 1.9-2.1 fewer abortions per 1,000 women of childbearing age. The CDC data results suggest approximately 1.5-1.9 fewer abortions per 1,000 women of childbearing age, which is slightly lower the estimates presented here.

The impact of neighboring states adopting a TRAP law is near zero in magnitude and statistically insignificant. While the coefficients are all positive, they are too small to draw any meaningful interpretations.

A natural next question is: if abortion rates are falling, and this is not *entirely* explained by traveling to neighboring states, is there an increase the birth rate?

5.2 Effect of TRAP Laws on Births

One benefit to using the birth rate as the primary outcome variable of interest, is the results may suffer less from the over-estimation problem that occurs when using in-state abortion rate. That is, in order for these point estimates to overestimate the effect of TRAP laws on births, women would have to travel to TRAP states from less restrictive states to give birth. To understand the impact of TRAP laws on the birth rate, I conduct the analysis described in Equations (1)-(3) and use the same models described in Section 5.1.1 using birth data from the CDC for years 1995-2015.

Figure 3 presents the results for the primary analysis, where the dependent variable is the natural log of the birth rate. The pre-treatment differences in birth rates are small in magnitude and statistically insignificant, except for the year before implementing the TRAP law. Models 1 and 2 show a generally positive impact of TRAP laws on birth rates. The positive effect on birth rates rises for the first two years post implementation, and levels off around year three.

I now turn to Table 5. Across models, the estimates for all the *pre-TRAP* variables presented in Panel A are statistically insignificant, and we also fail to reject that they are jointly significantly different from zero. For Models 1-2, The effect of TRAP laws the year the law is passed is positive and remains positive and significant in the years following adoption. However, when non-TRAP states are excluded, as presented in Column (3), the effect diminishes, though it remains positive.

Turning to Panel B of Table 5 shows a positive effect on birth rates when a neighboring state adopts a TRAP law, though the magnitude is near zero, thus it is difficult to draw any meaningful conclusions. The coefficient on $Post \times TRAP$ in Columns (1) of Panel B shows an approximate 3.2% increase in the birth rate. Before adopting a TRAP law, the average birth rate in a TRAP state was roughly 65.3 births per 1,000

Table 5: Effect of TRAP Laws on Log of Birth Rate

	(1)	(2)	(3)
Panel A: Grouped Event Study Estimates			
Pre-TRAP			
Year -8 or less	-0.023	-0.006	0.019
	(0.019)	(0.017)	(0.021)
Years -5 to -7	-0.004	-0.008	0.007
	(0.009)	(0.009)	(0.011)
Years -4 to -2	-0.005	-0.008	-0.004
	(0.006)	(0.005)	(0.006)
Year 0 × TRAP	0.007	0.010**	0.010*
	(0.006)	(0.005)	(0.005)
Post-TRAP			
Years 1 to 3	0.022***	0.011	0.013
	(0.007)	(0.007)	(0.009)
Years 4 to 6	0.026***	0.014	0.007
	(0.009)	(0.009)	(0.013)
Years 7 to 9	0.035**	0.018*	0.013
	(0.014)	(0.010)	(0.016)
Year 10+	0.035	0.023	0.012
	(0.029)	(0.023)	(0.031)
T-Test	0.166	0.494	0.453
Panel B: Difference-in-Difference Estimator			
% Neigh TRAP	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
Year 0 × TRAP	0.015	0.014*	0.006
	(0.011)	(0.008)	(0.006)
Post × TRAP	0.032***	0.019**	0.013
	(0.011)	(0.007)	(0.009)
Region by Year FE	N	Y	Y
TRAP States Only	N	N	Y
Panel C: Alternative approaches to TWFE			
ATT	0.025	0.019	0.035***
	(0.016)	(0.014)	(0.013)
CS Never Treated (2020)	Y	N	N
CS Not Yet Treated (2020)	N	Y	N
BJS (2021)	N	N	Y

There are 1071 observations in columns (1)-(2) of Panel A & B. All regressions include state and year fixed effects as well as demographic and policy controls. See Table 3 for details on demographic and policy variables. All regressions are weighted by the female population in each state-year. "CS Never Treated (2020)" presents the simple ATT from using the doubly robust DiD methods, and the control group is never treated units. "CS Not Yet Treated (2020)" presents the simple ATT from the doubly robust DiD methods and uses never and not-yet-treated groups as a control. BJS (2021) uses the imputation method for finding the simple ATT as proposed in Borusyak et al. (2021). Standard errors clustered at the state level are reported in parentheses. * p¡0.10; ** p¡0.05; *** p¡0.01.

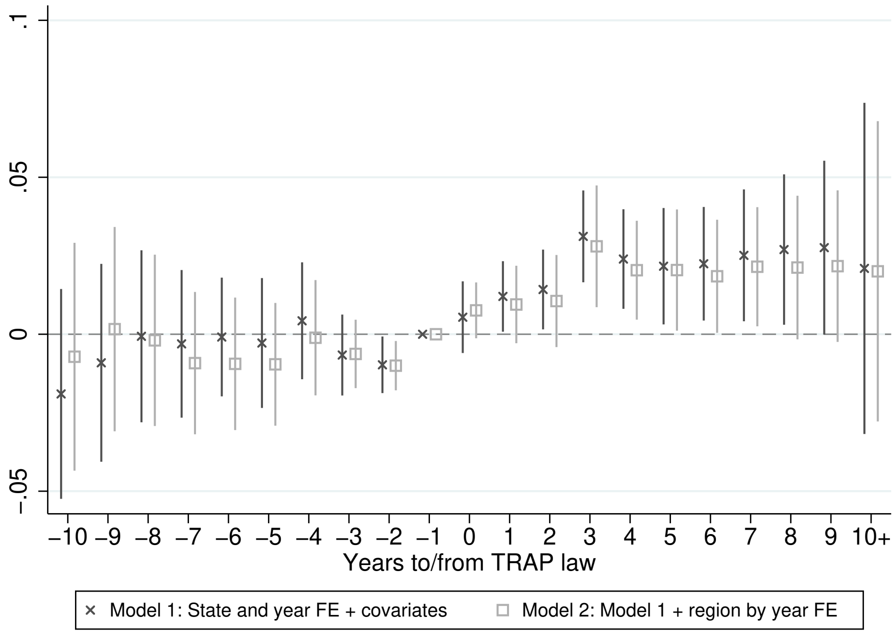

Figure 3: The Effect of TRAP Laws on Birth Rates

Notes: Model 1 controls for state and year fixed effects (FE) as well as state-level reproductive health policies (Kearney and Levine, 2015; Myers and Ladd, 2020) and demographic characteristics; see Table 3 for details. Model 2 adds region by year fixed effects to Model 1. The spikes represent the 95% confidence interval for the respective model. All regressions weight by the female population of childbearing age and cluster standard errors at the state level.

women of childbearing age; a 3.2% increase would imply 2 more births per 1,000 women of childbearing age. Once region by year fixed effects are included, the effect on $Post \times TRAP$ is slightly smaller at 1.9%, or roughly 1.2 more births per 1,000 women of childbearing age.

As was done in Table 3, Panel C presents the findings from the alternative estimators. The first two columns imply that adopting a TRAP law is associated with an approximate 2-2.5% increase in the birth rate, though the effect is not statistically significant at conventional levels. The point estimates are still quite similar to those presented in the TWFE model. The estimate in Column (3) is larger than any other estimate and statistically significant, implying that TRAP laws are associated with a 3.5% increase in the birth rate.

I can estimate what proportion of the observed decline in in-state abortions results in a child being carried to term using a quick back-of-the-envelope calculation. The coefficient on $Post \times TRAP$ in Panel B of Column (1) of Table 3 implies that the abortion rate falls roughly 14.7% after the TRAP law is passed, or approximately 1.9 fewer abortions per 1,000 women. The coefficient on $Post \times TRAP$ from Column (1) of Table 5 shows that the birth rate increases approximately 3.2%, or 2 more births per 1,000 women.[14]

[14] There is some missing data for the abortion rate since not all states report abortion data to the CDC every year. When the birth rate analysis is restricted to the observations for which there is a corresponding abortion rate observation, then the effect on births is slightly smaller, at roughly 2.3%, and the estimated fraction of abortions that result in a birth is roughly 79%.

Taken together, this would imply that approximately all of the observed decline in abortions results in a birth. Using the same process, but for the estimates from Column (2), when region by year fixed effects are included, implies that roughly 80% of the observed decline in the abortion rate results in a birth.

5.3 Threats to validity

One potential concern is that there is a correlation between adopting a TRAP law and some other policy or state characteristics. To address this concern, I conduct several falsification regressions. First, I use the following policies as potential outcome variables in the falsification analysis: mandatory waiting period for abortion ("Waiting Period"), restrictions on Medicaid funding for abortion ("Abortion Restriction"), and parental notification laws ("Parent Notification"). Next, I use the following state-level demographic and economic characteristics as potential outcome variables: the average age of the female, childbearing age population, the percentage that is black and white, and the unemployment rate.[15] Table 6 columns (1) -(3) report the results of the impact of TRAP laws on state policies and columns (4) through (7) report the results of the effect of TRAP laws on state-level demographic and economic characteristics. There is no evidence that the pre and post-treatment characteristics differ. Panel B of Table 6 restricts the analysis to states that always report abortion data to the CDC during the period covered.

Another potential concern is that one state is driving the results. To address this concern, I perform the difference-in-differences analysis, excluding one treatment state each time. I graph the point estimate from $Post \times TRAP$ and the 95% confidence interval for each of these 26 regressions in Figure 4. The dashed, horizontal, gray line represents the estimate from $Post \times TRAP$ in Panel B of Table 3. The gray, capped lines represent the 95% confidence interval from that estimate. I conduct this exercise for Models 1 and 2, presented in Column (1) and (2) of Table 3. The point estimates from each of these analyses fall within the 95% confidence interval of the main analysis. These results suggest that one state is not driving the effects found previously.

A concern with any difference-in-differences analysis is that the previous sections' results are due to the study's design. To address this concern, I conduct a placebo analysis. First, I drop observations for any treatment state after receiving treatment. Then, I randomly assign 26 states to receive treatment. Next, I randomly assign a treatment year that falls between 1995 and the last year that the state is in the dataset. I use this random "treatment" year to conduct a difference-in-differences analysis for each of the four models presented in Section 5.1. Finally, I iterate over this process 1000 times to collect a distribution of placebo effects for the two models. The method of randomly generating laws multiple times is similar to

[15]See Section 3 for more information about these variables. Also, please refer to Table 2 for summary statistics of these variables

Table 6: Effect of TRAP Laws on Policies and Demographic Variables

	Waiting Period (1)	Abortion Restriction (2)	Parent Notification (3)	Average Age (4)	Percent White (5)	Percent Black (6)	Unemployment Rate (7)
Panel A: All states							
Post × TRAP	-0.027	-0.006	0.225	0.048	0.095	-0.119	0.286
	(0.125)	(0.005)	(0.145)	(0.063)	(0.542)	(0.507)	(0.263)
Panel B: Exclude states with missing abortion data							
Post × TRAP	-0.080	-0.005	0.187	0.040	0.168	0.064	0.295
	(0.128)	(0.006)	(0.152)	(0.059)	(0.520)	(0.512)	(0.276)

There are 1071 observations in Panel A and 861 observations in Panel B. All regressions control for state and year fixed effects as well as region by year fixed effects. All regressions are weighted by the female population in each state-year. Standard errors clustered at the state level are reported in parentheses. * p¡0.10; ** p¡0.05; *** p¡0.01.

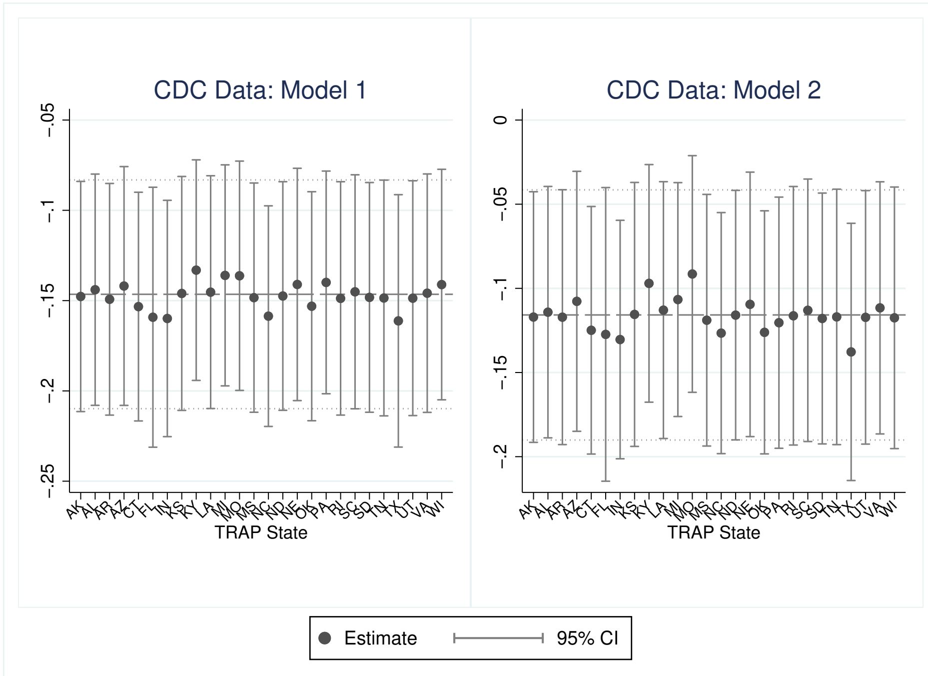

Figure 4: Robustness Check: The Effect of TRAP Laws on Abortion Rates using CDC data, Exclude One State

Notes: Model 1 controls for state and year fixed effects as well as state-level reproductive health policies (Kearney and Levine, 2015; Myers and Ladd, 2020) and demographic characteristics; see Table 3 for details. Model 2 adds region by year fixed effects to Model 1. All regressions weight by the female population of childbearing age and cluster standard errors at the state level.

that presented in Bertrand et al. (2004).

Figure 5 presents the results from this process using the CDC data. The vertical line denotes the $Post \times Trap$ estimate as presented in Panel B of Table 3. The point estimate from the main analysis falls in the far left tail of the placebo distribution in all specifications, suggesting that the main results do not simply reflect spurious estimates. In all cases, the main $Post \times Trap$ estimator from Panel B of Table 4, denoted by the vertical line in each of the figures, is in the far left tail of the placebo distribution.

5.4 Potential channels

Up to this point, the results presented have been reduced form estimates of the effect of the TRAP law on abortion and birth rates. Overall, the findings imply that TRAP laws are associated with an 11-14% decline in the abortion rate, and an increase in birth rates that may account for 80-100% of the observed decline in abortion rates. While these estimates represent the average effect, it may also be of interest to policymakers and economists to understand the mechanisms through which TRAP laws impact the abortion rate. For example, TRAP laws may cause some abortion providers to shut down if they cannot comply with the TRAP laws. Two ways shutdowns could cause a decline in the abortion rate by increasing the distance to the nearest abortion provider and increased congestion at the clinics that remain open (Lindo et al., 2019;

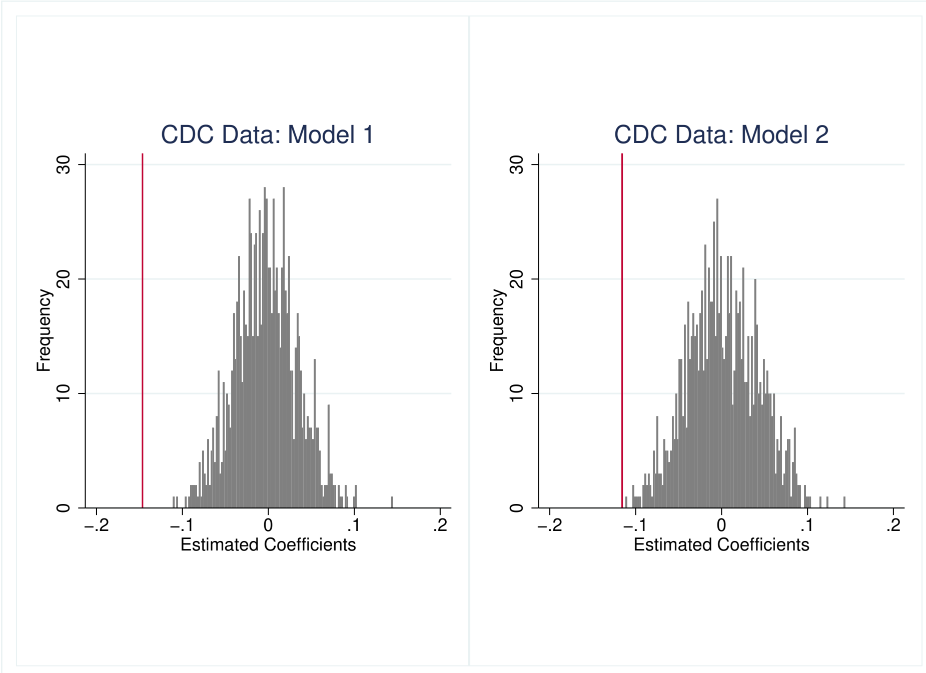

Figure 5: Placebo Analysis: The Effect of TRAP Laws on Log of Abortion Rate CDC data
Notes: Model 1 controls for state and year fixed effects as well as state-level reproductive health policies (Kearney and Levine, 2015; Myers and Ladd, 2020) and demographic characteristics; see Table 3 for details. Model 2 adds region by year fixed effects to Model 1. The spikes represent the 95% confidence interval for the respective model. All regressions weight by the female population of childbearing age and cluster standard errors at the state level.

Kelly, 2020). Alternatively, clinics may comply with the law and stay open, but the cost of complying with the law may be passed on to customers, increasing the procedure's price. Below, I attempt to estimate the extent to which the distance and congestion mechanisms may explain the observed decline in abortion rates documented in Section 5.1.2.

The potential mechanisms that I will examine are the median distance to the nearest abortion clinic and the average service population as a measure of congestion. In particular, I follow Lindo et al. (2019) and define the average service population as the number of women of childbearing age (in 100,000) divided by the number of abortion providers. I collected data on the number of abortion providers in a state-year from the Alan Guttmacher Institute. The limitation of this data is that it is not reported annually, but rather for select years.[16] For the distance to the nearest abortion provider mechanism, the Alan Guttmacher Institute collected data on the median distance to an abortion provider for each state for the years 2000, 2010, and 2014. It is important to note that due to limited information for either abortion provider distance or average service population, the results presented here are intended to be exploratory and descriptive; they should be interpreted with some caution.

Table 7 presents several ways to explore these potential mechanisms. The DiD estimator from Table 4,

[16]Those years are: 1980, 1981, 1982, 1984, 1985, 1987, 1988, 1991, 1992, 1995, 1996, 1999, 2000, 2005, 2008, 2011, and 2014.

$Post \times TRAP$, is shown again in Column (1) of Table 7 for easy reference.[17] This finding implies that TRAP laws are associated with an 11.5% reduction in the abortion rate. Column (2) of Table 7 shows the impact of adopting a TRAP law on the average service population. The point estimate suggests that adopting a TRAP law is associated with an approximate .306 increase in the average service population (or roughly 30,600 more women of childbearing age per provider). Column (3) shows the result from a simple regression of the average service population's impact on the abortion rate *before* states implement a TRAP law. This estimate implies that an increase of 100,000 women in the average service population is associated with a 24% reduction in the abortion rate. A back of the envelope calculation reveals that increases in the average service population could account for up to 63%[18] of the observed decline in the abortion rate. Column (4) presents an alternative way to measure how much of the observed decrease in abortion rates is explained by changes to the average service population. Column (4) shows the same analysis as Column (1), but now the average service population is a mediator variable in the regression. This additional variable allows us to visually inspect how the coefficient on Post × TRAP changes when the average service population is a control variable. It appears that the coefficient on Post × TRAP falls by approximately 44% between Column (1) and Column (4). For reference, Lindo et al. (2019) identify that congestion could explain approximately 59% of their observed decline in abortion rates, an estimate between the two proposed here.

As addressed above, TRAP laws may impact abortion rates by increasing the distance traveled to obtain an abortion. The results in Column (5) of Table 7 implies that TRAP laws are associated with an approximate 14% increase in the median distance to the nearest abortion clinic. The result presented in Column (6) shows the correlation between median distance and the abortion rate *before* the state adopted a TRAP law. The result suggests that a 1% increase in the median travel distance reduces the abortion rate by 0.172%. A simple, back of the envelope calculation using the results from Column (5) and (6) imply that increasing travel distance could account for roughly 21%[19] of the observed decline in abortion rate. Alternatively, Column (7) includes the log of the median distance to an abortion provider as a mediator variable. Here, we see that that estimated impact on $Post \times TRAP$ falls by roughly 23.4%. This estimate is substantially smaller than the findings presented by (Lindo et al., 2019), who find 41% of their observed decline is due to increased driving distance. The estimates presented here are considerably more imprecise and rough than those presented in either (Lindo et al., 2019; Fischer et al., 2018).

The back of the envelope calculations presented in this analysis implies that the impact of the average service population and median distance can account for 65%-86% of the observed decline in the abortion

[17] Since the distance and provider data is from the AGI, and the abortion provider data goes back to 1980, I use the main difference-in-differences effect using the AGI dataset.

[18] 24% × .306 = 7.3%. 7.3% is 63% of the estimated difference-in-differences effect (11.5%)

[19] .172% × 14.1% = 2.4; this is approximately 21% of the observed 11.5% decline

Table 7: AGI Data: Effect of TRAP Laws on Log Abortion Rate

Dependent Variable:	Log Abortion Rate (1)	Avg Service Population (2)	Log Abortion Rate (3)	Log Abortion Rate (4)	Log Median Distance (5)	Log Abortion Rate (6)	Log Abortion Rate (7)
Post × TRAP	-0.115***	0.306**	–	-0.064**	0.141*	–	-0.088
	(0.039)	(0.126)		(0.027)	(0.081)		(0.057)
Avg Service Population	–	–	-0.240*	-0.182***	–	–	–
			(0.128)	(0.020)			
Log of Median Distance	–	–	–	–	–	-0.172*	-0.220**
						(0.094)	(0.087)
N	1,071	867	753	867	150	99	150

Average service population is the number of women (in 100,000) divided by the number of abortion providers. Abortion providers and median distance to nearest abortion provider is from the AGI. All regressions control for state and year fixed effects as well as other state policy variables. All regressions are weighted by the female population in each state-year. Standard errors clustered at the state level are reported in parentheses. * p¡0.10; ** p¡0.05; *** p¡0.01.

rate. The remainder of the observed decrease could be potentially attributable to the increased driving distance that I cannot precisely measure with this data. However, other potential mechanisms could be at play here; for example, a portion of the observed effect could be due to an increase in the procedure's price.

6 Conclusion

The results of this study imply that adopting a TRAP law led to an approximate 11-14% reduction in the abortion rate or roughly 1.5-1.9 fewer abortions per 1,000 women of childbearing age. Furthermore, I find that birth rates potentially increased by roughly 1.9-3.2% or around 1.2-2 more births per 1,000 women of childbearing age. Together, these results imply that roughly 80-100% of the reduction in in-state abortions resulted in a child being carried to term. Overall, the finding that TRAP laws led to a decrease in abortion and an increase in births is consistent with the first scenario proposed in Section 2.2.

I use back-of-the-envelope calculations to explore two potential channels through which TRAP laws may reduce abortion rates. The first channel suggests that increased congestion at clinics may contribute to a large proportion of the observed decrease in abortion rates. The second mechanism, an increase in driving distance, appears to make up a smaller proportion of the observed decline in abortion rates.

Overall, the findings imply that TRAP laws decrease abortion rates, increase birth rates, and potentially increase interstate travel. However, it is important to note that while one benefit to the analysis presented here is that it explores the impact of TRAP laws across the US, more data with additional information on abortion providers and their location would be ideal for exploring this topic further.

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Ethics Declaration

The author declares that she has no conflicts of interest.