TRAP'd Teens:
Impacts of abortion provider regulations on fertility & education*

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Abstract

Following the 2022 decision of the U.S. Supreme Court, *Dobbs v. Jackson Women's Health,* several U.S. states have severely restricted or eliminated access to abortion. To shed light on the potential economic impacts of this landmark ruling, we estimate the impact of abortion access on women's educational attainment. We first codify the legal history of all targeted regulations of abortion providers (TRAP laws) ever implemented. We document that TRAP laws, which often result in clinic closures, increased teen births by more than 3 percent and offer evidence that these impacts are driven by reductions in abortion access and abortion use. We further document that exposure to TRAP laws before age 18 reduces college initiation by 2.1 percent and college completion by 5.8 percent among Black women. For White women, despite comparable impacts on teen births, educational impacts are on college completion only, are less than half as large, and are not robust. Our findings suggest that modern abortion restrictions affect women's economic participation and contribute to racial inequality.

JEL codes: J13 Fertility & Family Planning; I24 Education and Inequality; I14 Health and Inequality; J15 Economics of Minorities; J16 Economics of Gender

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1 Introduction

On June 24, 2022, the United States Supreme Court issued its ruling in the case *Dobbs v. Jackson Women's Health Organization* (henceforth, *Dobbs*). This decision overturned the previous rulings in *Roe v. Wade* and *Planned Parenthood v. Casey,* stating that the Constitution does not confer a right to abortion, giving states power to regulate any aspect of abortion not protected by federal law.[1] Within a year following the ruling, abortion had been banned in 13 U.S. states with 12 other states considering implementing gestational limits or full bans.

In states where opposition to abortion has been strong, legislators have long implemented various measures to restrict abortion access. The impacts of these restrictions can shed light on the potential consequences of the *Dobbs* decision for women. Existing evidence suggests that abortion restrictions, such as parental involvement laws, mandatory waiting periods, and reductions in Medicaid funding for abortion, reduce abortion use, delay abortion timing, and increase births, especially among young women.[2] We hypothesize that restricting access to abortion, especially among adolescents, may additionally impact women's educational attainment.

Access to family planning services such as contraception and abortion can impact women's economic outcomes through several mechanisms. The direct effects of these policies operate via impacts on fertility. When a young woman experiences an unintended birth, she may pause or abandon her educational or other career investments.[3] However, these services can also indirectly affect such investments even in the absence of an unwanted pregnancy. Expectations about one's future ability to control whether and when to have a child can affect aspirations, planning, and investment for the future. As such, abortion access may impact future welfare by changing the course of a young woman's life.

Prior to *Dobbs*, targeted regulations of abortion providers, or TRAP laws, were the fastest growing abortion restrictions in the U.S. These laws include a variety of requirements, such as admitting priviledges and detailed building specifications, with which abortion providers are typically unable to comply and often result in clinic closures. Between 2010 and 2017, the number of states that implemented these restrictions increased by 59 percent, resulting in the closure of many abortion clinics and likely preventing the new openings of others. TRAP laws remain relevant post-*Dobbs*, as 15 states that have not banned abortion are still enforcing TRAP laws.[4] Existing evidence has documented the detrimental impacts of clinic closures in Texas, Wisconsin, and Pennsylvania in terms of abortion access, abortion rates, and abortion timing (Quast, Gonzalez, and Ziemba, 2017;

[1] Section 2 provides more details on these Supreme Court decisions.

[2] See Clarke (2023) for a review of this evidence, also summarized in Section 2.1.

[3] One might argue that continuing with an unwanted pregnancy due to difficult or costly abortion access would be irrational, as even a heavily inflated cost of abortion would still be less than the discounted lifetime cost of raising a child. However, we note that teens in particular have been documented to demonstrate irrational behaviors due to myopia (Gruber and Köszegi, 2001) and additionally would face credit constraints, either of which may make paying a large (monetary or effort) cost in the present too undesirable or impossible.

[4] While some TRAP-enforcing states have passed bans or strict gestational limits that are enjoined (AZ, IN, NE, NC, OH, UT, WY), other TRAP-enforcing states have not (PA, WI) and many are considered by the Center for Reproductive Rights to have "expanded" or "protected" access (CT, FL, KS, MD, MI, RI, SC).

Fischer, Royer, and White, 2018; Lindo et al., 2020; Kelly, 2020; Venator and Fletcher, 2020). We test whether such impacts extend beyond these states and whether these have downstream impacts on fertility and education.

In this study, we estimate the impacts of twenty-seven targeted regulations of abortion providers implemented across twenty-one states since 1993. To do so, we first code the complete legal history of all TRAP laws in each U.S. state. We then estimate the impact of teen exposure to TRAP laws on teen births, using natality data from Vital Statistics, and the impact on women's educational attainment using data from the American Community Survey.

We exploit the fact that TRAP laws vary across states and over time, however, we acknowledge recent evidence suggesting that such "staggered adoption" estimations may be biased by heterogeneous treatment effects over time (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020a; Athey and Imbens, 2021; Goodman-Bacon, 2021). To address this concern, we use a pooled event study methodology to examine the difference in teen birth rates in each year leading up to and following a TRAP law between the state that implemented the law and other states that do not implement a law in that period. This methodology allows us to test the assumption of common trends and to examine heterogeneous effects of policies over time. Yet, we also acknowledge that pooled event studies may still be biased in the presence of heterogeneous treatment effects across units (Sun and Abraham, 2020; Borusyak, Jaravel, and Spiess, 2021). We demonstrate robustness of our findings to a stacked difference-in-differences methodology, which is not subject to this concern. We also explore differential effects by type, severity, and number of TRAP laws.

We find that the enforcement of a TRAP law increases births among teens by 3.5 percent. The effect is comparable across White and Black (non-Hispanic) teens. Among Hispanic teens, the increase in births following the onset of a law is not different from the pre-existing trend in teen births. This is consistent with evidence that Hispanic women are quite different, on average, between states that do versus do not implement TRAP laws. We also analyze potential mechanisms driving changes in fertility. Our findings suggest that decreases in abortion access and abortion use are channels through which TRAP laws increase teen birth rates. We also document that the impact of TRAP laws on fertility is greater in the presence of additional abortion restrictions.

We next explore the impacts of teen exposure to TRAP laws on the educational attainment of women, as measured at ages 25 and older, using information from the American Community Survey. We use a pooled difference-in-differences methodology that allows us to measure TRAP laws' longer-term impacts. As before, we demonstrate robustness of our findings in a stacked difference-in-differences methodology, to allay concerns about bias arising from heterogeneous treatment effects. We provide support for the assumption of common trends by testing for differences in trends in women's education in the years preceding a policy change.

Among Black women, we find that first exposure to an enforced TRAP law before age 18 reduces the probability of completing high school by 0.4 percent, reduces the probability of having ever entered college by 2.1 percent, and reduces the probability of completing college by 5.8 percent.

Among White women, impacts are smaller and less robust, with exposure reducing their probability of completing college by 1.6 percent.

Taken together, our findings indicate that TRAP laws increase teen births similarly for White and Black women, yet the impact of increased teen births has more significant effects on the human capital accumulation of Black women. We document that the race gap in the impacts on education is partially driven by differences in underlying poverty but we cannot rule out that it may also be partially explained by factors other than poverty.

We contribute to the literature in several ways. This study builds on a small but growing set of studies that estimate the national impacts of abortion restrictions in the U.S. Others in this space have documented impacts of parental involvement laws (Myers and Ladd, 2020), mandatory waiting periods (Myers, 2021b), and distance to clinic (Myers, 2021c); we document the impact of TRAP laws. Second, we contribute to a limited body of evidence on the causal impacts of U.S. abortion restrictions on women's outcomes beyond abortion use and fertility. This evidence includes only three studies of which we are aware: Borelli (2011) documents impacts of parental involvement laws on education, the Turnaway Study documents impacts of gestational limits on economic duress (Foster et al., 2018b; Miller, Wherry, and Foster, 2023), and Muratori (2021) studies the impacts of abortion clinic closures in Texas on instances of violence against women, all in the relatively short term. Our findings are consistent with the documented beneficial impacts of *increasing* access to abortion on the economic outcomes of women (Angrist and Evans, 2000; Kalist, 2004; Mølland, 2016; Gonzalez et al., 2018; Brooks and Zohar, 2021) and their children (Ananat et al., 2009; Foster et al., 2018a,c).

Finally, our study also updates our knowledge on the effects of early fertility on measures of socioeconomic success, a topic of long-standing interest across the social sciences. The most convincing studies in this area are those that have estimated the causal relationship between teen fertility and educational attainment by exploiting the implementation of family planning policies in the U.S. during the 1960s and 1970s.[5] However, the U.S. economic, social, and political landscapes have changed dramatically in the past 50 years. Thus, our paper also updates the knowledge base by providing evidence on the modern relationship between teen fertility and educational attainment as identified by exogenous shifts in teen births induced by policies occurring in recent decades.

In *Dobbs*, a central argument of one amicus brief was that "there is no adequate credible evidence that women have enjoyed greater economic and social opportunities because of the availability of abortion" (Collett, Alvare, and Bachiochi, 2021). Our findings provide direct evidence to the contrary.

The remainder of the paper is structured as follows: Section 2 discusses background existing evidence on the modern abortion restrictions and the relationship between teen motherhood and educational attainment. Section 3 describes TRAP laws and the creation of the legal data set. Section 4 presents the impacts on teen births and Section 5 presents the impacts on women's

[5]See Angrist and Evans (2000); Goldin and Katz (2002); Hock (2008); Bailey, Hershbein, and Miller (2012); Ananat and Hungerman (2012); Edlund and Machado (2015) for this evidence.

education. Section 6 documents abortion access and abortion use as key pathways and explores whether changes in expectations are also a contributing pathway. Section 7 concludes.

2 Background and existing evidence

2.1 Modern abortion restrictions in the U.S.

In 1973, the U.S. Supreme Court legalized abortion nationwide in the landmark case *Roe v. Wade*, which established the right to an abortion during the first trimester as protected under a constitutional right to privacy. The Court allowed states to place restrictions in the second trimester to protect a woman's health and, in the third trimester, to protect a viable fetus. In 1992, the abortion regulation landscape changed again with the next major Supreme Court ruling regarding abortion access. In *Planned Parenthood v. Casey* (henceforth *Casey*), the Court upheld the legality of abortion throughout the U.S. but dramatically changed regulatory standards in several ways. Under *Casey*, while a state could not prohibit a woman from obtaining an abortion prior to viability, states did have the right to restrict abortion, as long as those restrictions did not pose an "undue burden" on the woman seeking an abortion. Courts were now directed to consider the particular restriction and the degree to which it would interfere with the woman's ability to access abortion. States seeking to regulate or restrict abortion had a new standard to meet and a template for a law that met this standard. Following *Casey*, state and local legislatures began to pass more and more laws to restrict abortion access, and the Supreme Court has more often upheld them. Given the pivotal nature of the *Casey* decision, and our aim to provide evidence based on modern policy changes, this work focuses exclusively on restrictions in the post-*Casey* era.

Figure 1 documents the increase over time in the three of the most common types of abortion restrictions. Following *Casey*, there was rapid growth in parental involvement laws, which require that minors have parental notification or parental consent to access abortion. More recent and less common are mandatory waiting periods, which require that a women wait a specified period (usually 24 or 48 hours) after receiving specified information before she can access abortion services. Since 2010, the fastest growing abortion restriction is a category known as targeted regulations of abortion providers, or TRAP laws. Though our data end in 2017, we note that, by 2021, TRAP laws were more common than parental involvement laws across the U.S. (Guttmacher Institute, 2021c). As of mid-2023, TRAP laws remain more common than post-*Dobbs* abortion bans.

TRAP laws require providers to comply with various regulations including having specific agreements with hospitals for transfer and treatment of patients, locating within a specified proximity to a hospital, or meeting advanced surgical center requirements for building structure or utility systems. When clinics cannot comply with such requirements, they may be forced to close either temporarily or permanently, thereby reducing abortion access. Such requirements may also prevent potential new clinics from opening.

The most widely studied TRAP law is Texas HB2, implemented in 2013. It required the most

stringent form of hospital admitting privileges and mandated a minimum proximity to a hospital.[6] Several studies have documented that HB2 resulted in an increase in distance to the nearest provider, which decreased abortion rates and/or increased birth rates (Quast, Gonzalez, and Ziemba, 2017; Lu and Slusky, 2016; Fischer, Royer, and White, 2018; Lindo et al., 2020). Other impacts of HB2 have also been documented, such as increases in clinic congestion and delays in abortion timing (Lindo et al., 2020), and increases in violence against women (Muratori, 2021).[7] These findings are consistent with evidence from Wisconsin that abortion clinic closures (unrelated to TRAP laws) decreased abortion rates and increased births (Venator and Fletcher, 2020) and are consistent with national evidence that an increase in distance to the nearest abortion provider reduces abortion use and increases births (Myers, 2021c).

Outside Texas, TRAP laws have only been examined in Pennsylvania. In 2012, Pennsylvania implemented building regulations for abortion providers, causing the closure of almost half of the abortion facilities and creating increased congestion in those remaining open. Kelly (2020) shows that the reduced clinic capacity significantly shifted abortion timing, decreasing abortions within the first eight weeks of gestation and increasing abortions in later stages of pregnancy. It also caused a reduction in the total abortion rate of 14 percent and increases in total birth rates of 3 percent.

A first contribution of our study is to further a small but growing effort to document the national-level impacts of abortion access on abortion use and fertility, adding evidence regarding TRAP laws to other recent studies on parental involvement laws, mandatory waiting periods, and distance to clinic (Myers and Ladd, 2020; Myers, 2021b,c). In doing so, we also provide the first detailed, quantitative coding of the history of TRAP laws in all U.S. states.

We note that beyond TRAP laws, other abortion restrictions have also been documented to affect health outcomes. Parental involvement laws, particularly those post-*Casey,* have been shown to reduce minors' abortion use, delay their abortion timing, and increase early fertility.[8] Mandatory waiting periods, particularly those that require two trips to the clinic, have been shown to reduce abortion use, delay abortion timing, and increase births, particularly for young women and women of color.[9] Other studies have also documented that cuts to public funding of family planning affect fertility and contraceptive use, especially for those living in poverty.[10,11]

[6]It also required that all abortion facilities meet new ASC standards (see Section 3.1). The ASC standard was scheduled to be enforced in 2014, but it never happened. The case *Whole Woman's Health v. Hellerstedt* confirmed this requirement would not be applied.

[7]We show that our findings are not driven by Texas HB2 based on robust results in a model that excludes this policy change, ensuring that we are estimating a truly national effect.

[8]On minor's abortion use, see Cartoof and Klerman (1986); Haas-Wilson (1993); Ohsfeldt and Gohmann (1994); Kane and Staiger (1996); Joyce and Kaestner (1996); Haas-Wilson (1996); Ellertson (1997); Altman-Palm and Tremblay (1998); Tomal (1999); Levine (2003); Joyce, Kaestner, and Ward (2020); Myers and Ladd (2020).

On abortion timing, see Rogers et al. (1991); Bitler and Zavodny (2001); Joyce and Kaestner (2001); Colman and Joyce (2009).

On early fertility, see Tomal (1999); Myers and Ladd (2020).

[9]See Joyce, Henshaw, and Skatrud (1997); Bitler and Zavodny (2001); Lindo and Pineda-Torres (2021); Myers (2021b); Altındağ and Joyce (2022).

[10]See Stevenson et al. (2016); Packham (2017); Lu and Slusky (2016).

[11]Restricting the use of Medicaid for abortion decreases abortion use among minors and low-income populations, though is has not been shown to impact overall birth rates (Lundberg and Plotnick, 1990; Haas-Wilson, 1993; Meier

All of these studies have evaluated the impacts of restrictions on health and demographic outcomes. Only one study of which we are aware has examined the impact of modern abortion restrictions (post-*Casey*) on economic outcomes. The Turnaway Study examined the economic well-being of women who sought an abortion between 2008 and 2010. Relative to those who received a wanted abortion, those who were denied wanted abortions due to gestational limits had higher financial distress in the near-term (6 months and 4 years later) such as poverty, unemployment, public assistance, delinquent debt, bankruptcy, and eviction (Foster et al., 2018b; Miller, Wherry, and Foster, 2023). A key contribution of our study is to estimate the impacts of recent, fast-growing abortion restrictions on an important leading indicator of long-term productivity and economic welfare: educational attainment.

2.2 Early fertility and education

Identifying the relationship between early fertility and educational attainment requires separating the causal effects of the birth from other socio-economic factors that may drive both early pregnancy and educational attainment. Researchers have relied on various strategies to isolate the causal impacts of unintended pregnancy on educational attainment.

Studies have documented that teen mothers have lower educational attainment, even after controlling for individual and family characteristics, either through propensity score matching, estimating within families, or estimating within schools (Olsen and Farkas, 1989; Upchurch and McCarthy, 1990; Geronimus and Korenman, 1992; Hoffman, Foster, and Furstenberg, 1993; Levine and Painter, 2003; Holmlund, 2005; Schulkind and Sandler, 2019). Yet such comparisons may not fully account for unobservable factors that affect teen pregnancy and educational attainment. Other studies have relied on potentially exogenous sources of variation in the timing of childbearing, such as age at menarche or the experience of miscarriage. These studies have found negative but more modest impacts of early fertility on educational attainment (Ribar, 1994; Klepinger, Lundberg, and Plotnick, 1999; Chevalier, Viitanen, and Viitanen, 2003; Hotz, McElroy, and Sanders, 2005; Ashcraft, Fernández-Val, and Lang, 2013). However, even these sources of variation may still be related to individual characteristics, including health, which may also affect educational attainment.[12]

Researchers have also examined the relationship between early fertility and education by estimating the impacts of policy changes that create differential access to contraception. Variation in the geography and timing of such policies allows for the comparison of women exposed to these policies across and within states, examining differences in outcomes for exposed women versus the

and McFarlane, 1994; Blank, George, and London, 1996; Haas-Wilson, 1996; Levine, Trainor, and Zimmerman, 1996; Haas-Wilson, 1997; Matthews, Ribar, and Wilhelm, 1997; Tomal, 1999; Cook et al., 1999; Morgan and Parnell, 2002). Gestational limits contribute to worse health and economic outcomes for women who were denied needed abortion care (see Miller, Wherry, and Foster (2020) for an annotated list of findings from the Turnaway Study). Compulsory ultrasound requirements have not been found to affect abortion use (Gius, 2019). Fertility controls in the nineteenth century based on anti-obscenity laws led to increases in births (Lahey, 2014a,b).

[12] In addition, these studies were conducted in a context where abortion was relatively accessible, acting to prevent births that would be the most unwanted or costly; as such, the teen births that are observed in these studies are those with a lower potential to negatively impact the woman's life, suggesting these studies estimate lower bound effects.

unexposed. For example, during the time when marriage was a pathway to contraception for minors, reductions in the minimum age at marriage were documented to increase young women's college attendance (Edlund and Machado, 2015). Also using this approach, several researchers have studied the impacts of early legal access (ELA) to oral contraception. "The pill" was introduced in 1960, but most unmarried women under age 21 did not have access. However, in the 1960s and 1970s, states implemented laws that either lowered the age of majority or granted more rights to minors, making the pill accessible for single women ages 18-20. Researchers have documented that ELA increased enrollment of women in both college and professional schools, and increased attainment of bachelor's degrees (Goldin and Katz, 2002; Hock, 2008; Bailey, Hershbein, and Miller, 2012; Ananat and Hungerman, 2012). These effects were documented to be the greatest among high-income women and women with higher measured ability (Ananat and Hungerman, 2012; Bailey, Hershbein, and Miller, 2012). However, after conducting an extensive review of reproductive policy in the 1960s and 1970s, Myers (2017) replicated several of these analyses and concluded abortion policies were more influential than ELA in explaining the decline in early births, early marriages, and "shotgun marriages." While Myers' work does not estimate impacts on education or labor market outcomes, her findings raise questions regarding whether the estimated impacts of ELA on these outcomes may also have been at least partly driven by abortion access.

Indeed, legal access to abortion in the U.S. has been documented to both reduce early fertility and increase educational attainment. Before the nationwide legalization of abortion in 1973, five states legalized abortion by repealing anti-abortion laws and ten other states reformed (relaxed) their abortion restrictions.[13] Angrist and Evans (2000) use this state-level variation in legality of abortion to estimate the effects of abortion access on teen childbearing and women's schooling. Their findings indicate that three years of adolescent abortion access reduced births by 6 percent for White teens and 11 percent for Black teens (Table 3 in Angrist and Evans, 2000). Any adolescent abortion access increased educational attainment for Black women by 1.3 percent for high school completion, 3.7 percent for college initiation, and 9.6 percent for college completion, with no educational benefits for White women (Table 5 in Angrist and Evans, 2000).[14]

Findings from studies outside the U.S. context are consistent with these. Abortion legalization in Oslo in the 1960s and in Spain in 1985 also delayed fertility and increased women's education (Mølland, 2016; Gonzalez et al., 2018). More recently, abortion subsidies in Israel were expanded to include women aged 20 to 32 (they were previously available for women under 20). Brooks and Zohar (2021) estimate the impact of this policy change among young (aged 20-21), unmarried women who conceived a pregnancy. They find that the policy increased abortion use among these women by 4.6 percent, as well as reducing parenthood and marriage. Among women in this group who are from a religious family and have low socio-economic status, they find the policy increased

[13]These "repeal" states included California, New York, Washington, Alaska, and Hawaii. The states that relaxed their abortion restrictions are Oregon, Colorado, New Mexico, Delaware, Maryland, Virginia, North Carolina, South Carolina, Arkansas, and Kansas.

[14]We note that these findings may be downward biased by that fact that the authors do not distinguish between reform and repeal states, which have quite different implications for abortion access.

university enrollment.

Only one study of which we are aware has examined the impact of abortion restrictions (rather than legalization) on educational attainment. In an unpublished dissertation, Borelli (2011) examines the impact of teen exposure to parental involvement laws in the 1980s and 1990s on fertility and educational outcomes. She finds that exposure to these laws increased Black women's fertility by 4 to 6 percent; impacts on White women were not significantly different from zero. Exposure also reduced the probability of Black women completing high school by 2.4 to 3 percent and the probability of entering college by 5 to 7 percent.

These policy evaluations document that access to contraception and abortion in the 1960s to 1990s reduced early fertility and also exhibited positive impacts on women's educational attainment. However, the economic, social, and political landscape of the U.S. has changed dramatically since that time. Between 1960 and 2019, the rate of high school completion for women age 25 and over has increased from 42.5 percent to 90.5 percent, the share of women age 25 and over with a bachelor's degree has increased from 5.8 percent to 36.6 percent (NCES, 2019), and the share of women in the labor force has increased from 37.7 percent to 57.4 percent (BLS, 2021). Norms have shifted; many women expect and plan for a career. Motherhood no longer means an end to a women's economic life, in fact, in 2019, 72.4 percent of mothers with young children were in the labor force (BLS, 2021). Increased focus on the importance of education may reduce the willingness of pregnant teens to drop-out, and may reduce the social costs of continuing schooling while pregnant or parenting. Increased social supports may somewhat ease the difficult task of completing one's education while parenting. In addition, the decades since the civil rights movement has brought (somewhat) increased racial equity, indicating that previously documented differences by race may no longer apply.

The impacts of early fertility on education have been well identified through exogenous policy changes that occurred in the mid- to late-20th century. However, none of this evidence reflects changes in the past two decades. A primary contribution of this paper is to fill this gap by providing evidence on the causal impact of early fertility on educational attainment as identified by exogenous shifts in teen births induced by policies occurring in recent decades.

3 TRAP laws

Abortion providers are subject to strict evidence-based regulations explicitly created to ensure patients' safety. These include state licensing requirements, federal workplace safety requirements, association requirements, and medical ethics requirements. Despite these regulations, states have also enacted TRAP laws, which mandate requirements that are more stringent than those for other medical procedures of similar risk (Jones, Daniel, and Cloud, 2018).

TRAP laws may affect abortion access because some clinics and providers cannot comply with the requirements, which obliges them to stop operating. In addition, such laws may prevent the opening of new clinics, further suppressing supply over the longer run. In 2017, ninety-five percent

of all abortions reported were provided at clinics. So a change in the number of clinics is a good proxy for a change in abortion access overall.

According to Nash and Dreweke (2019), who examine the period between 2011 and 2017, TRAP laws and administrative regulations reduced the number of clinics providing abortions. During this period, the South and the Midwest had the largest share of new abortion restrictions, with nearly 86 percent of total restrictions nationwide enacted in those two regions. As a result, the South had a drop of 50 clinics, with 25 in Texas alone, and the Midwest had a decline of 33 clinics, mainly in Iowa, Michigan, and Ohio (*ibid*). Regulations also resulted in the closure of nearly half of all the clinics that provided abortions in Arizona, Kentucky, Ohio, and Texas and the closure of five clinics in Virginia, including two of the state's largest providers. Smaller changes in clinic numbers are also significant in states where access to abortion services is already extremely limited. Missouri, West Virginia, and Wisconsin, each lost one clinic out of an already small number in each state. In cases like this, the remaining clinics typically cannot absorb all the patients seeking abortion care, and patients face significant obstacles to obtaining an abortion, such as longer travel distances and increased financial costs (*ibid*).

TRAP laws are more common in Midwestern and southern states. These policies became more common in the post-*Casey* era. Tables 1 and A.2 present the complete list of states that have ever implemented a TRAP law.

The passage of TRAP laws is a function of complicated political processes. For instance, Texas HB2 provides an example of how politically influenced abortion restrictions are. Passage of this bill involved a governor's special session, a 10-hour filibuster, an after-hours vote later nullified by the Lieutenant Governor, a second special session, and a heavily partisan vote. As in Texas, abortion legislation in many other states is heavily charged by political decisions.

In the years before the *Dobbs* decision, efforts were made to strike down TRAP laws. For example, in June 2016, in *Whole Woman's Health v. Hellerstedt*, the Supreme Court ruled that two of the most burdensome TRAP laws that had been enacted in Texas were unconstitutional. The Supreme Court did not find any evidence to support the need for these requirements and concluded that the restrictions created an undue burden for women seeking abortion services (NARAL, 2021). Since 2016, TRAP laws have been overturned in at least four states.

3.1 Types of TRAP laws

A common method of enacting TRAP laws is to require abortion facility licensing, which is an additional requirement to the standard licensing for health facilities, and to enforce a number of regulations as part of abortion licensing. These may include some or all of the regulations as discussed below. A second method is to require abortion clinics to operate as ambulatory surgical centers (ASC), or ambulatory surgical facilities. ASCs are health facilities that perform surgical procedures that typically do not require an overnight stay. These facilities usually perform surgical procedures that are more invasive and use higher levels of sedation than abortion clinics do. They generally are equipped for emergencies and meet a high standard of sterility. Each state mandates

a different set of requirements for licensing as an ASC, including some or all of the regulations discussed below.

Admitting privileges This type of regulation requires that some or all of a clinic's physicians must have admitting privileges or staff privileges at a hospital. These privileges allow providers to admit patients and personally provide specific medical services at that hospital. Securing privileges may be difficult for abortion providers based on public relations concerns of hospitals. Further, privileges often require that providers live near the hospital and admit a certain number of patients per year. However, since fewer than 0.5 percent of abortion patients in the U.S. experience a complication requiring hospitalization (Guttmacher, 2020), it is difficult for providers to meet the admission threshold. Securing privileges may be particularly challenging for rural providers as there is often no hospital nearby.

Transfer agreements This type of regulation requires clinics to have a written agreement with a hospital for the transfer of patients in case of emergency. While admitting privileges are granted to individual physicians, a transfer agreement is signed between the hospital and the clinic itself. Opponents argue that transfer agreement laws are unnecessary, as federal law already requires hospitals to admit to anyone who needs emergency services. These agreements may also be difficult to secure due to hospitals' concerns over public relations. In an extreme case, after requiring clinics to secure transfer agreements, the state of Ohio prohibited all public hospitals from entering into such agreements with abortion clinics.

In some cases, states enact regulations that require clinics to meet either an admitting privileges requirement or a transfer agreement requirement.

Hospital proximity regulations This type of regulation requires clinics to be located within a certain proximity (by distance or driving time) to a hospital. This is sometime included as part of admitting or transfer regulations. For example, a "clinic must have a transfer agreement with a hospital that is located within 30 miles of the clinic," requires a transfer agreement, but also requires that the clinic be located within 30 miles of a hospital. However, these regulations are also sometimes enacted separately from admitting and transfer regulations. Clinics in rural or remote locations can rarely meet this regulation, as the requirements are typically 15 or 30 miles, or 15 or 30 minutes of driving time.

Building regulations This category includes a wide variety of regulations that may apply, typically as part of ASC requirements, though they also occur under licensing requirements. Opponents argue that building regulations are too restrictive as clinics and providers already comply with federal and state safety and building standards. The requirements considered in this analysis include minimum widths for hallways or doorways; requiring clinics to meet detailed specifications for hospital-grade ventilation or have an emergency source of electricity; requirements on rooms such as having an operating room, a dedicated recovery room, or separate clean and dirty laundry areas;

or specifying the minimum size of procedure, operating, or recovery rooms. In some states, the list extends well beyond these, even specifying less related aspects such as bathrooms, water fountains, staff locker rooms, parking and receptions areas, etc; these are rare and are not considered in our analysis.

Other regulations The four types of regulations discussed above are those we expect to have the greatest potential impact on clinic closures and abortion access. However, we also note here other types of provider regulations that are not codified or included in this study based on their lower potential impact. These include staffing requirements, such as specific required qualifications for physicians beyond training, experience, and state licensing, specific residency training or certification by specific professional boards, or specific levels of nursing staff for specific functions. These also include requirements about certain policies the clinic should have in place, including preventive maintenance, infection control, disaster preparedness, quality assurance, peer review of physicians, or patient satisfaction assessments. Finally, we note that many states prohibit the provision of abortion by advanced practice clinicians, such as nurse practitioners or physician assistants, who are trained and regularly perform procedures at comparable levels of complexity and risk. While we do expect this regulation to have a significant impact on abortion access, we do not include these regulations in our analyses due to the fact that they mostly originate in the 1970s and have little variation during our period of study, 1993 to 2015.

3.2 Legal coding of TRAP laws

We use as a starting point the information on State Abortion Laws from the Policy Surveillance Program at LawAtlas. This includes all ASC and licensing laws ever implemented as of March 2021. It also provides, for each, a breakdown of specific regulations included in each. A drawback of this information is that it does not provide the dates of implementation for specific requirements.[15]

Austin and Harper (2019) is the only available source on the history of effective dates of TRAP laws. Their database includes information on three types of laws: ASC, admitting privileges, and transfer agreements. As noted above, the specific regulations of an ASC law vary by state (and over time within state). As such, ASC, like licensing, is not a specific type of regulation, but rather, a method for enacting specific requirements. Therefore, for this study, we create a more comprehensive legal coding on TRAP laws, focusing on the nature of the requirement rather than the type of law from which it originates.

To obtain information on implementation dates, we collated the text and dates of each listed law, regulation, or set of rules using the information in WestLaw, LexisNexis, Justia Law, and Case-Text.com. In instances where implementation dates were not precise (especially when restrictions

[15]LawAtlas has three comprehensive TRAP laws data sets: ambulatory surgical center requirements, abortion facility licensing, and hospitalization requirements. For this study, we only focus on the information of the first two data sets. First, because hospitalization requirements usually target post-first trimester abortions and do not impose specific requirements on the building, staffing, policies, and hospital relationships, as ASC and licensing requirements do. Second, these requirements were enforced in most cases in the 1970s. As such, most of the population in our sample has been fully exposed to them, creating no variation for our estimations.

come not from legislation but rather from health department regulations, which are often undated), we relied on historical "Who Decides?" reports from NARAL Pro-Choice America.[16] In addition, we occasionally relied on older reports from the American Civil Liberties Union (ACLU) and the Center for Reproductive Rights (CRR). In those instances where we could not find any information on implementation dates from the sources described above, we contacted states' Departments of Health and/or state archives to request the laws' original and amended texts. This allowed us to identify the timing of the relevant changes to the laws.

We define the effective date of a law to be the date it was first enforced, either as prescribed in the law itself, or after any period of enjoinment, if applicable. *A period where a law was not enforced due to ongoing or completed litigation is not considered to have an effective law.* We record the effective date for each state law or regulation separately for requirements that apply to all facilities versus only facilities providing second-trimester abortions.[17] Providers of second trimester abortions are a small minority of all abortion providers, as the vast majority of abortions occur in the first trimester. Therefore, in this analysis, we focus on regulations coded as applying to all providers. We also focus on TRAP laws implemented post-*Casey,* as pre-*Casey* TRAP laws and were quite different politically and in enforcement.[18]

In our analysis, our main independent variable of interest is a binary indicator for the presence of any TRAP law. We also explore impacts of each of the four TRAP types individually in Appendix B. We treat each type of TRAP law as binary.[19] However, for three of the four types of TRAP laws we coded, the level of stringency can vary. For example, some states require all physicians in the clinic performing abortions to have admitting privileges. Other states require at least one physician in the clinic to meet the requirement. A less stringent version requires clinics to have an agreement with an external physician who has admitting privileges. Further, at each of these levels, some states require this without exception, while others allow that clinics meet this regulations *or* a separately specified transfer agreement regulation. In Appendix C, we define the levels of stringency and explore the impact of a change in the level of stringency for admitting, transfer, and building regulations. In Appendix F, we additionally test whether the impact of a TRAP law differs according to whether other abortion policies were in place at the time of TRAP law implementation.

Table 1 presents TRAP laws that apply to all providers and were implemented between 1993 and 2013, with information on type. This table also indicates which of these laws are excluded from each type of analysis in the paper and the reason for each exclusion. Other categories of excluded

[16] We used the information available on the 2002-2005 PDFs. For the following years, we relied on online archive information.

[17] A few laws are specific to surgical abortion providers, medication abortion providers, or first-trimester providers. In each of these cases we included these regulations with those that apply to all facilities because the vast majority of abortion providers do provide first-trimester abortions, surgical abortions, and medication abortions.

[18] In a robustness check, we exclude the two states from our analysis that implemented a post-*Casey* TRAP law following a pre-*Casey* TRAP law, given the potential concern that the state was already previously treated (see Appendix Table G.1).

[19] In our analysis, we define an admitting privileges regulation as one that requires at least one of the clinic's own physicians to have admitting privileges. We define a transfer agreement regulation as requiring either a formal agreement *or* a plan or protocol.

TRAP laws (pre-*Casey*, too recent for our data, or only applying to 2nd trimester or later providers) are presented in Table A.2. Figure A.1 shows the states included in any of our analyses and the corresponding years of TRAP law implementation. Brief descriptions of the policy history on which our legal coding is based are provided for each state in Appendix H.

4 Impacts on teen births

4.1 Data

We employ natality data from Vital Statistics, including a record for every birth in the United States from 1990 to 2018 (NCHS, 2018). We collapse this to the state-year-age group-race/ethnicity level. Primary outcomes include the number of births to women aged 15 to 19 in each state-year, separately for non-Hispanic White, non-Hispanic Black, and Hispanic women. We additionally employ population counts at the state-year-age group-race/ethnicity level from SEER (2018), which are based on census counts and extrapolated for intercensal years. The race/ethnicity-specific population of women aged 15 to 19 in a state-year is used to scale birth counts by relevant population size. The population data do not include information on Hispanic ethnicity before 1990. We do not disaggregate births by mother's educational attainment, as we are focusing on births to women aged 15 to 19, most of whom have not yet completed their educational attainment.

As described in Section 3.2, we create a data set that indicates the presence of each type of TRAP law for each state-year observation. In addition, we rely on existing data that indicate the presence of a parental involvement law or a mandatory waiting period in each state-year as controls (Myers, 2020). Given the evidence that these restrictions also impact fertility, it is important that we control for their implementation to accurately estimate the impacts of TRAP laws.[20] These same data indicate the presence of other relevant policies; we present estimations including these as controls and note that our findings are not measurably different when excluding these controls. These include other abortion-related policies (state Medicaid funding for abortion), other policies related to reproductive health care access (availability of over-the-counter emergency contraception, insurance mandates to cover contraception, and expanded Medicaid eligibility for family planning services), and welfare policies (welfare reform, maximum benefits, and family caps).[21]

[20] Based on the findings in Myers and Ladd (2020) and Myers (2021b), we ignore parental involvement laws in the pre-*Casey* period and mandatory waiting periods that do not require two trips.

[21] Although policies on public funding for family planning services could affect reproductive health care access, we do not control for them in our analyses for several reasons. First, since 1976, the Hyde Amendment prohibits federal funds from covering abortion services for people enrolled in Medicaid, Medicare, and the Children's Health Insurance Program (CHIP). In addition, by 1990, the first year of data included in our analyses, most of the states already banned the use of public funds for abortion (Guttmacher Institute, 2021b). Many states have implemented additional measures to limit funding of family planning services, most notably the severe cuts in Texas that increased birth rates and decreased women's preventive health services (Packham, 2017; Lu and Slusky, 2016; Fischer, Royer, and White, 2018). However, as most of the facilities affected by these cuts provided contraception and not abortion, the cuts could only serve to increase the demand for abortion rather than reducing the supply.

4.2 Estimation

We note that during the era of our analyses, 1990 to 2018, the U.S. experienced a strong declining trend in teen births from 61 to 17 births per 1,000 women aged 15 to 19. Kearney and Levine (2015) find that this trend is only marginally explained by relevant policies and the 2008 recession. Given such a strong secular trend, we employ an event study estimation to compare changes over time between states that did vs states that did not implement a TRAP law. This methodology allows us to clearly test for whether such secular trends differed across these state types in advance of the TRAP law enforcement.

We estimate an event study using

$$E\left[y_{it+1}|b_{it}, \boldsymbol{X_{it}}, \nu_i, \nu_t\right] = exp\left(\sum_{j=\underline{j}}^{\overline{j}} \beta_j b_{it}^j + 1ln(pop_{it+1}) + \boldsymbol{X_{it}'}\delta + \nu_i + \nu_t + \varepsilon_{it}\right) \tag{1}$$

where y_{it+1} is births to women aged 15 to 19 in state i in year $t+1$, focusing on $t+1$ because, in the majority of cases, abortion access in year t would affect births in year $t+1$. b_{it}^j is an indicator that a TRAP law in state i turned on j periods away from t, where $j \in [\underline{j}, \overline{j}]$. X_{it} is a vector of controls for other policies relevant to teen birth outcomes in state i in year t, as described in Section 4.1. ν_i represents the state fixed effects, which control for time-invariant differences across states. ν_t are the year fixed-effects, which control for time-varying factors affecting teen birth rates in all the states in the same manner.

We define

$$b_{it}^j = \begin{cases} \mathbb{1}[t \leq e_i + j] & \text{if } j = \underline{j} \\ \mathbb{1}[t = e_i + j\} & \text{if } \underline{j} < j < \overline{j} \\ \mathbb{1}[t \geq e_i + j] & \text{if } j = \overline{j} \end{cases}$$

That is, the treatment indicator is binned at the endpoints of the effect window. This assumes that the effect of the policy is constant over time outside of the effect window, that is, for all $j < \underline{j}$ and $j > \overline{j}$.

We set $\underline{j} = -5$ and $\overline{j} = 4$, that is, the effect window includes 5 years before, the year of, and 4 years after the policy change. We chose this range to ensure enough years to fully observe dynamic policy effects while avoiding potential contamination by other adjacent policies. We omit the year before the policy change as the comparison year, standardizing $b_{it}^{-1} = 0$. The parameter β_j indicates the impact of a TRAP law on teen births j years later.

We estimate Equation 1 using a Poisson regression, controlling for the exposure, pop_{it+1}, the population of women aged 15 to 19 in state i in year $t+1$, and constraining the coefficient on this control to be unity.[22] We present estimates as percent changes, β^*, where $\beta^* = 100 \times (\exp(\hat{\beta}) - 1)$, for which the standard errors are estimated using the Delta method. We present versions where the

[22] Algebraically, by including as the exposure variable the log of the corresponding population and constraining its coefficient to be equal to 1, this is equivalent to having the birth rate as the dependent variable. In Stata this is achieved using the *xtpoisson* command and specifying the relevant population in the *exposure* option.

standard errors are and are not clustered at the state level. We find no evidence for overdispersion, as we also performed a negative binomial estimation and the results are identical. Given concerns about bias induced by the use of fixed effects in a Poisson estimation, we demonstrate robustness to a weighted least squares estimation in Appendix G.[23] We note that this Poisson construction using birth counts as the outcome is preferable to using constructed birth rates as the outcome. This is because birth rates rely on population counts, which are interpolated in intercensal years and can be somewhat unreliable when focusing on small sub-groups of the population (such as Black women aged 15 to 19, who are very few in some states).

We focus on events occurring in the post-*Casey* period due to the pivotal nature of the *Casey* decision in 1992, and because our aim is to estimate the impact of recent policy changes. We note that, beginning in 2016, some TRAP laws were overturned in court and regulations were removed. We therefore exclude years after 2016 from our analysis. We employ data on births from 1990 to 2016; given our range of $j \in [-5, 4]$, we can estimate the impact of events occurring from 1995 to 2012.

This method assumes that in the absence of the policy, the trend in teen births would have been the same in treated states as what is observed in control states. We test this assumption by checking whether the trends are the same across these two groups prior to policy onset. That is, we check whether we fail to reject that $\beta_j = 0$ for $j \in [-5, -2]$.

We note that this methodology also relies on the assumption that the effects of a TRAP law are homogeneous across states (Sun and Abraham, 2020). We recognize that this is a strong assumption that may be violated if the severity of laws differs across states, or if some states enact multiple TRAP laws in succession. Appendix C presents an event-study analysis that accounts for the intensity of treatment based on the severity of the restriction. Further, to test whether effect heterogeneity is biasing our main results, we also implement an alternative estimation that is robust to this issue, as presented in Section 4.4.

4.3 Results

We begin by presenting estimated impacts on aggregate births for all women aged 15 to 44 in Figure 2a. While the figure indicates an increase in aggregate births following TRAP law implementation, the increase seems to be a continuation of a pre-existing upward trend in births in TRAP states relative to non-TRAP states. Therefore, we cannot conclude that TRAP law implementation impacted aggregate births.

We next present the event study for teens aged 15 to 19 in Figure 2b. For this age group, we observe a nearly flat pre-trend, indicating that teen births were trending similarly in TRAP and non-TRAP states prior to the onset of the policy. Following implementation, teen births increase in TRAP relative to non-TRAP states, by an average of about 6 percent over the first 5 years after implementation. Therefore, we focus the remainder of our analysis on teen births.[24]

[23] We note that slight differences in the estimates are expected when using WLS. These are attributable not only to the use of population weights but also to the difference in the estimator.

[24] While women aged 20-24 account for about 28 percent of abortions, abortions account for only 26 percent of

Given the evidence from previous studies documenting differential impacts of abortion access by race, we estimate the impacts separately for White (non-Hispanic), Black (non-Hispanic), and Hispanic teens, with the results presented in Figure 3.

We first examine whether there is any evidence of a pre-existing trend in the difference between treatment and control states prior to policy onset. Among Black teens we find no evidence for a pre-existing trend, failing to reject that the coefficients in the pre-period are zero regardless of standard error type. This provides support for the assumption of common trends among Black teens. Among White teens the pre-trend is mostly zero, with only one period significantly different from zero. However, we note that the weighted least squares estimates in Appendix Figure G.1 do indicate a possible non-zero pre-trend for White teens.[25] However, among Hispanic teens there is more evidence in Figure 3 of an upward trend prior to policy onset, with nearly all of the pre-period coefficients significantly different from zero.

We next examine the increase in teen births in TRAP states relative to comparison states following policy onset. Among Hispanic teens, coefficients in the post period are quite large, but appear to be consistent with the pre-existing trend. We do not find convincing evidence that TRAP laws increase teen births among Hispanic women. Among White and Black teens, coefficients are consistently positive and are significantly different from zero beginning one or two years following policy onset (depending on choice of standard errors). Policy impacts in year two represent a 4 percent increase in teen births, with impacts rising to 7 to 10 percent by year four. The average impact over the post period is 3.2 to 3.5 percent, with upper and lower bounds of the 90 percent confidence interval at 1 percent and 6 percent, respectively. Given a baseline teen birth rate of 60.3 per 1,000 (Kost, Maddow-Zimet, and Arpaia, 2017), this indicates an increase of 2 births per 1,000 (with an upper bound of 3.6 births per 1,000). We also estimate Equation 1 separately by TRAP law type and find the strongest impacts are of admitting privileges laws, though transfer agreements and hospital proximity requirements also have statistically significant impacts among Black teens (see Appendix B).

There are several reasons why we might observe policy impacts increasing over time. First, some policies include a grace period after enforcement, which may allow clinics to remain open while working on compliance; for clinics that are unable to comply, closure may occur after the grace period. Second, clinic closures may result in increased congestion in remaining clinics with some lag, as patients may take time to learn about their next-nearest option. Third, demand may be "chilled" as women hear about clinic closures and difficulties in securing appointments due to congestion and decide that abortion is too difficult to access; such "hearsay" effects may take time

total pregnancies for this age group. In contrast, although 15-19-year-old women account for around 10 percent of abortions, it is estimated that 29 percent of their pregnancies are aborted. This figure is 31 percent for 15-17-year-old women and as high as 52 percent for those under age 15 (Kost, Maddow-Zimet, and Arpaia, 2017). Therefore, lack of access to abortion has the highest potential to increase birth rates in the youngest reproductive years.

We note that the event studies for the 20-24 age group show pre-existing differential trends, and the post-intervention estimates are not statistically different from zero at conventional levels. Therefore, we cannot conclude that exposure to TRAP laws changed fertility outcomes for women aged 20-24.

[25] In Appendix B, we present these estimates separately by TRAP law type and find some evidence of a pre-trend for White teens when examining admitting privileges.

to develop. Further, supply may also be "chilled" over time as providers that are willing and able to meet the new restrictions at first may become unable to meet the restrictions later (e.g. admitting privileges were not renewed) or they may decide to stop providing services in a state that is making it increasingly difficult. Finally, as the requirements may prevent new clinic openings that may have otherwise occurred, the total impact on access may accumulate over time. The increase over time in impacts on births is potentially a combination of all of these factors. We also note that one might expect that these dynamic affects arise from the pattern whereby a state continues to implement additional TRAP laws over time following the first onset. In Appendix C we estimate a modified event study that takes into account the occurrence of multiple events and we find that the increasing effect over time remains.

We explore why it may be the case that we observe pre-existing trends in teen birth differentials by TRAP exposure among Hispanic (and possibly White) women but not among Black women. We draw on data from the 1990 American Community Survey to compare characteristics of women in TRAP vs non-TRAP states before the onset of most TRAP laws. We find that differences across state type are more pronounced for Hispanic women. For example, college initiation is lower by 28 percent among Hispanic women in future-TRAP states relative to Hispanic women in non-TRAP states (this figure is 13 percent for Black women). Similarly, average family income is lower by \$10,243 among Hispanic women from future-TRAP relative to non-TRAP states (\$3,121 for Black women).We find that Black women have been similarly disadvantaged across states, regardless of TRAP law implementation in the subsequent years. However, for Hispanic (and possibly White) women, those living in future-TRAP states have been historically more disadvantaged than those living in non-TRAP states. These comparisons suggest that Hispanic teen births in future-TRAP states have the potential to trend differently from Hispanic teen births in non-TRAP states.

Our national-level results differ somewhat from those of state-specific studies of the impacts of TRAP laws on birth rates. For Texas HB2, Lindo et al. (2020) do not find statistically significant increases in births, either in aggregate nor for any age or race/ethnicity group. Fischer, Royer, and White (2018) do find that HB2 increased aggregate births by 1.3 percent, however, they find this increase was concentrated among women in their 30s and 40s and do not find any evidence of impacts on teen births (they do not disaggregate findings by race/ethnicity). In Pennsylvania, Kelly (2020) finds that a TRAP law increases aggregate birth rates by 3.4 percent, seemingly driven by White women (but results are not disaggregated by age). Therefore, we provide the first evidence of TRAP law impacts on teen births, and the first evidence disaggregated by both age and race/ethnicity. Nonetheless, these findings are consistent with Arnold (2022), who documents that the implementation of TRAP laws across the U.S. increases birth rates by 2-3 percent.

4.4 Robustness

We test whether the results presented in Section 4.3 are biased by heterogeneous effects across units, as proposed by Sun and Abraham (2020) and Borusyak, Jaravel, and Spiess (2021). We implement a stacked difference-in-differences design (Baker, Larcker, and Wang, 2022), as described

in Appendix D.[26] This method addresses concerns of bias arising from treatment effect heterogeneity or controlling for multiple treatments. In addition, it documents that the findings are robust to the exclusion of Texas HB2 as it is not eligible for inclusion in this analysis (see Appendix D).

Overall, the percent increases in teen births estimated by the stacked difference-in-differences are larger than those estimated by Equation 1. This suggests that the average of the estimates in Figure 3 of 3.2 to 3.5 percent are not upward-biased by treatment effect heterogeneity or controlling for multiple treatments, nor are they driven by Texas HB2.

5 Impacts on women's educational attainment

5.1 Data

We employ nationally representative microdata from the American Community Survey as provided by IPUMS (Ruggles et al., 2021), using all available waves from the first year of the data, 2000, until 2019. These are monthly cross-sections covering 3.5 million households per year. We rely on information regarding state of birth, year of birth, race/ethnicity, and educational attainment.

We restrict our sample to women aged 25 or older at interview, as women younger than 25 may be still completing their education. We also exclude women born outside the U.S. as their adolescent exposure to TRAP laws is unknown. Exposure is determined by state and year of birth, relative to the year a TRAP law was implemented in that state. We acknowledge that some women may have spent adolescence in a state other than their state of birth. Lacking detailed information on state of residence in each year of life, we believe state of birth is a good proxy for state of residence during adolescence as most adolescents still reside in their parents home, and 75 percent of migration is intrastate (Frost, 2020; Dey and Pierret, 2014).[27] For robustness, we present a version of our analysis where we further restrict the sample to women who currently reside in their state of birth.

In order to evaluate the impact of a policy in year t, the data must include cohorts with and without teen exposure to that policy. That is, we must include cohorts born both before and after $t-19$. Therefore, in order to restrict our analysis to post-*Casey* TRAP laws, we do not consider cohorts born before 1973 ($1992-19$). Because we restrict our sample to those aged 25 and older and the last year of data employed is 2019, the youngest birth cohort in our sample was born in 1994. Our analysis sample includes women born in the U.S., aged 25-46 at interview, and born between 1973 and 1994. Following the results in section 4, we estimate impacts for non-Hispanic White and non-Hispanic Black women. Table A.1 presents the ages at which women are first exposed to TRAP

[26] Several articles have proposed estimators for staggered treatment timing settings, allowing for a more sensible aggregation of heterogeneous treatment effects (Baker, Larcker, and Wang, 2022; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020a; Sun and Abraham, 2020). The estimator proposed by Callaway and Sant'Anna (2021) is one of the most widely used, however, this estimator assumes a linear model, which prevents us from using it in our non-linear setting. Instead, we rely on a stacked DD method that provides more flexibility on the assumed model. The difference with other approaches is that the weights are determined by the number of treated units and treatment variance within each stacked event rather than by economic considerations (Roth et al., 2022).

[27] A potential implication of using state of birth as a proxy for the state of residence during adolescence is measurement error, and therefore, attenuation bias.

laws, separately by race. Those with any teen exposure comprise 25 percent of White women and 30 percent of Black women.[28]

Our outcomes of interest are high school completion, defined as having at least graduated high school or passed the GED, college initiation, defined as completing at least one year of college, and college completion, defined as completing at least a bachelor's degree.[29] In our sample, high school was completed by 95 percent of White and 90 percent of Black women, college was initiated by 69 percent of White and 57 percent of Black women, and college was completed by 41 percent of White and 24 percent of Black women.

5.2 Estimation

We estimate the impact of exposure to TRAP laws during adolescence on educational attainment. We cannot employ an event study for this estimation, given that the impacts of exposure manifest with lags of varying lengths. We begin with a high dimensional fixed effects estimation. Then, in section 5.3, we test the underlying assumption of this estimation, counterfactual common trends. In section 5.4, we demonstrate that our findings are robust to heterogeneous treatment effects using a stacked difference-in-differences approach.

We estimate the equation

$$y_{ibsa} = \beta exp_{bs} + \boldsymbol{X'_{bs}}\delta + \nu_b + \nu_s + \nu_a + \varepsilon_{ibsa} \tag{2}$$

where y_{ibsa} is the outcome of interest for individual i, born in year b in state s, and interviewed at age a. exp_{bs} is an indicator of teen exposure to a TRAP law, which is determined by birth year b and state s.[30] Teen exposure is defined as being under age 20 in any year when a TRAP law is enforced in one's home state. We present estimates for exposure to any TRAP law in Table 2 and present estimates by TRAP law type in Appendix Table B.1. $\boldsymbol{X_{bs}}$ is a vector of indicators of teen exposure to other relevant state-level policies, which are discussed in Section 4.1. We include fixed effects for the birth year to control for national cohort trends in outcomes. We also include state of birth fixed effects to control for time-invariant differences across states. In addition, we include age at interview fixed effects to control for natural increase in average educational attainment with age. Estimations are weighted using data-provided person weights to be nationally representative.

[28] We note that the birth cohorts used here (1973 to 1994) differ somewhat from the cohorts included in the analysis in Section 4 (women aged 15 to 19 in years 1990 to 2016 were born in years 1971 to 2001). For consistency, we re-estimate Equation 1, limiting the sample to births that occurred from 1992 to 2009 (women aged 15 to 19 in 1992 to 2009 born in years 1973 to 1994). Figure G.2 shows the results. Although the estimated treatment effects are less precise than those presented in Figure 3 due to the smaller sample size, overall, the patterns observed are similar.

[29] In order to separate high school diploma from GED, we must focus on the sub-sample of individuals interviewed in 2008 or later. Among this sub-sample, excluding those who passed the GED from the indicator for completing high school does not change the results.

[30] An alternative identification strategy would be to employ exposed men as the control group (instead of or in addition to unexposed women). However, we highlight that men are not unaffected by abortion access, as unintended births may also affect their education. As such, their use as a control group would significantly underestimate policy impacts. Nonetheless, we do estimate impacts on men as a falsification test, since impacts on men equivalent to impacts on women would indicate a violation of the estimation assumptions (see Section 5.4).

Standard errors are clustered at the state of birth level.

Panel A of Table 2 shows the estimates of β from Equation 2, where exp_{bs} indicates teen exposure (before age 20). Each coefficient comes from a separate estimation. Columns 1 to 3 show the impacts on non-Hispanic White women. We find no significant effects on their high school completion and college initiation. For college completion, we find a reduction of 0.7 percentage points, an effect of 1.6 percent relative to the mean (significant at the 5 percent level). Impacts on non-Hispanic Black women are presented in columns 4 to 6. We find negative and significant impacts on college initiation of 1.0 percentage point (1.8 percent relative to the mean), significant at the 5 percent level, as well as a negative impact on college completion of 0.5 percentage points, though we cannot reject that this effect is zero.

We note that the impact of an unintended birth may differ by age of occurrence, even within the age group 15 to 19. We thus explore whether impacts on education are exacerbated when exposure to a TRAP law occurs at an earlier age. In Panel B of Table 2, exp_{bs} indicates exposure before age 18. While the findings for White women are comparable, we find that effects are larger for Black women. Exposure before age 18 decreases their college initiation by 1.2 percentage points (2.1 percent) and decreases college completion by 1.4 percentage points (5.8 percent), significant at the 1 percent level. We note that these estimates are robust to controlling for later-life exposure (at ages 18-24 and 25-29; results not shown).

We confirm that the magnitudes of these estimates can be explained by the TRAP-induced increases in births to Black teens. The estimated impact of any TRAP exposure before age 20 on Black women's college completion is 0.5 percentage points, or 5 women per 1,000.[31] How many Black women were estimated to have a teen birth as a result of TRAP exposure? In section 4.3, we estimate that a TRAP law increases births among Black teens by 3.24 percent in a year when it is enforced. The translation of this into a number of women relies on the choice of a baseline birth rate to which the 3.24 percent can be applied. We note that the Black teen birth rate was consistently declining throughout our study period from a high of 111.3 in 1992 to a low of 39.0 in 2013 (Kost, Maddow-Zimet, and Arpaia, 2017)., This suggests a TRAP-induced increase between 1.3 and 3.6 births per 1,000 Black teens per year of TRAP law exposure. In our sample, the average Black woman with any TRAP exposure as a teen is exposed for a mean of 4.2 years between age 15 and 19. Taken together, this suggests that an additional 5 to 15 out of every 1,000 Black women had a teen birth as a result of TRAP law exposure. This is consistent with our finding that 5 out of every 1,000 Black women were unable to complete college as a result of a TRAP law. The large overlap in the intervals between number of women affected in terms of births and college completion suggests that TRAP-induced increases in teen births could be fully responsible for the estimated increases in Black women's college completion. However, it does not rule out the possibility of other relevant mechanisms by which TRAP laws could impact education (such as expectations), given that the

[31] We employ here the estimate from Panel A of Table 2 as it estimates the impact of exposure before age 20, which matches the ages of interest for the births analysis. While the impacts on education are larger for exposure before age 18, we are unable to conduct the analogous births analysis for this sub-group due to availability of population data only for pre-set age groups.

point estimates are slightly higher for college completion than for births.

To ensure that our estimated effects are not driven by potential differences between state of birth and state of residence during adolescence, Panel C of Table 2 shows the estimation from a sub-sample of women for whom state of residence at interview matches state of birth. For White women, the magnitudes are comparable but the estimates are less precise. For Black women, the magnitudes increase slightly and the precision is comparable. The remainder of Table 2 is discussed in Section 5.5, below.

5.3 Testing common trends

We estimate the causal impact of TRAP laws on education, conditional on the following assumption. In the absence of the TRAP law, conditional on state and year fixed effects, the trend over time in women's educational attainment would have been the same in an implementing state as it was in other states that did not implement a TRAP law. We note that level differences between these states in educational attainment would not violate this assumption. However, a violation that might explain our results would be that an implementing state had a pre-existing trend in education that was negative relative to the trend in other states.

To seek evidence of such a violation, we test for differential trends in education by TRAP law status prior to implementation. To do so, we select a relatively narrow age group for whom the outcome would have been recently determined to allow for dynamic analysis. We examine college initiation of women aged 18 to 24 and college completion of women aged 25 to 29.

We estimate

$$y_{ist} = \sum_{j=\underline{j}}^{\overline{j}} \beta_j b_{it}^j + \boldsymbol{X_{ist}'}\delta + \nu_s + \nu_t + \varepsilon_{it} \tag{3}$$

where y_{ist} is an indicator that women i in state s observed in year t had initiated (or completed) college. b_{it}^j is an indicator that a TRAP law in state i turned on j periods away from t, where $j \in [\underline{j}, \overline{j}]$. X_{it} is a vector of controls for relevant policies at the state by year level, as described in Section 4.2. ν_s and ν_t are state and year fixed effects, respectively.

Given our data range from 2000 to 2019, these contain information up to 16 years before the latest policies we examine (2016) and up to 25 years after the earliest policies we examine (1994). However, observations at the tails are few and so we set $\underline{j} = -10$ and $\overline{j} = 20$, accumulating at the end points as described for Equation 1. As before, we standardize $\beta_{-1} = 0$. Estimations are weighted using data-provided person weights to be nationally representative. Standard errors are clustered at the state level.

We note that while this is identical to an event study in estimation, we do not interpret it as such. Given the measurement of educational indicators by age group, there is not a single point in time where such a group switches from fully untreated to fully treated. Rather, there is a transitional period from the time when any of group is treated to the time when all of the group are treated. For example, for a policy that turns on at $j = 0$, women exposed to the policy before age 18 would

not be aged 25 until at least $j = 8$. Not until $j = 12$ would it be the case that all the women in that age group were exposed to the policy before age 18.

Despite its drawbacks in terms of identifying impacts on exposed women, this method can clearly test whether these indicators of women's education were trending similarly across TRAP and non-TRAP states prior to implementation. If that is the case, we should see that the β_j coefficients should be not different from zero.

Figure 4 presents the β_j coefficients for estimations of Equation 3 for college initiation and college completion separately for the non-Hispanic White and non-Hispanic Black samples. In all four sub-figures the coefficients for the years preceding policy implementation are not statistically different from zero (almost without exception). There is no evidence that women's education was trending negatively in TRAP-implementing states relative to other states in the years leading up to policy implementation.

While the purpose of this exercise is to examine prior trends, we also note that the trends after policy implementation are consistent with the evidence presented in Table 2. The effects for White women are suggestive at best and are non-zero only for college completion. In contrast, for Black women we see that college initiation becomes negative intermittently during the period when the age group of interest is partially treated, and becomes consistently negative in the years when the group is fully treated. The pattern for Black women's college completion is even more compelling, with a strong downward trend in all the years following implementation. We note that negative effects are estimated even in post-implementation years when none of the age group was officially treated (i.e. exposed before age 18); this is likely a result of the fact that, while early exposure has the strongest impacts, exposure even at ages 18-24 (25-29) may still impact college initiation (completion) for some women.[32]

We further test the robustness of these findings to possible violations of the parallel trends assumption using the method proposed by Rambachan and Roth (2023). We estimate bounds on the relative magnitude of deviations from parallel trends that the findings can withstand. Following the example provided in Rambachan and Roth (2023) section 6.3, we provide alternative confidence intervals for the coefficient at $j = 15$ under various assumptions of $\bar{M} \in [0.25, 1.5]$, the factor by which the maximum pre-trend deviation is multiplied. As shown in Appendix Table G.2, the test indicates that the findings are robust to violations up to the same size as the largest deviation observed in the pre-period, or $\bar{M} = 1$, with a "breakdown value" at $\bar{M} = 1.25$.

[32]By focusing on narrow ranges of ages, we partially address the concern that the birth cohorts used in these analyses do not have a balanced representation since we do not have a balanced panel of age cohorts. To document that imbalance across cohorts is not driving our results, Appendix Figure G.3 is analogous to Figure 4 but only includes a single age year (22 for college initiation and 26 for college completion). In this analysis, all birth cohorts have equal representation–each appears in exactly one year of data. While the standard errors are larger due to the smaller sample size, the pattern of coefficients is consistent with those in Figure 4.

5.4 Robustness & falsification

We test whether the results from the two-way fixed effects estimation presented in Section 5.2 may be biased by heterogeneous effects over time or across units, as recently proposed by several studies (de Chaisemartin and D'Haultfœuille, 2020a; Athey and Imbens, 2021; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). We implement a stacked difference-in-differences design (Baker, Larcker, and Wang, 2022), as described in Appendix D.

We find that all effects that are estimated to be significantly different from zero by Equation 2 are robust. In fact, the coefficients from the stacked DID estimate are larger in magnitude and more precisely estimated.

We also present a falsification test. If the estimated impacts on education are truly driven by reduced access to abortion, we should find that the impacts are substantially greater for women's education than for men's education. We do not necessarily expect a zero effect on men, as the education of young men may also be interrupted by early fatherhood, however, we do expect the potential interruption to be greater for young women. On average, mothers dedicate more time to parenting activities than fathers, and women are more likely to experience early parenthood than men.[33] If we find equivalent effects on men's and women's education, this would indicate the influence of confounding factors.

We estimate Equation 2 for men, separately for White non-Hispanic and Black non-Hispanic. The results are presented in Appendix Table G.3. The estimated effects on men's education are less than half the size of the effects on women's education and are not statistically different from zero.

5.5 Differences by race

While we find comparable impacts on teen births across White and Black women, the downstream impacts on educational attainment are strikingly different. Impacts on college initiation are statistically significant only for Black women, and impacts on college completion are more than twice as large for Black as for White women. This may reflect the fact that, in the U.S., race is highly correlated with economic disadvantage. For a disadvantaged woman, it is likely that those in her social support network also have fewer resources, and therefore lower ability to provide support and assistance. Financial and/or childcare support from family or friends may be critical for a teen mother's ability to continue her education. However, it may also be the case that the effects on education are stronger for Black women for reasons more directly related to race, such as systematic discrimination and structural racism.

We seek to test whether the different impacts by race are reflecting the correlation between race and economic status or whether the difference is more directly related to the social construct of race. We cannot rely on any indicators of economic status at the time of the interview, as these may also be affected by exposure to TRAP laws and the resulting impacts on educational attainment. We therefore focus on the only information we have about a woman prior to her adolescence: state of

[33] Based on the Vital Statistics data used in this study, children in the US are four times more likely to have a mother under age 18 at birth (2%) than to have a father under age 18 at birth (0.5%).

birth. For each race, we calculate the share of women in the state who are living below the federally defined poverty line (based on their household income and household size). This ranges from 15.6 to 35.3 percent for Black poverty (median 27.7 percent), and ranges from 6.5 to 19.7 percent for White poverty (median 11.7 percent). States with above median levels of Black women living in poverty are defined as "high Black poverty" states. We estimate Equation 2 separately for each state category.[34] Results are presented in Panels D and E of Table 2.

We find that the impacts of teen TRAP law exposure on educational attainment of Black women are concentrated among Black women born in states with above-median levels of Black poverty. For women from these states, we find significant effects for all categories of educational attainment with magnitudes of 1 percent decrease in high school completion, 3 percent decrease in college initiation, and 5.5 percent decrease in college completion. In contrast, impacts on Black women born in states with lower rates of Black poverty are one-sixth to one-half as large and are not statistically different from zero. This indicates that the impacts on educational attainment observed for Black women are concentrated among women from economically disadvantaged backgrounds.

We also present analogous results for non-Hispanic White women. We note however that the threshold for poor vs non-poor states is very different by race. No states have a White poverty rate above 20 percent. We thus define "high White poverty" states as those above the 75th percentile (14 percent White poverty rate).[35] We stress that poverty rates are much lower (roughly half) among White women in "high White poverty" states relative to Black women in "high Black poverty" states.

Among White women, we find a similar pattern whereby the estimated effects are found to be concentrated among White women born in states with higher rates of White poverty. Nonetheless, we note that a significant difference by race still remains among those born in states with high levels of race-specific poverty.

Ideally, we would compare estimates for Black and White women from a sub-sample born in states with comparable rates of race-specific poverty. However, the window of overlap in race-specific poverty rates is very narrow (15.6 to 19.7 percent – states with the highest levels of White poverty and the lowest levels of Black poverty). For Black women, this sub-sample would include DC, MT, NM, and UT, with 87 percent of observations being in DC.[36] Clearly this sample is too small for a representative analysis. Ultimately, the correlation between race and poverty in the U.S. is too strong to allow us to compare impacts across samples with comparable economic status at birth.

While we find clear evidence that economic disadvantage at birth predicts a stronger impact of TRAP laws on educational attainment, we cannot say that differences in poverty fully explain the differences in impact by race.[37]

[34]States with above-median rates of Black poverty include: AL, AR, GA, IA, ID, IL, IN, KY, LA, ME, MI, MO, MS, NE, OH, OR, PA, SD, TN, WI, and WV.

[35]States with White poverty in the 75th percentile include: AL, AR, IN, KY, LA, ME, MS, NC, OK, OR, SC, TN, and, WV.

[36]States with White poverty in this range include only: AR, KY, TN, and, WV.

[37]We also compare estimates for Black versus White women who are currently living in poverty, however, these sub-samples are endogenously determined as teen TRAP law exposure is likely to impact adult poverty status through its impact on educational attainment. Nonetheless, we note that the findings from such estimations are consistent with what is presented here.

6 Mechanisms

In this section we present evidence on the pathways by which the implementation of a TRAP law may impact educational outcomes. We first document mechanisms by which TRAP laws impact births. We next seek evidence for whether TRAP laws may impact educational outcomes in ways other than unintended births.

6.1 Pathways by which TRAP laws affect births

There is evidence that TRAP laws result in the closures of clinics that are unable to comply with the new regulations and prevent the opening of new clinics. This may increase distance to the nearest clinic and/or increase clinic congestion and wait times, thereby reducing access to abortion. Such impacts have been documented as a result of TRAP laws in Texas and Pennsylvania (Fischer, Royer, and White, 2018; Lindo et al., 2020; Kelly, 2020). We document that TRAP laws exhibit similar impacts nationally in terms of distance to clinic and abortion use.

6.1.1 Abortion access

We employ county-by-year data on distance to the nearest abortion provider from the Myers Abortion Facility Database (Myers, 2021a). These data begin in 2009, so, as a suggestive analysis, we test whether the most recent TRAP laws have increased average distance to the nearest clinic.[38] We estimate a simple, two-period difference-in-differences estimation

$$D_{cst} = \alpha + \beta_1 TRAP_s + \beta_2 post_t + \beta_3 (TRAP * post)_{st} + \varepsilon_{cs} \tag{4}$$

where D_{cst} indicates distance to the nearest abortion provider from county c in state s in year t, where $t \in \{2009, 2017\}$. $TRAP_s$ indicates that state s turned on a new TRAP law between 2010 and 2016, and $post_t$ indicates that $t = 2017$. β_3 estimates the impact of TRAP laws on average distance to provider from 2009 to 2017. Mean travel distance to the nearest provider in these data is 75 miles (mean travel time is 85 minutes).

We first document that TRAP laws increase the distance to the nearest provider in the average county by estimating Equation 4 at the county-year level, unweighted. Next, as we are interested in the extent to which increased travel distance affects the average person's access, we also estimate Equation 4 weighted by county population of women of reproductive age.

Estimates of β_3 are presented in Panel A of Table 3. We find that the enforcement of a TRAP law increases the distance to the nearest abortion provider in the average county by 12 to 13 percent when measured in geodesic or travel distance, respectively, and by 10 percent when measured by

[38] We note that the Alan Guttmacher Institute also provides data on the number of abortion providers per county in 28 separate years since 1973. However, these data are truncated for security purposes. For any county-year in which there were fewer than 400 abortions provided, the data reflect zero providers. This truncation is particularly salient in rural and remote counties, where TRAP laws such as admitting privileges and hospital proximity regulations are most likely to close clinics. As such, this data is not useful for our purposes.

driving time. These effects are significant at the 1 percent level. When weighting by population, the estimated change in distance is still positive, though about half as large and not statistically significant. This indicates that TRAP-induced increases in distance to provider are more severe in counties with smaller populations. In addition, TRAP-induced changes in abortion access in urban areas may be operating not only through increased distance but also through increased clinic congestion, as documented in Pittsburgh by Kelly (2020).

Our findings are in line with those of other studies of the impact of clinic closures on distance to nearest provider. In Wisconsin, the closure of a single clinic increased distance by 3 percent, and the closure of a second clinic increased distance by 43 percent (Venator and Fletcher, 2020). In Texas, HB2 closed nearly 20 clinics and increased distance by more than 100 percent (Lindo et al., 2020). We estimate a national average increase of about 6 percent, potentially indicating that the average TRAP law closes about one clinic. This finding is consistent with Caraher (2023), who finds that TRAP laws reduce abortion rates by 6 percent during a six-year period after the law was enacted. Similarly, Arnold (2022) finds an approximately 5 percent reduction in the year the TRAP law is adopted, and this effect remains and even increases in the subsequent years.

6.1.2 Abortion use

We employ state-by-year data on abortion counts provided by the Center for Disease Control and Prevention's Abortion Surveillance System (Kortsmit, 2020). These data are available from 1992 to 2018, disaggregated by age group.[39] CDC collects abortion counts at the state level both for residents of the state and occurrences in that state. We focus on abortions of residents to avoid compositional effects due to the impacts that policies could have on residents of bordering states. A downside of this information is that state reporting to CDC is voluntary and some states fail to report in some years. We supplement our analysis using state-by-year information on abortion use of state residents from Guttmacher Institute (Guttmacher Institute, 2021a). The advantage of this information is that it is collected through direct surveys of abortion providers, which usually results in higher counts, relative to CDC abortion counts. However, Guttmacher surveys are not conducted every year and information is not disaggregated by age group.[40]

We estimate

$$E\left[y_{st}|TRAP_{st}, \boldsymbol{X_{st}}, \nu_s, \nu_t\right] = exp\left(\gamma TRAP_{st} + 1ln(pop_{st}) + \boldsymbol{X'_{st}}\delta + \nu_s + \nu_t + \varepsilon_{st}\right) \tag{5}$$

where y_{it} is the number of abortions to residents of state s in year t, $TRAP_{st}$ indicates that any TRAP law was enforced in that state and year, X_{st} is a vector of state-year policy controls as described in Section 4.1, and fixed effects are included at the state and year levels. As in Equation 1, we estimate a Poisson model and control for the exposure, pop_{it}, the relevant population of women

[39] We note that these data are also disaggregated by race for some states in some years from 2007 to 2018. However, the coverage is too incomplete and too inconsistent for these data to be useful.

[40] For the post-*Casey* era, the years in which Guttmacher Institute did not collect data are 1993, 1994, 1995, 1997, 1998, 2001, 2002, 2003, 2006, 2009, 2012, 2015, 2016.

in state s in year t, and constrain the coefficient on this control to be unity. For consistency with our other analyses, we examine the impact of TRAP laws implemented between 1993 and 2013.

Estimates of γ are presented in Panel B of Table 3, both excluding $\boldsymbol{X_{st}}$ (column 1) and including $\boldsymbol{X_{st}}$ (column 2). Using CDC data in the fully controlled model, we find that abortion use overall declines by 4.2 percent, an effect that is significant at the 10 percent level. For teens, the effect is slightly larger (4.6 percent) and is also significant at the 10 percent level. Using the Guttmacher data, the aggregate estimate is identical: 4.2 percent (though not significantly different from zero).

Other work has documented moderate to large impacts on aggregate abortion rates arising from large changes in distance to nearest provider. For example, increasing distance by roughly 50 miles, reduces abortion use by 16 to 36 percent.

For example, in Texas, increasing distance by 100 miles decreased abortions by 10 percent (Quast, Gonzalez, and Ziemba, 2017). Other estimates based on HB2 suggest that an increased distance of roughly 50 miles reduces abortions by 16 percent and increasing distance by 200 miles reduces abortions by 44 percent (Lindo et al., 2020). In Wisconsin, an increase in distance of roughly 25 miles decreased abortion by 16 percent and an increase of roughly 50 miles decreased abortion by 36 percent (Venator and Fletcher, 2020). In section 6.1.1, we offer national-level evidence that that the implementation of a TRAP law increases distance to nearest provider by an average of 10 miles. As such, our estimated decreases in abortion use of roughly 4 percent are comparable to the 16 percent decreases estimated for distance increases of 25 to 50 miles.

Our estimates of TRAP-induced changes in abortion use are also consistent with our estimates of TRAP-induced changes in births. We estimate a reduction of 4.6 percent in teen abortion use, with an upper bound of 8.7 percent within the 90 percent confidence interval. Based on a baseline teen abortion rate of 35.7 per 1,000 (Kost, Maddow-Zimet, and Arpaia, 2017), this would represent an increase of 1.65 births (up to 3.09 births) per 1,000 teen girls. This is consistent with the increase in births of 2 per 1,000 as described in section 4.3.[41]

6.2 Non-birth pathway by which TRAP laws may impact education

An unexpected birth is not the only way in which abortion access might impact educational attainment. If a young woman expects that over her lifetime she will have full control over whether to have children, when to do so, and how many to have, this will impact her vision for her future. Such

[41] We note that the teen abortion rate was consistently declining throughout our study period from a high of 35.7 in 1992 to a low of 10.6 in 2013. This makes the selection of a baseline abortion rate challenging, as the statistic from 1992 may not be representative of the period, while the statistics from later years are attenuated by the impacts of existing TRAP laws. For example, a more conservative baseline teen abortion rate of 19.1 from 2005 also reflects the fact that TRAP laws were already in place in at least 10 states at that time. Nonetheless, we also report that employing the 2005 teen abortion rate (19.1) as the baseline, together with the estimated 4.6 percent reduction in teen abortion use, would generate at most an increase of 0.88 (up to 1.66) births per 1,000 teen girls. While this is slightly less than the estimated increase in births, we note that an effect of this size cannot be ruled out, given the standard errors. Finally, we highlight that it is also possible that TRAP laws increase teen births in ways other than reducing abortion use. If reproductive health clinics are less accessible (either due to distance, congestion, or perception) and/or teen girls are less likely to visit them (due to unavailability of abortion), then TRAP laws might also reduce teens' access to contraception. While the estimates in Section 6.2 do not provide evidence of this, they also do not rule it out.

expectations may lead her to aspire to a professional career, thereby impacting her decisions (and effort) regarding education. If a young woman instead observes the onset or presence of regulations that restrict abortion access (or more concretely, observes family members or friends who are unable to obtain wanted abortions), she will likely expect to have imperfect control over her future fertility outcomes.[42] Such expectations may likewise affect her aspirations and investments in her education and career. In this way, restrictions on abortion access may impact the educational outcomes of a woman even if she never experiences an unwanted pregnancy.[43]

In order for expectations to act as a significant pathway by which TRAP laws affect educational attainment, it must be true that teens are able to accurately perceive their access to abortion, or at least be aware of restrictions to access. We test whether teens perceive differences in abortion access by examining impacts of TRAP laws on teen sexual behavior and contraceptive use. This analysis is presented in Appendix E.

The results suggest that at least some teens do adjust their sexual behavior by delaying sexual debut in response to reduced abortion access (specifically, non-Hispanic teens). This indicates that they do perceive changes in abortion access. While this does not provide direct evidence for expectations as a pathway by which TRAP laws impact educational attainment, these findings indicate that we cannot rule out this pathway as potentially operable.

7 Conclusion

In this study, we provide new evidence on the modern relationship between women's ability to control their fertility and an important investment in economic welfare – educational attainment. Earlier studies have documented the causal impact of early fertility on education, but these studies rely on policy changes occurring five decades ago (Angrist and Evans, 2000; Goldin and Katz, 2002; Hock, 2008; Bailey, Hershbein, and Miller, 2012; Ananat and Hungerman, 2012; Edlund and Machado, 2015). Given the significant economic and social shifts since that time, it is not clear whether such estimated effects are still relevant today. Measuring the modern impact of early, unintended fertility on educational attainment is critical given the proliferation of abortion bans in the wake of *Dobbs*. We provide the first evidence of the impact of teen motherhood on education that relies on policy changes in recent decades.

We examine the impacts of teen exposure to TRAP laws as an exogenous shifter of teen fertility. To do so, we create a historical coding of TRAP law implementation that is more accurate and detailed than what previously existed. We provide national-level estimates of the impacts of TRAP laws on fertility, and we present a variety of estimates, taking into account the type, severity, and number of TRAP laws implemented in a given year. We also take seriously the recent concerns about the bias introduced by heterogeneous treatment effects in two-way fixed effects estimations. As

[42] Anand and Kahn (2022) find that teens' observations of friends' and siblings' fertility is highly salient, even affecting their own sexual behavior.

[43] As an example, Steingrimsdottir (2016) documents that increased access to contraception increased the probability of women in the first year of college aspiring to higher-earning, male dominated professions.

such, we employ an event study approach and show robustness to a stacked difference-in-differences approach as well.

Our results indicate that TRAP laws increase teen births to non-Hispanic women, both White and Black, by more than 3 percent, while they do not change existing trends in births to Hispanic teens. We provide evidence that these effects are operating through the expected mechanisms – decreases in abortion access and abortion use.

Our key finding is that adolescent exposure to TRAP laws reduces educational attainment. Using pooled and stacked difference-in-differences approaches, we find that exposure to TRAP laws before age 18 reduces Black women's educational attainment as measured at ages 25 to 40. Our results indicate that college initiation is reduced by 2.1 percent, and college completion is reduced by 5.8 percent. The impacts of TRAP laws on the educational attainment of White women are smaller and less robust. We find reductions in college completion of 1.6 percent, with no impacts on college initiation.

While we document unintended births as a predominant mechanism by which TRAP laws impact educational attainment, we also find evidence that teens perceive changes in abortion access, suggesting that expectations may also be a mechanism.

It is noteworthy that, while TRAP laws have similar impacts on teen birth for White and Black women, the downstream impacts on educational attainment are concentrated among Black women. We provide evidence that the race gap in the impact of TRAP laws is at least partially driven by racial differences in poverty at birth. However, we cannot rule out that a race gap remains when holding constant poverty at birth. We note that even in states without TRAP laws, there are significant race gaps in educational attainment.[44] Our findings suggest that TRAP laws are acting to exacerbate this existing racial inequality by preventing some Black women from completing their education.

We find that the negative impact of TRAP laws on Black women's educational attainment is strikingly comparable in magnitude to the positive impacts of legalization in the 1970s. Angrist and Evans (2000) found that any legal access during adolescence increased Black women's college initiation by 3.7 percent (as compared to the 2.1 percent we estimate) and college completion by 9.6 percent (as compared to 5.8 percent). Both studies find no robust impact on the educational outcomes of White women. This suggests that, for some women, TRAP laws are acting as a substantial barrier to abortion access – such that the impacts approach those of having no legal access. It also suggests that, despite fifty years of progress and social change, the economic futures of today's women are impacted by access to abortion in ways similar to those of women in the 1970s.

This may be surprising, given the differences in context between these two periods. However, the large changes in access following *Roe* were fairly homogenous in nature. Access was increased for nearly all women, regardless of socio-economic status. In contrast, the changes in access resulting from TRAP laws disproportionately affect disadvantaged women. While TRAP laws may leave

[44] Among women never exposed to TRAP laws, college completion is 18.4 percentage points (40 percent) lower among Black versus White women.

access unaffected for some (i.e., advantaged) women, they may leave other (i.e., disadvantaged) women with no access at all. If it is the disadvantaged population driving the impacts on educational outcomes, which seems likely, then it is not surprising that the impacts of changes in access on educational attainment would be comparable across these two periods.[45]

We also compare our findings to those from a more recent experiment, Brooks and Zohar (2021). They find that increasing abortion access among young women increased education of young, disadvantaged women (in Israel, these were women from religious families with low parental income). They estimate an increase in college enrollment of 4 to 11 percent – much higher than our estimate of a 2.1 percent change in college initiation. However, their sample is restricted to women who conceived at age 20 or 21. Assuming these are 6.9 percent of women in that age group, and assuming that abortion access does not affect educational attainment of women who do not conceive, this suggests an aggregate effect of 0.3 to 0.7 percent – considerably smaller than our estimate.[46] We note that the larger effects of "treatment on the treated" that are estimated when narrowing the sample to women with a conception are interesting and useful from a policy perspective. However, we also note that data limitations make possible only the estimation of an average treatment effect for the aggregate population in the U.S. context. Nonetheless, given the currently fluid state of abortion access in the U.S., estimates specific to this context are critical for policy.

As the reproductive health care landscape continues evolving and more states enforce abortion bans, evidence of these policies' impacts is critical. An amicus brief to the U.S. Supreme Court for the *Dobbs* case claimed that, as a result of social and economic changes in recent decades, access to abortion is no longer necessary in order for women in the U.S. to fulfill their economic potential (Collett, Alvare, and Bachiochi, 2021). The evidence provided here suggests otherwise. Access to abortion, especially in the early reproductive years, can be a significant predictor of investment in one's economic future. While it may be true that restrictions to access have a lesser impact on the most privileged women, women who already face the greatest barriers to economic advancement are the most harmed by restrictions. In light of this, it is clear that access to fertility controls is necessary for women's economic advancement.

[45] In addition, while access to safe, self-managed abortion may serve to partially offset changes in legal access, we note that the rise of safe, self-managed abortion largely occurred after the end of our study period. As of 2017, only 7% of women in the U.S. had ever self-managed an abortion (Ralph et al., 2020). Most of the increase in self-managed abortion has occurred post-*Dobbs*; the decision led to an increase of 167% in online searches for abortion medication (Poliak et al., 2022).

[46] These assumptions are based on U.S. statistics. Our calculation from American Community Survey data in years 2015-2019 suggest that 5.0 percent of women aged 21 and 22 had a birth in the past year. In addition, CDC estimates that the abortion rate for women aged 20 to 24 is 19 out of 1000 women (Kortsmit, 2020). Combined, these indicate that $5.0 + 1.9 = 6.9$ percent of women in the US conceive at age 20 or 21. Multiplying their estimated impact by 0.069 yields a range of 0.003 to 0.007.

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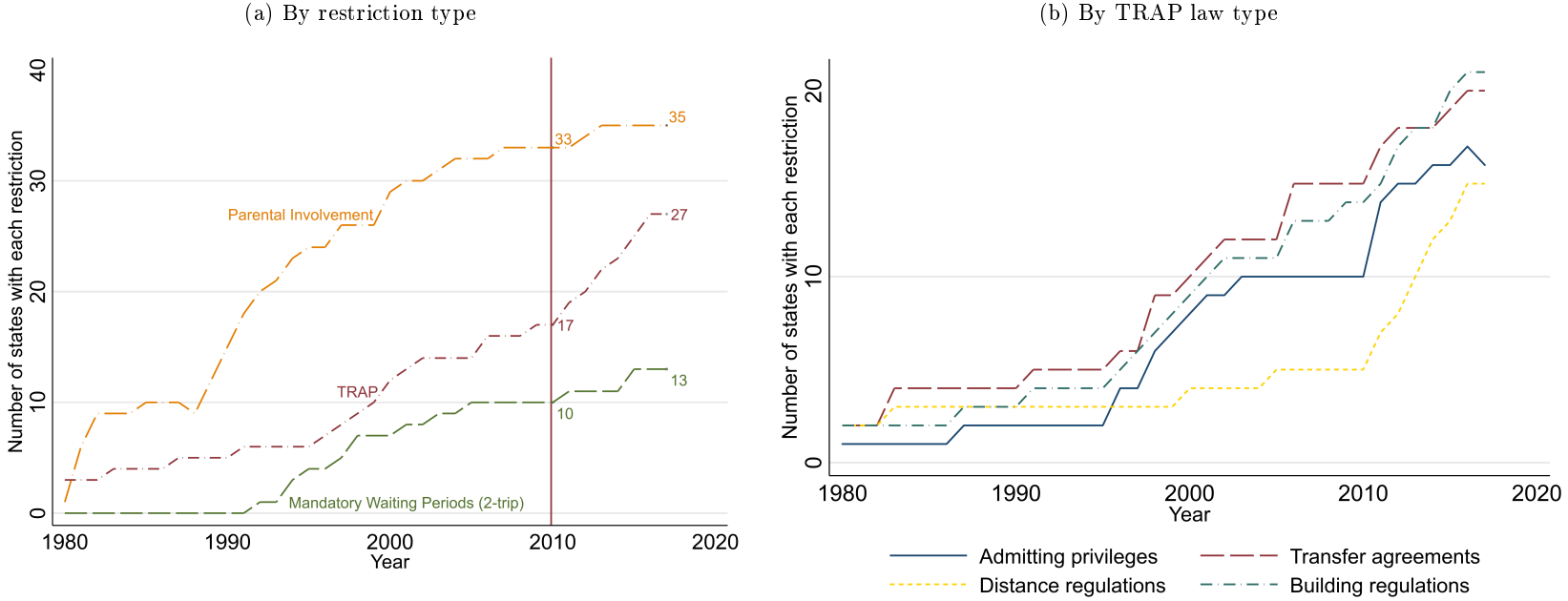
Figures

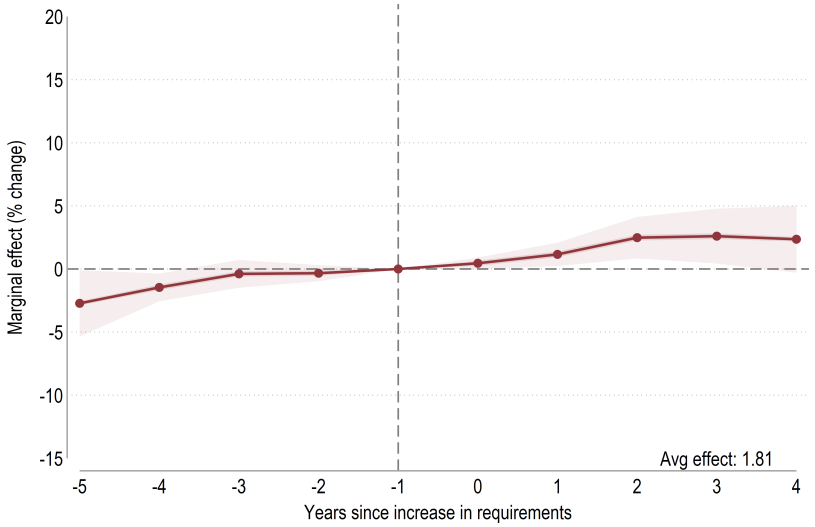
Figure 1: Recent increases in abortion restrictions over time



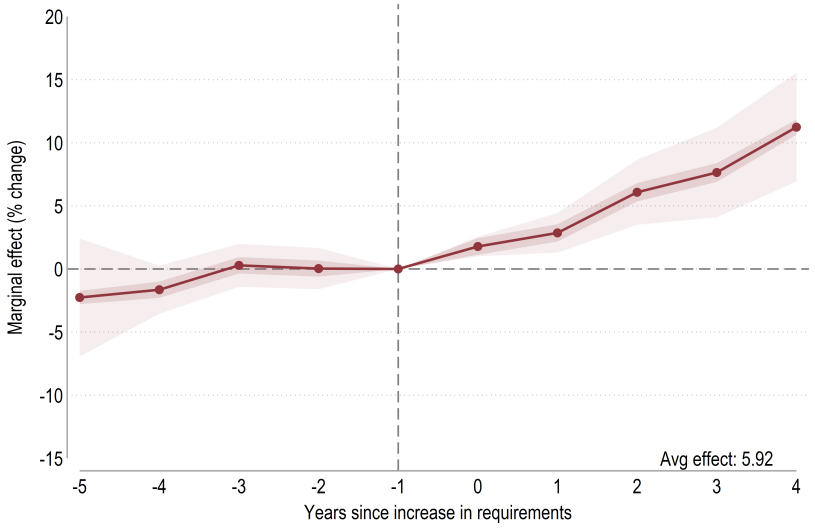
Note: Subfigure (a) compares the evolution of the number of states implementing three of the most common abortion restrictions types from 1980-2017. The vertical line at 2010 indicates the onset of a period where TRAP laws were the fastest growing abortion restriction. Though our data and analysis end in 2017, we note that, by 2021, TRAP laws were more common than parental involvement laws across the U.S. (Guttmacher Institute, 2021c). Other abortion restrictions excluded here are: laws allowing practitioners to refuse to provide abortion, because the refusal happens on a case-by-case basis and the law does not prevent or limit the existence of abortion facilities or providers; gestational limits, because the majority of abortions happen in the first trimester, so these are binding for a minority of women; so-called "partial-birth abortion" bans, because these apply to an abortion method used only after 20 weeks gestation (Kaiser Family Foundation, 2021); public funding bans, because these bans were triggered by the 1976 Hyde Amendment and, therefore, most of these laws have been already enforced for several decades (Salganicoff, Sobel, and Ramaswamy, 2021); state-mandated counseling laws, because they are usually implemented in combination with mandatory waiting periods; and bans of coverage by private insurance, because they have been implemented in few states and most states allow individuals to purchase additional abortion coverage at an additional cost.
Subfigure (b) compares the number of states implementing TRAP laws by type from 1980 to 2017. See section 3.1 for more information on TRAP laws.
Sources: The information on parental involvement laws and mandatory waiting periods comes from Myers and Ladd (2020). The information on TRAP comes from the authors' legal coding data set, described in detail in Appendix H.

Figure 2: Impact of TRAP laws on aggregate births

(a) All women (ages 15 to 44)



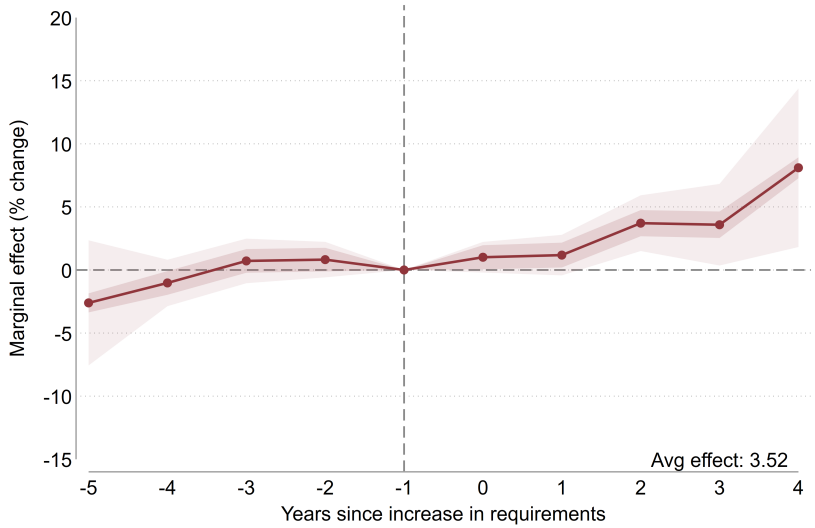
(b) Teens (ages 15 to 19)



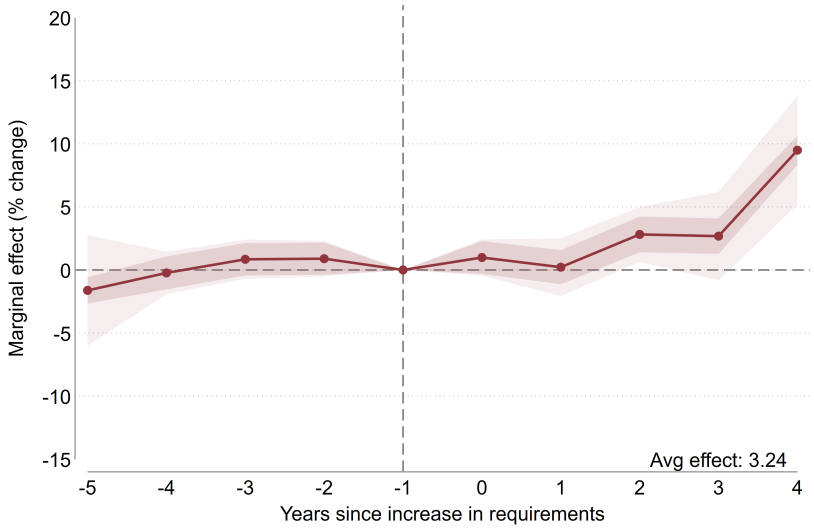
Note: These figures presents the estimates of $100 \times (\exp(\beta_j) - 1)$ from Equation 1. This is the percent change in the number of births of all women and teens, respectively, in state i and year $t + 1$. Year zero indicates the implementation of any TRAP law (admitting privileges, transfer agreement, hospital proximity regulations, and/or building regulations). The omitted year is the year before the policy change, $t = -1$. The standard errors corresponding to the percentage change of the estimates were obtained using the Delta method. The light-shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level; the dark-shaded area indicates the 95 percent confidence interval from standard errors that do not account for within-cluster correlation. The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 1. Sources: Figure created using the information on births recorded by state-age-race/ethnicity group in the U.S. from NCHS (2018), 1990 to 2016, information on population counts from SEER (2018), information on other policies from Myers and Ladd (2020), and the authors' legal coding on TRAP laws, as described in detail in Appendix H.

Figure 3: Impact of TRAP laws on teen births

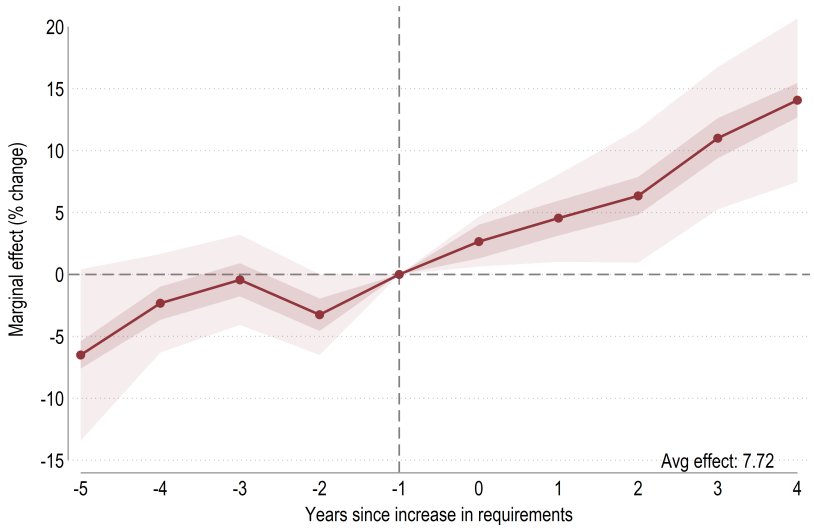
(a) Non-Hispanic White



(b) Non-Hispanic Black

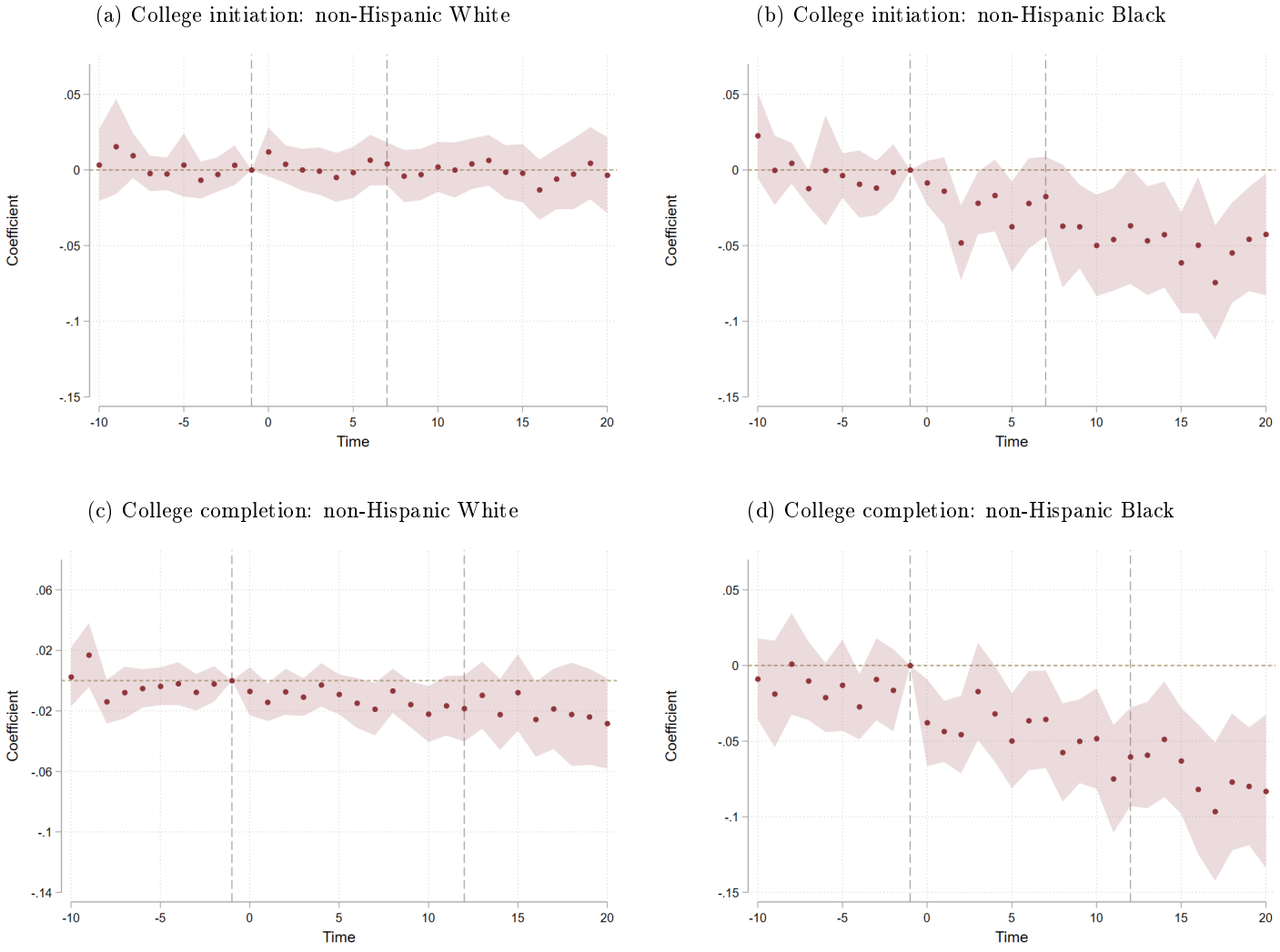


(c) Hispanic



Note: These figures presents the estimates of $100 \times (\exp(\beta_j) - 1)$ from Equation 1. This is the percent change in the number of births of 15 to 19-year-old women of the specified race/ethnicity group in state i and year $t + 1$. Year zero indicates the implementation of any TRAP law (admitting privileges, transfer agreement, hospital proximity regulations, and/or building regulations). The omitted year is the year before the policy change, $t = -1$. The standard errors corresponding to the percentage change of the estimates were obtained using the Delta method. The light-shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level; the dark-shaded area indicates the 95 percent confidence interval from standard errors that do not account for within-cluster correlation. The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 1. Sources: Figure created using the information on births recorded by state-age-race/ethnicity group in the U.S. from NCHS (2018), 1990-2016, information on population counts from SEER (2018), information on other policies from Myers and Ladd (2020), and the authors' legal coding on TRAP laws, as described in detail in Appendix H.

Figure 4: Test for prior common trends in women's education



Notes: This figure plots the β_j coefficients from Equation 3 estimated separately by outcome-group combination. College initiation is measured for ages 18-24; college completion is measured for ages 25-29. The right-hand vertical line marks the year when all women in the age group from the treated state were treated (exposed to the policy before age 18). Shaded areas show 95 percent confidence intervals based on standard errors clustered at the state level. The analysis includes the 27 policy changes shown in Table 1 to be included in the estimation of Equation 3. Source: Own calculations using ACS data from IPUMS (Ruggles et al., 2021), 2000-2019, women born in 1973 or later, and the authors' legal coding of TRAP laws, as described in detail in Appendix H.

Tables

Table 1: Implementation of TRAP laws

		TRAP type(s)				Included in estimation of			
Year	State	Admit	Transfer	Dist	Build	Eqns. 1, 3, & 4	Eqn. 2	Eqn. 5	Reason, if excluded
2013	Alabama	X				X			3
2013	North Dakota	X		X		X			4
2013	Ohio		X			X			4
2013	Texas	X		X		X			4
2013	Virginia				X	X	X		6
2012	Maryland		X		X	X	X		6
2012	Tennessee	X				X			3, 4
2012	Pennsylvania				X	X	X		6
2012	Arizona			X		X			5
2011	Indiana	X							1
2011	Kansas	X	X	X		X	X	X	
2011	Missouri				X	X			3
2011	North Dakota	X							1
2011	Utah	E	E	X	X	X			5
2009	Texas				X	X			4
2006	Indiana		X		X	X			4
2006	Ohio		X			X	X	X	
2006	South Dakota		X		X	X	X	X	
2005	Missouri			X		X	X	X	
2003	Alabama	X							1
2002	Rhode Island		X		X	X	X	X	
2001	Nebraska	E	E		X	X	X	X	
2000	Arizona	X			X	X			5
2000	Michigan		X	X		X	X	X	
1999	Arkansas				X	X	X	X	
1999	Pennsylvania	E	E						2
1998	Kentucky		X			X			5
1998	Oklahoma	1	X		X	X			5
1998	Texas	1	X			X			5
1997	Alabama				X	X	X	X	
1996	Mississippi	X							1
1996	South Carolina	E	E		X	X	X	X	
1994	North Carolina				X				1

Notes: Policies are excluded from all analyses if they are (1) below minimum stringency, or (2) a decrease in stringency. Policies are additionally excluded from both stacked DD analyses (Eqns. 7 and 8) if they are (3) blocked within 5 years, (4) have an adjacent TRAP within 5 years, or (5) have an adjacent PI or MWP within 5 years. Policies are additionally excluded from the ACS stacked DD (Eqn. 8) if (6) they are too recent to be evaluated in the ACS. E indicates that the law specifies a clinic can either comply with the admitting privileges requirement or the transfer agreement requirement.

Table 2: Impact of TRAP laws on women's education

	Non-Hispanic White			Non-Hispanic Black		
	High school completion	College initiation	College completion	High school completion	College initiation	College completion
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Full sample						
Exposure before 20	0.000	-0.001	-0.007**	-0.001	-0.010**	-0.005
	(0.001)	(0.003)	(0.003)	(0.004)	(0.005)	(0.004)
Obs	2,559,720	2,559,720	2,559,720	413,162	413,162	413,162
DV mean	0.948	0.690	0.421	0.901	0.566	0.240
Panel B. Full sample						
Exposure before 18	0.002	0.000	-0.007*	-0.004	-0.012**	-0.014***
	(0.002)	(0.003)	(0.003)	(0.004)	(0.006)	(0.005)
Obs	2,559,720	2,559,720	2,559,720	413,162	413,162	413,162
DV mean	0.948	0.690	0.421	0.901	0.566	0.240
Panel C. Women residing in state of birth						
Exposure before age 18	0.003	0.000	-0.006	-0.003	-0.015***	-0.015**
	(0.002)	(0.003)	(0.004)	(0.005)	(0.005)	(0.007)
Obs	1,622,719	1,622,719	1,622,719	296,228	296,228	296,228
DV mean	0.942	0.658	0.377	0.889	0.528	0.203
Panel D. Women born in states with higher race-specific poverty						
Exposure before 18	-0.001	-0.004	-0.008*	-0.008*	-0.017***	-0.012**
	(0.003)	(0.005)	(0.003)	(0.004)	(0.005)	(0.005)
	517,312	517,312	517,312	195,195	195,195	195,195
	0.926	0.633	0.349	0.893	0.552	0.223
Panel E. Women born in states with lower race-specific poverty						
Exposure before 18	0.001	0.002	-0.003	-0.003	-0.003	-0.006
	(0.002)	(0.004)	(0.004)	(0.008)	(0.008)	(0.005)
Obs	2,042,408	2,042,408	2,042,408	217,967	217,967	217,967
DV mean	0.954	0.705	0.440	0.909	0.579	0.256

Notes: Estimates of β in Equation 2. Each coefficient comes from a separate estimation. High school completion indicates being a high school graduate or having passed the GED or more. College initiation indicates completing at least one year of college or more. College completion refers to completing at least a bachelor's degree. High Black poverty indicates above the median level of Black women living in poverty (27.7 percent). High White poverty indicates above the 75th percentile level of White women living in poverty (14.0 percent). The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 2. The estimations are weighted by the person weight provided in ACS-IPUMS. The standard errors are clustered at the state of birth level. $^{*}p < 0.1$,$^{**}p < .05$, $^{***}p < .01$. Sources: ACS data from IPUMS (Ruggles et al., 2021), 2000-2019, women aged 25+ born in 1973 or later; information on other policies from Myers and Ladd (2020), and authors' legal coding on TRAP laws as described in detail in section H.

Table 3: Impact of TRAP laws on abortion access and use

		β	
Panel A. Distance to nearest abortion provider	Mean	Unweighted	Weighted
Geodesic distance (miles)	59.81	8.027***	3.773
		(2.545)	(3.073)
Travel distance (miles)	75.38	9.058***	4.512
		(3.097)	(3.813)
Travel time (minutes)	85.11	8.625***	4.484
		(2.969)	(4.141)

Panel B. Abortion use	(1)	(2)
<u>CDC data</u>		
All women	-4.329	-4.227*
	(2.698)	(2.457)
Women 15-19	-6.045*	-4.614*
	(3.534)	(2.707)
<u>Guttmacher Institute data</u>		
All women	-4.269	-4.198
	(3.774)	(2.626)
Controls PI & MWP	No	Yes
Controls other related policies	No	Yes

Notes: Panel A shows the estimates of β_3 from equation 4, which corresponds to an indicator for an observation from the year 2017, from state s that turned on a new TRAP law between 2010 and 2016. Geodesic distance, travel distance, and travel time represent the distances in miles and travel times in minutes, respectively, from a county to the nearest abortion provider. Weighted estimates are weighted by the county's average population of women of reproductive age.

Panel B shows the estimates of γ^* from equation 5, where $\gamma^* = 100 \times (\exp(\gamma) - 1)$ and represents the percent change in abortion use resulting from a TRAP law. Each coefficient is from a separate estimation, where the sample is shown in the row header. The estimations in this panel include state and year fixed effects. The second column additionally controls for the implementation of parental involvement laws and two-trip mandatory waiting periods and other abortion, contraception, and welfare policies. See section 4.1 for more information on these policies. The standard errors are clustered at the state level. $^{*}p < 0.1, ^{**}p < .05, ^{***}p < .01$.

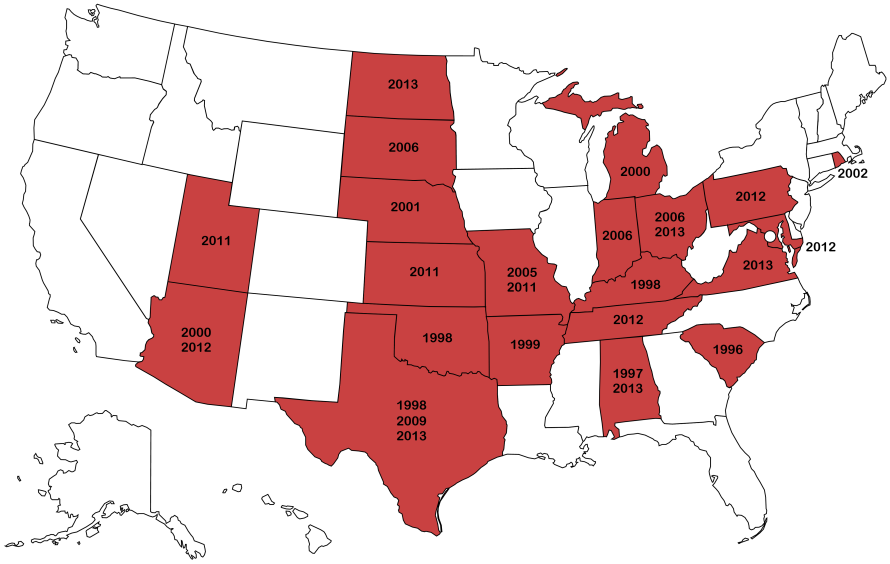
Sources: The information on distance and travel comes from the Myers Abortion Facility Database (Myers, 2020). Caitlin Myers also provided us with information on abortion counts, which was compiled from the CDC (Kortsmit, 2020). Information on abortion counts also comes from Guttmacher Institute (2021a). Information on other policies comes from Myers and Ladd (2020). The authors' legal coding on TRAP laws is described in detail in Appendix H.

Appendix

For Online Publication

A Additional Descriptives

Figure A.1: TRAP laws included in any of the analyses



Note: This map shows the TRAP laws included in our main analyses by state and year of implementation. These TRAP laws correspond to those listed in Table 1 as included in the estimation of Equations 1, 2, and 3. Source: TRAP laws legal coding, described in detail in Appendix H.

Table A.1: Age at first TRAP exposure

Age at first exposure	Non-Hispanic White	Non-Hispanic Black
15 or younger	0.146	0.1888
15 to 19	0.105	0.1158
20 to 24	0.119	0.1267
25 to 34	0.162	0.1669
35 or older	0.062	0.0878
Never	0.406	0.314

Notes: This table shows the distribution of women by age at first exposure to a TRAP law, by race. TRAP laws included are the 27 policy changes shown in Table 1 to be included in estimation of Equation 2. Source: Own calculations using ACS data from IPUMS (Ruggles et al., 2021), 2000 to 2019, including women born in 1973 or later, and the authors' legal coding of TRAP laws, as described in detail in Appendix H.

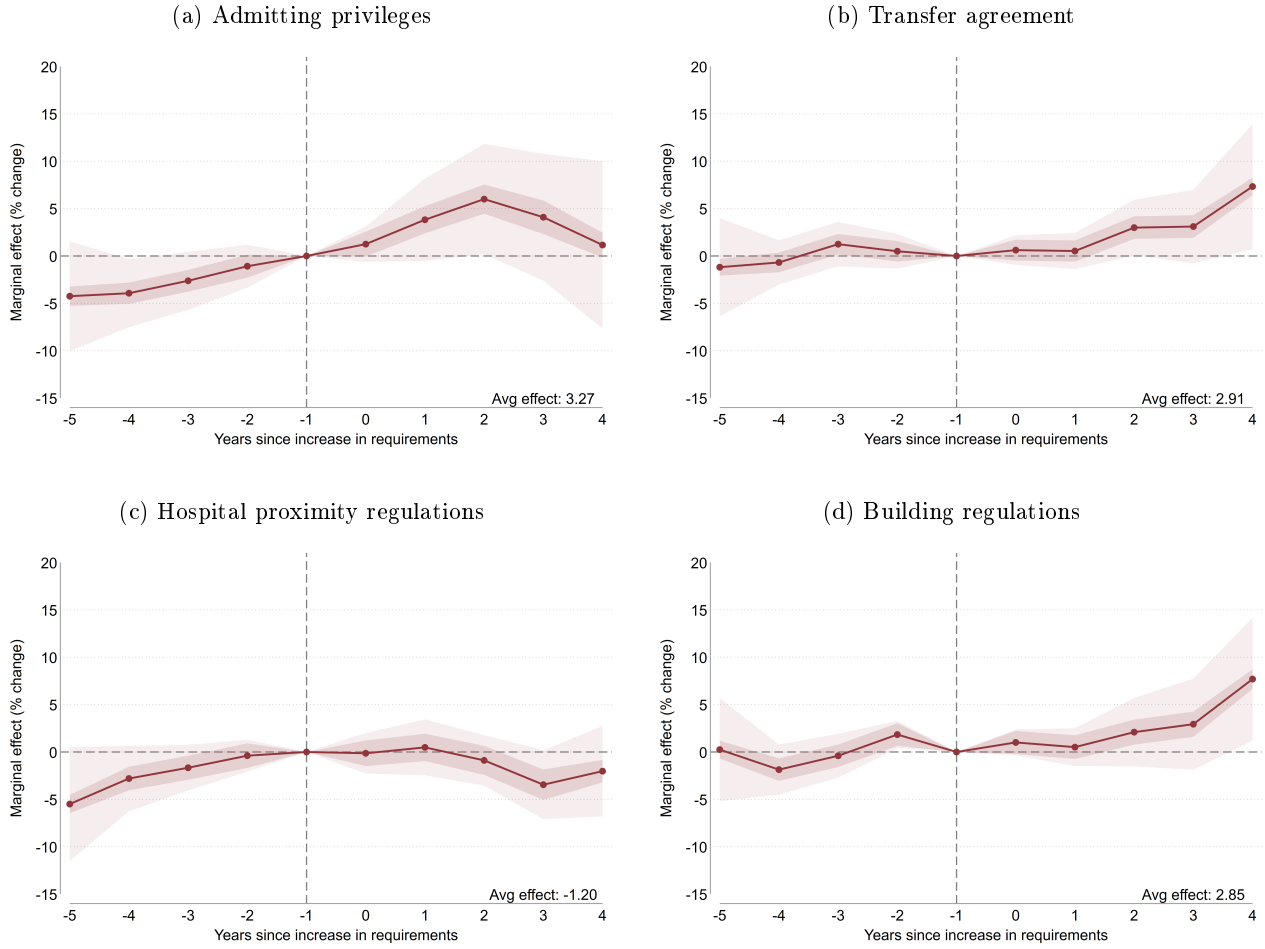
Table A.2: TRAP laws excluded from analyses

		Admit	Transfer	Hosp	Build
Pre-*Casey*					
1991	Mississippi		X		X
1987	Missouri	X			X
1983	Alaska		X		
1983	Pennsylvania		X	X	X
1976	North Carolina	E	E	X	X
1976	Wisconsin		X	X	
1974	Connecticut				X
1973	Idaho		X		
Too recent to evaluate					
2016	Florida	E	E	X	
2016	Illinois	X		X	X
2015	Louisiana				X
2015	North Carolina		X		
2015	Ohio			X	
2015	South Dakota				X
2015	Tennessee		X		X
2014	Louisiana	X		X	
2014	Oklahoma	X		X	
Applies to 2nd trimester providers only					
2017	Utah		X		
2013	Georgia	E	E		X
2012	Virginia		X		
2010	Utah	E	E		
2006	Florida				X
2005	Mississippi		X		X
1999	Indiana				X
1993	Indiana		X		X
1985	Utah	X	X	X	X
1976	South Carolina		X		
1973	Rhode Island	X	X		

Notes: *Admit* stands for admitting privileges, *Transfer* stands for transfer agreements, *Hosp* stands for hospital proximity regulations, and *Build* stands for building regulations. E indicates that the law specifies a clinic can either comply with the admitting privileges requirement or the transfer agreement requirement.

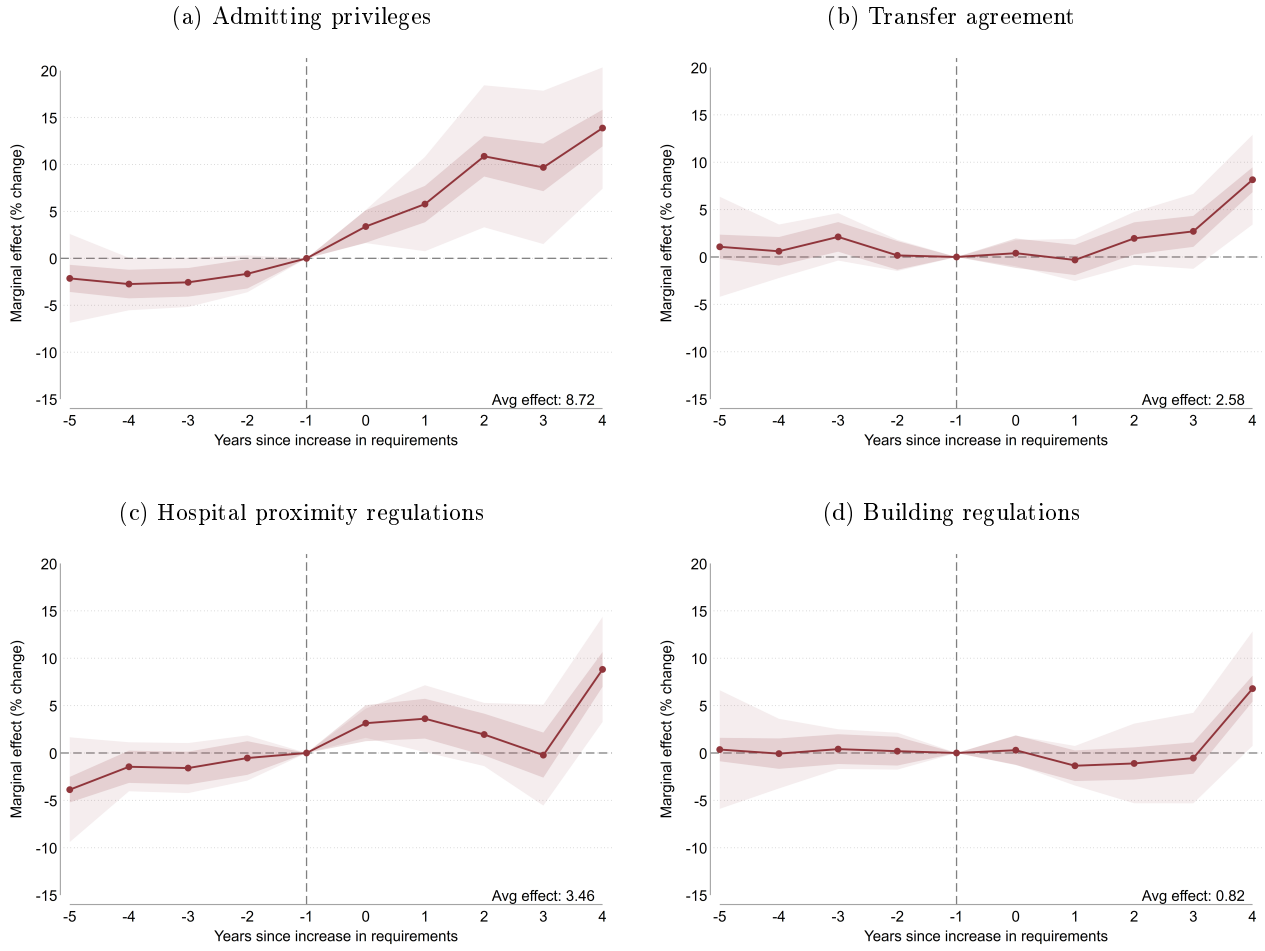
B Impacts by TRAP law type

Figure B.1: Impact of TRAP laws on teen births, by law type: Non-Hispanic White



Note: The figures present the estimates of $100 \times (\exp(\beta_j) - 1)$ from Equation 1. This is the percent change in the number of births to 15 to 19-year-old Non-Hispanic White women in state i and year $t + 1$. In each figure, year zero indicates the addition of a requirement, as follows: (a) that one or more staff members of a clinic providing abortion must have hospital admitting privileges, (b) a written transfer agreement or a plan/protocol for hospital transfer, (c) hospital proximity regulations, and (d) building regulations. The omitted year is the year before the policy change, $t = -1$. The standard errors corresponding to the percentage change of the estimates were obtained using the Delta method. The light-shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level; the dark-shaded area indicates the 95 percent confidence interval from standard errors that do not account for within-cluster correlation. The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 1. Sources: Figure created using the information on births recorded by state-age-race group in the U.S. from NCHS (2018), 1990-2016, information on population counts from SEER (2018), information on other policies from Myers and Ladd (2020), and the authors' legal coding of TRAP laws, as described in detail in Appendix H.

Figure B.2: Impact of TRAP laws on teen births, by law type: Non-Hispanic Black



Note: The figures present the estimates of $100 \times (\exp(\beta_j) - 1)$ from Equation 1. This is the percent change in the number of births to 15 to 19-year-old Non-Hispanic Black women in state i and year $t + 1$. In each figure, year zero indicates the addition of a requirement, as follows: (a) that one or more staff members of a clinic providing abortion must have hospital admitting privileges, (b) a written transfer agreement or a plan/protocol for hospital transfer, (c) hospital proximity regulations, and (d) building regulations. The omitted year is the year before the policy change, $t = -1$. The standard errors corresponding to the percentage change of the estimates were obtained using the Delta method. The light-shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level; the dark-shaded area indicates the 95 percent confidence interval from standard errors that do not account for within-cluster correlation. The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 1. Sources: Figure created using the information on births recorded by state-age-race group in the U.S. from NCHS (2018), information on population counts from SEER (2018), information on other policies from Myers and Ladd (2020), and the authors' legal coding of TRAP laws, as described in detail in Appendix H.

Table B.1: Impact of TRAP laws on women's education, by law type

	Non-Hispanic White			Non-Hispanic Black		
	High school completion	College initiation	College completion	High school completion	College initiation	College completion
Panel A. Admitting privileges						
Exposure before age 18	0.002	0.001	-0.005	-0.018***	-0.012**	-0.004
	(0.003)	(0.003)	(0.004)	(0.002)	(0.006)	(0.005)
Panel B. Transfer agreement						
Exposure before age 18	-0.001	0.001	-0.003	0.001	-0.007	-0.008
	(0.002)	(0.004)	(0.004)	(0.006)	(0.008)	(0.007)
Panel C. Hospital proximity regulations						
Exposure before age 18	0.000	-0.002	-0.010***	-0.007	-0.017***	-0.032***
	(0.002)	(0.002)	(0.002)	(0.005)	(0.004)	(0.006)
Panel D. Building regulations						
Exposure before age 18	0.003	-0.006	-0.011**	-0.015***	-0.018***	-0.009***
	(0.002)	(0.005)	(0.005)	(0.003)	(0.005)	(0.003)

Notes: Estimates of β in Equation 2 where *exp* is for a specific type of TRAP law as shown in the row header. High school completion indicates being a high school graduate or having passed the GED. College initiation indicates completing at least one year of college or more. College completion refers to completing at least a bachelor's degree. Each coefficient comes from a separate estimation. The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 2. The estimations are weighted by the person weight provided in ACS-IPUMS. The standard errors are clustered at the state of birth level. $^{*}p < 0.1, ^{**}p < .05$, $^{***}p < .01$. Sources: ACS data from IPUMS (Ruggles et al., 2021), 2000-2019, women aged 25+ born in 1973 or later; information on other policies from Myers and Ladd (2020). The authors' legal coding on TRAP laws is described in detail in section H.

C Impacts by TRAP law severity

In the event study methodology presented in Section 4.2, d_{it} takes the value 1 in the year the policy turned on, e_i, and zero otherwise. This treats TRAP laws as binary. This differs from reality in two ways: (1) a state may have more than one policy change in a narrow band of time, and (2) even within policy type, policies vary in intensity. In this Section we explore the impact of TRAP law accumulation and severity.

We follow the procedure proposed by Schmidheiny and Siegloch (2019) to modify the event study design to allow for varying treatment intensity and multiple sequential changes in treatment. In this case, d_{it} is no longer a binary variable, rather, it indicates the change in intensity of the law in state i in year t.[47] To accommodate this change we employ the suggested reformulation of b_{it}^{j}, that is,

$$b_{it}^{j} = \begin{cases} \sum_{s=t-\underline{j}}^{\bar{t}-\underline{j}-1} d_{is} & \text{if } j = \underline{j} \\ d_{i,t-j} & \text{if} \underline{j} < j < \bar{j} \\ \sum_{s=\underline{t}-\bar{j}+1}^{t-\bar{j}} d_{is} & \text{if } j = \bar{j} \end{cases} \tag{6}$$

This definition of b_{it}^{j} is equivalent to that presented in Section 4.2, except that it accommodates non-binary values of d_{it}. As before, the treatment indicator is binned at the endpoints of the effect window, summing the d indicators over the years extending beyond the effect window in each direction, respectively.

The number of TRAP laws takes the values $d_{it}^{NumTRAP} \in [0, 4]$, corresponding to the four categories of TRAP laws included in our analysis. Admitting privileges laws have an intensity of $d_{it}^{admit} \in [0, 6]$. Levels 1 and 2 indicate that a clinic is required to have an agreement with an external physician who has hospital admitting privileges, either with (1) or without (2) possible exceptions. An exception would be that a clinic can either meet this requirement or meet some level of a transfer agreement requirement. Levels 3 and 4 indicate that at least one clinic physician must have privileges (again, with or without exception). Levels 5 and 6 require that all clinic physicians have privileges. Transfer agreement laws have an intensity of $d_{it}^{Transfer} \in [0, 4]$. Levels 1 or 2 require the clinic to have a plan or protocol for transferring patients to hospitals. Levels 3 and 4 require the clinic to have a formal transfer agreement with a hospital. Building regulations have an intensity $d_{it}^{Build} \in [0, 10]$, indicating the number of building regulations in force (see Section 3.1 for a list). Hospital proximity regulations are excluded from this analysis because there is no variation in their intensity.

Multiple policy changes within a state are captured by the increase (or decrease) in intensity

[47] This modification is the reason for the reformulation of the definition of b_{it}^{j} in Equation 6, which also follows Schmidheiny and Siegloch (2019). In the standard case is mathematically equivalent to the more common definition:

$$b_{it}^{j} = \begin{cases} \mathbb{1}[t \leq e_i + j] & \text{if } j = \underline{j} \\ \mathbb{1}[t = e_i + j\} & \text{if } \underline{j} < j < \bar{j} \\ \mathbb{1}[t \geq e_i + j] & \text{if } j = \bar{j} \end{cases}$$

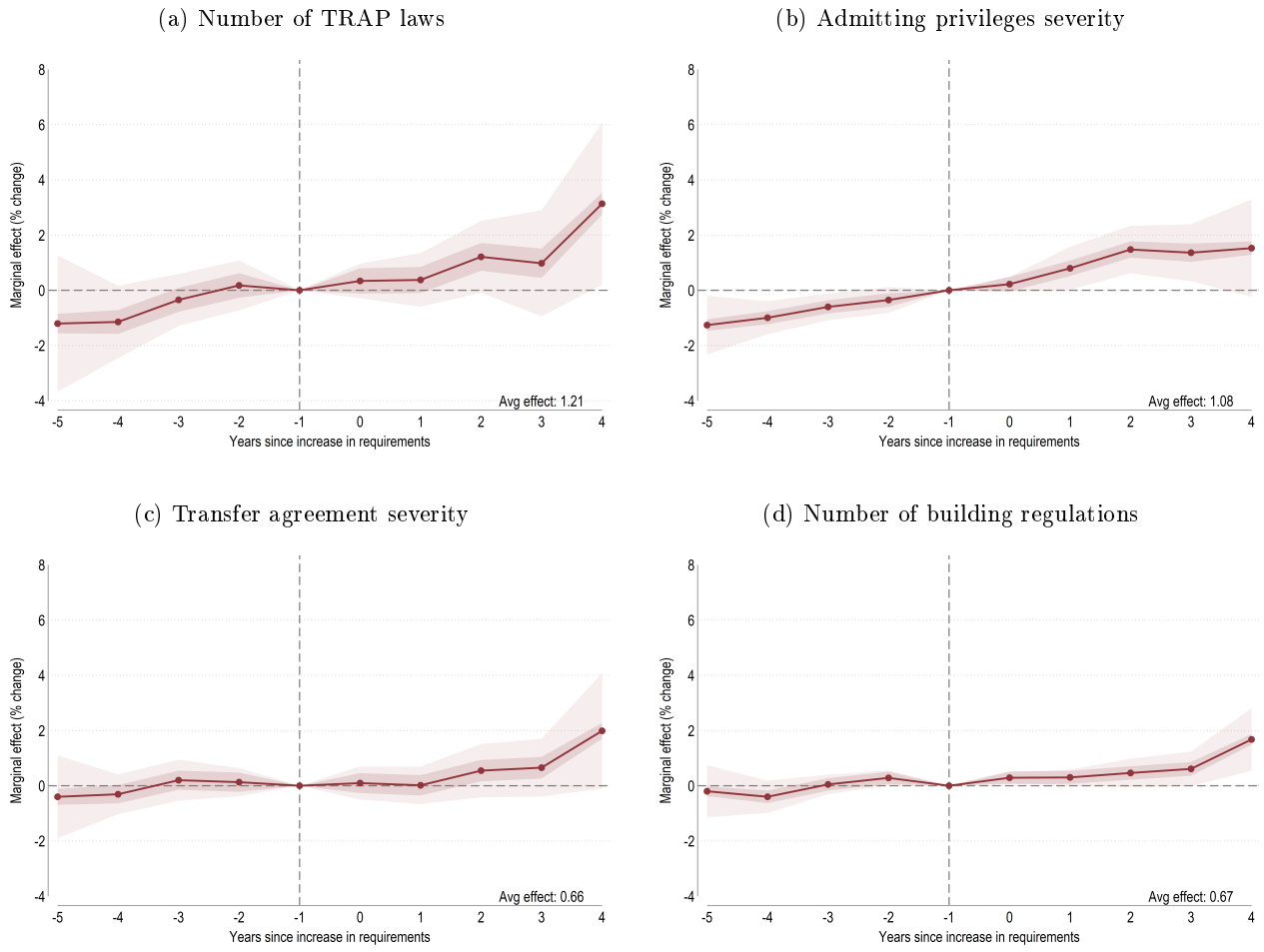
However, the reformulated definition can accommodate continuous or multi-valued treatments.

of the succeeding policy, relative to the former policy. For example, in North Dakota an admitting privileges law of level 2 turns on in 2011, and an additional admitting privileges law of level 6 turns on in 2013. In this case, (a selected set of) the values of b_{it}^{j} would be...

	b_{it}^{-5}	b_{it}^{-4}	$b_{it}^{0} = d_{it}$
	5 or more years before	exactly 4 years before	the year of
2005	6	0	0
2006	6	0	0
2007	4	2	0
2008	4	0	0
2009	0	4	0
2010	0	0	0
2011	0	0	2
2012	0	0	0
2013	0	0	4
2014	0	0	0
2015	0	0	0

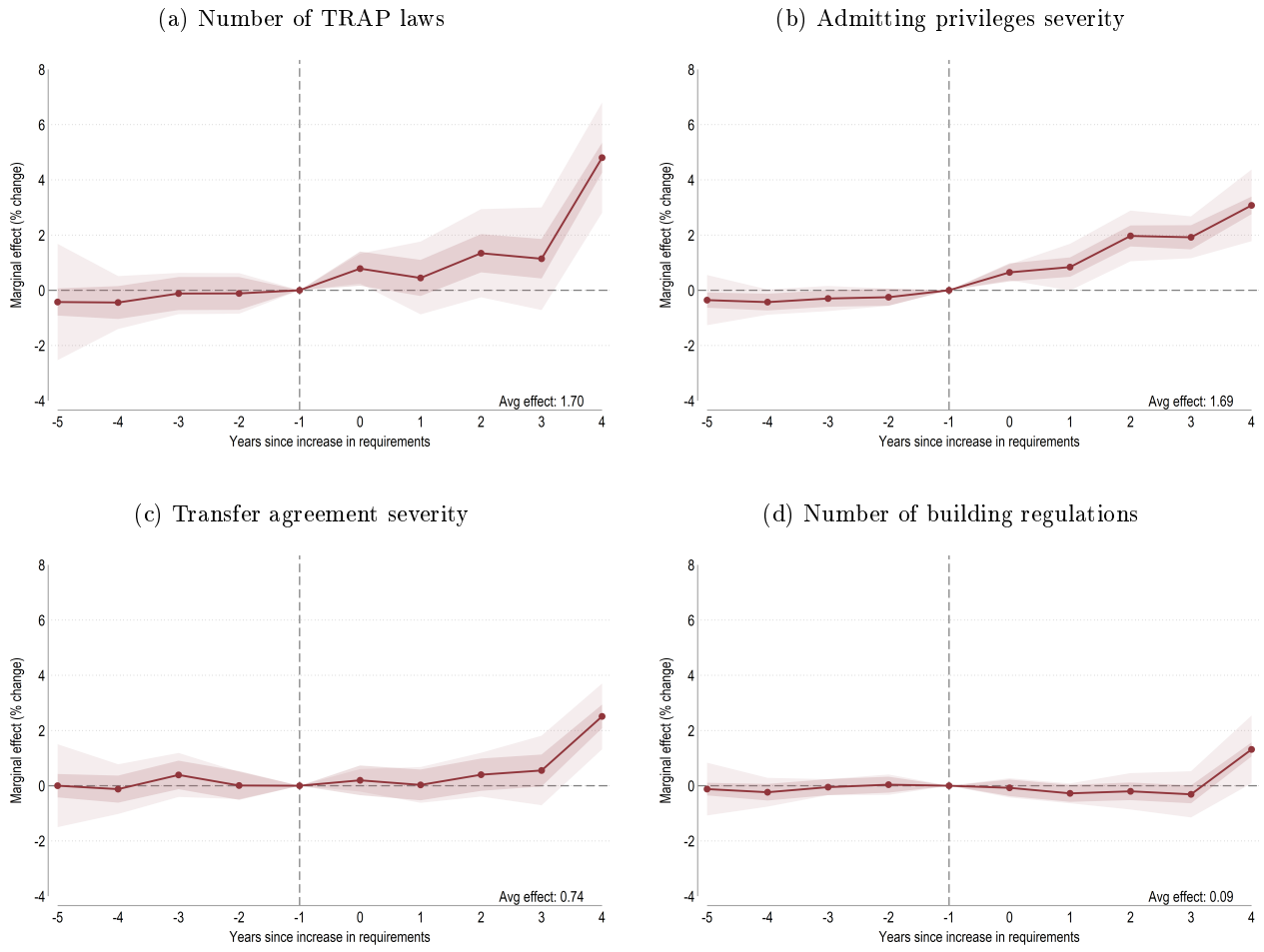
Other than the changes in d_{it} and the calculation of b_{it}, the analysis is identical to that presented in Section 4.2. The results are shown in Figure C.1. In these analyses, the event represents a one-unit change in d_{it}, for example, an increase in one TRAP law type, or an increase in admitting privileges severity from level 2 to level 3. We expect the estimated effects to be smaller than those estimated in Section 4.2, which capture binary changes from level 0 to level 1 but also binary changes from level 0 to level 6, for example.

Figure C.1: Impacts of TRAP laws on teen births, by severity: Non-Hispanic White



Note: This figure presents the estimates of $100 \times (\exp(\beta_j) - 1)$ from Equation 1. This is the percent change in the number of births to 15-19-year-old Non-Hispanic White women living in state i in year $t+1$ for each 1-unit increase in the intensity of the regulation, as defined in Appendix C. Year zero indicates the implementation of the corresponding change in severity. The omitted year is the year before the policy change, $t = -1$. The standard errors corresponding to the percentage change of the estimates were obtained using the Delta method. The light-shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level; the dark-shaded area indicates the 95 percent confidence interval from standard errors that do not account for within-cluster correlation. The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 1. Sources: Figure created using the information on births recorded in the U.S. from NCHS (2018), 1990-2016, information on population counts from SEER (2018), information on other policies from Myers and Ladd (2020), and the legal coding on TRAP laws, as described in detail in Appendix H.

Figure C.2: Impacts of TRAP laws on teen births, by severity: Non-Hispanic Black



Note: This figure presents the estimates of $100 \times (\exp(\beta_j) - 1)$ from Equation 1. This is the percent change in the number of births of 15-19-year-old Non-Hispanic Black women living in state i in year $t + 1$ for each 1-unit increase in the intensity of the regulation, as defined in Appendix C. Year zero indicates the implementation of the corresponding change in severity. Year zero indicates the implementation of a the corresponding TRAP law to the figure. The omitted year is the year before the policy change, $t = -1$. The standard errors corresponding to the percentage change of the estimates were obtained using the Delta method. The light-shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level; the dark-shaded area indicates the 95 percent confidence interval from standard errors that do not account for within-cluster correlation. The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 1. Sources: Figure created using the information on births recorded in the U.S. from NCHS (2018), 1990-2016, information on population counts from SEER (2018), information on other policies from Myers and Ladd (2020), and the legal coding on TRAP laws, as described in detail in Appendix H.

D Robustness to Stacked DID estimation

D.1 Impacts on teen births

We test whether the results presented in Section 4.2 are biased by heterogeneous effects across units, as proposed by Sun and Abraham (2020) and Borusyak, Jaravel, and Spiess (2021). We implement a stacked difference-in-differences design Baker, Larcker, and Wang (2022).[48]

For each policy change, we construct an event-specific data set containing only the treated state and selected comparison states, creating a symmetric panel of j years before the policy, the year the policy was implemented, and $j-1$ years after, $j \in \{4, 5, 6\}$. We follow the recommendation of Callaway and Sant'Anna (2021) to include in the comparison group those states that have never implemented a TRAP law, including during the pre-*Casey* era (never-treated).[49] We supplement this group by also including those that first implemented a TRAP law after the end of the included time window (future-treated). We construct an indicator for whether the year was before or after the relevant policy change and then stack the various data sets. Our approach is similar to Cengiz et al. (2019), who estimate the effects of minimum wages on low-wage jobs and Hamersma and Maclean (2021) who estimate the impact of children's public health insurance programs on provider behavior for substance abuse treatment. Both use an event-study analysis and check robustness using a stacked DD approach.

Table 1 shows the TRAP laws we are able to explore using this methodology. For some policy changes, we cannot estimate the impact using this methodology as the state enacted another TRAP law or other major abortion regulation within the period of years examined. For example, Texas enacted building regulations in 2009 and HB2 (admitting privileges and hospital proximity regulations) in 2013. These policy changes are within four years of each other and therefore must be excluded when $j \geq 4$. As such, this method demonstrates robustness to any concerns relating to states implementing multiple policy changes sequentially. In this analysis, only relative isolated changes are considered. We also note that these estimations also document the robustness of our findings to excluding Texas HB2, which was a far-reaching and impactful law.

Using the stacked data, we estimate the equation:

$$E\left[y_{i,t+1,d}|b_{it}, \nu_{id}, \nu_{td}\right] = exp\left(\beta b_{it} + 1ln(pop_{i,t+1,d}) + \nu_{id} + \nu_{td} + \varepsilon_{itd}\right) \tag{7}$$

where the subscript d indicates the policy change data set. We estimate Equation 7 using a Poisson

[48] Several articles have proposed estimators for staggered treatment timing settings, allowing for a more sensible aggregation of heterogeneous treatment effects (Baker, Larcker, and Wang, 2022; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020a; Sun and Abraham, 2020). The estimator proposed by Callaway and Sant'Anna (2021) is one of the most widely used, however, this estimator assumes a linear model, which prevents us from using it in our non-linear setting. Instead, we rely on a stacked DD method that provides more flexibility on the assumed model. The difference with other approaches is that the weights are determined by the number of treated units and treatment variance within each stacked event rather than by economic considerations (Roth et al., 2022).

[49] Never treated units are preferred as long as there is a sizable group of units that do not participate in the treatment in any period, and, at the same time, these units are similar enough to the "eventually" treated units (Callaway and Sant'Anna, 2021). In Table D.1, we demonstrate that our estimates are robust to the selection of different time windows and the comparison with either never-treated and future-treated or never-treated only.

regression where $y_{i,t+1,d}$ is the number of teen births in state i from data set d in period $t+1$, and we control for the population of women aged 15 to 19 in state i in year $t+1$, pop_{it+1}, and constrain the coefficient on this control to be unity. We also control for state-by-data set fixed effects, ν_{id}, and year-by-data set fixed effects, ν_{td}. We exclude controls for other policies from Equation 1 to avoid contamination issues, since these other policies may potentially represent other treatments. de Chaisemartin and D'Haultfœuille (2020b) point out that regressions with several treatments may be contaminated by the effect of other treatments, an issue that is not present in a regression with one treatment.

We note that the purpose of this exercise is to estimate the magnitude of the treatment effect using a method that is robust to various concerns. We compare the magnitudes of these alternative estimates to those presented in Section 4.2 to assess whether the primary estimators are biased. In particular, our concern is whether the primary estimators are upward-biased, potentially indicating that TRAP laws have an impact when they in fact do not.

Table D.1 presents estimates of $\beta^* = 100 \times (\exp(\hat{\beta}) - 1)$ from Equation 7, interpreted as percent changes in births. Estimates are presented for $j \in \{4, 5, 6\}$, where $r = 5$ is the estimation most comparable to the findings from Equation 1. These results are presented from estimations using only never-treated states as controls (NT), or using both never-treated and future-treated states as controls (NT+FT). Overall, the estimated percent increases in teen births are larger than those estimated by Equation 1. Increases for non-Hispanic White teens range from 5.0 to 7.9 percent and increases for non-Hispanic Black teens range from 3.7 to 7.6 percent. This suggests that the average of the estimates in Figure 3 of 3.2 to 3.5 percent are not upward-biased by treatment effect heterogeneity or controlling for multiple treatments, nor are they driven by Texas HB2.

D.2 Impacts on women's educational attainment

We test whether the results from the two-way fixed effects estimation presented in Section 5.2 may be biased by heterogeneous effects over time or across units, as recently proposed by several studies (de Chaisemartin and D'Haultfœuille, 2020a; Athey and Imbens, 2021; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). We implement a stacked difference-in-differences design Baker, Larcker, and Wang (2022).

For each policy change, we construct an event-specific data set containing only the treated state and selected comparison states, including 5 treated cohorts (aged 13 to 17 in the year of policy onset) and 5 control cohorts (aged 18 to 22 in that year). As in Section 4.4, we present estimations where the comparison group is only those states that have never implemented a TRAP law (even during the pre-*Casey* period; never-treated) and separate estimations that additionally include comparison states that first implemented a TRAP law after the last cohort in the treated group turned 18 (future-treated). We construct data set-specific treatment indicators by state and cohort then stack the various data sets.

Table 1 shows the TRAP laws we are able to explore using this methodology, which span the years 1993 to 2011. For policy changes after 2011, there is no variation in our birth cohorts

in exposure before age 18. For some policy changes, we cannot estimate the impact using this methodology as the state enacted another TRAP law or other major abortion regulation within five years of the policy examined. Table A.2 contains further information on the excluded policies. As with the analysis presented in section 4.4, this analysis demonstrates robustness to treatment effect heterogeneity over time, multiple sequential treatments, and the exclusion of Texas HB2.

We estimate the equation:

$$y_{ibsad} = \beta exp_{bs} + \nu_b + \nu_s + \nu_a + \nu_d + \varepsilon_{ibsad} \tag{8}$$

where the variables represent the same as in Equation 2, with the addition of data set fixed effects (ν_d). We employ the same sample restrictions as described in Section 5.1. Estimations are weighted using data-provided person weights to be nationally representative. Standard errors are clustered at the state level.

Results are presented in Table D.2, Panel A using never-treated controls and Panel B using never and future treated controls. We find that all effects that are estimated to be significantly different from zero by Equation 2 are robust to estimation using Equation 8. In fact, the coefficients from Equation 8 are larger in magnitude and more precisely estimated. Estimated effects are 2.3 percent for college completion of White women, 3.3 percent for college initiation of Black women, and 5.6 percent for college completion of Black women, all significant the 1 percent level.

Finally, we present a falsification test. If the estimated impacts on education are truly driven by reduced access to abortion, we should find that the impacts are substantially greater for women's education than for men's education. We do not necessarily expect a zero effect on men, as the education of young men may also be interrupted by early fatherhood, however, we do expect the potential interruption to be greater for young women. On average, mothers dedicate more time to parenting activities than fathers, and women are more likely to experience early parenthood than men.[50] If we find equivalent effects on men's and women's education, this would indicate the influence of confounding factors.

We estimate Equation 2 for men, separately for White non-Hispanic and Black non-Hispanic. The results are presented in Appendix Table G.3. The estimated effects on men's education are less than half the size of the effects on women's education and are not statistically different from zero.

[50]Based on the Vital Statistics data used in this study, children in the US are four times more likely to have a mother under age 18 at birth (2%) than to have a father under age 18 at birth (0.5%).

Table D.1: Impact of TRAP laws on teen births: Stacked DD

	Non-Hispanic White			Non-Hispanic Black		
	$j = 6$	$j = 5$	$j = 4$	$j = 6$	$j = 5$	$j = 4$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Never treated controls						
TRAP law	7.921***	6.26***	5.025***	7.551***	5.47***	4.223***
	(0.295)	(0.284)	(0.312)	(0.380)	(0.384)	(0.423)
Obs	2,790	2,880	2,304	2,790	2,880	2,304
Panel B. Never+Future treated controls						
TRAP law	7.535***	5.98***	5.384***	6.346***	4.38***	3.679***
	(0.284)	(0.268)	(0.291)	(0.363)	(0.357)	(0.388)
Obs	3,425	3,530	2,880	3,425	3530	2880

Notes: Estimates of $100 \times (\exp(\beta) - 1)$ from Equation 7. This is the percent change in the number of births of 15 to 19-year-old women of the given race/ethnicity group in state s and year $t + 1$. Each coefficient is from a separate regression. "Future treated states" are states that first implemented an abortion restriction more than five years after t. j indicates the number of pre-periods included in the analysis. The analysis includes the 13 policy changes shown in Table 1 to be included in estimation of Equation 7. $^{*}p < 0.1$,$^{**}p < .05$, $^{***}p < .01$. Sources: Table created using the information on births recorded by state-age-race group in the U.S. from NCHS (2018), population counts from SEER (2018), and the authors' legal coding on TRAP laws, as described in detail in Appendix H.

Table D.2: Impact of TRAP laws on women's education: Stacked DD

	Non-Hispanic White			Non-Hispanic Black		
	High school completion	College initiation	College completion	High school completion	College initiation	College completion
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Never-treated controls						
Exposure before 18	0.00188	-0.00369	-0.01073***	-0.00656	-0.01968***	-0.01601***
	(0.00180)	(0.00299)	(0.00302)	(0.00494)	(0.00437)	(0.00550)
Obs	3,767,929	3,767,929	3,767,929	573,960	573,960	573,960
DV mean	0.956	0.718	0.454	0.909	0.595	0.268
Panel B. Never+Future treated controls						
Exposure before 18	0.00163	-0.00408	-0.01038***	-0.01067**	-0.02127***	-0.01436***
	(0.00160)	(0.00270)	(0.00270)	(0.00509)	(0.00412)	(0.00525)
Obs	4,892,313	4,892,313	4,892,313	796,626	796,626	796,626
DV mean	0.956	0.717	0.454	0.906	0.582	0.257

Notes: The coefficients are estimate of β in Equation 8. Each coefficient comes from a separate estimation. High school completion indicates being a high school graduate or having passed the GED. College initiation indicates completing at least one year of college or more. College completion refers to completing at least a bachelor's degree. The analysis includes the 10 policy changes shown in Table 1 to be included in estimation of Equation 8. The estimations are weighted by the person weight provided in ACS-IPUMS. The standard errors are clustered at the state of birth level. $^{*}p < 0.1$,$^{**}p < .05$, $^{***}p < .01$. Sources: ACS data from IPUMS (Ruggles et al., 2021), 2000-2019, women aged 25+ born in 1973 or later; information on other policies from Myers and Ladd (2020), and authors' legal coding on TRAP laws as described in detail in section H.

E Non-birth pathway by which TRAP laws may impact education

In order for expectations to act as a significant pathway by which TRAP laws affect educational attainment, it must be true that teens are able to accurately perceive their access to abortion, or at least be aware of restrictions to access. We test whether teens perceive differences in abortion access by examining impacts of TRAP laws on teen sexual behavior and contraceptive use. We would expect that a reduction in abortion access would reduce sexual activity and/or increase contraceptive use.[51] While some studies have found that parental involvement laws and Medicaid restrictions do not affect adolescent sexual behavior (Sen, 2006; Colman, Dee, and Joyce, 2013; Sabia and Anderson, 2016), others have documented that parental involvement laws do reduce risky sexual behavior among adolescents (Klick and Stratmann, 2008; Meyerhofer, 2020). Evidence of such a behavioral response to TRAP laws would (i) indicate that individuals do perceive changes in access and (ii) suggest that the estimates of TRAP laws' impacts on fertility are a lower bound.

We rely on data from the Center for Disease Control's Youth Risk Behavior Surveillance System (YRBSS) (CDC, 2019), which includes a representative sample of students in grades 9 through 12 in odd-numbered years from 1991 to 2017. From these data, we employ information on sexual activity and contraceptive use, as well as information on gender, age, race, ethnicity, and state of residence.[52,53] We combine this with our state-by-year data set on TRAP law enforcement.

We estimate

$$Y_{isya} = \beta TRAP_{sy} + \boldsymbol{X'_{sy}}\delta + \nu_s + \nu_y + \nu_a + \varepsilon_{isya} \tag{9}$$

where Y_{isya} is the outcome of interest for individual i in state s interviewed in year y at age a. $TRAP_{sy}$ indicates the enforcement of any TRAP law in state s in year y. X_{sy} is the same policy controls as described in section 4.1. We include fixed effects for state, year, and age. The outcomes of interest are whether or not the individual has initiated sexual activity (had sexual debut) and whether or not contraception was used at last intercourse (conditional on having had sexual debut).[54]

Results are presented in Table E.1, with and without the full set of controls, separately for White, Black, and Hispanic teen girls. We find that TRAP laws delay sexual debut among White teen girls by 2 percentage points, an effect that is 4.7 percent of the mean of 42 percent and is significant at the 10 percent level. Among Black teen girls, the impacts on sexual debut are also 2 percentage points, though the smaller sample size does not allow us reject the null hypothesis. We

[51] We note that there is some potential for reverse causality between contraception use and unintended pregnancy. If a teenager faces an unintended pregnancy, she might respond by increasing contraception use to avoid future pregnancies. Under that scenario, we would also observe increases in contraception use, but indirectly driven by changes in teen fertility. Another possibility is that a teen mother may reduce her contraception use since her past incentives to use it might have been related to avoiding motherhood, which may not be relevant anymore.

[52] The YRBSS combined data set contains data for 44 states. The states whose information is not collected are Indiana, Massachusetts, Minnesota, Ohio, Oregon, and Washington.

[53] The YRBSS may suffer from selection because the information is collected among teenagers attending high school. Then, this sample does not represent teenagers who may have dropped out because of early childbearing.

[54] The contraception methods we consider to construct this variable are: condom, birth control pills, IUD, implant, injection, patch, and birth control ring.

do not find evidence of measurable changes in Hispanic girls' sexual debut as a response to TRAP law implementation. Finally, the impact of TRAP laws on teen contraceptive use are quite small and not statistically different from zero for any of these groups.

Taken together, these results weakly suggest that non-Hispanic teens do adjust their sexual behavior in response to reduced abortion access. This indicates that at least some teens do perceive changes in abortion access. While this does not provide direct evidence for expectations as a pathway by which TRAP laws impact educational attainment, these findings indicate that we cannot rule out this pathway as potentially operable.

Table E.1: Impact of TRAP laws on teen sexual behavior

	Mean	Obs	(1)	(2)
Panel A. White teen girls				
Sexual debut	0.42	292,048	-0.0214**	-0.0196*
			(0.0105)	(0.0108)
Contraception use	0.57	118,970	0.00305	0.00236
			(0.0109)	(0.0116)
Panel B. Black teen girls				
Sexual debut	0.54	62,879	-0.0238	-0.0204
			(0.0163)	(0.0166)
Contraception use	0.60	31,867	-0.0126	-0.0151
			(0.0189)	(0.0188)
Panel C. Hispanic teen girls				
Sexual debut	0.43	68,202	0.00747	-0.00213
			(0.0172)	(0.0156)
Contraception use	0.53	29,359	-0.000266	0.00088
			(0.0272)	(0.0273)
State, year, and age FE			Yes	Yes
Abortion policies controls			Yes	Yes
Other policies controls			No	Yes

Notes: The table shows the estimates of β from equation 9, which indicates that any TRAP law was enforced in state s in year y. The dependent variables are indicators of whether or not an individual has initiated sexual activity (had sexual debut) and whether or not contraception was used at last intercourse (conditional on sexual debut). Each estimate comes from a separate regression. All the estimations include state, year, and age fixed effects. The first column additionally controls for the implementation of parental involvement laws and two-trip mandatory waiting periods. The second column includes these and also controls for other abortion, contraception, and welfare policies. See section 4.1 for more information on these policies. The standard errors are clustered at the state level. $^{*}p < 0.1, ^{**}p < .05$, $^{***}p < .01$. Sources: The information on sexual activity and contraception use was obtained from CDC (2019). Information on other policies comes from Myers and Ladd (2020). The authors' legal coding on TRAP laws is described in detail in section H.

F Impacts by timing of other abortion policies

Across states, teen abortion access is not only determined by TRAP law implementation. Other policies are relevant as well, such as mandatory waiting periods, parental involvement laws, and Medicaid funds for abortions. Table F.1 shows the distribution of states according to whether they had one of these policies at the time they implemented a TRAP law. Overall, there is no clear pattern between TRAP law timing and the onset of other related policies. TRAP laws have been implemented both with and without these other policies.

We estimate equation 1 separately for states that did vs did not have another related policy in place at the time of TRAP law implementation. Figures F.1 and F.2 present the estimates for each state sample for Black and White women, respectively. As expected, we find that the estimated impact of a TRAP law is greater in the presence of parental involvement laws and lesser in the presence of Medicaid coverage of abortion policies. However, we cannot reject that the effect is the same regardless of these other policies.

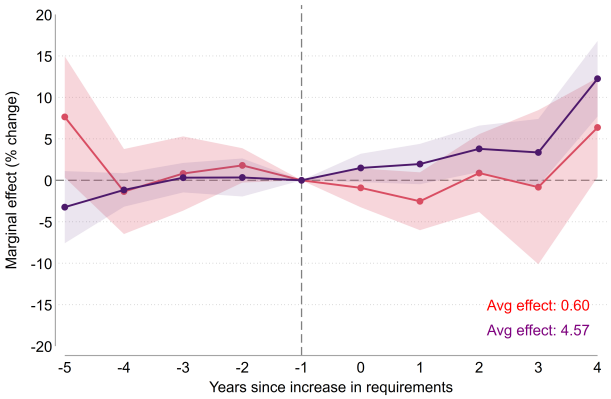
Table F.1: Coincidence of TRAP law implementation with related state policies

	Related policy of interest		
Number of states	Parental involvement	Mandatory waiting	Medicaid for abortion
Implementing TRAP in the presence of policy	19	11	4
Implementing TRAP law in the absence of policy	9	17	24
Never implementing TRAP law	20	20	20

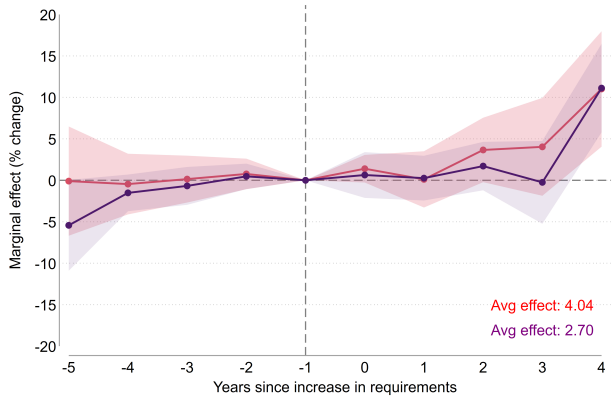
Notes: This table shows the number of states that implemented (or did not) TRAP laws according to the timing of parental involvement laws, mandatory waiting period laws, or Medicaid funding for abortion policies.

Figure F.1: Impact of TRAP laws on non-Hispanic Black teen births, disaggregated by presence of other abortion restrictions

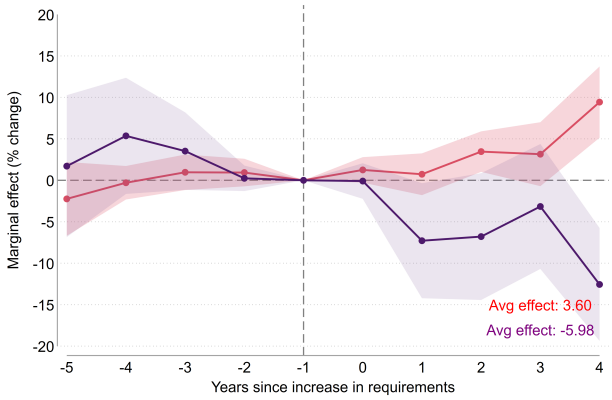
(a) By parental involvement law



(b) By mandatory waiting period law



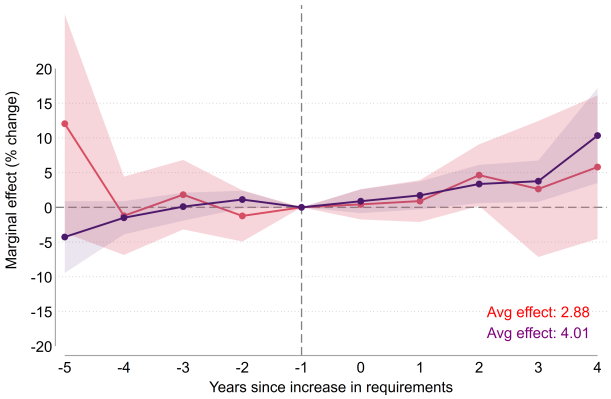
(c) By Medicaid coverage of abortion



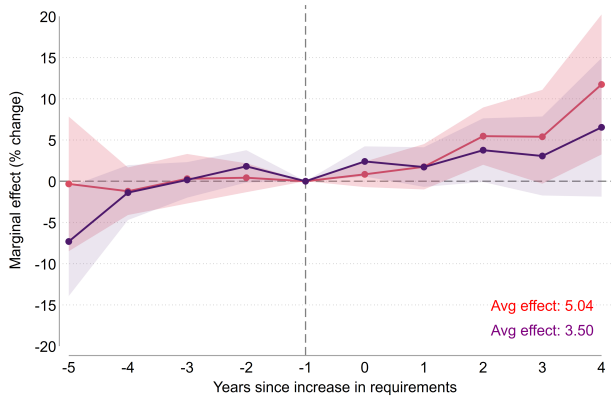
Note: The figures present the estimates of $100 \times (\exp(\beta_j) - 1)$ from Equation 1. This is the percent change in the number of births to 15 to 19-year-old Non-Hispanic Black women in state i and year $t + 1$. Year zero indicates the implementation of any TRAP law (admitting privileges, transfer agreement, hospital proximity regulations, and/or building regulations). The omitted year is the year before the policy change, $t = -1$. The purple line indicates the impact of a TRAP law in the presence of the other policy noted in the sub-figure title; the red line is the impact of a TRAP law in the absence of that other policy. The standard errors corresponding to the percentage change of the estimates were obtained using the Delta method. The light-shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level; the dark-shaded area indicates the 95 percent confidence interval from standard errors that do not account for within-cluster correlation. The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 1. Sources: Figure created using the information on births recorded by state-age-race/ethnicity group in the U.S. from NCHS (2018), 1990 to 2016, information on population counts from SEER (2018), information on other policies from Myers and Ladd (2020), and the authors' legal coding on TRAP laws, as described in detail in Appendix H.

Figure F.2: Impact of TRAP laws on non-Hispanic White teen births, disaggregated by presence of other abortion restrictions

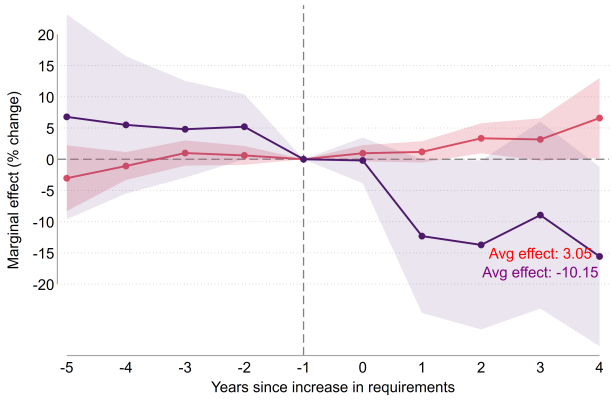
(a) By parental involvement law



(b) By mandatory waiting period law



(c) By Medicaid coverage of abortion

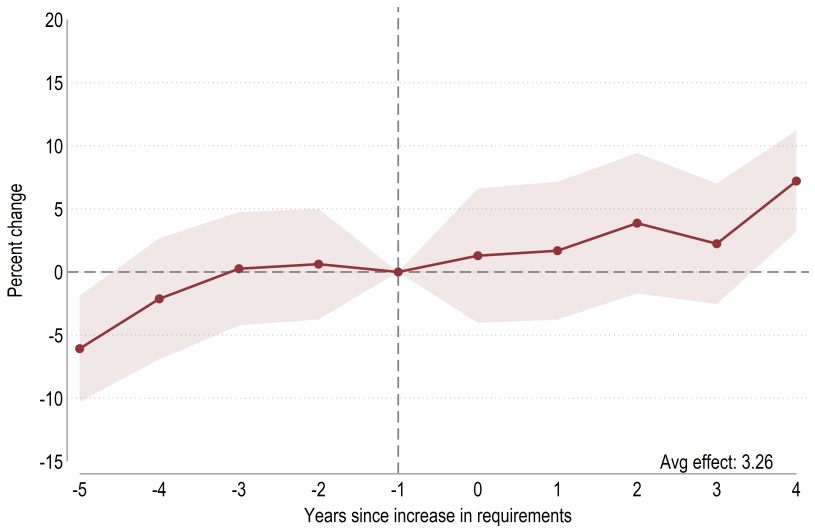


Note: The figures present the estimates of $100 \times (\exp(\beta_j) - 1)$ from Equation 1. This is the percent change in the number of births to 15 to 19-year-old Non-Hispanic White women in state i and year $t + 1$. Year zero indicates the implementation of any TRAP law (admitting privileges, transfer agreement, hospital proximity regulations, and/or building regulations). The omitted year is the year before the policy change, $t = -1$. The purple line indicates the impact of a TRAP law in the presence of the other policy noted in the sub-figure title; the red line is the impact of a TRAP law in the absence of that other policy. The standard errors corresponding to the percentage change of the estimates were obtained using the Delta method. The light-shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level; the dark-shaded area indicates the 95 percent confidence interval from standard errors that do not account for within-cluster correlation. The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 1. Sources: Figure created using the information on births recorded by state-age-race/ethnicity group in the U.S. from NCHS (2018), 1990 to 2016, information on population counts from SEER (2018), information on other policies from Myers and Ladd (2020), and the authors' legal coding on TRAP laws, as described in detail in Appendix H.

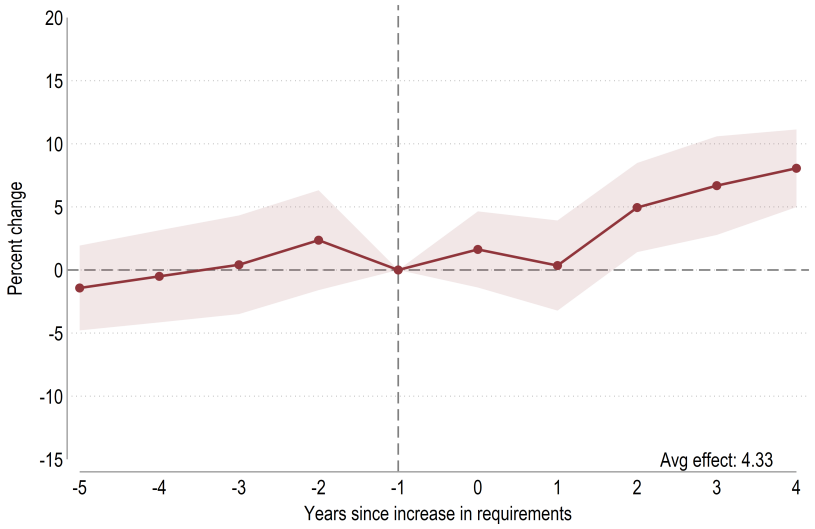
G Other robustness checks & falsification tests

Figure G.1: Impact of TRAP laws on teen births: Weighted Least Squares estimations

(a) Non-Hispanic White



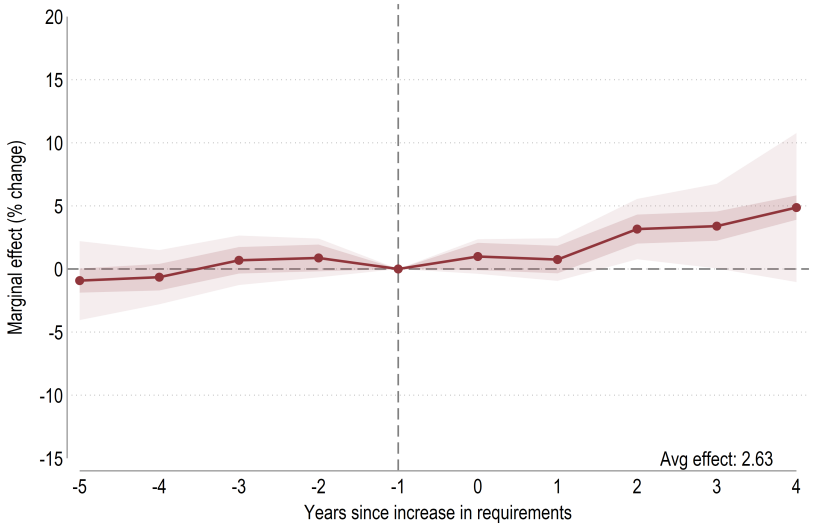
(b) Non-Hispanic Black



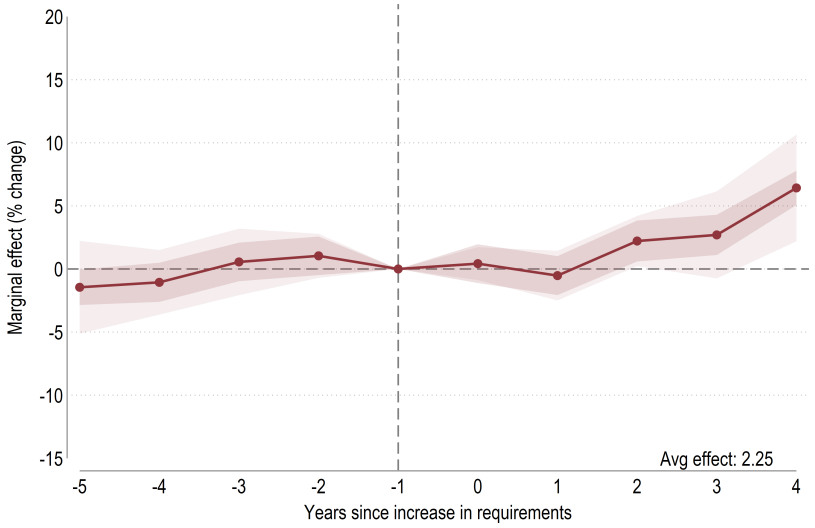
Note: This figure presents the estimates of β_j*100, which represents the percent change, from an equation similar to equation 1. In these figures, we estimate weighted least squares instead of Poisson regressions. The dependent variable is the log of the number of births per 1,000 women for 15-19-year-old non-Hispanic women (by race) living in state i in year $t+1$. The weight is the absolute value of the residual. Year zero indicates the implementation of a the corresponding TRAP law to the figure. The omitted year is the year before the policy change, $t = -1$. Equations are weighted by the 15-19 female population in the corresponding race group. The shaded area indicates the 95 percent confidence interval from robust standard errors. The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 1. Sources: Figure created using the information on births recorded in the U.S. from NCHS (2018), 1990-2016 information on population counts from SEER (2018), information on other policies from Myers and Ladd (2020), and the legal coding on TRAP laws, as described in detail in Appendix H.

Figure G.2: Impacts of TRAP laws on teen births: 1992-2009

(a) Non-Hispanic White



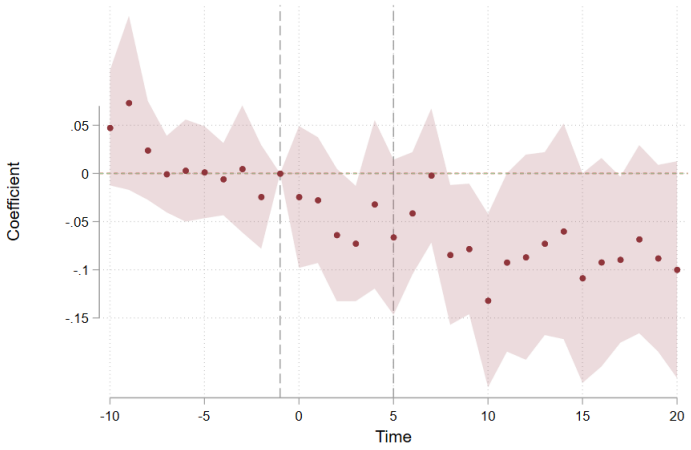
(b) Non-Hispanic Black



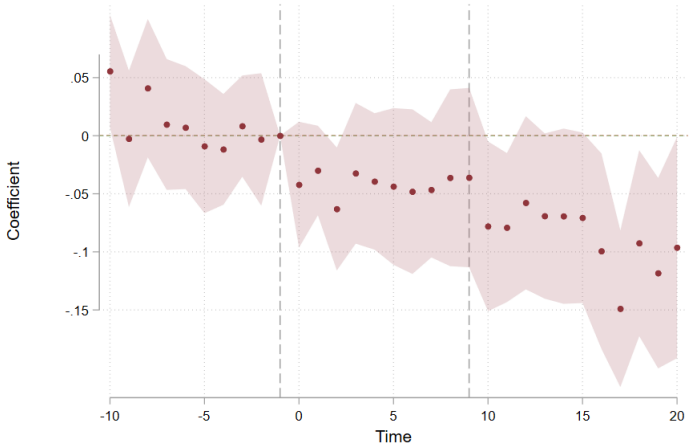
Note: These figures presents the estimates of $100 \times (\exp(\beta_j) - 1)$ from Equation 1. This is the percent change in the number of births of 15 to 19-year-old women of the specified race/ethnicity group in state i and year $t+1$. Year zero indicates the implementation of any TRAP law (admitting privileges, transfer agreement, hospital proximity regulations, and/or building regulations). The omitted year is the year before the policy change, $t = -1$. These figures include births data for the years 1992-2009 to mirror the birth cohorts included in the educational attainment analyses. The standard errors corresponding to the percentage change of the estimates were obtained using the Delta method. The light-shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level; the dark-shaded area indicates the 95 percent confidence interval from standard errors that do not account for within-cluster correlation. The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 1. Sources: Figure created using the information on births recorded by state-age-race/ethnicity group in the U.S. from NCHS (2018), 1992-2009, information on population counts from SEER (2018), information on other policies from Myers and Ladd (2020), and the authors' legal coding on TRAP laws, as described in detail in Appendix H.

Figure G.3: Estimated impact of adolescent TRAP law exposure by year relative to implementation. Black women, single-age group

(a) College initiation, as measured for 22-year-old women



(b) College completion, as measured for 26-year-old women



Notes: This figure plots the β_j coefficients from Equation 3 estimated separately for each outcome and single-age year for the sample of Black women. College initiation is measured at age 22, and college completion at age 26. The right-hand vertical line marks the year when all women in the age group from the treated state were treated (exposed to the policy before age 18). Shaded areas show 95 percent confidence intervals based on standard errors clustered at the state level. The analysis includes the 27 policy changes shown in Table 1 to be included in the estimation of Equation 3. Source: Own calculations using ACS data from IPUMS (Ruggles et al., 2021), 2000-2019, women born in 1973 or later, and the authors' legal coding of TRAP laws, as described in detail in Appendix H.

Table G.1: Stacked DD analyses excluding policy changes in states that also had a Pre-*Casey* TRAP law

Panel A. Teen births

	Non-Hispanic White			Non-Hispanic Black		
	$j=6$ (1)	$j=5$ (2)	$j=4$ (3)	$j=6$ (4)	$j=5$ (5)	$j=4$ (6)
Panel A1. Stacked DD (never treated controls)						
TRAP law	8.266***	6.612***	5.228***	8.680***	6.552***	5.298***
	(0.332)	(0.326)	(0.359)	(0.419)	(0.433)	(0.478)
Obs	2,358	2,340	1,872	2,358	2,340	1,872
Panel A2. Stacked DD (never+future treated controls)						
TRAP law	7.815***	6.264***	5.677***	7.194***	5.113***	4.467***
	(0.318)	(0.303)	(0.327)	(0.396)	(0.395)	(0.428)
Obs	2,933	2,930	2,400	2,933	2,930	2,400

Panel B. Educational attainment

	Non-Hispanic White			Non-Hispanic Black		
	High school completion	College initiation	College completion	High school completion	College initiation	College completion
Panel B1. Stacked DD (never-treated controls)						
Exposure before 18	0.00162	-0.00385	-0.01048***	-0.00752	-0.02095***	-0.01697***
	(0.00196)	(0.00328)	(0.00322)	(0.00529)	(0.00447)	(0.00568)
Obs	3,470,834	3,470,834	3,470,834	528,680	528,680	528,680
DV mean	0.956	0.719	0.456	0.909	0.595	0.268
Panel B2. Stacked DD (never+future treated controls)						
Exposure before 18	0.00129	-0.00431	-0.01019***	-0.01161**	-0.02209***	-0.01520***
	(0.00172)	(0.00297)	(0.00292)	(0.00535)	(0.00432)	(0.00541)
Obs	4,892,313	4,892,313	4,892,313	738,983	738,983	738,983
DV mean	0.956	0.718	0.456	0.906	0.582	0.257

Notes: This table presents estimates analogous to Table D.1 and Table 2 (Stacked DD panels), except that the set of treated states excludes MO and PA, due to their pre-*Casey* TRAP laws. Panel A shows estimates of $100\times(\exp(\beta)-1)$ from Equation 7. This is the percent change in the number of births of 15 to 19-year-old women of the given race/ethnicity group in state s and year $t+1$. Each coefficient is from a separate regression. "Future treated states" are states that implemented an abortion restriction more than five years after t. j indicates the number of pre-periods included in the analysis. The analysis includes the 13 policy changes shown in Table 1 to be included in estimation of Equation 7. The standard errors are calculated using the Delta method. $^{*}p<0.1$,$^{**}p<.05$, $^{***}p<.01$. Sources: information on births recorded by state-age-race group in the U.S. from NCHS (2018), 1992-2016, population counts from SEER (2018).

Panel B shows estimates of β in Equation 8. High school completion indicates being a high school graduate or having passed the GED or more. College initiation indicates completing at least one year of college or more. College completion refers to completing at least a bachelor's degree. The analysis includes the 10 policy changes shown in Table 1 to be included in estimation of Equation 8. The estimations are weighted by the person weight provided in ACS-IPUMS. The standard errors are clustered at the state of birth level. Sources: ACS data from IPUMS (Ruggles et al., 2021), 2000-2019, women aged 25+ born in 1973 or later.

Table G.2: Robustness test: Relative magnitude of parallel trend violation (for non-Hispanic Black women at $j = 15$)

College initiation

$\bar{M}$	Lower bound	Upper bound
0.00 (Original)	-0.094	-0.029
0.25	-0.098	-0.025
0.50	-0.103	-0.02
0.75	-0.11	-0.014
1.00	-0.117	-0.006
1.25	-0.125	0.004
1.50	-0.134	0.014

College completion

$\bar{M}$	Lower bound	Upper bound
0.00 (Original)	-0.097	-0.029
0.25	-0.102	-0.024
0.50	-0.108	-0.019
0.75	-0.116	-0.012
1.00	-0.124	-0.005
1.25	-0.133	0.003
1.50	-0.142	0.013

Notes: The tables present alternative confidence intervals for the coefficient on $j = 15$ as shown in Figures 4b and 4d using the relative magnitude method described by Rambachan and Roth (2023), implemented using the *honestdid* Stata command. Confidence intervals are calculated under the assumtion that there is a violation in parallel trends in the post-period that is as large as the maximum deviation in the pre-period multiplied by the factor $\bar{M}$. Findings indicate a "breakdown value" of $\bar{M} = 1.25$,meaning that in order for the statistically significant coefficient at $j = 15$ to be fully explained by confounding factors, a violation in parallel trends in the post period would need to be at least 125% of the size of the maximum deviation in the pre-period. The absolute value of the maximum deviation in the pre-period is 0.025 in both Figures 4b and 4d. Note that $j = 15$ is employed following the example in section 6.3 of Rambachan and Roth (2023) and because it is in the "fully treated post-period" for both estimations.

Table G.3: Falsification test: Impact of TRAP laws on men's education

	White non-Hispanic			Black non-Hispanic		
	High school completion	College initiation	College completion	High school completion	College initiation	College completion
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Women						
Exposure before 18	0.002	0.000	-0.007*	-0.004	-0.012**	-0.014***
	(0.002)	(0.003)	(0.003)	(0.004)	(0.006)	(0.005)
Obs	2559720	2559720	2559720	413162	413162	413162
DV mean	0.948	0.690	0.421	0.901	0.566	0.240
Panel B. Men						
Exposure before 18	-0.001	-0.002	-0.002	-0.004	-0.007	-0.006
	(0.002)	(0.003)	(0.004)	(0.006)	(0.007)	(0.005)
Obs	2549784	2549784	2549784	390954	390954	390954
DV mean	0.928	0.592	0.340	0.856	0.427	0.162

Notes: Estimates of β in Equation 2. Panel A is a reproduction of Panel B from Table 2, for ease of comparison. Each coefficient comes from a separate estimation. High school completion indicates being a high school graduate or having passed the GED or more. College initiation indicates completing at least one year of college or more. College completion refers to completing at least a bachelor's degree. The analysis includes the 27 policy changes shown in Table 1 to be included in estimation of Equation 2. The estimations are weighted by the person weight provided in ACS-IPUMS. The standard errors are clustered at the state of birth level. $^{*}p < 0.1$,$^{**}p < .05$, $^{***}p < .01$. Sources: ACS data from IPUMS (Ruggles et al., 2021), 2000-2019, women and men aged 25+ born in 1973 or later; information on other policies from Myers and Ladd (2020), and authors' legal coding on TRAP laws as described in detail in section H.

H Details on legal coding of TRAP laws

Alaska

- Transfer agreement: Since 11/19/1983, 7 AK Admin Code 7 AAC 12.370 allows ambulatory surgical facilities to terminate pregnancies. Also, effective on 11/19/1983, Alaska Admin. Code tit. 7, § 12.910 requires ambulatory surgical facilities to have a signed agreement with a general acute care hospital for transfer of patients who require medical or emergency care beyond the scope of the ability or license of the facility. So then, we coded 1983 as the effective year for the transfer agreement requirement for abortion facilities. However, since this is a pre-*Casey* TRAP law, we do not consider it in our analyses.

Alabama

- Admitting privileges:
  - On 5/22/2003, amends to Ala. Admin. Code r. 420-5-1-.03 became effective. These amends include a requirement for abortion providers to comply with their own admitting privileges or an agreement with an external physician with admitting privileges. We coded this restriction as the least stringent version of admitting privileges laws, but we do not account for this version of admitting privileges laws in our analyses.
  - According to NARAL Pro-Choice America, Admin. Code r. 420-5-1-.03 was amended in 2013 to require abortion providers to have admitting and staff privileges at an acute care hospital in the same standard metropolitan statistical area. No exceptions are made for rural areas, and nothing in the statute requires a hospital to agree to such an arrangement. A federal court has blocked this provision of the law. The U.S. District Court for the Middle District of Alabama, Northern Division issued a preliminary injunction after pro-choice activists challenged the law before it could go into effect (West Alabama Women's Center v. Williamson Planned Parenthood Southeast}, Inc. v. Bentley, 120 F.Supp.3d 1296, 2015 (M.D.Ala.)). Therefore, we coded the effective year as 2013 and the blocked year as 2015.

- Building regulations: On 3/27/1997, Alabama amended Ala. Admin. Code r. 420-5-1-.04 specifying requirements for doors and corridors width. It also indicates characteristics of examining facilities, procedure room, recovery room, clean workroom, and soiled workroom.

Arkansas

- Building regulations: Since 1999, the Rules and Regulations for Abortion Facilities §§12 require abortion facilities to comply with specific requirements for ventilation and temperature, examination, procedure, recovery, and soiled workrooms. We verified this information with NARAL's Who Decides? 2002.

Arizona

- Admitting privileges: 1999 Ariz. Legis. Serv. Ch. 311 (H.B. 2706) requires that at least one physician with admitting privileges at an accredited hospital in the state is available when a abortion procedure is performed. Also, a physician with admitting privileges at an accredited hospital in this state remains on the premises of the abortion clinic until all patients are stable and are ready to leave the recovery room and to facilitate the transfer of emergency cases if hospitalization of the patient or viable fetus is necessary. The effective date of this law was 31/3/2000. Then, we code the effective year as 2000.

- Building regulations: AZ ST § 36-449.03 requires abortion facilities to comply with specific characteristics on the procedure, examination, recovery, rooms, adequate lighting and ventilation, and areas for cleaning and sterilizing instruments. This law has been amended several times, but the requirements on lighting and ventilation appeared in the law in 1999. Given that this law occurs in the year before the admitting privileges law (discussed above), we code both laws as a single policy change in 2000.

- Hospital proximity regulations: Starting on 2012, Ariz. Rev. Stat. Ann. §§ 36-449.03, physicians providing surgical abortions must have admitting privileges at a hospital within thirty miles of the abortion facility, must remain on the premises of the abortion clinic until all patients are stable and are ready to leave the recovery room and to facilitate the transfer of emergency cases if hospitalization of the patient or viable fetus is necessary. Since this restriction applies to surgical abortions, we account for it law in our main analyses.

Connecticut

- Building regulations: Since 2/25/1974, CT ADC § 19-13-D54 specifies standards for operating and recovery rooms. We do not consider this law in our analyses because it was implemented in the pre-*Casey* era.

Florida

- Admitting privileges, transfer agreement, and hospital proximity regulations: Fla. Stat. Ann. § 390.012(2), H.B.1411 requires abortion clinics that perform abortions after the first trimester to have a written transfer agreement with a hospital within a reasonable proximity to the clinic, and requires physicians who perform abortions in the clinic to have admitting privileges with a hospital within a reasonable proximity to the clinic. Abortion clinics that perform only first trimester abortions must have such a transfer agreement, or physicians who perform abortions in the clinic must have such admitting privileges. This bill was effective on July 1, 2016. So, we code its effective year as 2016. However, this law is too recent to be included in our analyses.

- Building regulations: FL ADC 59A-9.022 requires abortion clinics providing second-trimester abortions to meet physical and plant requirements. This includes specifications on the procedure and recovery rooms and cleaning and sterilizing areas. This law was adopted on 9/25/2006. So, we coded 2006 as the effective year. However, we exclude this law from our main analyses because it only targets second-trimester abortion providers.

Georgia

- Since 1974, post-first-trimester abortions should be performed in a licensed hospital, ambulatory surgical center, or in a health facility licensed as an abortion facility by the Department of Community Health (Ga. Code Ann., § 16-12-141).

- Admitting privileges and/or transfer agreement: Effective on 3/12/2013, Ga. Comp. R. & Regs. 111-8-4-.09 requires ambulatory surgical facilities to have written procedures for emergency services. The centers should have a hospital affiliation agreement, and/or the medical staff must have admitting privileges or other acceptable documented arrangements to ensure the necessary back-up for medical complications. The centers must have the capability to transfer a patient immediately to a hospital with adequate emergency room services.

- Building regulations: Effective on 3/12/2013, Ga. Comp. R. & Regs. 111-8-4-.09 requires ambulatory surgical facilities to meet physical plant and operational standards. These include specific characteristics of procedure and recovery rooms.

- Since only abortion facilities providing post-first-trimester abortions licensed as ASCs are the ones complying with these regulations, we do not include these laws in our analyses.

Idaho

- Transfer agreement: Effective on 1973, Idaho Code § 18-608 requires that providers have "satisfactory" transfer arrangements with one or more acute-care hospitals within reasonable proximity. The provision makes no exception for clinics in rural areas, or if no local hospitals will agree to a transfer arrangement. We do not consider this law in our analyses because it was implemented in the pre-*Casey* era.

Illinois

- Admitting privileges, hospital proximity regulations, and building regulations: Effective 9/21/2016, 77 Ill. Adm. Code 205.710 states that facilities terminating pregnancies within 18 weeks should be considered pregnancy termination specialty centers. This law also requires the medical director or a physician practicing at the facility has a professional working relationship or agreement, maintained in writing at the facility and verifiable by the Department, with a physician who does have admitting or practice privileges at a licensed hospital within approximately 15-30 minutes from the facility and who will assume responsibility for all facility patients requiring hospitalization or inpatient hospitalization follow-up care. Additionally, it establishes standards for the plant, such as rooms sizes and corridor and hallway widths. This law was repealed in 2020. Given that these regulations were implemented recently, data limitations do not allow us to evaluate them. So, we do not include them in our analyses.

- We also found some indications that some abortion facilities may have to be licensed as ASCs if providing general anesthesia. It seems this decision is the result of *Ragsdale v. Turnock*, C.A.7 (Ill.) 1988, 841 F.2d 1358. Before that, abortion facilities were required to be licensed as ASCs. However, we did not find information on what ASC standards were then. After 1988, no law establishes all abortion facilities must operate as ASCs. Then, we do not code any hospital relationship requirements and building regulations applying to ASCs, because those requirements only potentially affect those few facilities providing general anesthesia.

Indiana

- Admitting privileges: Since 5/10/2011, the Ind. Code § 16-34-2-4.5 (P.L.193-2011, SEC.14) requires physicians working at facilities performing five or more medical abortions per year at any stage of pregnancy; or facilities performing surgical abortions at any stage of pregnancy to have admitting privileges at a hospital located in the county where abortions are provided or a contiguous county, or to have entered into an agreement with a physician who has admitting privileges at a hospital within the county or in a contiguous county, to manage possible complications arising from the abortion procedure. Effective on 7/1/14, the law was modified to specify that the admitting privileges should be provided in writing. Effective on 7/1/2016, it was additionally included that the agreement should be renewed annually. We code this law as the less stringent version of admitting privileges laws and consider 2011 as its effective year. However, this law is not included in our main analyses.

- Building regulations:

  - Since 1993, 410 IAC 15-2.5-7 establishes physical plant, equipment maintenance, and environmental services for ambulatory outpatient surgical center services following the Guidelines for Design and Construction of Hospital and Health Care Facilities. On 12/1/1999, it became also required for ambulatory outpatient surgical center services to have emergency power and lighting following the National Fire Protection Association standards. These regulations only apply to post-first-trimester abortions performed in ambulatory outpatient surgical centers. So then, we exclude this law from our main analyses.
  - 410 IAC 26-17-2 contains specifications of physical plants for abortion clinics. Among these specifications, it includes characteristics of procedure, examination, and recovery rooms. It also specifies minimum corridor and doorway widths. This law was filed on 5/11/2006. We are not sure this same year the law became effective. However, since we could not find further information, we consider 2006 the effective year.

- Transfer agreement:

  - Ind. Code § 16-34-2-1, effective in 1993, specifies that after the first trimester of pregnancy and before the earlier viability of the fetus or twenty (20) weeks of post-fertilization age, abortions should be performed in a hospital or ambulatory outpatient surgical center. Effective in the same year, Ind. Code § 16-18-2-14 requires ambulatory outpatient surgical centers to maintain a written agreement with at least one hospital for immediate acceptance of patients who develop complications or require postoperative confinement. Since only facilities providing post-first-trimester abortions must operate as ambulatory outpatient surgical centers, we exclude this law from our main analyses.

- 410 IAC 26-12-1 requires abortion clinics to have a readily accessible written protocol to manage medical emergencies that occur within the clinic and to transfer to a hospital a patient requiring further emergency care. This law was filed on 5/11/2006. We did not find information on the effective date. However, the 2006 version of the law is the same as today. So then, we assume the effective year is 2006 and consider it as a plan/protocol.

Kansas

- Admitting privileges and minimum hospital proximity regulations: Effective on 7/1/2011, Kan. Admin. Regs. § 28-34-132 requires that a physician performing or inducing abortion procedures in a facility has clinical privileges at a hospital located within 30 miles of the facility.
- Transfer agreement: Effective on 7/1/2011, Kan. Admin. Regs. § 28-34-140 requires developing written policies and procedures to transfer patients to a hospital. We consider this law a plan/protocol rather than a formal transfer agreement.

Kentucky

- Transfer agreement: Effective on 7/15/1998, Ky. Rev. Stat. § 216B.0435 requires written agreements between an abortion facility and acute-care hospital capable of treating patients with unforeseen complications related to an abortion facility procedure by which the hospital agrees to accept and treat these patients. The law also requires a similar agreement with a local ambulance service for the transportation of patients. We code 2008 as the effective year of this law.

Louisiana

- Admitting privileges and hospital proximity regulations: Effective on 9/1/2014, La. Rev. Stat. Ann. §40:1061.10 requires physicians performing or inducing abortions to have active admitting privileges at a hospital that is located not further than thirty miles from the location at which the abortion is performed or induced. The hospital should provide obstetrical or gynecological health care services. According to NARAL, "That decision was appealed to the U.S. Supreme Court and the high court granted an emergency stay—blocking the law from going into effect. June Medical Serv., et al. v. Gee, Sec., LA DHH, 577 US 15A880 (2016). The Fifth Circuit did not oppose the motion—pending the ruling in the *Whole Woman's Health v. Hellerstedt* case—challenging a similar law in Texas. The U.S. District Court for the Middle District of Louisiana issued a permanent injunction against the law and any implementing regulations in April 2017. June Medical Services LLC v. Caldwell, No. 3:14-CV-525 (M.D. La. April 26, 2017)." There were some other appeals later, but they are not relevant for our analysis, as it runs up to 2016. Then, for our legal coding, we consider this law as blocked in 2016.
- Building regulations: Promulgated on April 2015, La. Admin. Code tit. 48 § I-4445 specifies general requirements for abortion providers. It includes specifications on procedure room and recovery area size and characteristics of a clean utility room used for clean or sterile supplies. Unfortunately, we could not locate any information on the effective date. So, we assign as the effective year the promulgation year 2015.
- Both of these policies are not included in our analyses because they are too recent to evaluate.

Maryland

- Building regulations: Md. Code Regs. 10.12.01.15 contains some requirements on the physical environment of surgical abortion facilities. In particular, it includes specifications on the procedure and recovery rooms.
- Transfer agreement: Md. Code Regs. 10.12.01.10 requires surgical abortion facilities to have an effective procedure for transferring patients to a nearby hospital when care beyond the facility's capabilities is required. Since the law only requires written protocols and procedures related to emergency transfer procedures but not a formal transfer agreement with a hospital, we consider this law a plan/protocol. Therefore, we exclude this law from our analyses.

- COMAR 10.12.15 became effective on 7/23/2012. Both subsections 15 and 10, mentioned above, are part of it. So, we assign 2012 as the effective year. Also, since its content applies to surgical abortions, we consider both building regulations and transfer agreements in our main analyses.

Michigan

- Since 2000, Michigan Compiled Laws, Chapter 333. Health § 333.20115 requires abortion facilities to be licensed as freestanding outpatient facilities if they perform a certain number of abortions a year. Before 2013, the rules applied to facilities where 50 percent or more of the patients served annually undergo an abortion. After 2013, the facilities required to operate as ASCs performed 120 or more surgical abortions per year and publicly advertised outpatient abortion services.

- Hospital proximity regulations: Mich. Admin. Code r. 325.3832 requires freestanding surgical outpatient facilities to be located not more than 30 minutes normal travel time from the hospital with which written emergency admission arrangements are made.

- Transfer agreement: Mich. Comp. Laws Ann. § 333.20821 requires the freestanding surgical outpatient facility to have a written agreement with a nearby licensed hospital to provide for the emergency admission of post-surgical patients who may require hospital admission and care for unpredictable reasons.

- Since these two regulations only apply to freestanding surgical outpatient facilities, and abortion facilities became required to be licensed as such in 2000, we coded 2000 as the effective year for both requirements. Also, we consider the pre-2013 laws as applying to all abortion facilities and the post-2013 law as applying to surgical facilities only. However, for our main analyses, we consider the transfer agreement and hospital proximity regulations as effective in 2000, without distinction. Both laws were rescinded in 2020.

Missouri

- Building and hospital proximity regulations:

  - In 1987, of Mo. Code Regs. Ann. tit. 19, §30-30 was created, and it included some physical plant requirements and surgical privileges with a hospital for physicians providing abortions. We coded these restrictions as admitting privileges and building regulations, respectively, with 1987 as the effective year. However, we do not consider these policies in our analyses because they were implemented in the pre-*Casey* era.
  - A hospital proximity regulation was created in 2004. However, it was restrained until a case dropped due to a clinic closure in 2005. This law required abortion facilities to be located within 30 miles of a hospital. Therefore, we code 2005 as the effective year of this law.
  - In 2007, the state revised Mo. Rev. Stat. § 197.200 to require all abortion providers to operate as ASCs. This increased the physical plant requirements and changed the hospital proximity regulation to be located within 15 minutes from a hospital. Later this law was challenged in court (*Drummond*). This challenge implies that the 1987 abortion provider regulations continued applying without modification, given that the 2007 law was enjoined.. In 2010, the parties executed an agreement to end the injunction. Beginning 16 months after the May 2010 agreement, the 2007 ASC requirements were enforced with some exceptions and modifications for Columbia Center and Brous Center clinics. We consider these requirements more stringent building regulations than those from the 1987 law and coded 2011 as their effective year.
  - There were posterior changes to the ASC law. However, in terms of our analyses, they are irrelevant since they happened after 2016.

Mississippi

- Transfer agreement and building regulations:

- 1991 amend to Miss. Code Ann. § 41-75-1 establishes that abortion facilities shall make arrangements with a local ambulance service, duly licensed by the State of Mississippi, to transport emergency patients to a hospital and provide documentation to the Department of proof of such arrangements. We considered this requirement a plan/protocol to transfer patients since it does not require a formal transfer agreement with a hospital. However, since it was implemented in the *pre-Casey* era, we do not consider it in our analyses.
- 1996 Miss. Laws Ch. 442 (S.B. 2817) required abortions performed at 16+ weeks to be performed in ambulatory surgical facilities (ASF). 2004 Miss. Laws Ch. 584 (H.B. 1038), effective in 2005, required post-first trimester abortions to be performed in ambulatory surgical facilities. Then, for ASF providing abortion services, it became relevant to comply with 15 Miss. Code R. § 16-1-42. It requires a transfer agreement for the immediate transfer to a hospital of patients requiring medical care beyond the capabilities of the ASF. It also includes other regulations applying to operating and recovery rooms, surgical suites. Then, in 2005, transfer agreements and building regulations were implemented for ASFs providing abortions. However, since the ASF requirement only applies to post-first-trimester abortions, we do not include these policies in our main analyses.
- Effective on 7/1/2012, House Bill 1390 requires that all physicians performing abortions in abortion facilities have admitting privileges at a local hospital and must be board certified in obstetrics and gynecology. This law was never enforced and ultimately blocked in 2017. According to NARAL: "A court held that the admitting-privileges requirement was valid, but temporarily prohibited the state from enforcing the civil or criminal penalties while the abortion facility attempted to comply with the law. The state appealed, but a three-judge panel of the Fifth Circuit Court of Appeals upheld the temporary injunction. The full Fifth Circuit denied a rehearing in the case, so the admitting privileges requirement did not go into effect. The state appealed to the U.S. Supreme Court, but the court held the case for over a year, the day following the Supreme Court's decision in a similar TRAP case out of Texas (*Whole Woman's Health v. Hellerstedt*) denied cert. In March 2017, the state was blocked from permanently enforcing the admitting privileges requirement, though summary judgment in the case has not yet been granted."
- 15 Miss. Code R. § 16-1-44.12.1 requires abortion facilities to have a written agreement with one or more physicians for the express purpose of ensuring that patients who have complications will be immediately transferred to the physician's care. The physician who enters the written agreement with the abortion facility shall have full admitting privileges with one or more acute general hospitals that shall be located within 30 minutes travel time of the abortion facility. This is the least stringent version of admitting privileges laws, so we do not consider this policy in our analyses. Furthermore, we do not consider the 30 minutes travel time a hospital proximity regulation because this is a requirement for the physician's office, not the clinic location. Unfortunately, we could not track any information on the effective date of this law. However, the history of the law shows that the last effective date was 7/1/1996. So then, we assign 1996 as the effective year.

North Carolina

- Admitting privileges, transfer agreement, hospital proximity and building regulations: Effective on 2/1/1976, 10A N.C. Admin. Code 14E established that abortion clinics are considered freestanding facilities if performing abortions during the first 12 weeks of pregnancy. Also, effective on the same date, the law requires some emergency back-up services requirements. In particular, a written transfer agreement between free abortion clinics and a licensed North Carolina hospital, was required to transfer patients in need of emergency care. In the absence of a transfer agreement, all the physicians operating in a freestanding abortion clinic shall document that they have adequate admitting privileges at a hospital. The hospital should also be located no more than 15 minutes travel time from the freestanding abortion facility. It is also required to meet minimum standards for construction and equipment. This includes standards for sanitation, elevator, corridors, doors, and rooms. We do not include any of these laws in our analysis because they were implemented in the *pre-Casey* era.

- Effective on 7/1/1994, 10A N.C. Admin. Code 14E, some modifications to the existing building regulations in 10A N.C. Admin. Code 14E and additions such as ventilation requirements were made. We coded this as building regulations, with 1994 as the effective year. However, since this is a single and minor requirement, we did not account for it in our analyses.

- Effective on 10/1/2015, 10A N.C. Admin. Code 14E modified the emergency back-up services requirements. Now, it requires clinics to have either a written agreement between the clinic and a hospital to facilitate the transfer of patients who require emergency care, or documentation of their efforts to establish such a transfer agreement with a hospital and has been unable to secure such an agreement. Since clinics can document their effort to get a transfer agreement without actually getting it, we code this law as the less stringent version of transfer agreements. However, this policy is too recent to evaluate, so we exclude it from our analyses.

North Dakota

- Admitting privileges and hospital proximity regulations:
  - Effective on 8/1/2011, N.D. Cent. Code § 14-02.1-03.5. 4, requires any physician who gives, sells, dispenses, administers, prescribes, or otherwise provides an abortion-inducing drug shall enter a signed contract with another physician who agrees to handle emergencies associated with the use or ingestion of the abortion-inducing drug. The physician who contracts to handle emergencies must have active admitting privileges and gynecological and surgical privileges at the hospital designated to handle any emergencies associated with the use or ingestion of the abortion-inducing drug. This law focuses on medical abortion only.
  - Effective on 8/1/2013, N.D. Cent Code § 14-02.1-04 requires all physicians performing abortion procedures to have admitting privileges at a hospital located within thirty miles of the abortion facility and staff privileges to replace hospital on-staff physicians at that hospital. These privileges must include the abortion procedures the physician will be performing at abortion facilities.

- We code the 2011 admitting privileges law as the least stringent version because it only requires an agreement with another physician with active admitting privileges. So then, we exclude it from our main analyses. We code the 2013 laws as admitting privileges and hospital proximity regulations.

Nebraska

- Admitting privileges or transfer agreement: Effective on 1/1/2001, Neb. Admin. R. & Regs. Tit. 175, Ch. 7, § 006. 7-006.14D requires abortion facilities to have a written agreement for emergency care with a hospital that provides obstetrical services. Otherwise, each medical practitioner practicing at the facility must have admitting privileges at a transferring hospital. This requirement is triggered by facilities performing ten or more abortions of any method per week. Then, we consider it as applying to all clinics. We code 2001 as the effective year.

- Building regulations: Effective on 1/1/2001, 175 Neb. Admin. Code § 7-006. "Standards of operation, care, and treatment specify requirements on physical plan standards," requires abortion facilities to comply with requirements on examination rooms, procedure, and recovery rooms, as well as the corridor and hallway width and ventilation. We code 2001 as the effective year.

Ohio

- Hospital proximity regulation: Effective on 9/25/2015, Ohio Revised Code Section 3702.3010 requires that local hospitals with a written transfer agreement with an ASF shall not be further than thirty miles from the ASF. Therefore, we code 2015 as the effective year of this restriction. However, this policy is too recent to evaluate, so we exclude it from our analyses.

- Transfer agreement: The 2006 decision on the case *Women's Medical Professional Corp (WMPC). v Baird* mentions that abortion facilities were not required to operate as ambulatory surgical facilities (ASF) before 1999. Attempts to enforce licensing for abortion clinics as ASFs began in 1999, which would have triggered a transfer agreement requirement. Note that there is no legal code requiring abortion clinics to operate as ASFs; it seems some clinics operating as ASFs has more to do with interpreting the definition of an ASF than an ASF requirement for abortion facilities. In the same court decision, it is mentioned that the state was regularly granting waivers to clinics regarding the transfer agreement until a waiver was denied to WMPC in 2003. The waiver denial was enjoined until it was held

constitutional in this court decision. Then, based on this information, we consider 2006 as the transfer agreement effective year. This is consistent with the fact that this TRAP law is not mentioned in any NARAL documentation until 2007.

- Effective 9/29/2013, Ohio Rev. Code Ann. §3727.60 prohibited public hospitals from entering into a written transfer agreement with an ASF where non-therapeutic abortions are performed or induced. This prohibition made it harder for abortions facilities operating as ASFs to comply with the transfer agreement requirement. We also code this transfer agreement restriction as a separate one from the 2006 transfer agreement, and as effective in 2013 to capture the increase in stringency of the law.

Oklahoma

- Admitting privileges, hospital proximity regulation, and transfer agreement:
  - Effective on 11/1/2014, Okla. Stat. tit. 63, § 1-748. B requires physicians performing or inducing abortions to have admitting privileges at a general medical-surgical hospital that offers obstetrical or gynecological care in this state located within thirty (30) miles of where the abortion is being performed. It also requires physicians to remain on the facility's premises to facilitate the transfer of emergency cases if hospitalization of an abortion patient or a child born alive is necessary and until all abortion patients are stable and ready to leave the recovery room. According to Guttmacher Institute, NARAL, and Austin and Harper (2019), this law was enjoined and blocked in 2016. Therefore, we coded 2014 as the effective year.
  - Effective on 7/13/1998, Okla. Admin. Code 310:600-9-6 indicates that each abortion facility shall establish a written protocol for the transfer of patients requiring emergency treatment that cannot be provided on-site. The protocol shall include procedures to contact the local ambulance service and expedite the transfer to the receiving hospital. Appropriate clinical patient information shall be provided to the receiving facility. If the attending physician does not have admitting privileges at a local general hospital, the physician shall attest arrangements have been made with a physician having hospital privileges to receive emergency cases. Since the law only requires an agreement with a physician with admitting privileges, we consider this law as the least stringent version of admitting privileges laws, and exclude it from our analyses. However, we still consider the transfer agreement in our analyses because it is not a substitute for the admitting privileges.

- Building regulations: Effective on 7/13/1998, Law 1. 310:600-11-1. Facility design and construction guidelines establish specific requirements on the procedure, operating, and recovery room characteristics. Therefore, we code 1998 as the effective year.

Pennsylvania

- Admitting privileges, hospital proximity regulation, and transfer agreement:
  - 28 Pa. Code § 29.33 requires freestanding clinics to have a written transfer agreement. The agreement shall be entered into with a hospital that is capable of providing routine emergency services. The location of the hospital holding the agreement to supply emergency services shall not be farther than 30 minutes by ambulance from the clinic. It is not clear what the effective year is. However, 1983 corresponds to the last amendment. Also, abortion clinics became required to be licensed in 1983. Then, we assign this year as the effective year for the transfer agreement and hospital proximity regulation, but exclude them from our main analyses because they were implemented in the *Pre-Casey* era.
  - 28 Pa. Code § 555.23(d) requires ASFs to have a written transfer agreement with a hospital that has an emergency and surgical services available, or physicians performing surgery in the ASF shall have admitting privileges at a hospital in close proximity to the ASF, to which patients may be transferred. This law applies to ASFs performing surgical abortions. The last amendment to this law was on 11/22/1999, and we could not trace down the previous version of the law. Then, we assign 1999 as the effective year of the written transfer agreement or admitting privileges requirement.

- Building regulations:
  - 28 Pa. Code § 29.33 also require ASFs to meet some conditions in terms of the building and plant that include specification on corridor doors, elevators, and other passages shall be adequate in size and arrangement to allow a stretcher-borne patient to be moved from each procedure room and recovery room to a street-level exit. As mentioned above, we do not know the effective date of this law. So, we assign 1983 as the effective year.
  - Effective on 6/19/2012, Dec. 22, P.L. 563, No. 122, § 2 requires all facilities performing surgical abortions to operate as ASF. The ASF requirement triggers 28 Pa. Code § 571.1, which requires ASFs to comply with the "Guidelines for Design and Construction of Hospital and Health Care Facilities." Therefore, we cod 2012 as the effective year of these building regulations on surgical abortion facilities operating as ASFs.

Rhode Island

- Building regulations: Department of Health regulations (31-4 R.I. Code R. § 6:30.0, 31-1 R.I. Code R. § 2:3.0, 31-4 R.I. Code R. § 6:21.0), dated 2002 and not amended, created some rules regarding operating, procedure, and recovery rooms. They also include standards for emergency lights and power in the operating room. We assign 2002 as the effective year.
- Transfer agreement:
  - Since 1973, abortions from 15 to 18 weeks of gestation should be performed in freestanding ambulatory surgical centers (FASC). 216-40-10 R.I. Code R. § 5.5 requires FASC to have a written transfer agreement for transferring patients to a nearby hospital when hospitalization is indicated or permit elective surgery only by licensed practitioners who have similar privileges at a nearby licensed hospital and approved by the governing body of the FASC. This rule applies to abortions between 15 to 18 weeks of gestation. Then, we exclude this law from our analyses.
  - Effective on 1/2/2002, 216-20-10 R.I. Code R. § 6.3. 6.3.2, requires making provisions for the prompt and safe transfer of patients for back-up services. We consider this law a plan/protocol, as it does not require a formal transfer agreement with a hospital.

South Carolina

- Admitting privileges and/or transfer agreement:
  - Effective in 1996, S.C. Code Ann. Regs. 61-12.309 requires physicians to have admitting privileges at one or more hospitals that have appropriate obstetrical/gynecological services. However, this law only applies to abortions beyond 14 weeks. So then, we do not include this law in our analyses.
  - The 1976 version of S.C. Code Ann. Regs. 61-12. 205, requires clinics providing second-trimester abortions to have a written agreement with at least one certified general hospital for immediate admission and care of patients with complications. The clinic shall have arrangements for transporting the patient within ten minutes from the clinic to the hospital with which it has an agreement for surgical services for emergency care. From 1976 to 1995, this law focused only on second-trimester abortions. Then, we do not consider its implementation during this period in our analyses. Then, in 1996, amends to chapter 61, Section 62 indicated that the facility shall enter into a signed written agreement with at least one physician board-certified in obstetrics and gynecology who has admitting privileges at one or more local hospitals with OB/GYN services to ensure his/her availability to the staff and patients during all the operating hours.
  - The 1996 version of S.C. Code Ann. Regs. 61-12.305 requires that all staff and/or consulting physicians shall have admitting privileges at one or more local hospitals that have appropriate obstetrical/gynecological services or shall have in place documented arrangements approved by the Department for the transfer of emergency cases when hospitalization becomes necessary. This law then requires all physicians providing abortion to have admitting privileges or to have a plan/protocol to admit patients to a hospital in case of emergency. Also, requiring all physicians to have admitting privileges is more stringent than requiring a signed agreement with at least one

physician, such as S.C. Code Ann. Regs. 61-12. 205 does. Then, we code the most stringent version of admitting privileges laws and a plan/protocol with 1996 as their effective year.

  - The 2003 version of S.C. Code of Regulations R. 61-91.504 indicates that at least one physician at an ambulatory surgical facility (ASFs) should have admitting privileges at one or more local hospitals. However, no law requires abortion facilities to operate as ASFs. S.C. Code Ann. Regs. 61-91.103 establishes that abortions cannot be performed in an ASF unless licensed as an abortion facility. In the event an ASF provides abortions, then being an ASF would trigger the ASF admitting privileges. However, abortion facilities are already required since 1996 to have admitting privileges due to the laws described above. So then, we do not code this law because it only applies to the subset of ASFs providing abortion services.

- Building regulations: As of 1996, all clinics must follow strict building regulation rules. Also, starting in 1996, abortion clinics performing abortions after 18 weeks must be ASCs, which triggers additional rules from 1983. The rules include standards for operating and procedure rooms (S.C. Code of Regulations R. 61-91.2001), emergency power generator (S.C. Code of Regulations R. 61-91.1902), corridors width (S.C. Code of Regulations R. 61-91.2004), and ventilation (S.C. Code of Regulations R. 61-91.2017). We code the first building regulations and ignored the second because they only apply to post-first trimester abortions.

South Dakota

- Transfer agreement: Effective on 12/26/2006, S.D. Admin. R. 44:67:04:07 requires abortion facilities to establish and implement policies and procedures for emergency care and arrange for transport to a licensed hospital sufficiently close to provide prompt care to the facility's patients if needed. We consider this law a plan/protocol since it does not require a formal written agreement with a hospital to transfer patients. Therefore, we assign 2006 as the effective year.

- Building regulations: A set of rules effective on 11/26/2006 establish different building regulations for abortion facilities such as standards for recovery rooms (S.D. Admin. R. 44:67:05:03), procedure rooms (S.D. Admin. R. 44:67:05:02), ventilation (S.D. Admin. R. 44:73:02:13), lighting (S.D. Admin. R. 44:73:02:14). Then, S.D. Admin. R. 44:73:02:03, effective on 10/13/2015, requires written procedures for cleaning and sterilization and a separate clean and soiled utility room. Since the first building regulations started in 2006, we assign this year as the effective year. We do not account for the 2015 building regulations because they are too recent to evaluate.

Tennessee

- Admitting privileges: In 2012, it was added to Tenn. Code § 39-15-202 that a physician performing surgical abortions must have admitting privileges at a licensed hospital in the county where the abortion is performed or in an adjacent county. This law was enjoined in 2017. Since this law applies to surgical abortions, we include it in our analysis and code 2012 as its effective year.

- Transfer agreement: In 2015, surgical abortion facilities performing more than 50 surgical abortions in a calendar year became required to operate as ambulatory surgical treatment centers (ASTC) (Tenn. Code § 68-11-201). Tenn. Comp. R. & Regs. 1200-08-10-.05 indicates that ASTC must have a written transfer agreement with a local hospital. So then, since the ASTC requirement for surgical abortion facilities triggers the written transfer agreement, we assign 2015 as its effective year.

- Building regulations: Tenn. Comp. R. & Regs. 1200-08-10-.06 indicates that ASCTs shall provide one or more surgical suites. It also indicates ASTCs should have separate areas for waiting rooms, recovery rooms, and treatment/examining rooms. The effective date of this law is 8/22/1977. However, surgical abortion facilities became required to comply with it until the ASCT requirement was enforced in 2015. Then, we assign 2015 as the effective year. This law was enjoined in 2018.

- Both the transfer agreement and the building regulations are too recent to evaluate, so we do not include them in our analyses.

Texas

- Admitting privileges, hospital proximity regulation, and transfer agreement:
  - Effective on 8/13/1998, 25 Tex. Admin. Code § 139.56 require abortion facilities to have a readily accessible written protocol for managing medical emergencies and transferring patients requiring further emergency care to a hospital. In addition, the facility shall ensure that the physicians who practice at the facility have admitting privileges or have a working arrangement with a physician(s) who has admitting privileges at a local hospital to ensure the necessary back-up for medical complications. We code the admitting privileges as the least stringent version because it allows an arrangement with an outside physician with admitting privileges in a hospital. We only consider the written protocol as a plan/protocol because it does not imply a formal written agreement with a hospital. Therefore, we assign 1998 as the effective year for both restrictions. However, we exclude them from the analyses because they are below the minimum stringency level in our analyses.
  - Effective on 11/1/2013, Tex. Health & Safety Code Ann. §171.0031 requires physicians performing or inducing abortions to have active admitting privileges at a hospital located not further than 30 miles from the location at which the abortion is performed or induced. This law was blocked in 2016 after the Supreme Court decision in *Whole Woman's Health v. Hellerstedt*. Therefore, we assign 2013 as the effective year for both requirements.

- Building regulations: 25 Tex. Admin. Code § 139.48 establishes physical and environmental requirements for licensed abortion facilities. Among the different requirements, it includes standards for recovery rooms. 25 Tex. Admin. Code § 135.52 specifies standards for the electrical system. 25 Tex. Admin. Code § 135.11 requires written policies and procedures for decontamination, disinfection, sterilization, and storage of sterile supplies. All these laws became effective on 6/18/2009. Therefore, we code 2009 as the effective year for building regulations.

- The following regulations are not included in our analyses as they only apply to post-first trimester abortions. The 2004 "Women's Right to Know" Act (Tex. Health & Safety Code § 171.004) required abortions beyond 16 weeks to take place in an ASC. This triggered a number of requirements (25 Tex. Admin. Code § 135.52), such as staff training and facility safety and cleanliness, but did not include any of the building (or other) requirements coded in this paper. As of 2009, (25 Tex. Admin. Code § 135.11) additionally required ASCs to have a transfer agreement, though this continued to apply only to clinics providing 16 weeks+ abortions.

Utah

- Admitting privileges, transfer agreement, and building regulations:

  We faced difficulties in tracking down the potential laws that require the implementation of TRAP laws. However, based on information from NARAL, it seems that early requirements applied only to second-trimester abortions.

  The first versions of admitting privileges laws, transfer agreements, and hospital proximity regulations can be traced down to the 1985 version of Utah Admin. Code r. R432-600. In the 1991 version of Utah Admin. Code r. R432-600 it is mentioned that clinics should follow the 1987 Guidelines for Design and Construction of Health Care Facilities, which dictated what building regulations should be implemented. However, we could not trace down the 1987 version of these guidelines. We code these regulations as implemented in 1985. However, all these early regulations seem to only apply to post-first-trimester abortions. They were also implemented in the *pre-Casey* era. So, we do not include them in our main analyses.

- We also code a version of the law that allows for either admitting privileges laws or transfer agreement regulations, which seems to only apply to the second-trimester before 2011, as implemented in 2010. However, we do not include them in our analyses.

  The 2011 version of Utah Admin. Code r. R432-600 updates to the 2010 Guidelines for Design and Construction of Health Care Facilities. These guidelines include building regulations for recovery room, procedure room size, sterility room, and doorway and hallway widths. These building regulations apply to all abortion facilities. This version of the administrative code also includes a requirement for admitting privileges to a hospital within a specified distance of the facility's medical director or an alternative transfer agreement. Therefore, we assign 2011 as the effective year for admitting privileges or transfer agreement, hospital proximity regulations, and building regulations.

- In 2017, the admitting privileges were enjoined, and the transfer agreement requirements changed to a plan/protocol that only applies to second-trimester providers. However, we do not consider this change in our analysis because it runs up to 2016.

Virginia

- Transfer agreement and building regulations:

  Effective in 2012, Va. Code § 18.2-73 requires second-trimester abortions to be provided in a hospital. 12 Va. Admin. Code 5-410-1240 requires outpatient surgical hospitals to have a written agreement with a general hospital to ensure that any patient receives needed emergency treatment. Then, triggered by Va. Code § 18.2-73, second-trimester abortion facilities must comply with a written transfer agreement. We code 2012 as the transfer agreement effective year. However, we do not include this law in our analyses because it only applies to second-trimester abortions.

  Effective on 6/20/2013, 12 Va. Admin. Code 5-412-370 requires all abortion facilities to comply with the Virginia Uniform Statewide Building Code. It also requires them to comply with Part 3 of the 2010 Guidelines for Design and Construction of Health Care Facilities of the Facilities Guidelines Institute, which establishes some room requirements and provides external guidelines and standards. Therefore, we code 2013 as the effective year of these building regulations.

Wisconsin

- Hospital proximity regulations and transfer agreement: Effective on 11/1/1976, Wis. Admin. Code, MED. § 11.04 requires abortion facilities providing abortions within the first 12 weeks of gestation to make arrangements with a hospital for admission of patients needing hospital care. Such hospital shall be located sufficiently near the facility so that the patient could be transferred to and arrive at the hospital within 30 minutes of the time when hospitalization appears necessary. Since this law was implemented in the *pre-Casey* era, we do not include it in our main analyses.