ESSAYS ON THE ECONOMICS OF REPRODUCTIVE HEALTH CARE

A Dissertation

by

MAYRA BELINDA PINEDA TORRES

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Chair of Committee,	Jason M. Lindo
Committee Members,	Steve Puller
	Joanna Lahey
	Mark Hoekstra
	Laura Dague
	Kelly Jones
Head of Department,	Steve Puller

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ABSTRACT

Reproductive health care is the ability to have a satisfying and safe sex life, the capability to reproduce, and the freedom to decide if, when, and how often to do so. Access to reproductive health care has improved due to technological advances and information dissemination in the last few decades. However, this access is still heterogeneous and, therefore, not everyone has the same ability to use reproductive technology and control their fertility choices. In this dissertation, I use quasi-experimental methods and non-experimental data to study the health and economic implications of access to reproductive controls in the US.

In chapter 2, we study the long-run impacts of access to oral contraception and abortion on women's education and earnings. We find evidence that access to these reproductive controls improved high school graduation among Black women. We also observe increases in women's probability of working in a Social Security-covered job in women's 20s and 30s associated with early access to oral contraception and abortion, but we find no evidence of positive effects on women's earnings in their 50s.

In chapter 3, we evaluate the effects of a Tennessee law enacted in 2015 that requires women to make an additional trip to abortion providers for state-directed counseling at least 48 hours before obtaining an abortion. We find that the introduction of this policy caused increases in the share of abortions obtained during the second trimester, and we find inconclusive evidence of changes in overall abortion rates.

Finally, in chapter 4, we study how women's exposure to targeted regulations to abortion providers (TRAP laws) in adolescence affects their fertility and educational attainment. We find that the exposure to these policies increases Black teen births in states that implemented these policies relative to states without such restrictions. We offer evidence that these impacts are driven by reductions in abortion access, abortion use, and contraception use among Black teens. We further document that adolescent exposure to TRAP laws before age 18 reduces the probability of initiating and completing college.

DEDICATION

A mis padres, Juan Pineda González y Laura Torres Villa. Siempre en mi mente y en mi corazón.

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TABLE OF CONTENTS

Page

LIST OF FIGURES

FIGURE Page

LIST OF TABLES

TABLE Page

1. INTRODUCTION

"When sexual and reproductive health needs are not met, individuals are deprived of the rights to make crucial choices about their bodies and futures, with cascading impacts on their families' welfare and future generations..." (United Nations Population Fund, 2016).

Reproductive health care is the ability to have a satisfying and safe sex life, the capability to reproduce, and the freedom to decide if, when, and how often to do so. Technological advances and information dissemination have improved access to reproductive health care. However, this access is heterogeneous and, therefore, not everyone has the same ability to use reproductive technology and control their fertility choices.

In these chapters, I use quasi-experimental methods and non-experimental data to study the health and economic implications of access to reproductive controls in the US. The US reproductive health care landscape has faced massive changes in the last few decades. However, many of these changes are too recent to evaluate their causal impacts. Nonetheless, the economic effects of historical policies can inform us of the merits and costs of subsidizing or restricting access to reproductive health care today.

In the second chapter, I present a study that revisits the evidence provided in previous studies on the economic impacts of the expansion of access to oral contraception to unmarried women in the 1960s. We extend this evidence by evaluating the effects of both access to oral contraception and legal abortion on longer-run outcomes. Our findings on educational attainment indicate improvements in high school graduation among Black women, aligning with prior work but are not statistically significant. The findings for earnings indicate increases in the probability of working in a Social Security covered job in women's 20s and 30s associated with early access to contraception and abortion, and we find no evidence of positive effects on women's earnings in their 50s.

Some of the policies implemented in the last decades restrict access to reproductive health

care services. Since abortion became nationwide legal in 1973, those states in which opposition to abortion has been historically strong have implemented different policies restricting access to abortion services. Demand-side restrictions—those targeting patients—have been implemented since the 1970s.

In the third chapter, I present a study that investigates the effects of a demand-side restriction—a 2015 Tennessee law requiring women to have in-person counseling and wait at least 48 hours before the abortion procedure on abortion timing and abortion rates. Our findings indicate that the introduction of the mandatory waiting period caused an increase in the share of abortions obtained during the second trimester. Our analysis examining overall abortion rates is less conclusive but suggests a reduction caused by the waiting period.

Restrictions on the supply side of the abortion market—the clinics—have become more common in the last few years, becoming the fastest-growing abortion policy since 2010. These policies target the operation and physical structure of abortion facilities and clinics.

In the fourth chapter, I present a study on the impacts of exposure to targeted regulations of abortion providers (TRAP laws) during adolescence on teen fertility and educational attainment. We find that TRAP laws increase Black teen births, while they do not change existing trends in births for White teens. We provide further evidence that these effects operate through decreases in abortion access, abortion use, and contraceptive use. We also document Black women exposed to TRAP laws before age 18 are less likely to initiate and complete college.

2. LEGAL ACCESS TO REPRODUCTIVE CONTROL TECHNOLOGY, WOMEN'S EDUCATION, AND EARNINGS APPROACHING RETIREMENT

2.1 Introduction

The landscape for reproductive health care in the United States has undergone massive changes in recent years. In 2017, the set of employers and insurers who are exempt from the Affordable Care Act's contraceptive coverage mandate was broadened to include those with moral objections. In 2019, Title X rules were changed to deny funding to family-planning providers that refer patients for abortion, which could restrict women's access to both contraception and abortion care. At the same time, several states, including Delaware, Massachusetts, South Carolina, and Washington, have launched major initiatives to expand access to the full range of contraceptives, including intrauterine devices and implants, which can be difficult for some women to obtain because of costs and a lack of trained providers. A variety of state restrictions have made it harder for women to access abortion, including restrictions that have caused abortion clinics to close. Telemedicine for consultation and/or medication abortion has expanded access in some states. Questions about the economic effects often come up when the desirability of such policies is discussed. The economic effects are relevant to considering the merits of subsidizing access and to considering the costs imposed by regulations that limit access.

What do historical changes in contraception and abortion access tell us about the long-run effects of such changes? In this study we investigate this question using data from the Health and Retirement Study and an identification strategy that leverages variation in exposure to legal changes in access across cohorts born in the same states during the 1960s and 1970s. We follow the methodology of Bailey et al. (2012) (hereafter "BHM") who used the National Longitudinal Survey of Young Women and documented significant increases in contraception use at ages 18-20 associated with unmarried women's ability to consent for contraception at such ages. They also documented increased educational attainment and increased earnings in women's 30s and

40s associated with this confidential access to contraception. Our analysis revisits the effects on education and earnings. We also investigate the sensitivity of the estimated effects to the legal coding and control variables used in Myers (2017)'s study of the effects on fertility and marriage.

The results for educational attainment align with prior work but are not statistically significant. The results for earnings indicate increases in the probability of working in a Social Security (SS) covered job in women's 20s and 30s associated with early access to contraception and abortion, but we find no evidence of positive effects on women's earnings in their 50s.

2.2 Data and Methodology

Our analyses use restricted-use data from the Health and Retirement Study (HRS).[1] HRS is a longitudinal survey of Americans over age 50 and their spouses. The study interviews approximately 20,000 respondents every two years on subjects like employment, health care, housing, assets, pensions, and disability. We use restricted-use data from HRS that includes individuals' earnings histories from 1951-2013 based on information provided by the Social Security Administration. The HRS has collected information on six groups of birth cohorts across multiple survey waves since they began conducting surveys in 1992. Our analysis of educational outcomes follows the approach used in Goldin and Katz (2002), Bailey (2006), Bailey (2009), Guldi (2008), Hock (2008), and Myers (2017) who analyze the effects of legal access to contraception and abortion on women's marital and fertility outcomes using within-state-across-cohort variation. Following Myers (2017), our analysis of education focuses on women born 1935-1958 and considers two measures of access to each reproductive control method (contraception and abortion): the method being legal and young unmarried women being able to provide legal consent ("pill consent" or *PiCon*, "abortion consent" or *AbCon*), and the method being legal but young unmarried women not being able to provide legal consent ("pill legal" or *PiLeg*, "abortion legal" or *AbLeg*). We measure a woman's exposure to legal access based on the legal circumstances in her state of residence between the ages 18-20, allowing

[1]The HRS (Health and Retirement Study) is sponsored by the National Institute on Aging (grant number NIA U01AG009740) and is conducted by the University of Michigan.

variables to range from zero to one for the proportion of years of legal access during these years. We infer a woman's state of residence at these ages based on her state of residence at age 10 for the vast majority of women for whom this is available and based on state of birth for the remainder. Our regression model, identical to Myers (2017), is as follows:

$$Ed_{ics} = PiLeg_{cs}\gamma + PiCon_{cs}\beta + AbLeg_{cs}\theta + AbCon_{cs}\delta + \eta_c + \psi_s + X_{ics}\lambda + \epsilon_{ics} \tag{2.1}$$

where Ed_{ics} measures the educational attainment for woman i born in cohort c who lived in state s as a youth, the legal access measures are as defined above, η_c are cohort fixed effects, ψ_s are state fixed effects, and X_{ics} includes a rich set of additional controls including state-linear cohort trends.[2] In constructing standard error estimates, we allow the error term ϵ_{ics} to be correlated across cohorts from the same state. In addition to reporting estimates based on Myers (2017)'s legal coding, we also report estimates that use BHM's legal coding for contraception access.[3]

Our analysis of women's economic outcomes across the life-cycle follows BHM. This methodology also leverages variation in access across cohorts of women from the same state but focuses on variation in young women's ability to provide consent to access contraception and extends the model to assess the effects on women's outcomes that are measured at different ages. Specifically, we estimate

$$\begin{aligned} Y_{iacs} = &\sum_g \beta_g PiCon_{cs} D_{g(a)} + \sum_g \gamma_g EAA_{cs} C50_c D_{g(a)} + \\ &\sum_g \theta_g PiCon_{cs} EAA_{cs} C50_c D_{g(a)} + \delta lnDist_s C50_c + \\ &\gamma_{g(a)} + \theta_s + \psi_c + \epsilon_{iacs} \end{aligned} \tag{2.2}$$

[2]The additional control variables include race, ethnicity, the interaction of "early pill legal" and "abortion legal" and the interaction of "early pill legal" and "early abortion legal." They also include exposure (measured as the fraction of years from age 18-20) to: state abortion reforms, which were enacted in 13 states prior to *Roe vs. Wade* and permitted abortion under limited circumstances; state policy permitting no-fault divorces; state equal pay law prior to the enactment of federal legislation in 1963; and state fair employment practices act (FEPA) prohibiting racial discrimination in hiring, discharge, and compensation.

[3]BHM's coding is based on Bailey et al. (2011).

where g corresponds to 5-year age groups (20-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, and 55+), $D_{(g(a))}$ is an indicator if an observation is in age group g based on its corresponding age a, EAA_{cs} is an indicator for early legal access to abortion (defined as residing in an early-legalizing state[4] before age 21), $C50_c$ is an indicator for being born in 1950 or later and thus potentially being affected by abortion legalization before age 21 for women residing in an early legalizing states, and the other variables are defined as in Equation 2.1. For this analysis we follow BHM by considering women born no later than 1954.[5]

Two notable difference between the models characterized by Equation 2.1 and Equation 2.2 are that the latter model: (i) does not distinguish between legal access to abortion and minors' ability to consent for abortion and (ii) does not consider the degree to which there may be effects of legal access when these women are themselves older. We intend to examine these possibilities in future work. In this study, we replicate BHM, extend the analysis to consider effects at older ages, and we examine the sensitivity of the estimates to using legal coding and additional control variables based on Myers (2017).[6] When we do so, we measure early abortion access when women were age 18-20 based on whether unmarried of such ages could consent to abortion according to Myers (2017)'s coding.

2.3 Results

2.3.1 Educational attainment

Table A.1 reports our estimated effects on years of education (up to 17) based on Equation 2.1. Consistent with estimates reported in BHM, and previously in Goldin and Katz (2002) and Hock

[4]Early-legalizing states are states that legalized in 1969-1971: Alaska, California, the District of Columbia, Hawaii, New York, and Washington.

[5]BHM was restricted to using data on from the 1943-1954 cohorts because those were the cohorts covered by the National Longitudinal Survey of Young Women (NLS-YW), which was first conducted in 1968 and focused on 5,159 women ages 14 to 24 at the time. The results reported in the tables in this paper are based on an expanded set of cohorts, 1930-1954. These results are consistent with our analysis of the 1943-1954 cohorts, which produce estimates that are slightly smaller in magnitude but with much larger standard errors.

[6]These additional control variables include indicators for the race and ethnicity of the respondent, state-linear cohort trends, and measures of the fraction of years of exposure (from age 18-20) to: state abortion reforms and consent to state abortion reforms (enacted in 13 states prior to Roe vs. Wade and permitted abortion under limited circumstances); state policy permitting no-fault divorces; state equal pay law prior to the enactment of federal legislation in 1963; and state fair employment practices act (FEPA) prohibiting racial discrimination in hiring, discharge, and compensation.

(2008), our estimates suggest that both legal access and being able to consent for contraception from age 18-20 is associated with increased levels of education. With that said, we note that these estimates are only marginally statistically significant when we use BHMs' coding of legal access to contraception (Column 1) and that the estimates are somewhat smaller and are not statistically significant when we use Myers (2017)' coding (Column 2). Our analysis of black women also suggests positive effects of greater legal access to reproductive control technology, and to legal access to abortion in particular (columns 3 and 4).

2.3.2 Earnings

We examine earnings using two types of data available in the HRS. Earnings based on social security (SS) records and earnings based on HRS surveys. The former has the advantage of a large sample size covering a very broad set of age groups; however, it will vastly understate earnings for women working in jobs that are not covered by SS. For this reason, we use this measure simply to evaluate whether a woman had any earnings in a SS-covered job in a given year, which is measured without error.[7] In 1981, 90 percent (98 million) of all wage and salary workers and 62 percent (13 million) of workers in the public sector were covered under SS (Heeringa and Connor, 1995). We use HRS's survey-based measure of earnings to evaluate women's earning levels in their 50s.[8]

Table A.2 reports the estimated effects on whether a woman is working in a SS-covered job. Column 1 shows the results following BHM's methodology and Column 2 shows the results using Myers (2017)' coding and the additional control variables described in Footnote 6. As a whole, these estimates indicate that early legal access to contraception increased women's probability of working in a SS-covered job, particularly in their late 20s and early 30s. While any such effects may reflect increased labor force participation, it could also arise from substitution from SS-uncovered jobs to SS-covered jobs.

The results also indicate that gaining early legal access to abortion is similarly associated with

[7] If instead evaluated earnings levels based on this measure, it could cause us to understate the economic benefits of legal access to reproductive control technology if such access led women into higher paying jobs that are not covered by Social Security.

[8] The analysis includes younger women but we only report estimates for women in their 50s, because younger women are only included in the HRS if they are married to someone who is older than 50.

an increased probability of working in a SS-covered job. The estimates again suggest effects for women in their 20s and early 30s. As discussed above, an important caveat to these results is that the estimates could be picking up long-run effects of the conditions when a woman was 18-20 or the effects of having access at older ages.[9] Table A.3 shows estimates focusing on the log of women's hourly wages. As a whole, the estimated effects on this outcome indicate no statistically significant effects on women's earnings in their 50s. These results are not inconsistent with BHM who find positive effects of early access to the pill when women were in their 30s and 40s. We also do not find evidence of statistically significant positive effects if we evaluate hourly wages (not taking the logarithm), hourly wages excluding zeroes, weekly wages (taking the logarithm or not, excluding zeroes or not), or if we restrict the sample to the 1943-1954 cohorts (as in BHM).

2.4 Conclusions

Given major gaps in access to contraception and abortion care, understanding the economic effects of such access is likely to continue to be relevant to policy. In this paper, we build on the knowledge base by evaluating how changes in access resulting from policy changes in the 1960s and 1970s affected educational attainment and women's very-long run earnings. We hope that future work will go deeper in assessing the robustness of these results.

[9]Estimated effects of both contraception access and abortion access are slightly smaller in magnitude with much larger standard errors if we instead analyze the 1944-1954 cohorts (like BHM) instead of the 1930-1954 cohorts.

3. NEW EVIDENCE ON THE EFFECTS OF MANDATORY WAITING PERIODS FOR ABORTION

3.1 Introduction

Currently, 27 states (shown in Figure B.1) require women to wait 18-to-72 hours between pre-abortion consultation and the actual procedure. Fourteen of these states require women to make an additional trip to the facility so that this consultation can be done in person (Guttmacher Institute, 2016b). Proponents of mandatory waiting periods (MWPs) argue that they ensure women receive information about pregnancy and abortion and that they have ample time to weigh their options before deciding to terminate a pregnancy. Others have argued that women requesting abortion are already making informed decisions without the MWP; therefore, these laws impose an unnecessary burden that has the potential to delay or prevent women from accessing abortion care. In particular, women may have difficulty making arrangements for transportation, time off work, child care, or paying for any additional costs associated with the consultation appointment.[1] In addition, requiring a separate consultation appointment for all women seeking an abortion may present logistical challenges for providers thereby reducing the number of women they can serve—as a result, women may experience difficulties obtaining an appointment from their preferred provider.[2]

Together, these issues suggest that a MWP may cause delays for women seeking abortions because of: (i) the mandatory wait time after their first appointment; (ii) any additional wait time for appointments due to capacity constraints at facilities; (iii) and any extra time it takes them to make arrangements for another trip to a facility. Any such delays are particularly important

[1] See Lupfer and Silber (1981); Althaus and Henshaw (1994); Karasek et al. (2016); Roberts et al. (2016); Sanders et al. (2016); and White et al. (2016).

[2] For women requesting an abortion at the end of their first trimester, the MWP could represent an additional burden because they may be in their second trimester by the time they can get an abortion. Consequently, they might not be able to have their preferred type of abortion (Roberts et al., 2016), since medical abortion is less effective after the first trimester. They are also likely to be referred to another clinic or to face difficulties finding an abortion provider because fewer providers are available for women at later stages of pregnancy (Drey, Foster, Jackson, Lee, Cardenas, and Darney, 2006; Jones and Jerman, 2014). This could prevent them from terminating their pregnancies.

because they can limit the types of procedures available which depend on gestational age, and because monetary costs and health risks tend to be higher as gestational age increases.[3] The same set of issues suggest that a MWP may *prevent* some women from obtaining abortions if: (i) they are unable to make arrangements for an additional trip; (ii) the delays push them beyond the maximum gestational age for an abortion; or (iii) the consultation and/or waiting period changes beliefs or preferences. On the other hand, such effects could be mitigated to some degree if forward-looking women who know they are pregnant and know they may want an abortion begin to make arrangements more quickly in anticipation of these obstacles. It is also possible that barriers to abortion access could change other behaviors that mitigate the risk of pregnancy, such as the use of contraception, but prior work has found minimal evidence of such effects (Fischer, Royer, and White, 2018).

Ultimately, understanding the effects of MWPs on abortion timing and abortion rates requires careful empirical analysis. Notably, a 2009 review of the evidence found just seven studies of the impacts of MWPs on abortion rates and/or abortion timing, and only four of these used approaches that are typically thought to be credible for identifying causal effects (Joyce, Henshaw, Dennis, Finer, and Blanchard, 2009). Those four studies focused on the effects of MWPs in early-adopting states through the mid-1990s. To our knowledge, no new papers estimating the causal effects of MWPs have been written since. Thus, the evidence base arguably has become dated.

More recent evidence on the effects of MWPs is particularly important because these effects may be different in today's context, given the major changes in the landscape for women seeking an abortion. One major change is that the number of abortion providers has declined dramatically in many states. Thus, a restriction requiring women to make a second trip to a clinic may involve more travel today than it would have in the past. Another key difference is that the nearest out-of-

[3]The counseling visit may represent 11 percent of the actual cost of the abortion and 9 percent of the total cost of the two appointments (Roberts et al., 2016). Procedures performed after 12 weeks of pregnancy pose higher risks of medical complications and mortality than abortions performed earlier (Althaus and Henshaw, 1994; Drey, Foster, Jackson, Lee, Cardenas, and Darney, 2006). The type of procedure used to interrupt pregnancy depends on the stage of pregnancy. Medical abortion is most effective within the first nine weeks of gestation (UCLA Obstetrics and Gynecology (2017)), and its success rate decreases as gestational age increases (Rørbye, Nørgaard, and Nilas, 2004). If a woman delays abortion, she becomes more likely to require a surgical abortion which can be more than twice as expensive as a medical abortion (Jones and Jerman, 2014; Roberts, Turok, Belusa, Combellick, and Upadhyay, 2016)

state provider likely would not have had a MWP for women in early-adopting states. Today it is less likely that a woman can avoid the MWP by driving to her nearest out-of-state provider; most states have a MWP, and these states are clustered geographically (as shown in Figure B.1). These two features of the current landscape imply that many women would have to travel farther today to avoid their states' MWP laws than in years past. While these factors suggest that MWPs may have a greater impact today, it is possible that their effects may be smaller, perhaps because of changes in access to transportation, improved information on navigating the process of obtaining an abortion, or for other reasons. Regardless, given the massive changes in the abortion landscape since the 1990s, including a surge in legislation since 2011,[4] we believe it is important to expand on the existing base of knowledge by documenting the effects of MWPs enacted in recent years.

Towards this end, we evaluate the effect of Tennessee's MWP, which went into effect in 2015 and requires women to wait at least 48 hours after getting in-person counseling from a physician before they can obtain an abortion. With the enactment of this law, Tennessee joined its neighboring states with similar laws on the books. To evaluate causal effects, we use both a difference-in-differences approach and a synthetic control design comparing changes over time for residents of Tennessee to changes over time in two sets of comparison states. We focus on the share of women obtaining abortions in the second trimester of their pregnancy and on abortion rates.

Our difference-in-differences and synthetic-control estimates indicate that Tennessee's MWP caused a 53–69 percent increase in the share of abortions obtained in the second trimester, completely or almost completely closing the pre-existing gap between women residing Tennessee and women living in the comparison states (approximately 5 percentage points). We highlight the statistical significance of this estimate through permutation tests indicating that no state in the comparison group experienced such a large increase relative to other states. Our analyses of the second-trimester abortion rate indicate that the MWP increased the number of number women having such abortions, though these estimates are not always statistically significant at

[4]States passed more abortion restrictions from 2011–2013 than in the entire previous decade (205 versus 189). See Nash et al. (2013).

conventional levels. Our analysis of the overall abortion rate yields suggestive evidence of reductions caused by the MWP though these analyses should be viewed with more caution because of limited statistical power. We also find suggestive evidence that effects on delays are larger in relatively disadvantaged counties.

Motivated by our findings indicating that Tennessee's MWP led to significant delays in abortion, we provide a number of back-of-the-envelope calculations of the additional monetary costs of the MWP to provide these findings some additional context. We conservatively estimate that the additional consultation appointment increased women's monetary costs by $173–256 accounting for fees charged by providers, transportation costs, and lost wages or childcare. We also estimate that a delay of one week, which can alter the procedure type and the clinics available to a woman, can increase the monetary cost of obtaining an abortion by up to a total of over $502.

3.2 Prior research on MWPs

Descriptive studies provide some strong reasons to believe that MWPs, particularly those that substantially increase costs, may delay and/or prevent women from obtaining abortions. Survey data indicate that among women who would have preferred to have their abortions earlier, 60 percent report that delays occurred because it took time for them to make arrangements (Finer, Frohwirth, Dauphinee, Singh, and Moore, 2006). This is perhaps unsurprising given that low-income women make up a large share of all women seeking abortions. In 2014, half had incomes less than the federal poverty line, and three-quarters had incomes less than 200 percent of the poverty line (Jones and Jerman, 2017).

Surveys of women having to make additional trips to an abortion clinic because of mandatory waiting periods highlight the challenges they faced. In Utah, 47 percent of women obtaining abortions reported negative effects due to lost wages from needing to take extra time off work, 30 percent reported negative effects due to increased transportation costs, 27 percent reported negative effects due to lost wages by family or friends, and 33 percent reported that they had to disclose their abortion to someone who they would not have told otherwise (Sanders, Conway, Jacobson, Torres, and Turok, 2016). Women in Louisiana stated similar challenges, reporting concerns about missing

work, encountering traffic or bad weather, thinking their car would not be able to make the trip, and having to lie about their absence to their parents or partners (Carroll and White, 2020). Some of these women also reported that challenges making arrangements resulted in them being unable to obtain their preferred abortion method and/or made them worry that they would have to continue an unwanted pregnancy. Notably, these surveys of women's experiences with mandatory waiting periods do not include women who were unable to obtain abortions and, thus, likely understate the burdens imposed on women.

As noted earlier, just a few prior studies have evaluated the *causal* effects of MWPs on abortion rates and/or abortion timing using commonly accepted approaches to estimating causal effects: Joyce et al. (1997); Joyce and Kaestner (2000); Bitler and Zavodny (2001); and Joyce and Kaestner (2001).[5]

Bitler and Zavodny (2001) is the largest of these studies in scope, examining a wide variety of abortion restrictions and using annual abortion data for nearly all U.S. states from 1974–1997. Their estimates—based on a generalized difference-in-differences model that controls for state and year fixed effects—indicate that MWPs increase the proportion of abortions performed in the second trimester by 2.3 percentage points and increase the rate of second-trimester abortions by 41-percent. They do not find evidence of reductions in overall abortion rates. As noted in Joyce et al. (2009), the vast majority of the MWPs analyzed in Bitler and Zavodny (2001) did not require an additional clinic visit. Therefore, those findings may mask more severe effects of MWPs that do require an additional clinic visit.

Consistent with this notion, causal studies of Mississippi's MWP, which went into effect in 1992 and required an additional clinic visit, have found larger effects on the proportion of abortions obtained in the second trimester and stronger evidence of reductions in abortion overall (Joyce, Henshaw, and Skatrud, 1997; Joyce and Kaestner, 2000; Joyce and Kaestner, 2001). Further supporting the idea that requiring women to travel more to obtain an abortion has significant effects

[5]Other studies reviewed in Joyce et al. (2009) are described as lacking a comparison group to evaluate how outcomes would have changed in the absence of the MWP (Althaus and Henshaw, 1994); estimating effects primarily based on cross-sectional variation (Medoff, 2007); or inappropriately controlling for lagged abortion rates (Meier, Haider-Markel, Stanislawski, and Mcfarlane, 1996.)

on abortion rates, several recent studies have demonstrated that increases in travel distance to the nearest provider significantly reduce abortion rates (Quast, Gonzalez, and Ziemba, 2017; Fischer, Royer, and White, 2018; Lindo, Myers, Schlosser, and Cunningham, 2019).

As a whole, this body of work suggests that when they require women to make an additional trip to their provider, MWPs increase the proportion of abortions obtained in the second trimester and that they can reduce abortion rates overall. That said, this summary statement is based solely on analyses that find large effects of Mississippi's MWP (Joyce, Henshaw, and Skatrud, 1997; Joyce and Kaestner, 2000; Joyce and Kaestner, 2001), a study pooling together MWPs that do and do not require additional travel that finds more moderate effects (Bitler and Zavodny, 2001); and another analysis finding little evidence that South Carolina's MWP affected adolescents (Joyce and Kaestner, 2001).

3.3 Background on Tennessee's MWP

In May 2015, Tennessee's Gov. Bill Haslam approved a law that required women to wait at least 48 hours after counseling with a physician before they could obtain an abortion.[6]

As a result, Tennessee joined 26 states that already had a mandatory waiting period, including all of its neighboring states. Thirteen out of these 26 states required in-person counseling, necessitating an additional trip to the clinic, like the MWP in Tennessee.[7] Some of Tennessee's neighboring states had laws that required women to wait 24 hours (Arkansas, Georgia, Kentucky, Mississippi, Missouri, North Carolina, and Virginia), and one required 48 hours (Alabama). Only

[6]That 48-hour period excludes the day on which the information was provided. For the consultation appointment, the law requires physicians to inform a woman requesting an abortion on the following: 1) that according to the physician's best judgment, the woman is pregnant; 2) the weeks elapsed from the probable time of conception of her unborn child; 3) that if more than 24 weeks have elapsed from the time of conception, the child may be capable of surviving outside the womb; 4) that abortion may constitute a major surgical procedure; 5) information on the public and private agencies and services available to assist her during pregnancy and after the birth of the child, if she decided not to have an abortion, and whether women wish to keep the child or place the child for adoption; 6) that there are risks associated with her pregnancy and childbirth, and the abortion or child delivery technique to be employed, as well as a general description of the medical instruction to be followed subsequent to the abortion or childbirth in order to ensure her safe recovery; and 7) the existence of a two-day waiting period (2010 Tennessee Code. Chapter 15, Part 2, 39-15-202).

[7]These states are Arizona, Arkansas, Indiana, Kentucky, Louisiana, Mississippi, Missouri, Ohio, South Dakota, Texas, Utah, Virginia, and Wisconsin. Information on state MWPs over time are based on the Guttmacher Institute's "Counseling and Waiting Periods for Abortion" accessed at different points in time via Wayback Machine.

Alabama, North Carolina, and Georgia had MWPs that did not require an additional visit to the clinic.[8] Figure B.2 depicts the MWPs across the United States before the enactment of Tennessee's MWP. It demonstrates that Tennessee was one of the few states in the South without a MWP for abortion.[9]

In addition to MWPs, the states in this region (Alabama, Arkansas, Georgia, Kentucky, Mississippi, Missouri, North Carolina, and Virginia) have many other abortion regulations. For instance, all require abortions to be performed by a licensed physician, and some require second-trimester abortions to be performed in a hospital (Alabama, Kentucky, Missouri, North Carolina, Tennessee, and Virginia). All of these states prohibit abortion during the third trimester, except in cases of life or health endangerment. All restrict public funding to abortions except in cases related to life endangerment, rape, or incest, and only Kentucky and Missouri allow private insurance to cover abortion. Except for Alabama, all allow providers to refuse to perform an abortion, and all require parental consent or notice for minors (Guttmacher Institute, 2016a). Except for Georgia, all of these states have imposed so-called targeted regulation of abortion providers ("TRAP laws") (Guttmacher Institute, 2016d). Therefore, the circumstances are relatively challenging for women seeking abortions—and for abortion providers—in the setting, we study.[10,11]

[8] Virginia did not require in-person counseling for women living more than 100 miles from an abortion provider.

[9] The latest changes to MWP laws in nearby states are: Alabama increased the MWP from 24 to 48 hours in 2014; Arkansas increased its MWP from 48 to 72 hours in April 2019; Georgia enacted a 24-hour mandatory period in 2012; in Kentucky, a 24-hours MWP law was amended in 1998; Mississippi imposed a 24-hour MWP in 1992; Missouri increased the MWP from 24 to 72 hours in 2014; North Carolina voted an increase from 24 to 72-hour MWP in 2015, and Virginia amended the law that requires 24-hour abortion delay in 2003. See Guttmacher Institute (2016a) for more information.

[10] Since 2010, the ACA has allowed states to broaden Medicaid eligibility, creating a foundation of coverage for low-income Americans with incomes up to 138 percent of the federal poverty level (FPL). With the elimination of categorical eligibility, low-income women who are not pregnant nor have children can qualify for Medicaid coverage. In December 2014, Gov. Bill Haslam announced a plan to expand the state's Medicaid program under the ACA. However, a Senate committee voted against this proposal in February 2015. Tennessee does not provide Medicaid Family Planning Program either. Regarding its neighboring states, in 2013, Arkansas and Kentucky passed the state's expansion plans. By 2016, these were the only two bordering states that expanded Medicaid under ACA. The remaining bordering states (Alabama, Georgia, Mississippi, Missouri, North Carolina, and Virginia) did not expand Medicaid but do offer Medicaid Family Planning Programs. See Ranji et al. (2015) for more information.

[11] We are not aware of any restrictions on out-of-state residents ability to obtain abortions in any state. However, it is typically the case that Medicaid cannot be used for health care obtained out of state (Backman, 2021). It is also the case that Medicaid does not cover abortions except in cases of rape, incest, or life endangerment. Currently, 16 states cover abortion beyond these relatively limited circumstances. Neither Tennessee nor any of its neighboring states are

Tennessee did not enforce any other regulations in 2015 and 2016 that we would expect to generate significant changes in abortion timing or rates.[12] In 2017, however, there were some other changes that might have affected these outcomes, which we address in our empirical analysis by showing results separately by year.[13]

3.4 Data

In this section, we describe the process by which we collected annual data on abortions by gestational age for various states and how we ultimately arrived at the set of states used in our analysis. We use two main sources of data. Primarily, we use data from 2010–2017 that we collected from state reports. We supplement these data with data from the Centers for Disease Control and Prevention (CDC)'s Abortion Surveillance System which are available from 2010 through 2016.[14] We refer to the comparison group using both sources of data and spanning 2010–2016 as "Comparison Group 1," and the comparison group based solely on state reports and spanning 2010–2017 as "Comparison Group 2." See Figure B.3 for a depiction of these states.

Our data collection effort based on state reports identified 38 states providing information on the number of abortions by gestational age in the form of Vital Statistics reports and/or abortion reports from their state health departments. Details regarding the type of information provided by each of these states are shown in Table B.1. For the remaining states, the data were not made available by the state or were not collected.[15] States vary in the type of abortion information they

amongst this group of states. Also, none of these 16 states had any policy changes in regards to this coverage during the time period spanned by our analysis (Salganicoff, Sobel, and Ramaswamy, 2019).

[12]A federal judge blocked a 2015 law requiring abortion clinics to be regulated as ambulatory surgical centers. In 2016, the laws that went into effect required written consent from the woman for any fetal tissue research or photographs of the fetus, and another one requiring facilities performing more than 50 surgical abortions a year to conduct mandatory interim assessments, report on serious injuries or deaths of patients, and be subject to regular inspections during which they must show their record of the disposition of fetal tissue (Guttmacher Institute, 2019).

[13]In April 2017, a court issued a partial judgment to permanently enjoined ambulatory surgical center and admitting privileges requirements that were previously blocked. Also, in May 2017, the state passed a law that would require a doctor to evaluate whether a fetus is viable after 20 weeks of pregnancy and which would presume that a fetus would be viable after 24 weeks of gestation. Finally, in June 2017, a Planned Parenthood clinic opened in Shelby county.

[14]We do not use data from the Alan Guttmacher Institute (GIA) because those data do not report abortions by gestational age.

[15]Connecticut, District of Columbia, Georgia, South Carolina, and Virginia do not release information on the number of abortions by gestational age. California and Maryland do not collect information on abortions. A Wyoming law restricts the sharing of abortion data to only local, state, or national public health officials or physicians. We contacted the health departments of Massachusetts, New Hampshire, and Rhode Island, but we did not receive any

collect and release. There are four types of "abortion data" that we identified: 1) the number of "occurrences," which represents the number of abortions obtained from providers within the state; 2) the number of abortions obtained by residents of the state from providers within the state; 3) the number of abortions obtained by residents of the state from providers within the state plus the (known) number of abortions obtained by residents of the state from out-of-state providers;[16] and 4) the number of occurrences plus the (known) number of abortions obtained by residents of the state from out-of-state providers. These differences can make it difficult to make comparisons across states. Furthermore, they highlight the reality that abortion information released by states may not be very informative about the abortions obtained by their residents, particularly for states with clinics providing abortions to many out-of-state women and states with many residents who obtain abortions in other states.

Of these 38 states for which we obtained abortion data from state agencies, 16 provide abortion data focusing on the number of abortions obtained by residents, regardless of where they are obtained, including Tennessee.[17] Our analyses use data on the 13 of these states that report data for all years from 2010–2017.[18] We refer to this set of states, for which we have data spanning 2010–2017, as "Comparison Group 2."

Our preferred estimates are based on analyses of "Comparison Group 1," which draws on data from the CDC to expand on the number of comparison states, but which restricts the analyses to 2010–2016 because more recent years of CDC data are not presently available. Another important

answer. The information on abortions from Florida is incomplete. We had difficulty contacting New Jersey's health department.

[16]States can get information on abortions obtained by their residents in other states through information exchange agreements with those states.

[17]The other 15 states are Alabama, Arizona, Delaware, Illinois, Minnesota, Missouri, New Mexico, New York, North Carolina, Oklahoma, Pennsylvania, Texas, Utah, Washington, and Wisconsin. Arizona, Delaware, Minnesota, Missouri, New Mexico, New York, Oklahoma, Pennsylvania, Texas, Utah, and Wisconsin release information on abortions performed on residents in the state. Alabama, Illinois, North Carolina, and Washington release information on abortions performed on residents both in the state and out-of-state.

[18]We do not include Delaware in our analysis because of missing data in 2010. However, we examine the sensitivity of our estimates to the inclusion of Delaware data (from 2011-2017) and show that the results are very similar if these data are included in the analyses. We do not use data from Texas because Texas HB2 led to the closure of nearly half of the abortion clinics in the state in 2013, which has been shown to have led to delayed abortions (Lindo, Myers, Schlosser, and Cunningham, 2019). We also do not use data for Alabama due to its 2013 law imposing regulations on outpatient clinics and private doctor's offices providing surgical and medical abortions and imposing requirements on the facilities and clinics. See Guttmacher Institute (2019) for more information.

drawback of these CDC data is that they only report abortions by gestational age *obtained in* each state, and thus, they may not accurately reflect abortions obtained by *residents of* each state. For this reason, we use CDC data for states in which no more than 20 percent of the abortions are provided to out-of-state women for our main results, though we show that results are similar if we use alternative thresholds.[19] We exclude from Comparison Group 1 states with potentially important abortion-related policies enacted during the period of our analysis.[20] Ultimately, this yields a set of 26 states that are included in Comparison Group 1.[21]

The information we use to measure outcomes for Tennessee's residents, provided by Tennessee's Department of Health, reflects abortions obtained by its residents in clinics both in Tennessee and in other states. We note that these data do not include information on *all* out-of-state abortions. States typically have data-sharing agreements with one another, but not all states participate, and those that do participate do not always end up sharing their data for unknown reasons. States reporting abortions to Tennessee include Alabama (2010–2016), Georgia (2016), Mississippi (2016), and North Carolina (2010–2014). According to CDC data, Tennessee residents most frequently seek out-of-state abortions in Georgia (548 annually), Arkansas (268 annually), and Alabama (111 annually).[22] To accommodate for irregularities in the

[19] See Figure B.4 for the distribution of percent of abortions to out-of-state residents reported in these CDC data.

[20] Specifically, we exclude six states from Comparison Group 1 that have abortion data available because of their own policy changes regarding the number of trips to a provider required or due to laws that are typically associated with abortion clinic closures. These states are Arizona (enforced MWP requiring two trips to the provider in 2011), Arkansas (altered MWP to require two visits to the provider in 2015), Illinois (implemented an admitting privileges law in 2014), Indiana (implemented an admitting privileges law in 2014), Pennsylvania (implemented ambulatory surgical center requirement, admitting privileges, and transfer agreement laws in 2012), and Virginia (ambulatory surgical center requirement and a transfer agreement law in 2012). Note that Arizona, Illinois and Pennsylvania *are* included in Comparison Group 2, for which the sample size is a greater concern—that said, this leads to more conservative estimated effects on the share of abortions obtained in the second trimester and on the second-trimester abortion rate.

[21] Those 26 states are: Alaska, Colorado, Georgia, Hawaii, Idaho, Iowa, Kentucky, Michigan, Minnesota, Missouri, Montana, Nevada, New Jersey, New Mexico, New York, North Carolina, Ohio, Oklahoma, Oregon, Rhode Island, South Carolina, South Dakota, Utah, Washington, West Virginia, and Wisconsin.

[22] CDC data confirm far fewer Tennessee residents obtaining abortions in Missouri, North Carolina, and Kentucky. While we could not find information on the number of Tennessee residents obtaining abortions in two other bordering states—Mississippi and Virginia—we expect this number to be extremely small because providers in these states were quite distant. In particular, based on historical data on providers that Caitlin Myers generously shared with us, a Tennessee resident would have had to travel at least 200 miles (multiple times) to Jackson to obtain an abortion in Mississippi as opposed to getting an abortion in Southwest Tennessee in Memphis (Shelby County). Similarly, a Tennessee resident would have had to travel at least 130 miles to Roanoke to obtain an abortion in Virginia as opposed to getting an abortion in Northeastern Tennessee in Bristol (Sullivan County).

data caused by this sort of irregular reporting, we analyze data on all abortions reported by Tennessee's Department of Health and data on a subset of Tennessee for which we can be especially confident that the data are reliable, which we call "Refined Tennessee." This is made possible based on data provided by Tennessee's Department of Health reporting on abortions obtained by residents of 14 "health areas," each comprised of 1–15 Tennessee counties. "Refined Tennessee" excludes data from four health areas–two in southeastern Tennessee from which a majority of women would have an Atlanta provider as their nearest option and two in northeastern Tennessee from which many women may have traveled to Virginia to obtain an abortion.[23] While we think analyzing this "Refined Tennessee" is helpful towards obtaining estimates that are as accurate as possible, this refinement has very little influence on the estimated effects on timing and has only a modest impact on the estimated effects on abortions overall. This is probably unsurprising given that women 15-44 years old residing in the excluded areas account for just 17 percent of such women residing in the state (and based on the data we have available to us, just 5.9 percent of abortions).[24]

We evaluate the effects of the MWP on three outcomes: the percent of abortions obtained in the second trimester;[25] the second-trimester abortion rate, constructed as the number of second-

[23]Clinic locations are depicted in Figure B.5 and Tennessee health areas are depicted in Figure B.6. The lack of consistent data on abortions obtained in Atlanta is a clear problem for the first two areas (Southeast and Sullivan), which is evident in the large unusual jump in 2016 in Figure B.7 and which corresponds to the single year in which Georgia provided data to Tennessee. The lack of consistent data for residents of the Hamilton and Northeast areas is also evident in the same figure, which shows an extremely implausible drop in the number of reported abortions from 2010 to 2011 (from 227 to 127 for Northeast and 77 to 33 for Sullivan) and an extremely implausible jump from 2015 to 2016 (from 107 to 214 for Northeast and 30 to 98 for Sullivan). To corroborate our suspicion about these data, we reached out to Bristol Women's Health Center, which has been in operation in Sullivan county since 1980. In contrast to the patterns evident in Tennessee reports, this clinic reported an increase (not a large drop) in abortions provided to Tennessee residents in 2011 (from 425 to 540), and a very small increase (not a more-than-doubling) in 2016 (from 359 to 374). We also discussed these data with Tennessee's Director of Vital Statistics, who could not determine the reason for these anomalies. We also note that the owner of the clinic in Sullivan said that they viewed clinics in Roanoke, Virginia–where Tennessee residents would only need to wait 2 hours as opposed to 24 hours before having an abortion after mandatory counseling–as their primary competition. Notably, abortions obtained by Tennessee residents in Virginia will not be captured in the data we use because Virginia does not share data with Tennessee.

[24]Authors' calculation using data from 2014.

[25]Due to differences in what is reported by each state, we are unable to use exactly the same definition of "second trimester" for all states. We do, however, use a consistent definition for each state over time. We define second-trimester abortions as abortions at 13+ weeks of gestation for Tennessee, Alaska, Colorado, Georgia, Hawaii, Idaho, Iowa, Kentucky, Michigan, Minnesota, Missouri, Montana, Nevada, New Jersey, New York, North Carolina, Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Utah, Washington, West Virginia, and Wisconsin; 12+ weeks of gestation for Arizona and Illinois; and 14+ weeks gestation for New Mexico.

trimester abortions per 1,000 women aged 15-44; and the overall abortion rate, generated as the total number of abortions per 1,000 women aged 15-44.[26]

Table B.2 shows the means for each of these variables, in addition to covariates used in our analyses, for Tennessee and the comparison states for the pre-intervention period (2010–2014) and the post-intervention period (2015–2017).[27] Most notably, 6.5 percent of abortions were obtained in the second trimester for Tennessee residents in 2010–2014, and this number rose to 9.9 percent in 2015-2017. In the comparison states, the share of abortions obtained in the second trimester fell slightly over the same period.

3.5 Empirical strategy

We first evaluate the effects of Tennessee's MWP on abortion timing and abortion rates using a difference-in-differences approach, which exploits within-state variation over time while controlling for aggregate time-varying shocks. The identifying assumption underlying this approach is that changes in abortion outcomes observed in the comparison states over time provide a good counterfactual for the changes that would have been observed in Tennessee if it did not implement the MWP.

Our estimating equation is as follows:

$$y_{s,t} = \alpha_s + \gamma_t + Treated_{s,t}\beta_0 + \mathbf{X'}_{s,t}\eta + \epsilon_{s,t} \tag{3.1}$$

where $y_{s,t}$ represents an outcome for residents of state s in year t; α_s are state fixed effects, which control for observed and unobserved state characteristics with time-invariant effects on the outcome; γ_t are year fixed effects, which control for time-varying factors affecting the outcomes in all the states in the same manner; $Treated_{s,t}$ represents the share of year t in which the MWP was in effect for Tennessee;[28] $X_{s,t}$ can include time-varying characteristics of states, including measures of demographics and economic conditions; and $\epsilon_{s,t}$ is the error term. The parameter of

[26]Population estimates are from the United States Census Bureau (2017).

[27]Unemployment rates are from the Bureau of Labor Statistics.

[28]Because Tennessee's law went into effect in May 2015, this variable takes the value of 7/12 for Tennessee in 2015, it takes the value of one for Tennessee in 2016 and 2017, and otherwise takes the value of zero.

interest is β_0, which captures the effects of Tennessee's MWP law on its residents.

Because we analyze a setting in which one state changes treatment status, we conduct randomization inference in addition to reporting standard errors clustered at the state level.[29]

Our randomization inference approach allows us to conduct exact inference without relying on large-sample approximations and without making assumptions about the distributions of the error terms. To do so, we consider the distribution of possible treatment effect estimates that could be obtained if we apply our estimating equation to each state, one-by-one. We then compare the estimate for Tennessee to this distribution to assess its statistical significance and to calculate *p*-values following the definition provided in Young (2019), which specifies the *p*–value to be uniformly distributed, and which offers an exact test with a rejection probability equal to the nominal level of the test.[30] Since this definition has a random component from a uniform distribution, we report the upper bound of each *p*–value, which corresponds to a draw from the uniform distribution equal to one. A downside of this approach is that 1/27 and 1/13 are the minimum p-values that can be obtained in analyses using Comparison Group 1 and Comparison Group 2, respectively, where 27 and 13 are the number of states used in those analyses.

In addition to reporting the results from Equation 3.1, we also report event-study estimates; estimates that separately evaluate the effects for each year from 2015–2017; and estimates documenting the sensitivity to the inclusion of each of the states in the comparison group. We

[29]In the ideal difference-in-differences setting, in which there are many clusters including many that change treatment status, cluster-robust standard errors will lead to correct inference (Bertrand, Duflo, and Mullainathan, 2004; Roodman, Nielsen, MacKinnon, and Webb, 2019). In our setting, we have only one that changes treatment status. This is relevant because *t*-tests based on cluster-robust standard errors tend to over-reject severely when the number of treated clusters is small (MacKinnon and Webb, 2018; MacKinnon and Webb, 2020). In the extreme case, when only one cluster is treated, cluster-robust standard errors would severely underestimate the variance of the difference-in-difference estimator (Ferman and Pinto, 2019). Wild-bootstrap provides an alternative when conventional inference methods are unreliable because large-sample assumptions do not hold. However, in difference-in-differences models with few treated clusters, the unrestricted wild bootstrap over rejects while the restricted one under rejects (Roodman, Nielsen, MacKinnon, and Webb, 2019).

[30]The definition is as follows:

$$p - value = \frac{1}{M}\sum_{S=1}^{M} I_S(> T_E) + U * \frac{1}{M}\sum_{S=1}^{M} I_S(= T_E) \tag{3.2}$$

where T_S are equally probable potential treatment allocations, T_E is the true treatment effect, M is the total number of potential treatment allocations, $I_S(> T_E)$ is an indicator function for $T_S > T_E$, $I_S(= T_E)$ is an indicator function for $T_S = T_E$, and U is a random variable drawn from a uniform distribution (0,1).

discuss the specific details of each of these exercises below.

As an alternative strategy to estimate the effects of Tennessee's MWP, we use a synthetic control design (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010; Abadie, Diamond, and Hainmueller, 2015), comparing the outcomes of residents of Tennessee to the outcomes of residents of a "Synthetic Tennessee." The intuition behind our implementation of this strategy is to use data from 2010–2014 to identify the weighted average of comparison states that provides the best match for the outcomes observed in Tennessee over this period, i.e., the synthetic control. Under the assumption that the synthetic control also provides a good match for the outcomes that would have been expected in Tennessee if it had not enacted a MWP, the difference between the outcomes observed for Tennessee and the outcomes observed for the synthetic control provides a valid estimate of the causal effect of the mandatory waiting period. We implement this strategy by determining the non-negative weights for each potential "donor state" that minimize the function:

$$(X_{TN} - X_{SC}W)'V(X_{TN} - X_{SC}W) \tag{3.3}$$

where X_{TN} is a (5×1) vector of variables measuring outcomes from 2010–2014, X_{SC} is a $(5 \times K)$ matrix containing the same variables for the K states in the donor pool (corresponding to Comparison Group 1 or Comparison Group 2 in different analyses), W is a $(K \times 1)$ vector of weights given to each each "donor state" (summing to one), and the diagonal matrix V are the "importance weights" assigned to each variable in X. We follow Ferman and Pinto (2017) recommendation to demean the data using information from the pre-intervention period, and then construct the SC estimator using the demeaned data.[31]

To conduct statistical inference for our synthetic control estimates, we follow Abadie et al. (2010) and estimate the distribution of estimated treatment effects under the null hypothesis of no effect by reassigning treatment to each state in the donor pool and applying the same method to

[31] Ferman and Pinto (2017) point out that, otherwise, the synthetic control's restriction to convex combinations of the control units may lead to bias even if treatment assignment is only correlated with time-invariant unobserved variables.

estimate a placebo effect for each state.

3.6 Results

3.6.1 Difference-in-Differences

3.6.1.1 Graphical evidence of changes over time

Our difference-in-differences approach relies on the identifying assumption that the changes in these outcomes observed in the comparison states provide a good counterfactual for the changes that would have been observed in Tennessee if it did not enact its MWP.

To assess the plausibility of this assumption, Figure B.8 compares the percent of abortions obtained in the second trimester for residents of Tennessee and residents of the comparison states. This graph shows that this percentage was quite stable in both Tennessee and the comparison states from 2010–2014. In other words, they exhibited parallel trends in the lead up to Tennessee's policy change. This provides support for our assumption that they would have continued to exhibit parallel trends in subsequent years in the absence of Tennessee's MWP.

Figure B.8 also shows some initial evidence of the effect of Tennessee's MWP, which we subsequently confirm in our regression analyses. In particular, it shows that the percentage of abortions obtained in the second trimester fell slightly after 2014 in the comparison states. In stark contrast, in Tennessee, it grew from 6-7 percent from 2010–2014 to almost 8 percent in 2015 before increasing to roughly 12 percent in 2016 and 2017. Thus, after several years in which its percentage was roughly half of the comparison states, Tennessee converged to or nearly to the levels of the comparison groups after it implemented a MWP.

Figure B.9 is similar but focuses on the log of the second-trimester abortion rate and the log of the overall abortion rate, respectively, in different panels. These graphs also provide support for the common trends assumption, as they demonstrate similar trends for Tennessee and the comparison groups from 2010–2014. They also provide some evidence that Tennessee's MWP increased second-trimester abortions, but less clear evidence of effects on abortions overall (the majority of which are first-trimester abortions).

3.6.1.2 Event-study estimates

As an alternative approach to showing how Tennessee's outcomes evolve over time relative to the comparison groups, Figure B.10 shows event-study estimates of the effects over time, in the years leading up to and following Tennessee's MWP.[32] Specifically, Figure B.10 shows estimates that control for state and year fixed effects and also adjusting for demographics and economic conditions. The graphs in the first column compare Tennessee, including information on all the health areas, with each comparison group. The graphs in the second column compare "Refined Tennessee", which excludes information on the Hamilton, Northeast, Southeast, and Sullivan health areas, to each comparison group. These graphs provide additional evidence of common trends and also additional evidence that Tennessee's MWP resulted in substantial changes in abortion timing and second-trimester abortion rates after it went into effect. This is true for estimates based on both comparison groups and both model specifications. Figure B.11 reports the one-sided and two-sided randomization inference p-values associated to each estimated treatment effect in Figure B.10.

3.6.1.3 Difference-in-differences main results

Table B.3 presents our main results based on Equation 3.1. Columns 1 through 3 show estimates of the effect of Tennessee's MWP on the percent of abortions obtained in the second trimester: Column 1 shows the results from our baseline difference-in-differences model with state fixed effects and year fixed effects; Column 2 shows estimates that additionally adjust for changes in states' economic conditions (the unemployment rate). These two columns compare Tennessee, which includes information on all the health areas, to the states in each comparison group. Column 3 shows estimates that compare "Refined Tennessee" to the states in each comparison group. These estimates control for state and year fixed effects, and adjust for demographics and the unemployment rate and they represent our preferred specification. The subsequent columns of Table B.3 are organized similarly but show estimated effects on the log of

[32]The year before the law went into effect (2014) serves as the reference period.

the second-trimester abortion rate (in columns 4–6) and the log of the overall abortion rate (in columns 7–9).[33]

Like the graphical evidence presented in the previous subsection, our regression-based estimates indicate that Tennessee's MWP increased the percent of abortions obtained in the second trimester by its residents. The estimated effect shows the MWP increased this share by 3.2–4.9 percentage points when Tennessee to each comparison group (Columns 1 and 2), which represents a 48–73 percent increase over its 2014 level (6.7 percent), depending on the comparison group and specification. The comparison of "Refined Tennessee" to each comparison group (Column 3) indicates an increase in the percent of second-trimester abortions of 3.6–4.6 percentage points, which represents a 53–69 percent increase over its 2014 level (6.6 percent). Panel A of Figure B.12 shows the results of our randomization-inference procedure for the estimates using the specification that compares "Refined Tennessee" to the two groups of states (Column 3). In particular, it shows the distribution of treatment effects that are possible with different permutations of the treatment variable across states, demonstrating that no other permutation yields such a large estimated impact as the one we obtain for "Refined Tennessee", using either Comparison Group 1 or Comparison Group 2.[34]

Our estimates of the effect on the number of second-trimester abortions per 1,000 women and the overall abortion rate indicate that these effects are a result of more women having abortions in the second trimester and fewer women having abortions overall. Specifically, the point estimates indicate that Tennessee's MWP increased the second-trimester abortion rate by 25–39 percent (Column 6) and reduced the overall abortion rate by 13–18 percent (Column 9), depending on the comparison group that is used.[35] However, we note that these estimated effects are typically not statistically significant at conventional levels when we conduct randomization inference for these

[33] Since weights should reflect the size of the underlying population upon which the measures are based, we weighted the percent of abortions by the total number of abortions in each state and the abortion rates by the total number of women (ages 15-44) in the state. The estimates are similar if we instead use common weights–the number of female residents aged 15-44–when analyzing these different outcomes.

[34] As evident in the p-values presented in Table B.3, this is also true for the models that compare Tennessee as a whole with the two groups of comparison states.

[35] Percent effects are calculated as $100 \times (e^{estimate} - 1)$.

estimates, the results of which are shown in panels B and C of Figure B.12.[36]

3.6.1.4 Difference-in-differences robustness tests

Table B.5 shows estimated effects separately for the year in which Tennessee's MWP went into effect and in subsequent years, based on the following model:

$$y_{s,t} = \alpha_s + \gamma_t + TN15_{s,t}\beta_1 + TN16_{s,t}\beta_2 + TN17_{s,t}\beta_3 + \mathbf{X'}_{s,t}\eta + \epsilon_{s,t} \tag{3.4}$$

where $TN15_{s,t}$, $TN16_{s,t}$, and $TN17_{s,t}$ are indicator variables for Tennessee in 2015, 2016, and 2017, respectively, and $y_{s,t}$, α_s, γ_t, $X_{s,t}$, and $\epsilon_{s,t}$ are defined the same as they were in Equation 3.1. Our analyses using Comparison Group 1 omits the variable $TN17_{s,t}$ because they only use data from 2010–2016. In any case, the primary parameter of interest from this model is β_2, which we expect to capture the effect of the MWP being fully in effect. In contrast, β_1 captures the effect of the policy going into effect midway through the year and β_3 may in part capture the effects of other changes in access to abortion in Tennessee that occurred in 2017.[37] For this reason, we focus our discussion on the estimated effect for 2016, or β_2.

These estimates indicate that Tennessee's MWP increased the percent of abortions obtained in the second trimester by its residents by 4.3–5.2 percentage points (Column 3) when it was fully in effect (in 2016), which is consistent with the estimated effects reported in Table B.3, and which represents a 64–78 percent increase over Tennessee's 2014 level (6.6 percent). Panel A of Figure B.13 shows the results of our randomization inference procedure for these estimates, the results of which demonstrate that no other permutation of the treatment variables yields such a large estimate as the estimated effect we observe for "Refined Tennessee".[38] The estimated effects on

[36]We evaluate the log of abortion rates and not "natural abortion rates" because the pre-treatment trends for Tennessee and the comparison states appear slightly more similar for the log of abortion rates. Though we note that Barkowski (2021) emphasizes that researchers should *not* evaluate both natural and logged outcomes because the common trends assumption can not hold for both (except in special circumstances), we find qualitatively results if we instead evaluate abortion rates. Table B.4 shows these results.

[37]Because these changes include state restrictions and also the opening of a clinic, the expected net effect of these changes is ambiguous.

[38]This is also the case for the estimated impact from our baseline model (Column 1) and our model that adjusts for demographics and the unemployment rate (Column 2), for Tennessee as a whole.

the second-trimester abortion rate and the overall abortion rate in 2016 are also consistent with the estimates reported in Table B.3, though in this analysis we find estimates that are somewhat larger in magnitude for the effect on the second-trimester abortion rate and somewhat smaller in magnitude for the impact on the overall abortion rate.

As another way of assessing the robustness of our main results, we verified that they are not sensitive to the inclusion of any specific state from the comparison group. Figure B.14 reports the results from this analysis. In particular, it shows how our main results (shown in columns 3, 6, and 9 of Table B.3) compare to the distribution of estimates that are possible using the same methodology if any single state from the comparison group is omitted from the analysis.

Along similar lines, we have verified that our estimates are similar if we adopt different rules for the inclusion of states in Comparison Group 1. Specifically, our main results incorporate CDC data for states whose data reflect no more than more than 20 percent of abortions to out-of-state residents. For comparison, in Tables B.6 and B.7 we report the states that would be included under thresholds ranging from 5 percent to 40 percent, and the estimates corresponding to these alternatives.

We also assess the robustness of the estimates to the use of data from Delaware, which is excluded from our main analyses in order to maintain a balanced panel. That said, in Table B.8 we show that our main results are extremely similar if Delaware data from 2011–2017 are included in the analyses.

3.6.2 Synthetic control estimates

In this section we present estimated effects based on a synthetic control design (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010; Abadie, Diamond, and Hainmueller, 2015). In implementing this design, we follow Ferman and Pinto (2017) and construct synthetic controls for "Refined Tennessee" by matching on the demeaned outcomes observed in the pre-intervention period (2010–2014) when evaluating each outcome variable.

Figure B.15 compares "Refined Tennessee" to “synthetic Tennessee” for each of the outcomes we consider, using both Comparison Group 1 and Comparison Group 2 to construct different

versions of "synthetic Tennessee."[39] This graph demonstrates that we are able to identify a synthetic control that is very similar to "Refined Tennessee" before its MWP, particularly for evaluating the percent of abortions obtained in the second trimester and the second-trimester abortion rate, and also indicates that these outcomes rose dramatically for Tennessee relative to its synthetic controls after its MWP went into effect.

Figure B.16 shows how these estimated effects compare to the distributions of estimates that are possible if the same methodology is applied to any state included in the analyses. Underscoring the statistical significance of the estimated effect on the percent of abortions obtained in the second trimester, none of the placebo tests yields an estimate as large and positive as the estimated effect for Tennessee, for 2016 or 2017 or using Comparison Group 1 or Comparison Group 2. Moreover, none of the placebo tests yields an estimate as large and positive as the ones we obtain for Tennessee when we evaluate the second-trimester abortion rate in 2016, and just one is as large and positive as the one we obtain when we evaluate the second-trimester abortion rate in 2017. The estimated effects on the overall abortion rate are less conclusive.

In Figures B.17 to B.19 we show that we get very similar synthetic control estimates if we adopt different rules (based on the degree to which the data may reflect out-of-state abortions) for the inclusion of states in Comparison Group 1. In figures B.20 and B.21, we show that we get very similar synthetic control estimates if we incorporate into the analysis data from Delaware (which requires modifying the analysis to begin in 2011).

3.7 Analysis of effect heterogeneity

As noted in our data description above, Tennessee reports data on abortions obtained by its residents for each of its "health areas" which are comprised of 1–14 counties in different parts of the state (as shown in Figure B.6). We use these data to explore the degree to which effects of the MWP may be greater for low-income women and minorities. To do so, we compare outcomes across areas which vary in measures of socioeconomic status but which have the same average travel distance to an abortion clinic. Specifically, we compare outcomes across the three single-

[39] Weights given to each state for the analysis of each outcome are reported in Table B.9.

county health areas with reliable data—Shelby (where Memphis is located), Davidson (where Nashville is located), and Knox (where Knoxville is located)—all of which have a clinic in the county and thus short average travel distances of 4-5 miles.[40]

Our analysis of abortion outcomes across these areas, the results of which are shown in Figure B.22, is consistent with the notion that socioeconomic status is an important factor associated with the effect of the MWP. Specifically, it shows that the largest post-regulation increase in the share of abortions obtained in the second trimester is in Shelby, which has the highest poverty rate, lowest median income, and highest share of women who are Black. That said, we also note that there is not clear evidence that the effects are greater in Davidson than Knox, though Davidson is more disadvantaged based on some measures of socioeconomic status.[41] It will be important for future studies to further consider the degree to which the effects of MWPs vary, perhaps leveraging data on more-populous areas and with more variation in underlying characteristics to enhance precision and/or to consider other types of heterogeneity.[42]

3.8 Monetary costs of the MWP

Previous studies have reported on women's perceptions about and experiences with MWPs based on surveys. In these surveys, women reported problems associated with transportation, employment, school, and childcare (Lupfer and Silber, 1981; Karasek, Roberts, and Weitz, 2016; Roberts, Turok, Belusa, Combellick, and Upadhyay, 2016; and Sanders, Conway, Jacobson, Torres, and Turok, 2016). Notably, our estimates suggest additional monetary costs are likely to result from women being delayed from obtaining abortions.

Delayed abortions can increase monetary costs for two main reasons. First, delays can increase

[40]The same three health areas are also the three most populous areas which is helpful for precision. Their (all-age and gender) populations range from 446,000 to 935,000. The next most populous health area (Mid-Cumberland) has 187,000 residents and all others range from 34,000 to 98,000.

[41]Shelby has the lowest median household income ($45,700 versus approximately $49,000 in Davidson and Knox), the highest unemployment rate (7.6 percent versus approximately 5.5 percent in Davidson and Knox), the highest poverty rate (21.0 percent versus 17.8 and 15.3 percent in Davidson and Knox, respectively), and the lowest population share that is non-Hispanic White (31.9 percent versus 54.8 and 81.2 percent in Davidson and Knox, respectively). Descriptive statistics for these—and other—Tennessee health areas are shown in Table B.10.

[42]Unfortunately, it is not feasible for us to consider heterogeneity based on travel distance to clinics because travel distance is strongly related to measures of socioeconomic status in Tennessee (as shown in Table B.10).

the cost of the procedure which typically rise with gestational age.[43] Second, delays can require women to travel to more distant providers because providers vary in the types of procedures they offer and the gestational ages at which they offer them. Women's options become more limited at higher gestational ages. As a result, delays can increase transportation costs, potential lost wages, and potential childcare expenses. To gain a better sense of the additional monetary costs faced by women seeking abortions under Tennessee's MWP, we have done several back-of-the-envelope calculations that take into account costs associated with the additional consultation appointment and the costs associated with being delayed from obtaining an abortion. It is worth mentioning these calculations are not welfare costs; we are providing information on the financial costs women can face due to the MWP.

3.8.1 Additional costs ignoring costs of delays

To begin, we focus on the costs that would be expected for a woman who is not delayed from obtaining an abortion. For such a woman, the additional costs consist solely of the additional costs associated with the mandatory counseling appointment.

We estimate the additional amount that clients had to pay to providers based on the change in the cost of obtaining an abortion—inclusive of all appointments—between July 2015 and October 2015. In particular, the fees for getting an abortion—inclusive of all appointments—rose by $20–70 between July 2015 and October 2015.[44] We use this range as a conservative estimate of the additional fees resulting from the MWP, though we note that providers charged $180–275 for consultation appointments after the MWP was enacted, followed by additional fees at the time of the procedure, which would vary depending on the procedure type and the gestational age.

We estimate transportation costs as a function of where a woman lives and the gestational age for which she is seeking an abortion. These jointly determine the nearest provider that could provide a woman with abortion care and the travel time required to reach this provider using the

[43] Figure B.23 shows the costs of obtaining an abortion at a clinic in Tennessee in October 2015, a few months after the MWP law went into effect.

[44] The information on abortion prices is from Knoxville Center for Reproductive Health, one of the seven clinics providing abortion services in Tennessee as of 2017. We used Wayback Machine to obtain information on the prices women faced in 2015.

provider-search tool from Goldenberg et al. (2017), which provides calculations for women living in Tennessee's twelve largest cities.[45] The tool provides information on the nearest provider, disregarding if this provider is in the state or out-of-state. Because a more-limited set of clinics provide care to women seeking abortions at higher gestational ages, women seeking abortions at such gestational ages often have to travel farther to reach their nearest provider. Thus, the transportation costs are comparatively high for women living relatively far from clinics (e.g., women in rural areas) and for women seeking abortions at higher gestational ages. Notably, some women in Tennessee have to travel three hours roundtrip to reach their nearest provider regardless of gestational age at which they are seeking an abortion (e.g., those residing in Chattanooga). We calculate a maximum of six hours of travel required for Tennessee residents seeking an abortion at different gestational ages (e.g., women in Knoxville or Jackson seeking an abortion at 20 weeks, women in Johnson City or Kingsport seeking an abortion at 16 weeks). As such, we estimate that the additional transportation costs could be up to $66, based on the cost of gas for a woman who needs to travel an extra six hours roundtrip for the counseling appointment.[46] Naturally, women without access to a car may incur additional costs.

We estimate costs associated with lost wages and/or childcare for women who have to take time off work similarly, based on the amount of time it is expected to take for a woman to reach the nearest provider that can serve her, and additionally factor in the amount of time that the appointment takes itself which can take 3–6 hours.[47] Given the possibility of driving up to six

[45]Goldenberg et al. (2017) was published in September 2017. Therefore, we expect the estimations on travel distance to reflect the abortion landscape as of that date. The tool provides information for cities as small as approximately 50,000. Specifically, it provides information for the following twelve cities in Tennessee, ordered by population: Nashville, Memphis, Knoxville, Chattanooga, Clarksville, Murfreesboro, Franklin, Jackson, Johnson City, Bartlett, Hendersonville, and Kingsport.

[46]We assume a gas cost $2.75 per gallon, and woman drives to a clinic at 60 miles per hour on average in a vehicle with a fuel efficiency of 30 miles per gallon.

[47]This range of times is based on the Knoxville Center for Reproductive Health's website: https://kcrh.com/frequently-asked-questions/. It states, "with new laws and restrictions in place and a required 48 hour waiting period, two visits are now required. You can expect to be at the center for 3-6 hours for each visit. The first visit will include medical history, ultrasound, lab, informed consent, video, and educational materials to review as well as individual counseling sessions. The second visit will include the procedure itself, whether aspiration or medical and recovery time for aspiration patients. So there are a lot of 'other' time-consuming parts of your visit here. We strive to give you the individual attention you deserve but remember on busy days, there are many other women needing all these services too."

hours due to the second trip to the clinic, and a total appointment time of up to six hours, women could lose up to twelve hours of wages or could have to pay for up to twelve hours of childcare to attend the consultation appointment. At Tennessee's minimum wage, which has remained at $7.25 per hour since 2015, twelve hours of lost wages would amount to $87. At a cost of $10 per hour for a babysitter, twelve hours of childcare would amount to $120.[48]

Given the calculations described above, we conservatively estimate that costs associated with the consultation appointment range from $20–70 if we focus solely on the fees required by providers. Additionally, accounting for transportation costs and the fact that some women will have to travel six hours to reach the nearest provider who can serve them, the costs range from $86–136. Additionally, accounting for costs associated with lost wages, the costs rise to $173–223, or childcare brings the range from $206–256.

3.8.2 Additional costs accounting for delays

Though our empirical analysis indicates that the MWP delayed women from obtaining abortions, it does not tell us precisely how long the delays typically were. For this reason, we provide estimates of the costs for one-week delay as a benchmark while noting that delays may be longer or shorter than one week.[49]

Based on the fees charged for different abortion procedures at different gestational ages, a one-day delay can increase the fees by up to $175.[50]

As we highlighted earlier, delays can also reduce the set of clinics from which a woman can obtain care because clinics vary in the types of procedures they offer and the gestational ages at which they offer them. As such, delays can increase travel time and also costs associated with transportation, lost wages, and childcare. Based on information from the same provider search

[48] According to Guttmacher Institute (2016c), as of 2014, 59 percent of women who got an abortion in the U.S. had one or more children.

[49] Prior research shows that 40 percent of women obtaining an abortion had a week or more between their consultation and procedure visits and 12 percent had two weeks or more between such visits (White, Turan, and Grossman, 2017). Sanders et al. (2016) found that 63 percent of women obtaining abortion had a week or more between their consultation and procedure visits. For more information about delays up to a month caused by financial difficulties, see White et al. (2016).

[50] This cost is based on the fee schedule from Knoxville Center for Reproductive Health as of October 1st, 2015 which is shown in Figure B.23.

tool described earlier, we estimate that a one-week delay could increase travel time by up to five hours. Making the same conservative assumptions as we have previously, we estimate that such a delay could increase transportation costs by up to $21; lost wages by up to $36.25; and childcare costs by $50.

As a whole, these calculations indicate that a one-day delay could increase a woman's cost by $175 based on the cost of the procedure alone. If the delay restricts the clinics at which a woman can obtain abortion care, her additional costs could rise by up to $196, when accounting only for additional transportation costs. These costs could rise by up to $232.5, accounting for additional transportation costs and lost wages or by $246 accounting for transportation costs and childcare.

The total costs of the MWP would be a combination of the additional costs associated with the counseling appointment and the costs of the delay. According to our calculations, the total cost of the MWP would be up to $245 based on the cost of the procedure alone. If we account for transportation costs, this could rise to $332, by up to $455.25, also accounting for lost wages, and by up to $502 if accounting for childcare expenses and not lost wages.

3.8.3 Costs in context

To put our cost calculations in context, it is important to note that the majority of abortion patients are low income, and even in the absence of the waiting period or other restrictions, many women incur financial hardship to obtain an abortion (Karasek, Roberts, and Weitz, 2016). A family of three at the federal poverty level in 2014 had a monthly income of $1,649. As such, the *additional* financial costs for a woman to obtain an abortion caused by Tennessee's MWP *over and above prior costs* can amount to a substantial share of women's monthly income.

3.9 Conclusions

In this study, we provide the first estimates of the effects of a MWP for abortion since studies evaluating the effects of states adopting such policies in the early 1990s. Consistent with rigorous research on Mississippi's MWP (Joyce, Henshaw, and Skatrud, 1997; Joyce and Kaestner, 2000), which also required an additional trip to the provider for an in-person consultation before the

waiting period began, we find significant effects on the share of abortions obtained in the second trimester, and we also find some suggestive evidence of impact on the overall abortion rate. Relative to this earlier work, our point estimates indicate somewhat larger effects on the percent of abortions obtained in the second trimester and somewhat smaller effects on the overall abortion rate.[51]

These findings are consistent with a number of recent studies documenting the causal effects of barriers to accessing reproductive health care. In particular, they are consistent with research on the impact of physical attacks on abortion providers (Jacobson and Royer, 2011) and with several recent studies showing that increases in the distance women have to travel to reach their nearest abortion provider cause significant reductions in abortion rates (Quast, Gonzalez, and Ziemba, 2017; Fischer, Royer, and White, 2018; Lindo, Myers, Schlosser, and Cunningham, 2019). Other studies have found significant effects of the distance that women have to travel to reach family planning clinics on birth rates (Lu and Slusky, 2019; Fischer, Royer, and White, 2018; Kelly, Lindo, and Packham, 2020). Recent studies have also demonstrated that parental involvement laws can have significant effects on abortion rates among minors (Joyce et al., 2020).

As more data become available, and as the landscape for reproductive health care continues to change at a rapid rate, it will be important for researchers to continue to evaluate important policy changes. With regards to Tennessee's MWP, future research could examine whether the effects persist into subsequent years, whether they extend to residents in neighboring states, whether they lead to any measurable impacts on birth rates, and whether they affect other outcomes for women and their families. For MWPs more broadly, future research could examine the effects in other states, the effects of changes in the duration that women are required to wait, and the margins of

[51] Joyce et al. (1997) found that Mississippi's requirement increased the share of abortions performed after 12 weeks LMP by 39 percent and reduced the abortion rate among Mississippi residents by approximately 12 percent. Joyce and Kaestner (2000) find that increased the proportion of abortions obtained in the second trimester by 45 percent for Mississippi residents living nearest to Mississippi clinics as compared to Mississippi residents living nearest to out-of-state clinics. Our point estimates also indicate larger effects on the percent of abortions obtained in the second trimester and smaller effects on the overall abortion rate compared to those reported Myers (2021). The results of this working paper, which are based on an analysis of a broader set of state requirements, indicate that two-trip requirements reduce proportion of abortions obtained in the second trimester by 19 percent and reduces the overall abortion rates by 9 percent, on average.

adjustment that are available to providers who might struggle to cope with the requirement that they have an additional appointment for each patient.

4. TRAP'D TEENS: IMPACTS OF ABORTION PROVIDER REGULATIONS ON FERTILITY & EDUCATION

4.1 Introduction

Access to abortion services in the United States has long been a divisive issue and a political flash-point. When the Supreme Court decision *Roe v. Wade* declared that an individual's right to privacy provided a right to abortion, the national debate only intensified. Since that time, in states where opposition to abortion is strong, various measures have been implemented to restrict abortion access. Existing evidence suggests that modern 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.[1] 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 are reflected in their fertility impacts. When a young woman experiences an unintended birth, she may pause or abandon her educational or other career investments.[2] 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.[3] As such, abortion access may impact future welfare by changing the course of a young woman's life.

Targeted regulations of abortion providers or TRAP laws are the fastest growing abortion restrictions in the U.S. Between 2010-2017, the number of states that implemented these

[1] See Section 4.2.1 for a review of this evidence.

[2] Upchurch and McCarthy, 1990; Geronimus and Korenman, 1992; Ribar, 1994; Klepinger, Lundberg, and Plotnick, 1999; Chevalier, Viitanen, and Viitanen, 2003; Levine and Painter, 2003; Kaplan, Goodman, and Walker, 2004; Holmlund, 2005; Hotz, McElroy, and Sanders, 2005; Sanders and Smith, 2007; Ashcraft, Fernández-Val, and Lang, 2013; Schulkind and Sandler, 2019.

[3] Evidence suggests that early legal access to contraceptives in the 1970s affected women's educational choices and attainment (Goldin and Katz, 2002; Hock, 2008; Bailey, Hershbein, and Miller, 2012; Ananat and Hungerman, 2012.)

restrictions increased by 53 percent. Existing evidence has documented the detrimental impacts of such restrictions in Texas and Pennsylvania in terms of abortion access, abortion rates, and abortion timing (Quast, Gonzalez, and Ziemba, 2017; Fischer, Royer, and White, 2018; Lindo, Myers, Schlosser, and Cunningham, 2019; Kelly, 2020). We test whether such impacts extend beyond these two states and whether such restrictions have downstream impacts on fertility and education.

In this study, we estimate the impacts of twenty-seven targeted regulations of abortion providers (TRAP laws) implemented across twenty-one states since 1994. 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. 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, 2020; Athey and Imbens, 2022; 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, 2021; 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.[4] We also explore differential effects by type, severity, and number of TRAP laws.

We find that the onset of a TRAP law increases births among Black teens by 3 percent. Among White teens, the onset of a law has no impact on the pre-existing trend in teen births. This is consistent with previous evidence that Black women are disproportionately affected by restrictions

[4]See Cengiz et al. (2019) for an application of this methodology to the study of the effects of minimum wages on on low-wage jobs.

to family planning access.[5] We also analyze potential mechanisms driving changes in fertility. Our findings suggest that decreases in abortion use, abortion access, and contraception use are channels through which TRAP laws increase Black teen birth rates.

We next explore the impacts of teen exposure to TRAP laws on the educational attainment of Black 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 also offer evidence that the assumption of common trends holds, demonstrating that the impacts of first exposure at ages 30 or older are zero, with effects becoming negative and monotonically increasing in magnitude as the age of first exposure falls. We find that first exposure to a TRAP law before age 18 reduces the probability of having ever entered college by 1 to 3 percentage points, relative to a mean of 55 percent, and reduces the probability of completing college by 1 to 3 percentage points, relative to a mean of 24 percent.

We contribute to the literature in several ways. This is the first study to estimate the impacts of TRAP laws on abortion access, abortion use, and teen fertility nationally. Second, we contribute to a limited body of evidence on the causal impacts of modern abortion restrictions on women's outcomes beyond abortion use and fertility. This evidence includes only two papers of which we are aware: Borelli (2011) documents impacts of parental involvement laws on education and the Turnaway study documents impacts of gestational limits on economic duress, both in the relatively short term (Foster, Biggs, Ralph, Gerdts, Roberts, and Glymour, 2018b; Miller, Wherry, and Foster, 2020a).[6]

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. Despite

[5] See Bailey, Malkova, and Norling, 2014; Bailey, Malkova, and McLaren, 2018; Browne and LaLumia, 2014; Myers, 2017; Myers and Ladd, 2020.

[6] Additionally, Angrist and Evans (2000) document impacts of abortion *legalization* in the 1970's on women's educational and labor market outcomes. Further, some studies have also documented the economic impacts of abortion access on the *next generation* (Ananat, Gruber, Levine, and Staiger, 2009; Foster, Biggs, Raifman, Gipson, Kimport, and Rocca, 2018a; Foster, Raifman, Gipson, Rocca, and Biggs, 2018c).

great interest in the relationship between teen fertility and educational attainment, a challenge in studying this relationship is that socio-demographic characteristics of women often are strong predictors of both fertility-related behaviors and economic outcomes. Researchers have relied on various strategies to isolate the causal impacts of teen pregnancy on educational attainment, such as controlling for individual and family characteristics by comparing women within families or within schools,[7] or by relying on potentially exogenous sources of variation in the timing of childbearing, such as age at menarche or the experience of miscarriage.[8] Other studies have estimated the causal relationship between teen fertility and educational attainment by exploiting the geographic and temporal variation in the implementation of family planning policies implemented in the 1960s and 1970s.[9] However, the U.S. economic, social, and political landscapes have changed dramatically in the past 30 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 the case *Mississippi v. Jackson Women's Health*, currently under consideration by the US Supreme Court, a central argument of one amicus brief is that "there is no adequate credible evidence that women have enjoyed greater economic and social opportunities because of the availability of abortion." Our findings provide direct evidence to the contrary.

The remainder of the paper is structured as follows: Section 4.2 discusses existing evidence on the modern abortion restrictions and the relationship between teen motherhood and educational attainment. Section 4.3 describes TRAP laws and the creation of the legal data set. Section 4.4 presents the impacts on teen births and explores the mechanisms driving these impacts. Section 4.5 presents the impacts on women's education and Section 4.6 concludes.

[7]Studies that employ this methodology include Olsen and Farkas (1989); Upchurch and McCarthy (1990); Geronimus and Korenman (1992); Hoffman et al. (1993); Levine and Painter (2003); Holmlund (2005); Sanders and Smith (2007); and Schulkind and Sandler (2019).

[8]Ribar, 1994; Klepinger, Lundberg, and Plotnick, 1999; Chevalier, Viitanen, and Viitanen, 2003; Kaplan, Goodman, and Walker, 2004; Hotz, McElroy, and Sanders, 2005; Ashcraft, Fernández-Val, and Lang, 2013 rely on these quasi-exogenous sources of variation.

[9]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.

4.2 Existing evidence

4.2.1 Modern abortion restrictions

In 1973, the US 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 dramatically changed 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 US 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 (Mercier et al., 2016). 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.

Figure C.1 documents the increase over time in the three of the most common types of abortion restrictions.[10] Following *Casey*, there was rapid growth in parental involvement laws,

[10]According to Guttmacher Institute (2021b), the most common types of abortion laws are: 1) refusal, which allows health care providers or institutions to refuse to participate in an abortion, 2) gestational limits, which prohibit abortions after a specific point in pregnancy, usually 20+ weeks since last menstrual period (LMP) or fetal viability (Kaiser Family Foundation, 2021b), 3) TRAP laws, 4) parental involvement laws, and 5) mandatory waiting periods. We do not include in Figure C.1 refusal laws 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. We also exclude from Figure C.1 gestational limits because the majority of abortions happen in the first trimester, so these are binding for a minority of women. Other abortion laws we do not consider in our analysis are so-called "partial-birth abortion" bans, because these apply to an abortion method used only after 20 weeks since LMP (Kaiser Family Foundation, 2021a). Additionally, we do not include 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 et al., 2021). We also exclude state-mandated counseling laws as they are usually implemented in combination with mandatory waiting periods. Finally,we do not include 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.

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 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, 2021b). 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 distance 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. The number of states enforcing TRAP laws increased by 53 percent between 2010 and 2017.

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 distance to a hospital.[11] 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, Myers, Schlosser, and Cunningham, 2019). One study also documents increases in clinic congestion and delays in abortion timing as a result (Lindo et al., 2019). 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).

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

[11] It also required that all abortion facilities meet new ASC standards (see Section 4.3.1). 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.

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 provide the first national-level evidence regarding the impact of TRAP laws on abortion access, abortion use, and fertility. In doing so, we also provide the first detailed, quantitative coding of the history of TRAP laws in all US states.

We note that beyond TRAP laws, other abortion restrictions have also been documented to affect abortion use, abortion timing, and fertility. Parental involvement laws, particularly those post-*Casey,* have been shown to reduce minors' abortion use, delay their abortion timing, and increase early fertility.[12] 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.[13] Other studies have also documented the harmful consequences of cuts to public funding of family planning, especially for those living in poverty.[14,15]

[12]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); and Joyce et al. (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); and Myers and Ladd (2020).

[13]See Joyce et al. (1997); Bitler and Zavodny (2001); Lindo and Pineda-Torres (2021); and Myers (2021).

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

[15]Prior evidence on the impacts of other abortion restrictions include studies on the use of Medicaid for abortion, showing these restrictions decrease abortion use among minors and low-income populations, though they have not been shown to impact overall birth rates (Lundberg and Plotnick, 1990; Haas-Wilson, 1993; Meier 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, Parnell, Moore, and Pagnini, 1999; and Morgan and Parnell, 2002).Also, gestational limits have been shown to contribute to worse health and economic outcomes for women who were denied needed abortion care (see Miller et al., 2020b for an annotated list of findings from the Turnaway Study). Compulsory ultrasound requirements have not been found to affect abortion use (Gius, 2019). We do not discuss these policies since most of them were implemented several decades ago. Some other evidence focuses on access to fertility controls in the nineteenth century based on anti-obscenity laws, which restricted access to information, products, and services to control fertility, particularly abortion. Findings indicate that these policies led to increases in births (Lahey, 2014a; Lahey, 2014b).

Differences by race

Existing evidence documents that the impacts of abortion access may be different across demographic groups. This is true both for the impacts of abortion legalization, as well as the impacts of modern abortion restrictions.

Abortion legalization has been found to decrease births to Black women more than births to White women (Gruber, Levine, and Staiger, 1999; Levine, Staiger, Kane, and Zimmerman, 1999), including teen births (Joyce and Mocan, 1990; Donohue, Grogger, and Levitt, 2009; Ozbeklik, 2014; Myers, 2017). It has also been documented to increase women's education and labor market participation, but only among Black women (Angrist and Evans, 2000; Kalist, 2004).

Parental involvement laws increase births for both White and Black teens. However, at higher avoidance distances, the estimated effects are 50 to 100 percent larger for Black women Myers and Ladd (2020).[16] Parental involvement laws also negatively impact educational attainment for Black but not White women (Borelli, 2011).

Distance to the nearest abortion provider affects abortion use and births more among Black women. Joyce et al. (2013) document that distance to a provider has a greater impact on abortion use for non-White vs. White women. Venator and Fletcher (2020) find that a distance increase of 100-miles increases births by five times as much among Black women compared to White women.

Reductions in abortion funding also differentially affect Black women. Funding cuts in North Carolina increased births by twice the amount among Black women as White women (Cook, Parnell, Moore, and Pagnini, 1999; Morgan and Parnell, 2002), and reductions in Medicaid funding for abortion increased fatal injuries to Black children but not White children (Sen, 2007).

Why does research find greater impacts of abortion access among Black women? Black women have higher rates of unmet need for contraception, higher rates of unintended pregnancy, and report higher use of abortion (Bernstein and Jones, 2019). Lower access to contraception is likely to increase the potential need for access to abortion. Black women are also more likely to be living in

[16]Myers and Ladd (2020) define "avoidance distance" as the difference between the distance to the nearest provider of a confidential abortion and the nearest abortion provider.

poverty than White women and so generally face greater barriers to accessing reproductive health care and are less able to overcome abortion access restrictions.

These disparities stem from the broader environment of structural racism and oppression facing Black women in the United States. Throughout the history of the country, there have been efforts to control the fertility of women of color and low-income women, including through involuntary sterilization and long-acting contraception for public assistance recipients.[17] This legacy of reproductive coercion perpetuates mistrust of the health care system and may deter women from seeking care.

Given the evidence of sizable impacts of abortion access on Black women relative to other women, it seems likely that TRAP laws would also differentially affect these two groups. Our analysis explores the impacts separately by race.

4.2.2 Early fertility and education

A second literature to which this study contributes is one that assesses the economic impacts of early births on economic outcomes such as educational attainment. Identifying this relationship 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; Sanders and Smith, 2007; 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

[17] See Rocca and Harper (2012); Thorburn and Bogart (2005); Gold (2014).

educational attainment (Ribar, 1994; Klepinger, Lundberg, and Plotnick, 1999; Chevalier, Viitanen, and Viitanen, 2003; Kaplan, Goodman, and Walker, 2004; 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.

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 unexposed. A policy widely studied using this approach is 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, women with higher measured ability, and women from less-advantaged backgrounds (Bailey, Hershbein, and Miller, 2012; Ananat and Hungerman, 2012). Other laws that increased contraceptive access for young women have also been documented to increase college attendance, such as reductions in the minimum age at marriage (in a time where marriage was a pathway to contraception for minors) (Edlund and Machado, 2015).

Legal access to abortion has also been documented to both reduce teen 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 relaxed their abortion restrictions.[18]

[18]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.

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 and labor market outcomes. Their findings indicate that abortion access reduced births by 5 percent for White teens and nearly 10 percent for Black teens. Instrumenting teen childbearing with abortion access, the authors find that Black teen mothers are 17 to 35 percentage points less likely to complete high school and 52 percentage points less likely to ever attend college. Findings from studies outside the US 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; González, Jiménez-Martín, Nollenberger, and Vall-Castello, 2018).

These policy evaluations document that access to contraception and abortion in the 1960s to 1980s 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 in the past 30 years. 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 (NCES, 2019), 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 are 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. It is not clear that the existing evidence about the impact of early fertility on education still applies today.

We turn to modern abortion restrictions as an exogenous change in teen childbearing that can offer evidence on the links between the risks of early fertility and education today. Only one study

of which we are aware examines the impact of early fertility on women's educational outcomes using a modern policy as an exogenous change.[19] In an unpublished dissertation, Borelli (2011) examines the impact of exposure to parental involvement laws during adolescence in the 1980s and 1990s on fertility and educational outcomes. She finds that exposure to these laws for Black women increased fertility by 4 to 6 percent; impacts on White women were not significantly different from zero. This exposure 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.

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.

4.3 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 et al., 2018).[20]

The passage of TRAP laws is a function of complicated political processes. For instance, Texas

[19]While they do not study early fertility or educational attainment, we note that the investigators of the Turnaway Study do provide evidence of the impact of unwanted births on economic outcomes. The researchers compared medium-term outcomes of women who were denied wanted abortions due to gestational limits to the outcomes of women who were able to receive abortions. They find that abortion access increased the probability of full-time employment and reduced the probability of living in poverty at six months and four years after the abortion was sought (Foster et al., 2018b). They also document that being denied an abortion results in a large and persistent increase in financial distress for these women that is sustained for up to four years after the birth year. An abortion denial increases the probability of bankruptcies and evictions by 81 percent and increases the amount of debt 30 days or more past due by 78 percent, relative to women who interrupted their pregnancies (Miller et al., 2020a).

[20]The leading medical groups uniformly oppose TRAP laws. They state that, far from making women safer, they put women's health in jeopardy by shutting down clinics and making it more difficult for women to access safe and legal abortion care. The American Medical Association and the American College of Obstetricians and Gynecologists have said, "there is simply no medical basis" for the TRAP law and that it "does not serve the health of women..., but instead jeopardizes women's health by restricting access to abortion providers" (American Civil Liberties Union, 2019).

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.

Figure C.2 displays the enforcement of the first TRAP law in each state across the years. The map indicates that most of the states that have implemented TRAP laws are in the Midwest and the South. States in the South generally have higher percentages of Black population relative to states in other regions. Also, the map reveals that the implementation of these laws became more frequent post-*Casey*.

TRAP laws may affect abortion access because some clinics and providers cannot comply with the requirements, which obliges them to stop operating. 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*).

In recent years, efforts have been made to strike down TRAP laws. 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.

4.3.1 Types of TRAP laws

A common method of enacting TRAP laws is to require abortion facility licensing (AFL) that is in addition to standard licensing for health facilities, and to enforce a number of regulations as part of AFL. 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 (AP): 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.[21] 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 abortion is a very safe procedure, it is difficult for providers to meet the admission threshold.[22] Securing privileges may be particularly challenging for rural providers as there is often no hospital nearby.

Transfer agreements (TA): 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

[21] Missouri was the first state to enact such a policy in 1986, and these laws remained relatively rare until 2011.

[22] Fewer than 0.5 percent of abortion patients in the United States experience a major complication that requires hospitalization (Guttmacher Institute, 2020)

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.

Distance regulations (DR): This type of regulation requires clinics to be located within a certain distance (or a certain driving time) from a hospital. This is sometime included as part of AP or TA regulations, for example, a "clinic must have a transfer agreement with a hospital that is located within 30 miles of the clinic," requires a TA, but also requires that the clinic be located within 30 mi of a hospital. However, these regulations are also sometimes enacted separately from AP and TA 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 (BR): This category includes a wide variety of regulations that may apply, typically as part of ASC requirements, though they also occur under AFL 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, those these 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, such as 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 in the modern period.

4.3.2 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 AFL 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.[23]

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, AP, and TA. As noted above, the specific regulations of an ASC law vary by state (and over time within state). As such, ASC, like AFL, 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

[23]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 datasets. 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 AFL requirements do. Second, these requirements were enforced in most of the states in the 1970s. As such, most of the population in our sample has been fully exposed to them, creating no variation for our estimations.

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 CaseText.com. In instances where implementation dates were not precise (especially when restrictions 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.[24] 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 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.[25] 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.

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. We treat each type of TRAP law as binary.[26] 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

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

[25]A few laws are specific to providers that provide surgical abortion, providers that provide medication abortion, 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.

[26]In our analysis, we define an AP regulation as one that requires at least one of the clinic's own physicians to have admitting privileges. We define a TA regulation as requiring either a formal agreement *or* a plan or protocol. In Figure C.7, we show that our findings are robust to defining AP to also include the minimum stringency and defining TA to exclude the minimum stringency.

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 TA regulation. In Appendix C.3, we define the levels of stringency and explore the impact of a change in the level of stringency for AP, TA, and BR.

4.4 Impacts on teen births

4.4.1 Data

We employ natality data from Vital Statistics, which include a record for every birth in the United States from 1989 to 2018 (NCHS, 2018). We collapse this to the state-year-race-age group level. Primary outcome include the number of births to women aged 15 to 19 in each state-year, separately for White and Black women.[27] We additionally employ population counts at the state-year-race-age group level from SEER (2018), which are based on census counts and extrapolated for inter-censal years. The race-specific population of women aged 15 to 19 in a state-year is used to scale birth counts by relevant population size.

As described in Section 4.3.2, 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.[28] These same data indicate the presence of other relevant policies; we test the robustness of our findings to their inclusion as 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).

[27]The selection of the age group 15 to 19 is constrained by the available population data, which provide counts for this age group, but not for single-age groups.

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

4.4.2 Estimation

We estimate an event study using

$$E\left[y_{it+1}|b_{it}, \boldsymbol{X_{it}}, \nu_i, \nu_t\right] = exp\left(\sum_{j=\underline{j}}^{\overline{j}} \beta_j b_{it}^j + 1ln(pop_{it+1}) + \boldsymbol{X_{it}'}\delta + \nu_i + \nu_t + \varepsilon_{it}\right) \tag{4.1}$$

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 4.4.1. 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.

Following Myers (2021), we estimate Equation 4.1 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.[29]

We define

$$b_{it}^j = \begin{cases} 1[t \leq e_i + j] & \text{if } j = \underline{j} \\ 1[t = e_i + j\} & \text{if } \underline{j} < j < \overline{j} \\ 1[t \geq e_i + j] & \text{if } j = \overline{j} \end{cases} \tag{4.2}$$

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 $j > \overline{j}$. As such β_j captures the change in the teen birth rate.

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

[29] In Stata this is achieved using the *xtpoisson* command and specifying the relevant population in the *exposure* option.

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 cluster the standard errors at the state level.

We note that, beginning in 2016, some TRAP laws are overturned in court and regulations are removed. We therefore exclude years after 2016 from our analysis. We employ data on births from 1989 to 2016, so we can estimate the impact of events occurring from 1994 to 2012. This aligns with the post-*Casey* period, which has been shown to exhibit greater impact of abortion restrictions (Myers and Ladd, 2020).

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 $b_{it}^{j} = 0$ for $j \in [-5, -2]$.

We note that this methodology also relies on the assumption that the effects of a law of a given type are homogeneous across states (Sun and Abraham, 2021). We recognize that this is a strong assumption that may be violated if the severity of laws differs across states, or if some states enactmultiple TRAP laws in succession. Section C.3 in the Appendix presents event-study analyses that accounts for the intensity of treatment based on the severity of the restrictions. 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.

4.4.3 Results

We estimate the impact separately for each of the four regulation types described in Section 4.3.1, separately and altogether, which generates five separate analyses Consistent with previous studies documenting differential impacts of abortion access by race, we estimate the impacts separately for Black and White teens. We note that estimates for the aggregate sample are indistinguishable from the effects for White teens, given their strong majority status.

White teens: When estimating effects among White teens, we find strong violations of the assumption of common trends, for four out of five types of regulations examined. The event study

plots do not indicate any break in the pre-existing trend as a result of policy onset. As such we find no evidence that our underlying assumption hold for this group, i.e., we find no evidence that TRAP laws shift births to White teens in any substantial way. We also conclude that further analysis for this sample is not valid. These estimations are presented in figures C.3 and C.4. The following sections will focus on the results for Black teens.

Black teens: Figure C.5 shows the event-study estimates of the impacts of TRAP laws on Black teen births. When estimating the impact of any TRAP law, we observe a very clear zero trend in the pre-period, suggesting that the onset of TRAP laws is unrelated to existing trends in Black teen births. Following the onset of a policy, we see a clear divergence in the trend between those who are, versus are not, exposed. The average effect over the post-period is an increase in Black teen births of 3 percent. This represents an increase of 1.8 births per 1,000 women, relative to the median birth rate for Black teens, 60 births per 1,000 women.[30] We observe that the effect develops over time. The first significant effect appears two years after policy onset.

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 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. The increase in impacts over time 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.

[30] Authors' estimates using natality and population data at the state-age-race level, 1993 to 2016.

In Appendix C.3 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.

Figure C.6 presents the event study for each type of TRAP law. For admitting privileges regulations we also observe a clear zero trend prior to policy onset. The estimated effects of AP on Black teens are increases in births between 2.5 to 13 percent, relative to Black teens not exposed. The average effect over the post-period is 7.39 percent. For transfer agreement regulations, we also observe a clear zero trend in the pre-period. The implementation of TA increases Black teen births with a lagged response, with significant effects beginning three years after the effective date. The average effect over the post-period is 2.07 percent.

For distance and building regulations, we observe an approximate, but not exact, zero trend in the pre-period. DR increases Black teen births in four out of five of the post periods. The average impact is 3.48 percent. However, a robustness check in Section 4.4.4 suggests that this estimate may be upwardbiased, with an unbiased estimate that is smaller, though still positive.

Finally, for building regulations, we again observe a pre-trend that is approximately, though not exactly, zero. However, we find that BR results in a temporary reduction in Black teen births, followed by a spike increase four years after onset. The average impact over the post-periods is 0.07 percent. However, the results in Section 4.4.4 suggests that this effect is potentially downward-biased, with an unbiased effect as large as 2.33 to 2.41 percent.

Table C.1 presents suggestive evidence to explain why the underlying assumption–teen births in states that implemented TRAP laws trended similarly to teen births in states that did not implement TRAP laws before implementation–does not hold for White teens, but it does for Black teens. This table shows the averages of different variables by race and future state-TRAP status, using information from the 1990 American Community Survey; information collected before the first treatment occurred (1994). The variables presented in this table are potential determinants of unintended pregnancy for women who responded to the 1990 American Community Survey. The information in this table suggests that before TRAP law implementation, potential determinants of births for Black women were similar across TRAP and non-TRAP

states. In contrast, means differed for White women living in TRAP states vs. White women living in non-TRAP states. For example, White women living in states that eventually implemented TRAP laws had lower educational attainment than White women living in non-TRAP states. In comparison, the percentage of Black women by educational attainment is similar across TRAP vs. non-TRAP states. Regarding employment and labor force participation, we observe similar patterns; White women who lived in future-TRAP states were less likely to be employed or in the labor force than White women who lived in future-non-TRAP states. However, Black women were similarly likely to be employed or in the labor force disregarding on the future TRAP law status in their states of residence. The most striking difference is observed in the real family income. Black women living in future-TRAP and future-non-TRAP states had a family income of around $22,000 in 2012 USD, with a difference between groups of less than 200 dollars. In contrast, White women living in future-non-TRAP states had a family income of $44,395 in 2012 USD, and White women in future-TRAP states had a family income of $39,808, a difference of almost $5,000 between these two groups. Overall, this table indicates that Black women have been similarly disadvantaged across states and over time, disregarding TRAP law implementation in the subsequent years. However, White women living in TRAP states have been historically more disadvantaged than White women living in non-TRAP states. These comparisons suggest that White teen births in future-TRAP states were likely to have trended differently from White teen births in future-non-TRAP states because the determinants of such births behaved differently across these groups of states. In contrast, Black teen births trended similarly before TRAP law implementation because the determinants of Black teen births were similar across states before TRAP laws were implemented.

Our results differ somewhat from the evidence presented in previous studies on the impacts of TRAP laws on birth rates. Findings by Fischer et al. (2018) indicate that the impacts of Texas HB2 were concentrated among women in their 30s and 40s. However, they do not find any evidence of impacts on teen births, and they do not provide evidence of impacts by race. Lindo et al. (2019) also explore HB2's impacts on different demographic groups. Overall, their findings do not provide

robust evidence of effects on births among teens or Black women. Finally, Kelly (2020) finds that a TRAP law in Pennsylvania increases birth rates for women overall, though her results are not disaggregated by age. However the effects she estimates appear to be driven by White women. Therefore, we provide the first evidence of TRAP laws impacts disaggregated by both age and race.

4.4.4 Robustness tests

We next test whether the results presented in Section 4.4.2 are biased by heterogeneous effects across units, as proposed by Sun and Abraham (2021) and Borusyak et al. (2021).

We separately estimate the impact of each TRAP law implementation using a difference-in-differences design, selecting the appropriate comparison group for each estimation. We construct event-specific data sets containing only the treated state and the selected comparison states, creating a symmetric panel of r years, centered on the year of the policy change, where $r \in \{6, 8, 10, 12\}$. 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 (never-treated).[31] We supplement this group by also including those that implemented a TRAP law after the end of the included time window (future-treated). Our approach is similar to Cengiz et al. (2019), who estimate the effects of minimum wages on low-wage jobs using an event-study analysis and check robustness using estimates of separate treatment effects for each minimum wage change.

Table C.2 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. Table C.3

[31] To deal with the bias induced by the heterogeneity of treatment, Callaway and Sant'Anna (2021) propose that practitioners should favor the comparison with the never treated group rather than comparisons with future treated only. Never treated units are preferred as long as there is a sizeable 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. These authors consider that the comparison with future-treated only may create issues because pre-treatment trends may capture differences in the economic environment between "early periods" and "later periods" of treatment. In such cases, the outcomes of different groups may evolve in a non-parallel manner during "early periods" because the groups were exposed to different shocks, while trends become parallel in the "later periods." Our preferred comparison is with never-treated+future-treated. However, in Tables C.4 and C.10, we demonstrate that our estimates do not suffer from these issues since they are robust to the selection of different time windows and the comparison with never-treated only.

shows other TRAP laws excluded from our analyses because they occurred before our after our period of analysis, or because they apply only to providers of second-trimester (or later) abortions.

For each policy change, we estimate the equation:

$$E\left[y_{it+1}|b_{it}\nu_i,\nu_t\right] = exp\left(\beta b_{it} + 1ln(pop_{it+1}) + \nu_i + \nu_t + \varepsilon_{it}\right) \tag{4.3}$$

We estimate Equation 4.3 using a Poisson regression where y_{it+1} is the number of teen births in period $t+1$, controlling for the population of women aged 15 to 19 in state i in year t, pop_{it}, and constraining the coefficient on this control to be unity. We also control for state fixed effects, μ_i, and year fixed effects, θ_i. We exclude controls for other policies, X_{it}, from Equation 4.1 to avoid contamination issues, since, these other policies may potentially represent other treatments. de Chaisemartin and D'Haultfoeuille (2021) 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 treatment effect heterogeneity. We compare the magnitudes of these alternative estimates to those presented in Section 4.4.2 to assess whether the primary estimations are biased. In particular, our concern is whether the primary estimations are upward-biased, potentially indicating that TRAP laws have an impact when they in fact do not. As such, statistical inference on these alternative estimates is not our objective. Nonetheless, we present Fisher exact p-values beside each estimate (Fisher, 1922).[32] Given the significantly

[32]Since one state is treated in each estimation, using traditional inference methods such as clustered-standard errors would lead to incorrect inference (Bertrand, Duflo, and Mullainathan, 2004,Roodman, Nielsen, MacKinnon, and Webb, 2019). Inference using clustered standard errors requires a sample with many clusters in which many of them change treatment status. Clustered standard errors at the state level tend to over reject the zero effect null hypothesis when the number of treated units is small. (MacKinnon and Webb, 2018; MacKinnon and Webb, 2020). In the extreme case, when only one cluster is treated, cluster-robust standard errors would severely underestimate the variance of the difference-in-difference estimator (Ferman and Pinto, 2019).

Therefore, we conduct exact inference without relying on large-sample approximations and without making assumptions about the distribution of the error terms. We consider the distribution of possible treatment effect estimates obtained by estimating Equation 4.3 where we have reassigned treatment to each state in the sample, one-by-one. We then compare the estimate for the actual treated state with the distribution of possible treatment effect estimates. We follow the p-values definition in Young (2019), which specifies the exact p-values to be uniformly distributed, and which offers an exact test with a rejection probability equal to the nominal level of the test.

reduced sample size in each estimation one should expect much less precision on these estimates.

We estimate the impact separately for each of the five types of TRAP laws described in Section 4.3.1. Estimations for $r = 10$ are presented in Figures C.8 and C.9, as $r = 10$, or $j = [-5, 4]$, is the most comparable to the event studies presented in Section 4.4.2. For each state, we show in brackets the one-sided and two-sided Fisher exact p-values for treatment permutation at the state level, as described above. We present the average effect across all the estimations presented in the figure for comparison with the average effects presented in Section 4.4.2.

Estimates for other values of r are presented in Table C.4, including estimations employing two different sets of controls: (i) never treated states only and (ii) never or future treated states. For each type of TRAP law, Table C.4 compares the primary estimate from Section 4.4.2 to the alternative "stacked DiD" estimate, that is, the unweighted average of the coefficients from the separate estimations of Equation 4.3 for each policy change.

This exercise confirms that estimated impacts of exposure to any TRAP law are not upward-biased. The stacked DiD estimates are nearly all larger in magnitude than the average of post-intervention treatment effects in the primary estimation of 3 percent, with median values across the various specifications of 1.96 percent and 3.02 percent. The same is true for admitting privileges, transfer agreement laws, and building regulations, where the stacked DiD estimates are all larger in magnitude than the primary estimation. Only for distance regulations we find that the stacked DiD estimations are smaller than the event study estimation. Nonetheless, nearly all of the variations of stacked DiD estimates for distance regulations remain positive, with median values of 0.99 percent

The definition is as follows:

$$p = \frac{1}{M}\sum_{S=1}^{M} I_S(> T_E) + U * \frac{1}{M}\sum_{S=1}^{M} I_S(= T_E)$$

where T_S are equally probable potential treatment allocations, T_E is the true treatment effect, M is the total number of potential treatment allocations, $I_S(> T_E)$ is an indicator function for $T_S > T_E$, $I_S(= T_E)$ is an indicator function for $T_S = T_E$, and U is a random variable drawn from a uniform distribution (0,1).

Since this definition has a random component from a uniform distribution, we report the upper bound of each p-value, which corresponds to a draw from the uniform distribution equal to one. Based on the potential control states in each "quasi-experiment," the minimum p-values will be between 128 and 119. SC 1996 and AL 1997 are compared to 28 states; AR and PA 1999 are compared to 25 states; MI 2000 and NE 2001 are compared to 24 states; RI 2002, MO 2005, IN, OH, and SD 2006 are compared to 23 states; KS, MO, and UT 2011, MD and PA 2012, and VA 2013 are compared to 18 states. These 18 states are the never-treated states.

and 1.79 percent. This suggests that while the event study estimate of 3.48 percent may be an overestimate, there is still good reason to believe that the impact of distance regulations on Black teen births is positive.

4.4.5 Mechanisms

In this section we present evidence on the pathways by which the implementation of a TRAP law may impact teen births. There is anecdotal evidence that TRAP laws result in the closures of clinics that are unable to comply with the new regulations. 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, Myers, Schlosser, and Cunningham, 2019; Kelly, 2020). We document that TRAP laws exhibit similar impacts on distance to clinic and reduce abortion use nationally.

A second mechanism by which TRAP laws may affect teen fertility is through changes in teen sexual behavior. If teens perceive a reduction in abortion access, they may reduce sexual activity or increase contraception use to avoid unintended pregnancy. Such behavioral responses may dampen the impact of TRAP laws on teen births.[33]

4.4.5.1 Abortion access

We employ county-by-year data on distance to the nearest abortion clinic compiled by Caitlin Myers. 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.[34] We estimate a simple, two-

[33]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 may have been related to avoiding motherhood, which may not be relevant anymore.

[34]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 distance regulations are most likely to close clinics. As such, this data is not useful for our purposes.

period difference-in-differences estimation

$$D_{cst} = \alpha + \beta_1 TRAP_s + \beta_2 post_t + \beta_3 (TRAP * post)_{st} + \varepsilon_{cs} \tag{4.4}$$

where D_{cst} indicates distance to the nearest abortion clinic 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 clinic from 2009 to 2017.

Estimates of β_1 are presented in Panel A of Table C.5. We find that the enforcement of a TRAP law increases the average distance to the nearest abortion provider by 10 to 12 percent when measured in distance and by 15 percent when measured by driving time.

4.4.5.2 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, both aggregated and disaggregated by age. They are additionally available disaggregated by race from 2008 to 2018.[35] 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 states 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 race or age group.[36]

[35] We thank Caitlin Myers for sharing with us a version of these data already extracted from the CDC's website and compiled.

[36] 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.

We estimate

$$E\left[y_{st}|TRAP_{st}, \boldsymbol{X_{st}}, \nu_s, \nu_t\right] = exp\left(\gamma TRAP_{st} + 1ln(pop_{st}) + \boldsymbol{X'_{st}}\delta + \nu_s + \nu_t + \varepsilon_{st}\right) \quad (4.5)$$

where y_{it} is the number of abortions in 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 4.4.1, and fixed effects are included at the state and year levels. As in Equation 4.1, we estimate a Poisson model and control for the exposure, pop_{it}, the relevant population of women in state s in year t, and constraining the coefficient on this control to be unity.

Estimates of γ are presented in Table C.5 for all women, White women, Black women, and all teens. Column 1 controls for other major abortion restrictions; Column 2 additionally controls for other relevant state policies. Whether using CDC or Guttmacher data, we consistently find a reduction in abortion rates of 4 to 4.3 percent in the fully controlled model; this effect is statistically significant at the 10 percent level when estimated with the CDC data. Consistent with our findings in Section 4.4.2, effects are entirely driven by reductions in abortion use among Black women. Effects for White women in the fully controlled model are very close to zero (though with a wide confidence interval). Among Black women, the estimated effect is 4.7 percent, although not statistically significant at convenient levels. Among all teens, TRAP laws reduce abortion use by 5.5 percent, an effect that is significant at the 10 percent level. Unfortunately, data on abortion counts by race *and* age are not available. Nonetheless, given the fact that impacts are larger for teens than for all women and are almost entirely driven by Black women, we speculate that the impacts on Black teens are larger than 5.5 percent.

4.4.5.3 Teen sexual behavior

We rely on data from the Center for Disease Control's Youth Risk Behavior Surveillance System (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, and state of

residence.[37] We combine this with our state-by-year data set on TRAP law enforcement. We estimate

$$Y_{isya} = \beta TRAP_{sy} + \boldsymbol{X'_{sy}}\delta + \nu_s + \nu_y + \nu_a + \varepsilon_{isya} \tag{4.6}$$

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 4.4.1. 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.[38]

Results are presented in Table C.6, with and without the full set of controls, separately for Black and White teen girls. We find that TRAP laws delay sexual debut among White teen girls 2.3 percentage points, a 5.5 percent effect relative to the mean of 42 percent. Among Black teen girls, the impacts on sexual debut are 50 percent smaller and not statistically significant. This may be one reason why we observe impacts of TRAP laws on births for Black teens and not White teens. White teens are more likely to delay sexual debut as a response to TRAP laws.

We also find that Black teen girls experience a large and significant decline in contraception use as a result of TRAP laws. This decline is not observed among White teens, whose change in contraceptive use is approximately zero. This is the opposite of the expected behavioral response, which would be to increase contraceptive use in response to a reduction in abortion access. However, it is important to note that many clinics that close as a result of TRAP laws, such as Planned Parenthood clinics, are providing not only abortion services but also other reproductive health services. Planned Parenthood and other Title X clinics are a primary source of contraception for young women and low-income women. If TRAP laws result in the closure of some of these clinics, it is not surprising to see decreases in contraceptive use among Black teens

[37]The YRBSS combined dataset does not contain information on all the states. Some state and district health and education agencies that conducted the surveys did not give CDC permission to include their data in this dataset. Furthermore, some states did not collect information on specific questions or did not release this information to CDC. The combined dataset contains data for 44 states. The states whose information is not collected are Indiana, Massachusetts, Minnesota, Ohio, Oregon, and Washington.

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

as a result in reduced access. This offers a second reason why we observe impacts of TRAP laws on Black teen births: not only are these young women less able to access abortion, they are also potentially less able to access contraception.

4.5 Impacts on women's educational attainment

4.5.1 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, and educational attainment.

Based on the findings in Section 4.4, we restrict our sample to Black women.[39] We further 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 i 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).[40] For robustness, we present a version of our analysis where we define exposure based on state of residence at interview rather than state of birth in Table C.9 and the results are consistent.

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 much include cohorts born both before and after

[39] 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.

[40] 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.

$t - 19$. Therefore, in order to restrict our analysis to post-*Casey* TRAP laws, we do not consider cohorts born before $1992 - 19 = 1973$. 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. Or analysis sample includes Black women born in the U.S., aged 25-46 at interview, and born between 1973 and 1994.

Our outcomes of interest are college initiation, defined as completing at least one year of college or more, and college completion, defined as completing at least a bachelor's degree. In our sample, 55 percent of women initiated college and 24 percent of women completed college. In terms of exposure to TRAP laws, 23 percent of the sample were exposed to a TRAP law in adolescence, 39 percent were exposed later in life, and 38 percent were never exposed, as shown in Figure C.10.

4.5.2 Estimation

We estimate the impact of exposure to TRAP laws during adolescence on college initiation and college completion. We begin with a pooled difference-in-differences estimation. In section 4.5.4, we demonstrate that our findings are robust to heterogeneous treatment effects using a stacked difference-in-differences approach. The pooled estimation is found to be biased towards zero but is nonetheless our preferred specification given its greater statistical power.

We estimate the Equation:

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

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 adolescent exposure to a TRAP law, which is determined by birth year b and state s. $\boldsymbol{X_{bs}}$ is a vector of indicators of adolescent exposure to other relevant state-level policies, which are discussed in Section 4.4.1. We include fixed effects for the birth year to control for national cohort trends in outcomes. We include state fixed effects to control for time-invariant differences across states. We include age at interview fixed effects to control for natural increase in average educational attainment with age, which occurs even after our threshold of age 25. Standard

errors are clustered at the state level.

Our data have too few observations in each single-year category of age at first exposure to estimate an event study. However, we can test the assumption of common trends by replacing exp_{bs} with a set of indicators for first exposure at various age groups. The results are shown in Figure C.11. Consistent with the assumption of common trends, first exposure to a TRAP law at ages 30 to 34 or ages 25 to 29 has a near-zero impact on educational attainment. We see that first exposure at ages 20 to 24 or 18 to 19 reduces college initiation by 1 percentage point, whereas exposure before age 18 reduces college initiation by nearly twice as much. A similar pattern applies to college completion. Based on these findings, we feel confident in the assumption of common trends, and we recognize that exposure as a minor (under 18) rather than as a teen (under 20) may be the more relevant exposure. Our preferred findings are shown in Table C.7, which correspond to exposure as a minor. As a robustness check, we also present findings for exposure as a teen in Table C.8.

4.5.3 Results

Table C.7 shows the estimates of β from Equation 4.5.2, where exp_{bs} indicates exposure before age 18. Each coefficient comes from a separate estimation of the impact of each TRAP law type. Estimations in column (1) include controls for exposure to parental involvement laws and two-trip mandatory waiting periods. Estimations in column (2) additionally control for exposure to the other policies described in Section 4.4.1.

Panel A shows the impacts on college initiation. These estimates indicate that exposure to an TRAP law before age 18 reduces the probability of initiating college by 1.1 to 1.2 percentage points, significant at the 5 percent and 10 percent levels, respectively. These reductions represent 2 to 2.2 percent reductions in college initiation relative to the mean of 55 percent. Examining each type of TRAP law, we find that early exposure to admitting privileges laws, distance regulations, and building regulations reduce the probability of college initiation by 0.9 to 2 percentage points. This is equivalent to reductions of 1.6 to 3.8 percent relative to the mean. In addition, early exposure to transfer agreement laws shows moderate reductions in college

initiation of 0.6 to 0.9 percentage points. However, these estimates are not large enough to be statistically significant at conventional levels.

Panel B indicates that exposure to any TRAP laws before age 18 reduces college completion by 1.2 percentage points. This represents a 5 percent reduction relative to a mean of 24 percent and is significant at the 5 percent level in the model with full controls. Considering TRAP laws by type, we find this effect is driven by distance and building regulations. Impacts of exposure to admitting privileges laws and transfer agreement laws are also negative but not significant at conventional levels.

4.5.4 Robustness tests

We next test whether the results presented in Section 4.5.2 may be biased by heterogeneous effects over time or across units, as recently proposed by several studies (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Athey and Imbens, 2022; Goodman-Bacon, 2021). We separately estimate the impacts of exposure to each policy change, selecting the appropriate set of control states in each estimation. We then "stack" these separate estimates by averaging their coefficients, absent the complex weighting schemes inherent in the pooled analysis.

We separately estimate the impact of each TRAP law implementation using a difference-in-differences design, selecting the appropriate comparison group for each estimation. We construct event-specific data sets containing only the treated state and the selected comparison states. The comparison group includes states that have never implemented a TRAP law (never-treated) and those that implemented a TRAP law more than five years after the policy of interest (future treated). Table C.2 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. We also must exclude policy changes in states for which the ACS data include fewer than 150 Black women in the sample (due to very small Black populations in

these states). Table C.3 contains further information on the excluded policies.

We estimate the Equation:

$$y_{ibsa} = \beta exp_{bs} + \nu_b + \nu_s + \nu_a + \varepsilon_{ibsa}$$

where the variables represent the same as in Equation 4.5.2. We estimate the impact separately for each of the five types of TRAP laws. We employ the same sample restrictions as described in Section 4.5.1. We present estimations including all available cohorts, and an alternative estimation limiting the sample to at most 10 treated and 10 comparison cohorts.

Figure C.12 presents the estimates for each policy change of any TRAP law for college initiation and college completion. We report in each figure the average across estimates. For each outcome, nearly all DiD estimates are negative and most estimates are between 1 and 5 percentage points. For each outcome there is one outlier state with a very large estimated effect (MO 2011 for college initiation and KS 2011 for college completion). While these outliers pull up the mean effect to 4 or 5 percentage points, most estimates are in the range of 1 to 3 percentage points. Table C.10 presents alternative versions of the stacked DiD estimates, varying the nature of the control group, and whether there is a limit on the number of cohorts.

For both college initiation and college completion, we find that all variations of the stacked DiD estimates are larger in magnitude than the pooled estimations presented in Table C.7, with only one exception out of forty comparisons. This rules out the possibility that the primary methodology is overestimating the impact of TRAP laws on Black women's education. If fact, it suggests that the estimates in Table C.7 are biased towards zero and are lower bounds for the true magnitude of the impacts. This is consistent with the issues recently identified with the pooled DiD, two-way fixed effects method: in the presence of heterogeneous effects over time the earliest and latest events are underweighted and events in the middle are overweighted. As shown in Figure C.12, the largest estimates are for the most recent events. Nonetheless, despite the attenuation bias, the pooled DiD estimates are useful given their superior statistical power for hypothesis testing.

Overall, we conclude that TRAP law exposure before age 18 negatively impacts college initiation and college completion. The median impact on college initiation across all law types

and specifications is a reduction of 2.4 percentage points, representing a 4.4 percent decrease in the probability of college initiation. Likewise, the median impact on college completion is a reduction of 4.8 percentage points, which corresponds to a 20 percent decline in the probability of completing college.

4.6 Conclusions

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 two to 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. 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 the first national-level estimates of the impacts of TRAP laws on teen 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 recent concerns about the biasing effects of heterogeneity in two-way fixed effects estimations. As 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 birth to Black teens by 3 percent, while they do not change existing trends in births to White teens. We provide evidence that these effects are operating through the expected mechanisms – decreases in abortion access and abortion use – and through an unexpected mechanism, decreases in contraceptive use. We speculate that TRAP-induced clinic closures reduced Black teen's access to not only abortion services but also contraception.

Our findings are consistent with previous evidence that TRAP laws in Texas and Pennsylvania reduced abortion use and increased birth rates, either through increasing the average distance to the nearest abortion clinic or increasing clinic congestion (Quast, Gonzalez, and Ziemba, 2017; Lu and Slusky, 2016; Fischer, Royer, and White, 2018; Lindo, Myers, Schlosser, and Cunningham, 2019; Kelly, 2020). The impacts of TRAP laws are consistent with evidence that other abortion restrictions, such as parental involvement laws and mandatory waiting periods, also decrease abortion use and increase births (Myers and Ladd, 2020; Myers, 2021). Across all these findings, a consistent theme is the stronger impacts of abortion restrictions on Black women. This likely reflects the higher level of unmet need for contraception and higher level of abortion use among Black women. In turn, these factors likely reflect the structural racism facing Black women in the United States, including a history of reproductive coercion that perpetuates mistrust of the health care system and may deter women from seeking care.

We also document the impacts of TRAP laws on educational attainment. This study is among the first to present evidence on the impacts of abortion restrictions in recent decades on women's long-term economic welfare.[41] Using pooled and stacked difference-in-differences approaches, we find that exposure to TRAP laws before age 18 reduces Black women's college initiation and college completion as measured at ages 25 to 40. Our results indicate that college initiation is reduced by 1 to 3 percentage points, an effect of at least 2 percent, relative to the percent of Black women initiating college. Similarly, their probability of completing college decreases by 1 to 3 percentage points, an effect of at least 4 percent, relative to the percent of Black women completing college. As of 2015, Black women's college completion rate was only 25 percent, as compared to 45 percent for White women (Guyot and Reeves, 2017). Our findings suggest that TRAP laws are acting to exacerbate this existing racial inequality by preventing Black teens from avoiding unwanted motherhood and thereby reducing their ability to complete their education.

[41] Other evidence on the impacts of modern abortion restrictions include the Turnaway study, which has documented that gestational limits harm the economic wellbeing in the near-term of women who were denied wanted abortions between 2008 and 2010, focusing on outcomes 6 months and 4 years later (Miller et al., 2020a) and unpublished dissertation that documents that minors' exposure to parental involvement laws in the 1990s reduced high school completion and college initiation for Black women, as measured at ages 21 to 32 (Borelli, 2011).

As the reproductive health care landscape continues evolving and more states enforce restrictions on abortion providers, evidence of these policies' impacts is critical. A recent amicus brief to the U.S. Supreme Court claimed that, as a result of social and economic changes in recent decades, access to abortion is no longer necessary in order for women to fulfill their economic potential. 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.

5. SUMMARY AND CONCLUSIONS

In this dissertation, I use quasi-experimental approaches and non-experimental data to study the health and economic implications of access to reproductive health care in the US. These studies provide evidence of the causal impacts of reproductive health care policies and how such impacts extend beyond access and use, potentially altering women's life trajectories. As such, I investigate the long-run economic impacts of historical policies and the health and economic effects of more recent policies targeting the demand and supply-side of the abortion markets.

The US landscape for reproductive health care has undergone massive changes in the last decades. Different policies expanding and restricting access to reproductive technologies have been recently implemented. Studying their economic effects is fundamental to understanding the potential merits and costs of subsidizing or restricting their access. However, some of these policies are so recent that we cannot explore their causal economic impacts yet. Nonetheless, historical changes in contraception and abortion access can inform us on what we can expect in the long run from current policies.

In chapter 2, we revisit the evidence provided in previous studies on the economic impacts of the expansion of access to oral contraception to unmarried women in the 1960s. We extend this evidence by evaluating the effects of both access to oral contraception and legal abortion on longer-run outcomes and using information from the Health and Retirement Study. We find evidence of improvements in educational attainment among Black women, although these estimates are not statistically significant. Regarding employment and earnings, we find increases in the probability of working in a Social Security-covered job in women's 20s and 30s, but we find no evidence of positive effects on women's earnings in their 50s. These findings indicate that current policies that expand access to reproductive controls have the potential to improve economic outcomes for women.

Since abortion became legal in the US in 1973 after the Supreme Court case *Roe vs. Wade*, those states where opposition to abortion has been historically strong have implemented different

policies that restrict access to abortion services. These policies can target the demand side of the market—the patients—or the supply-side of the market—the abortion clinics and providers.

In chapter 3, we study the impacts on abortion timing and abortion use of a demand-side restriction—a mandatory waiting period implemented in Tennessee in 2015. This policy requires women to have in-person counseling and wait at least 48 hours before the abortion procedure. We find that the introduction of the mandatory waiting period increased the share of abortions obtained during the second trimester in Tennessee relative to other states. We also find inconclusive evidence of increases in overall abortion rates, but our results suggest a reduction caused by this policy.

In chapter 4, we study the impacts of exposure during adolescence to supply-side restrictions on fertility and educational attainment. These policies, commonly known as targeted regulations of abortion providers (TRAP laws), require abortion facilities to comply with specific requirements to be allowed to provide abortion services. Some of these policies are onerous, obliging abortion facilities to close and increasing the costs of entering the abortion market to new providers. We find that the implementation of TRAP laws increases Black teen births, while they do not change existing trends in births for White teens. These changes in births operate through decreases in abortion access, abortion use, and contraceptive use. We further document that adolescent exposure to TRAP laws before age 18 reduces Black women's probability of initiating and completing college.

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APPENDIX A

FIGURES AND TABLES FOR CHAPTER 2

A.1 Tables

Table A.1: Effects of the Pill and abortion on years of education

	Full sample		Black women	
Contraception coding:	BHM (2012) (1)	Myers (2017) (2)	BHM (2012) (3)	Myers (2017) (4)
Pill consent	0.3677	0.2030	0.6627	0.3537
	(0.2157)	(0.1782)	(0.4279)	(0.3579)
Pill legal	0.2488	0.2282	0.0722	0.0288
	(0.1384)	(0.1548)	(0.324)	(0.4031)
Abortion consent	-0.3104	-0.3837	0.7801	0.6052
	(0.3482)	(0.3180)	(0.5444)	(0.5454)
Abortion legal	-0.2276	-0.2724	1.4631	1.3402
	(0.2665)	(0.2704)	(0.3490)	(0.3440)
Observations	9,390	9,390	2,095	2,095

Notes: The table reports coefficients and robust to heteroskedasticity clustered at the state-level standard errors in parenthesis. The dependent variable is years of education up to a maximum of seventeen. Pill (abortion) consent measures the proportion of years from ages 18 to 20 in which the pill (abortion) was legally available and allowed minors to legally consent for them. Pill (abortion) legal and abortion legal measures the proportion of years from ages 18 to 20 in which the pill (abortion) was legally available but unmarried minors of these ages could not consent. See the text, including Footnote 2, for additional details on the models.

Table A.2: Effects of the Pill and abortion on working in a social security covered job

	Pill consent		Early access to abortion	
Legal	BHM (2012)	Myers (2017)	BHM (2012)	Myers (2017)
coding:	(1)	(2)	(3)	(4)
20-24	0.037	0.025	0.053	0.042
	(0.018)	(0.017)	(0.018)	(0.017)
25-29	0.076	0.055	0.138	0.070
	(0.019)	(0.024)	(0.026)	(0.040)
30-34	0.044	0.054	0.056	0.049
	(0.019)	(0.018)	(0.033)	(0.036)
35-39	0.017	0.027	0.015	0.021
	(0.015)	(0.019)	(0.017)	(0.016)
Observations	305,877	305,877	305,877	305,877

Notes: The table reports coefficients and standard errors robust to heteroskedasticity clustered at the state-level in parenthesis. The sample includes 7,608 unique women. The dependent variable is an indicator variable that takes value of one if the respondent showed zero earnings in the Social Security information. This information comes from the SSA supplement to the HRS. "Pill consent" is equal to one if a woman would have could legally consent for contraception before age 21 in her state of residence as a youth. Early access to abortion represents early access to abortion. In column (3), it is equal to one if a woman lived in an early-legalizing state before age 21, and in column (4), it is equal to one if a woman could legally consent to having an abortion before age 21.

Table A.3: Effects of the Pill and abortion on the log of real hourly wage on the previous year

Legal coding:	BHM (2012)	Myers (2017)
	(1)	(2)
Pill consent x age 50-54	0.018	0.014
	(0.049)	(0.049)
Pill consent x age 55+	-0.029	-0.032
	(0.034)	(0.041)
Early access to abortion x age 50-54	-0.0056	-0.031
	(0.083)	(0.105)
Early access to abortion x age 55+	-0.077	-0.146
	(0.094)	(0.066)
Observations	24,907	24,907

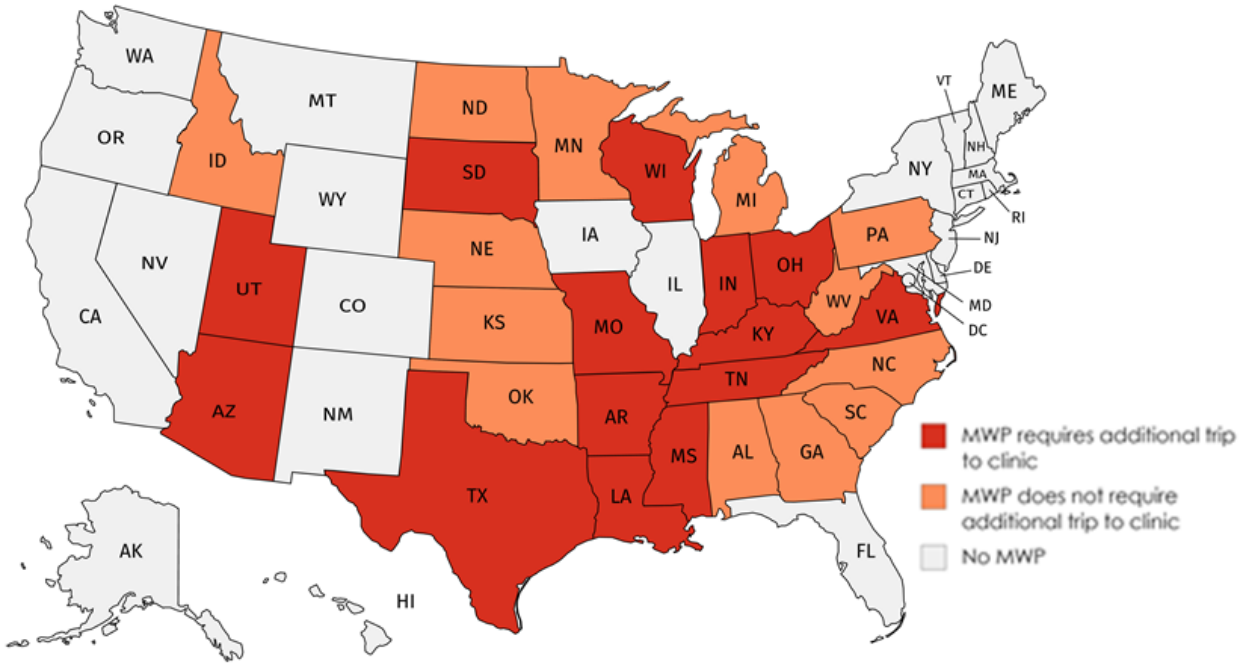
Notes: The table reports coefficients and standard errors robust to heteroskedasticity clustered at the state-level in parenthesis. The sample includes 6,533 unique women. The dependent variable is the log the real hourly wage (2000's dollars) of the previous year. Observations with zero wages are excluded from these estimations. "Pill consent" is equal to one if a woman would have could legally consent for contraception before age 21 in her state of residence as a youth. "Early access to abortion" in column (1), it is equal to one if a woman lived in an early-legalizing state before age 21 and, in column (2), it is equal to one if a woman could legally consent to having an abortion before age 21.

APPENDIX B

FIGURES AND TABLES FOR CHAPTER 3

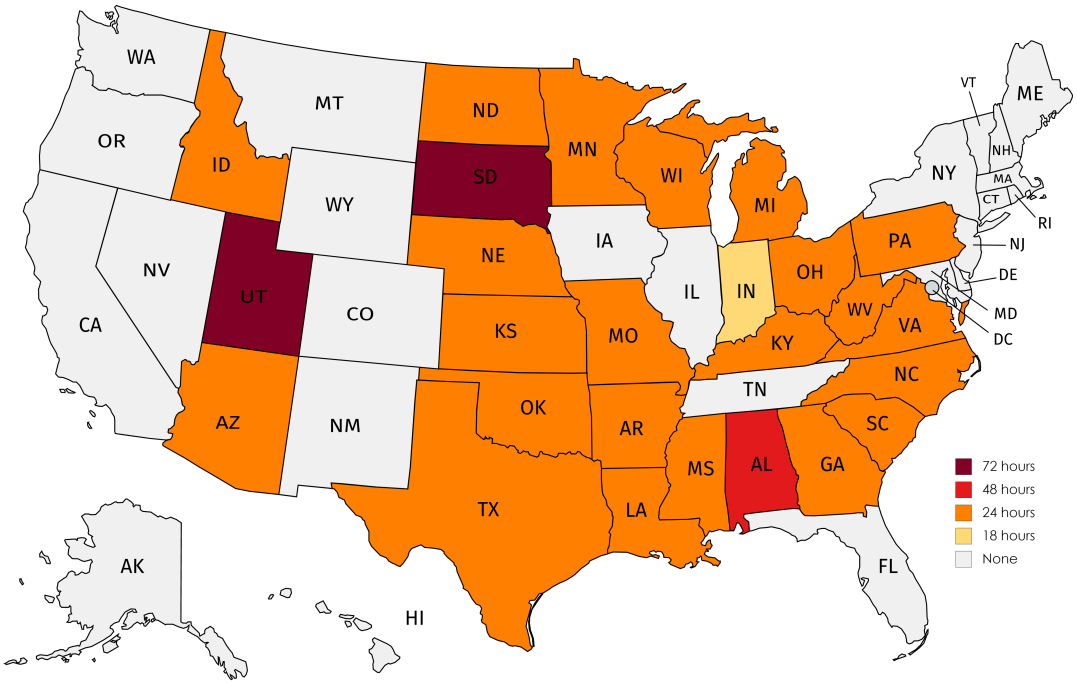
B.1 Figures

Figure B.1: Mandatory waiting periods for abortion, 2019



Source: The map shows the mandatory waiting period in each state as of August 19th, 2019. The map was created by the authors using information on *Counseling and Waiting Periods for Abortion* from the Guttmacher Institute.

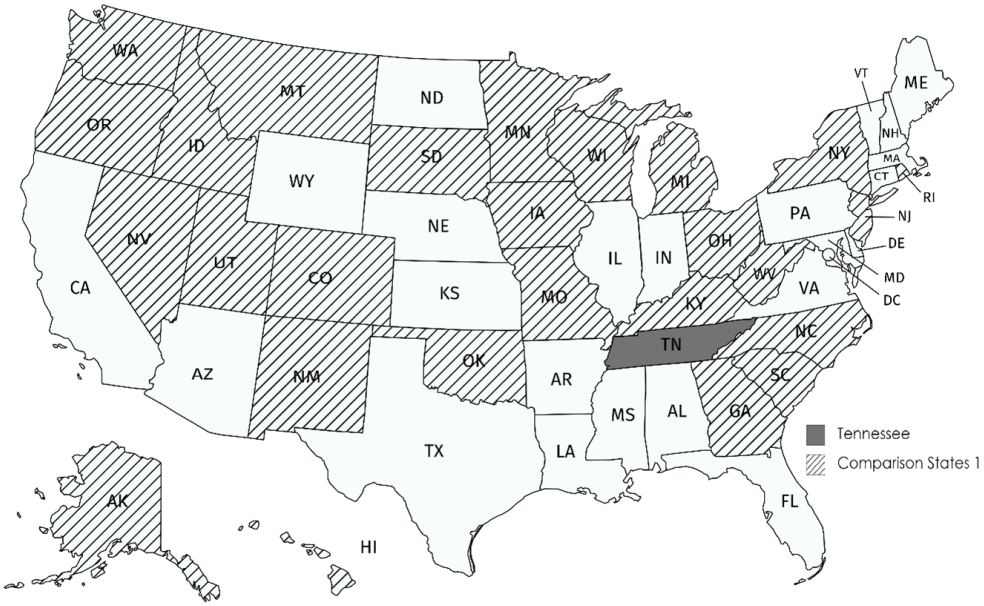
Figure B.2: Mandatory waiting periods by length of waiting period, 2014



Note: The map shows the mandatory waiting period in each state at the beginning of 2014. Source: Huffington Post.

Figure B.3: States used in analyses

Panel A. Comparison States 1, data from 2010-2016



Panel B. Comparison States 2, data from 2010-2017

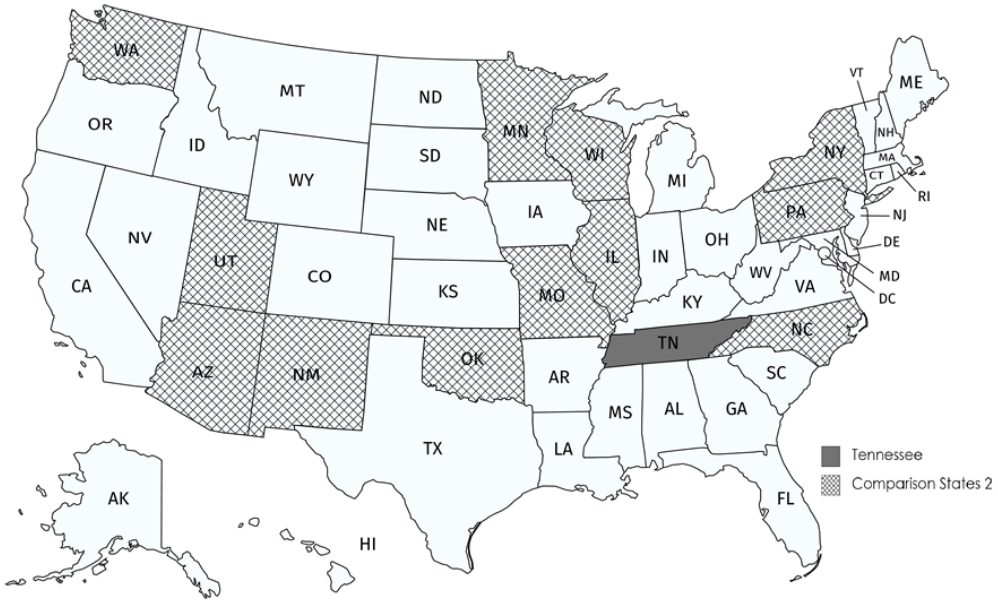


Figure B.4: Distribution (across states) of the percent of out-of-state abortions in data reported to CDC

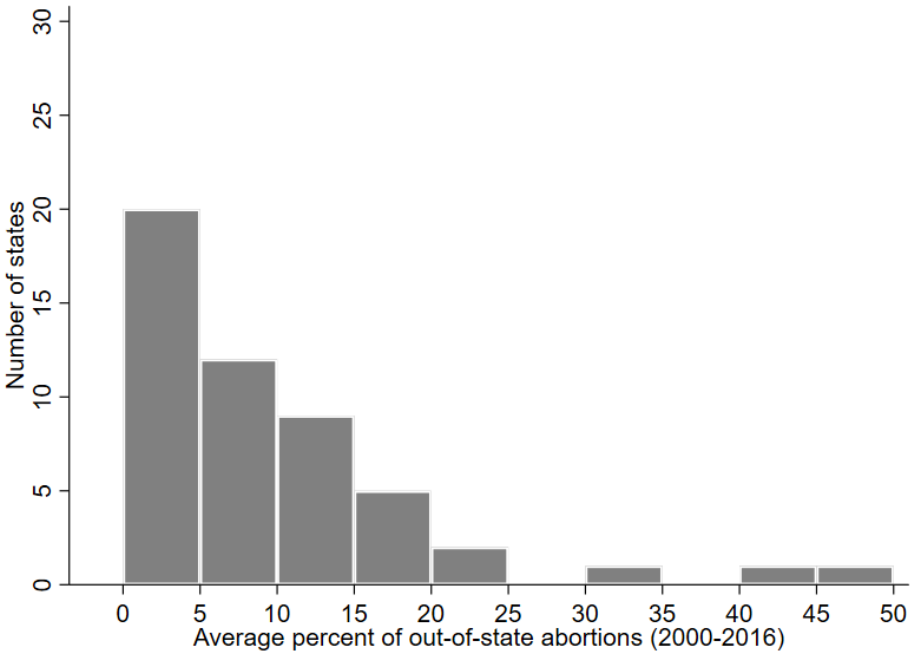
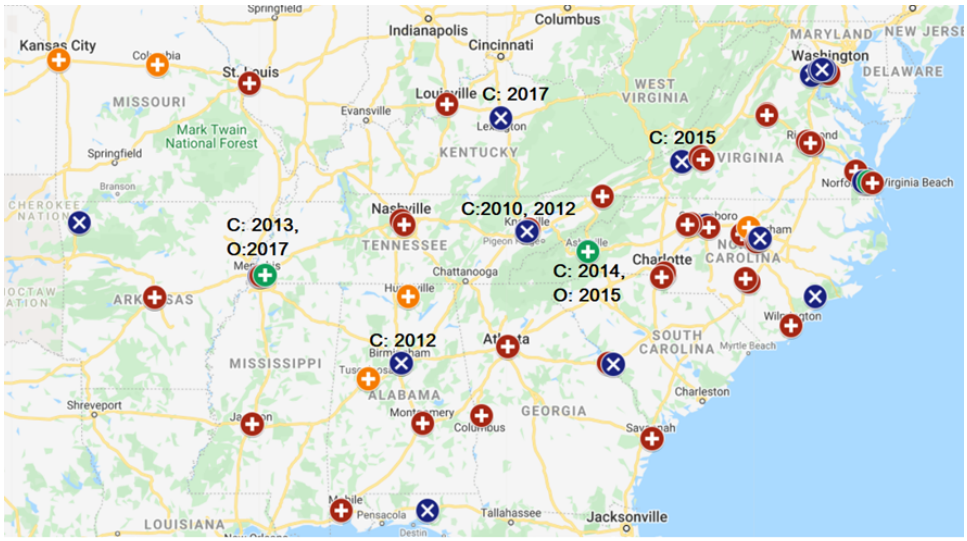
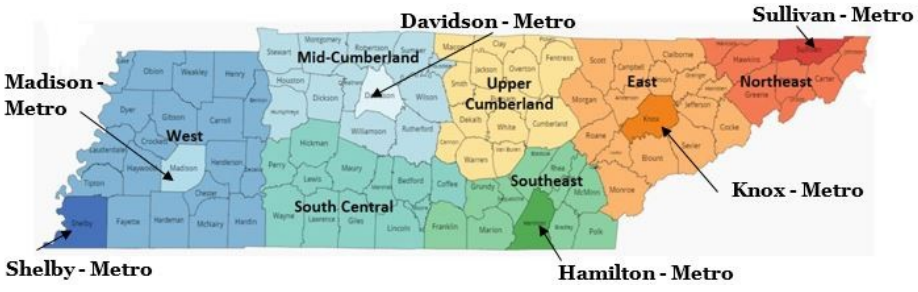


Figure B.5: Location of abortion providers in Tennessee and bordering states



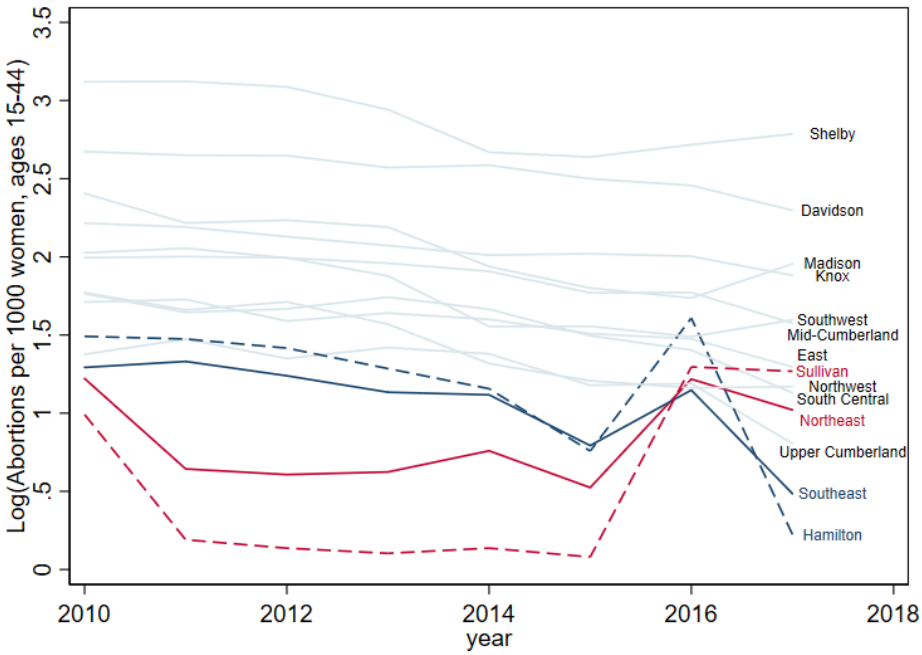
Notes: This map shows the location of abortion providers in Tennessee and bordering states who operated between 2010–2017. The providers located with a red "+" were opened during 2010–2017. The providers located with a green "+" opened after 2010 and remained opened until 2017. The providers located with an orange "+" closed at some point between 2010-2017, but they also reopened within the same period. The providers located with a "x" closed without reopening at some point during 2010-2017. Locations are only displayed for Tennessee and its bordering states. The years displayed in the map correspond to clinic openings and closures within Tennessee and those areas in bordering states closed the most to Tennessee. "C" and "O" indicate if a displayed year corresponds to a clinic closure or opening, respectively.

Figure B.6: Tennessee health areas



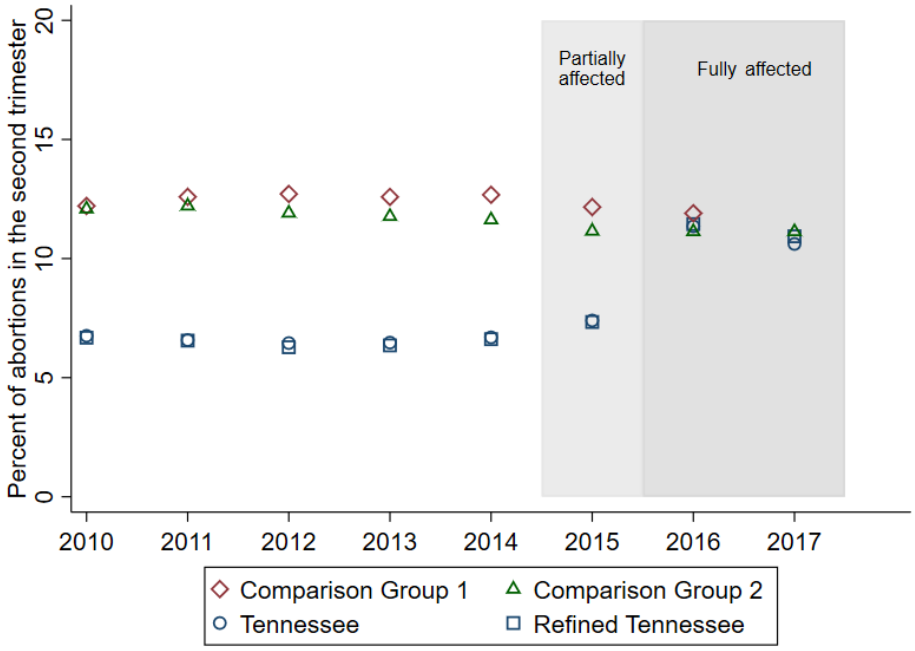
Notes: The map shows which Tennessee counties are included in each "Health Area" defined by Tennessee. Our analysis of "Refined Tennessee" excludes data from Northeast, Sullivan, Hamilton, and Southeast. Based on data from 2014, women 15-44 years old residing in these areas account for just 17 percent of such women residing in the state (and based on the data we have available to us, just 5.9% of abortions). The most populous cities in Tennessee are Memphis in Shelby County and Nashville in Davidson County. This graphic is reproduced from the Tennessee Department of Health website.

Figure B.7: Log of abortions per 1000 women (ages 15-44) by health regions



Notes: The number of abortions were collected from abortion reports from the Tennessee Department of Health. Annual state-level population estimates were obtained from the United States Census Bureau (2017).

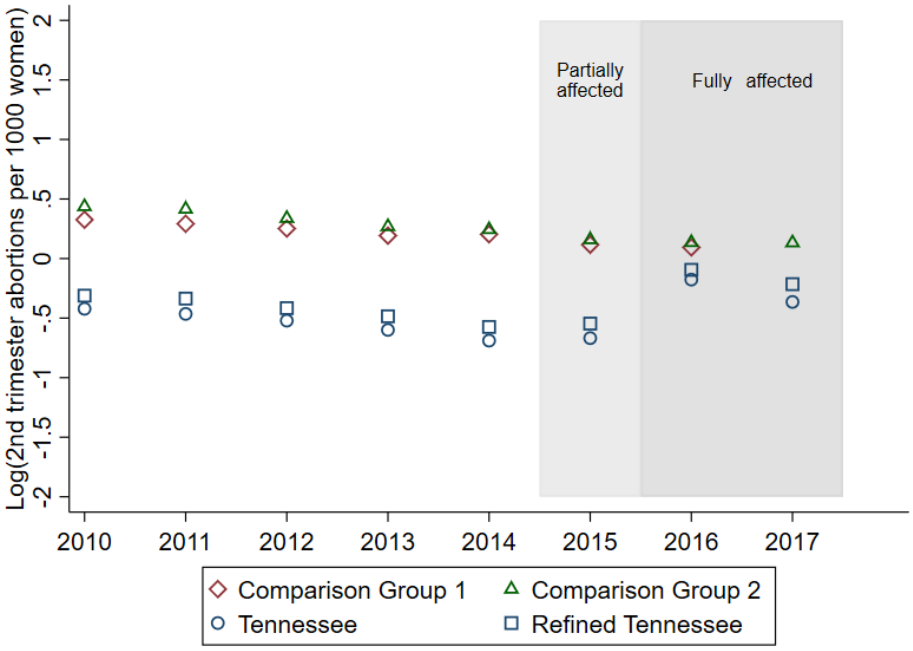
Figure B.8: Percent of abortions in the second trimester



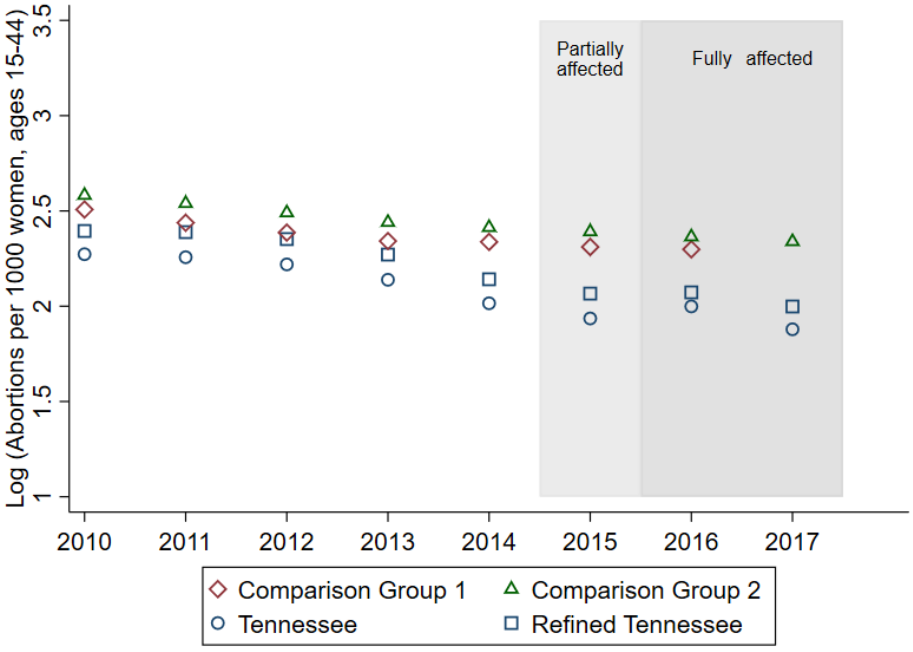
Notes: Tennessee's mandatory waiting period law went into effect in May 2015. See Figure B.3 for the states included in Comparison Group 1 and Comparison Group 2. For the sets of comparison states, we report the average weighted by the total number of women (ages 15-44) in the state. Sources: Number of abortions by gestational age were collected from state health departments by the authors and CDC Abortion Surveillance Reports. Annual state-level population estimates were obtained from the United States Census Bureau (2017).

Figure B.9: Log of abortions per 1,000 women aged 15-44

Panel A: Log of second trimester abortion rate



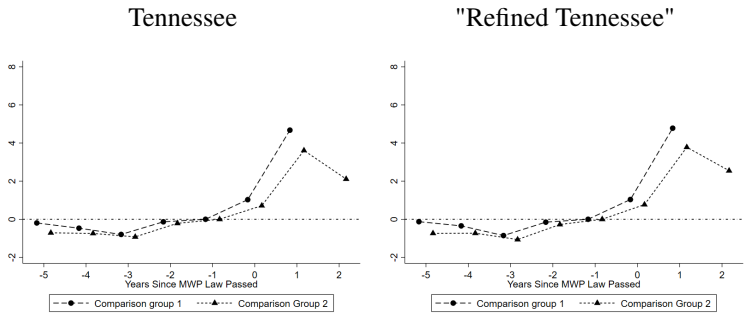
Panel B: Log of overall abortion rate



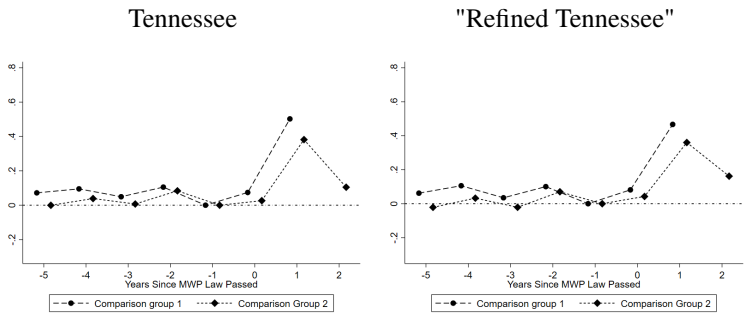
Notes: For the sets of comparison states, we report the average weighted by the total number of women (ages 15-44) in the state. See notes in Figure B.8 for more information.

Figure B.10: Event-study estimates of effects of Tennessee's mandatory waiting period

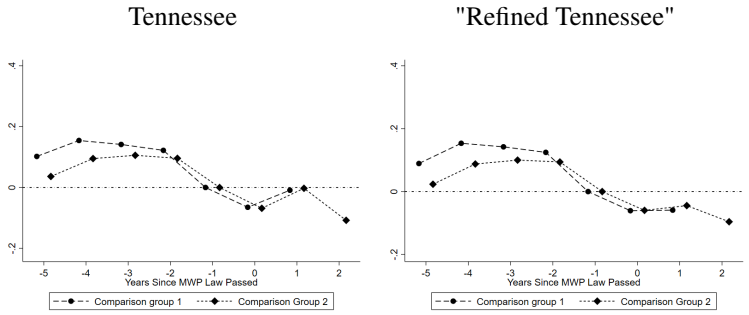
Panel A: Percent of abortions in the second trimester



Panel B: Log of the second-trimester abortions per 1,000 women (ages 15-44)



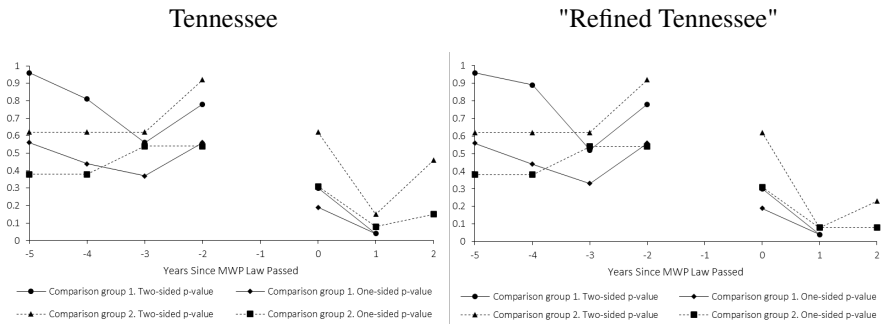
Panel C: Log of the number of abortions per 1000 women (ages 15-44)



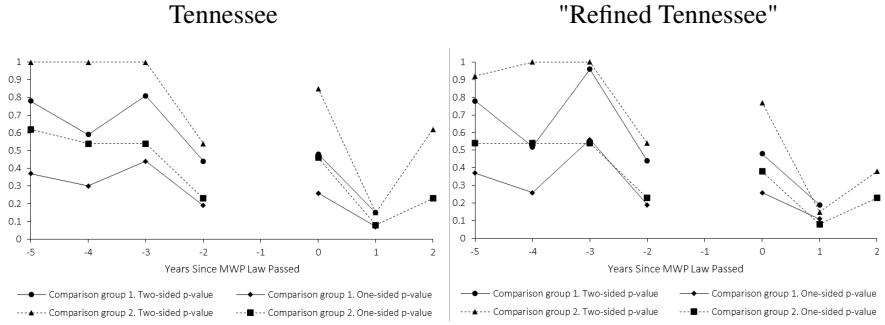
Note: See Figure B.3 for the states included in Comparison Group 1 and Comparison Group 2. Tennessee includes information on all the health areas in the state. "Refined Tennessee" excludes information on the Hamilton, Northeast, Southeast, and Sullivan health areas. See section 3.4 for more information on these health areas. The percent of abortions in the second trimester is calculated as the percent of all abortions that were obtained after 12 weeks of gestation. The second trimester abortion rate is the number abortions obtained after 12 weeks of gestation per 1,000 women (ages 15-44). The overall abortion rate is the number abortions per 1,000 women (ages 15-44). Estimates are based on indicators for Tennessee g years from its MWP going into effect in 2015, with 2014 serving as the omitted category. Estimates in both columns control for state fixed effects, year fixed effects, demographics, and the unemployment rate.

Figure B.11: Randomization Inference *p*-values associated to event-study estimates of effects of Tennessee's MWP

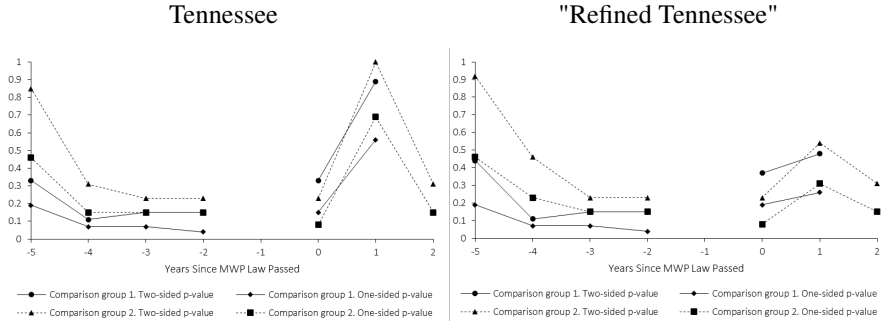
Panel A: Percent of abortions in the second trimester



Panel B: Log of the second-trimester abortions per 1,000 women (ages 15-44)



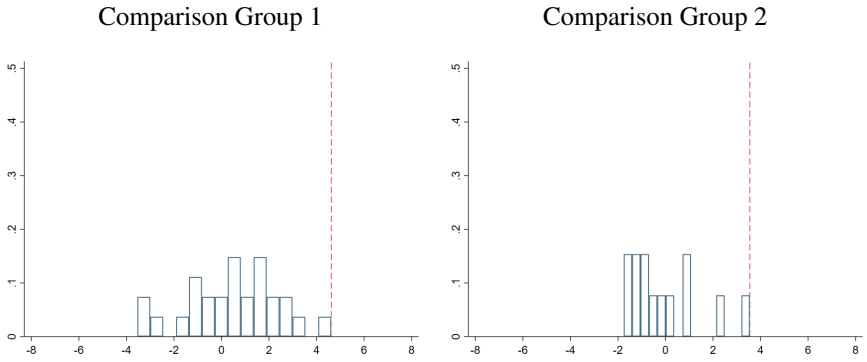
Panel C: Log of the number of abortions per 1000 women (ages 15-44)



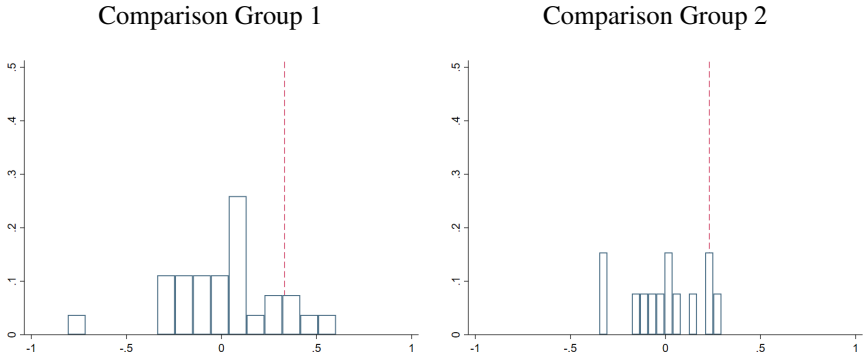
Notes: These figures show the randomization inference *p*-values associated to the event-study estimates in Figure B.10. See section 3.5 for more information on the calculation of the *p*-values. See Figure B.3 for the states included in Comparison Group 1 and Comparison Group 2. Tennessee includes information on all the health areas in the state. "Refined Tennessee" excludes information on the Hamilton, Northeast, Southeast, and Sullivan health areas. See section 3.4 for more information on these health areas. The percent of abortions in the second trimester is calculated as the percent of all abortions that were obtained after 12 weeks of gestation. The second trimester abortion rate is the number abortions obtained after 12 weeks of gestation per 1,000 women (ages 15-44). The overall abortion rate is the number abortions per 1,000 women (ages 15-44).

Figure B.12: Permutation test results associated with Table B.3's estimates

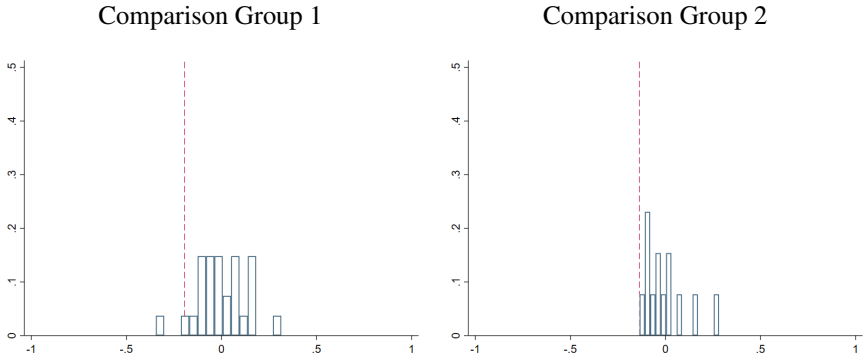
Panel A: Percent of abortions in the second trimester



Panel B: Log of the second-trimester abortions per 1,000 women (ages 15-44)



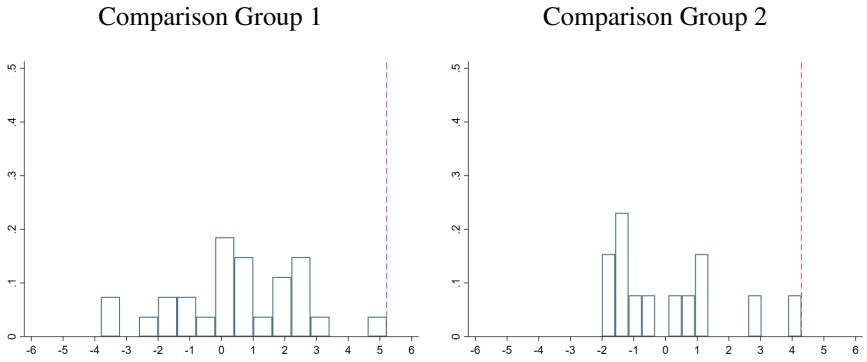
Panel C: Log of the number of abortions per 1000 women (ages 15-44)



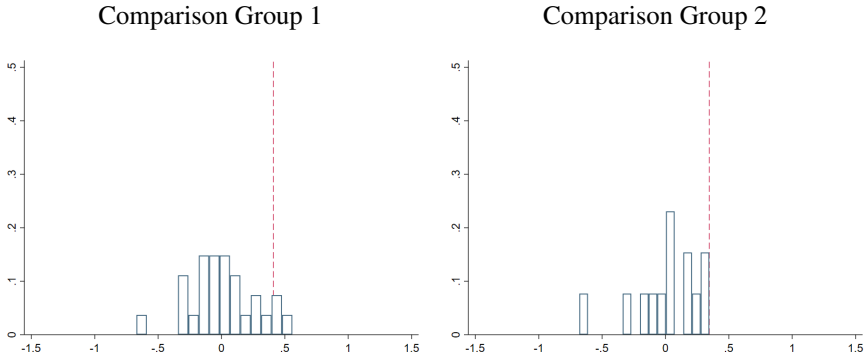
Note: Each graphic shows the results of permutation tests for the estimated treatment effects in Table B.3 that correspond to the comparison of "Refined Tennessee" with each comparison group (columns 3, 6, and 9). These estimations control for state fixed effects, year fixed effects, demographic controls, and the unemployment rate. The vertical line indicates where the estimated treatment effect for "Refined Tennessee" lies in the distribution of possible estimated treatment effects that could be estimated for any state.

Figure B.13: Permutation test results associated with Table B.5's estimates for 2016

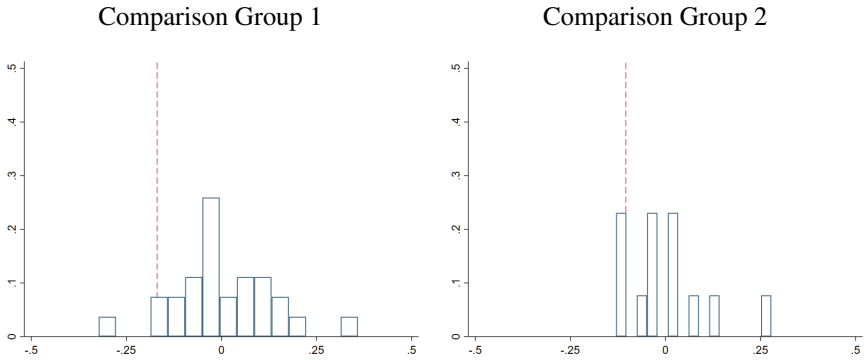
Panel A: Percent of abortions in the second trimester



Panel B: Log of the second-trimester abortions per 1,000 women (ages 15-44)



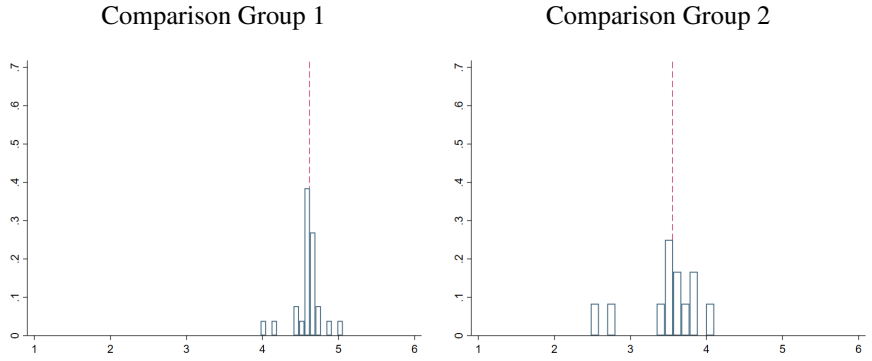
Panel C: Log of the number of abortions per 1000 women (ages 15-44)



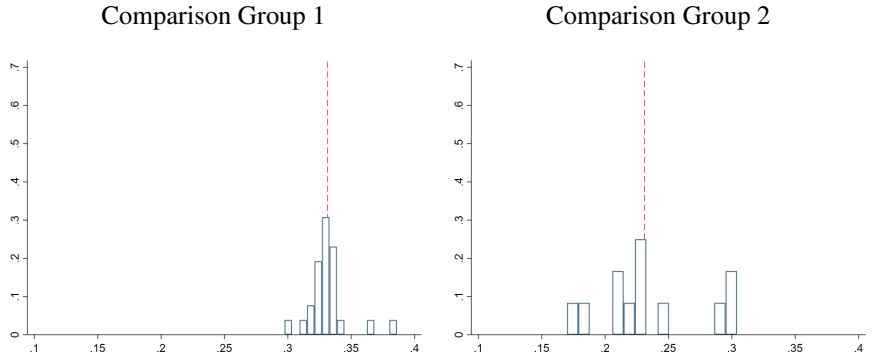
Note: Each graphic shows the results of permutation tests for the estimated treatment effects for 2016 in Table B.5 that correspond to the comparison of "Refined Tennessee", which excludes information on the Hamilton, Northeast, Southeast, and Sullivan health areas, with each comparison group (columns 3, 6, and 9). These estimations control for state fixed effects, year fixed effects, demographic controls, and the unemployment rate. The vertical line indicates where the estimated treatment effect for Tennessee lies in the distribution of possible estimated treatment effects that could be estimated for any state.

Figure B.14: Comparison of main difference-in-differences estimates to the distribution of estimates omitting one state from the comparison group

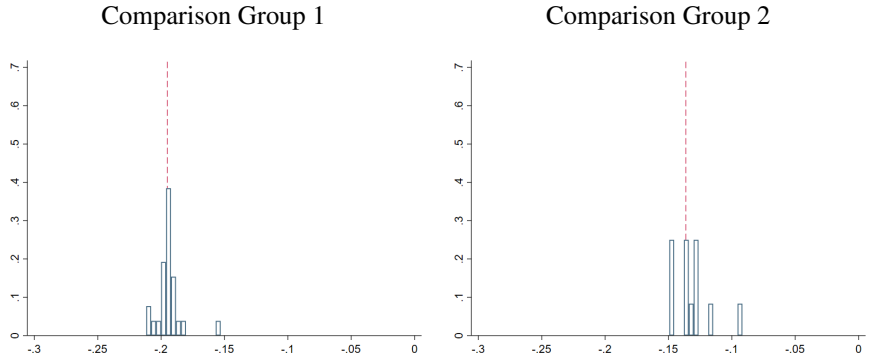
Panel A: Percent of abortions in the second trimester



Panel B: Log of the second-trimester abortions per 1,000 women (ages 15-44)



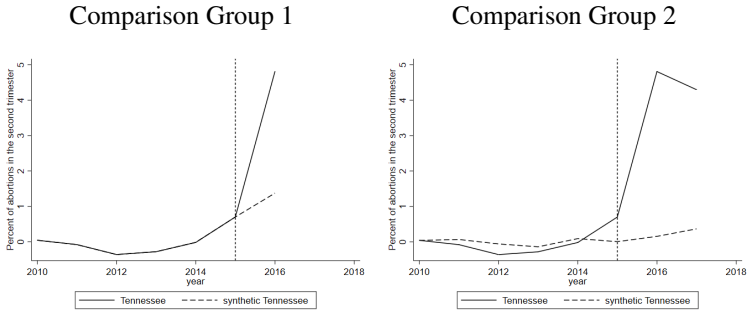
Panel C: Log of the number of abortions per 1000 women (ages 15-44)



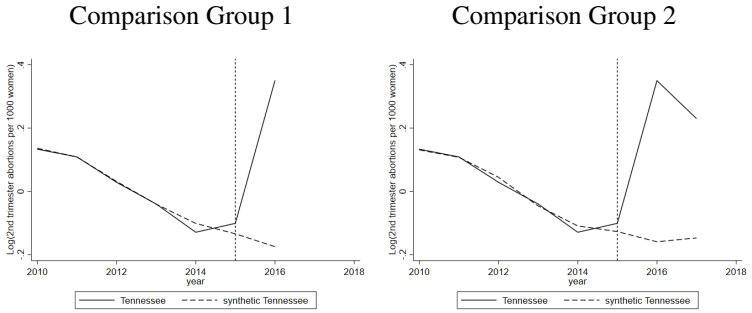
Note: The vertical line shows the estimates from Table B.3 that compare "Refined Tennessee", which excludes information on the Hamilton, Northeast, Southeast, and Sullivan health areas, with each comparison group. These estimations control for state fixed effects and year fixed effects, demographic controls, and unemployment rates (columns 3, 6, and 9). The distribution around this line shows the effects that we obtain if we omit any single state from the comparison groups.

Figure B.15: Demeaned outcomes for Tennessee and synthetic controls

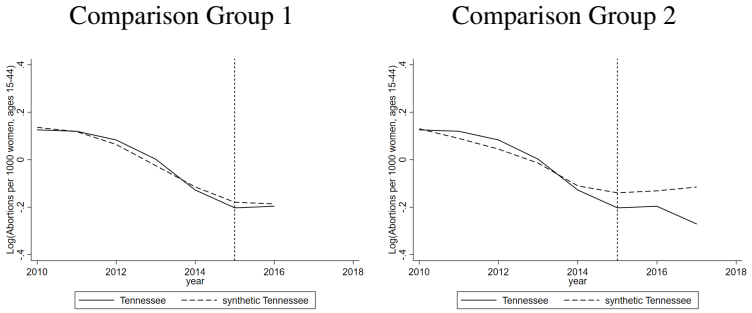
Panel A: Percent of abortions in the second trimester



Panel B: Log of the second-trimester abortions per 1,000 women (ages 15-44)



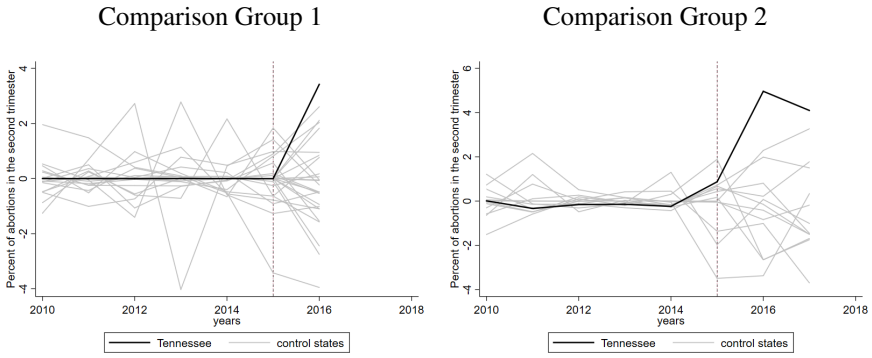
Panel C: Log of the number of abortions per 1000 women (ages 15-44)



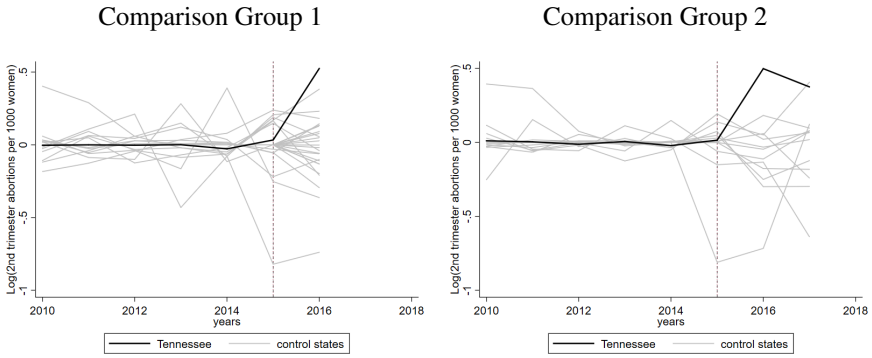
Note: Tennessee's mandatory waiting period law went into effect in May 2015. The synthetic controls were constructed following Ferman and Pinto (2017), matching on demeaned outcomes prior to 2015. See Figure B.3 for the states included in Comparison Group 1 and Comparison Group 2. These two groups are compared with "Refined Tennessee", which excludes information on the Hamilton, Northeast, Southeast, and Sullivan health areas. See section 3.4 for more information on these health areas. The percent of abortions in the second trimester is calculated as the percent of all abortions that were obtained after 12 weeks of gestation. The second trimester abortion rate is the number abortions obtained after 12 weeks of gestation per 1,000 women (ages 15-44). The overall abortion rate is the number abortions per 1,000 women (ages 15-44). Sources: The number of abortions by gestational age were collected from states' health departments by the authors and CDC Abortion Surveillance Reports. Annual state-level population estimates were obtained from the United States Census Bureau (2017).

Figure B.16: Permutation tests associated with synthetic control estimates

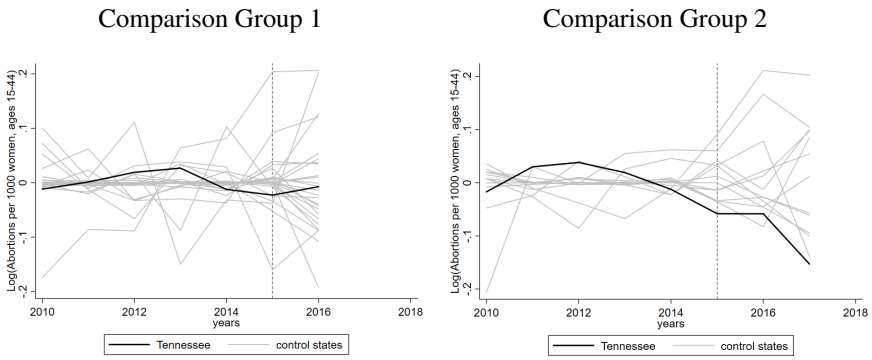
Panel A: Percent of abortions in the second trimester



Panel B: Log of the second-trimester abortions per 1,000 women (ages 15-44)



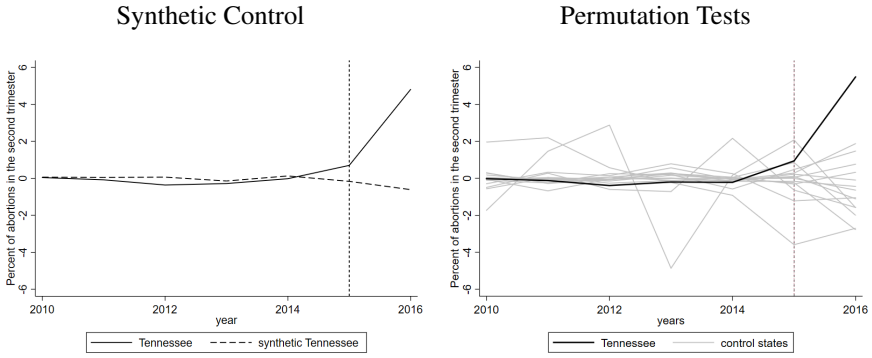
Panel C: Log of the number of abortions per 1000 women (ages 15-44)



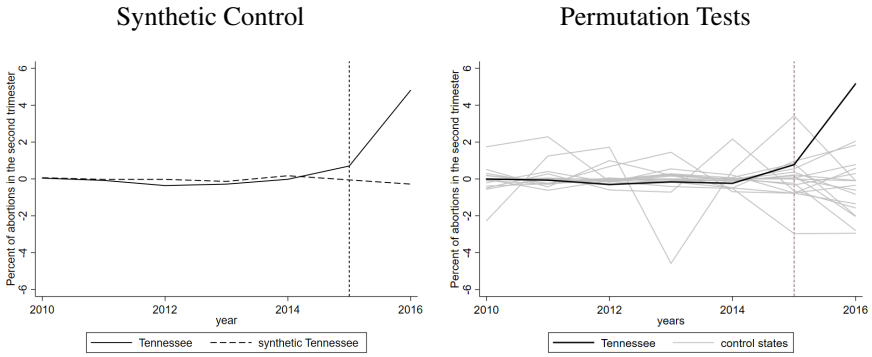
Note: The figure depicts estimates evaluating "Refined Tennessee" using the synthetic control design, as in Figure B.15, along with the set of placebo estimates (in gray) that can be obtained by applying the same methodology to each of the states in the comparison groups. See Figure B.15 for additional information.

Figure B.17: Synthetic control estimates and permutation tests for the percent of abortions in the second trimester

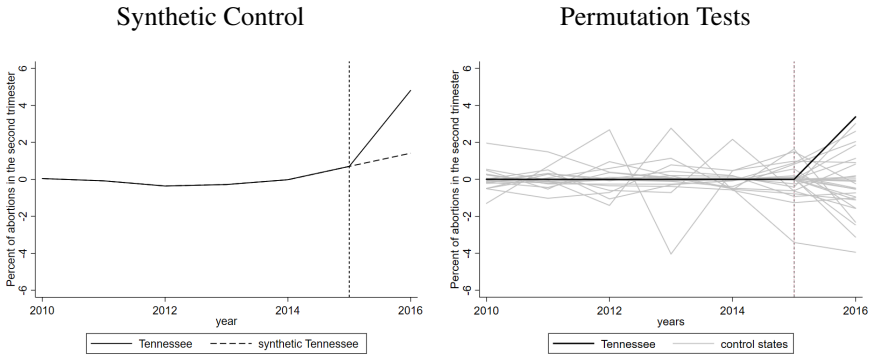
Panel A: 5% out-of-state abortions



Panel B: 10% out-of-state abortions



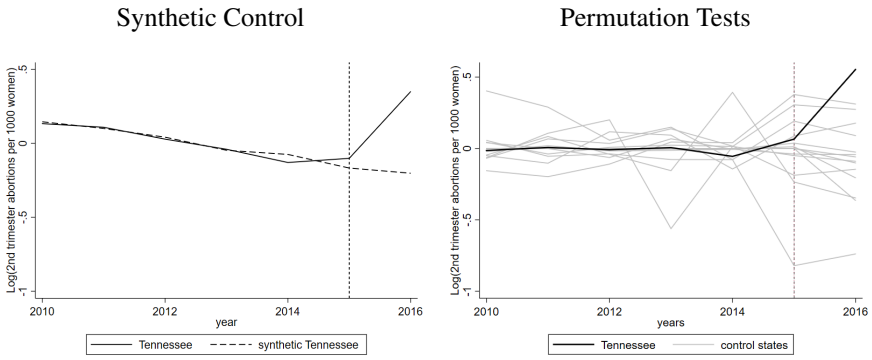
Panel C: 40% out-of-state abortions



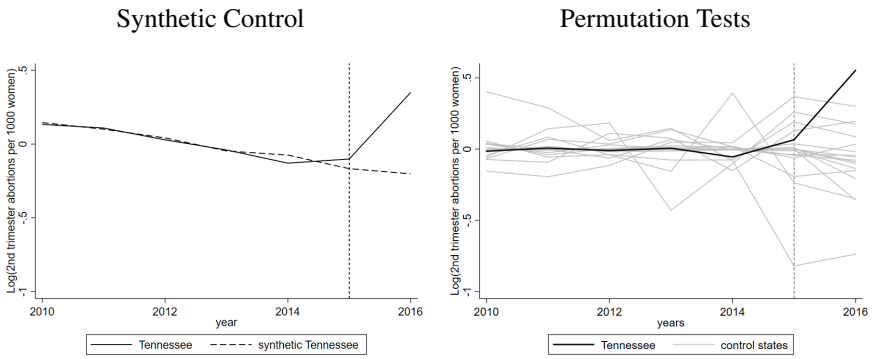
Note: Panel A, B and C compare "Refined Tennessee", which excludes the information from the Hamilton, Northeast, Southeast, and Sullivan health regions, with the states listed in columns (1), (2), and (4) of Table B.7, respectively. The percent of abortions in the second trimester is calculated as the percent of all abortions that were obtained after 12 weeks of gestation. See Figures B.15 and B.16 for additional information.

Figure B.18: Synthetic control estimates and permutation tests for the log of second-trimester abortion rates

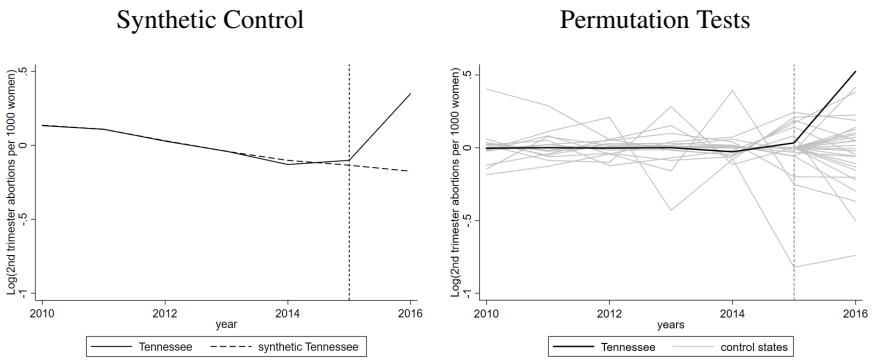
Panel A: 5% out-of-state abortions



Panel B: 10% out-of-state abortions



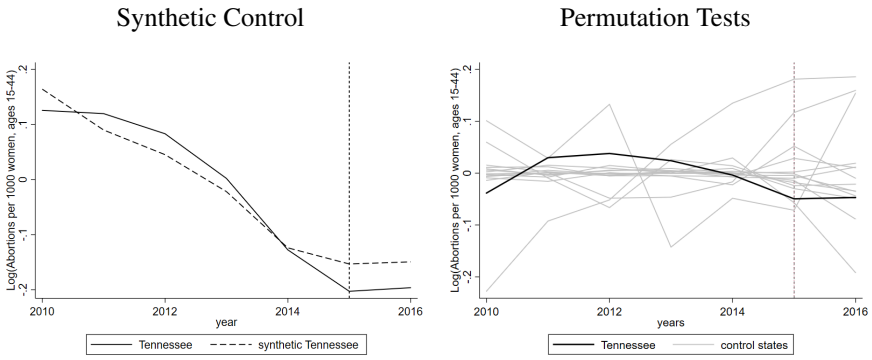
Panel C: 40% out-of-state abortions



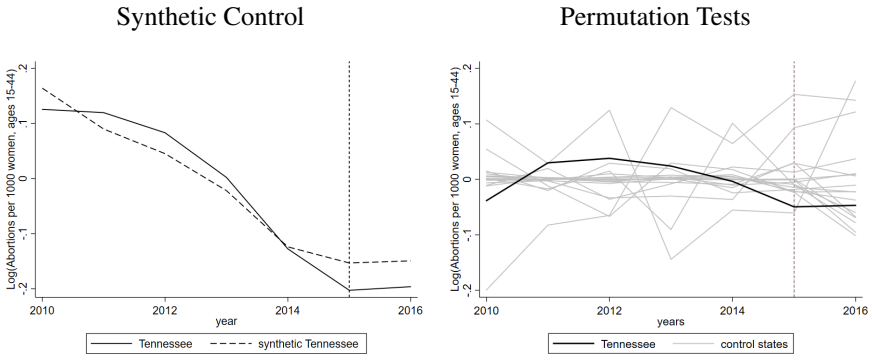
Note: Panel A, B and C compare "Refined Tennessee", which excludes the information from the Hamilton, Northeast, Southeast, and Sullivan health regions, with the states listed in columns (1), (2), and (4) of Table B.7, respectively. The second trimester abortion rate is the number abortions obtained after 12 weeks of gestation per 1,000 women (ages 15-44). See Figures B.15 and B.16 for additional information.

Figure B.19: Synthetic control estimates and permutation tests for the log of abortion rates

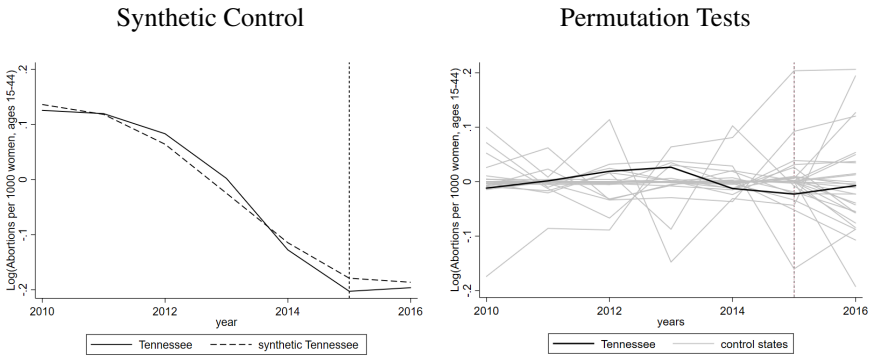
Panel A: 5% out-of-state abortions



Panel B: 10% out-of-state abortions



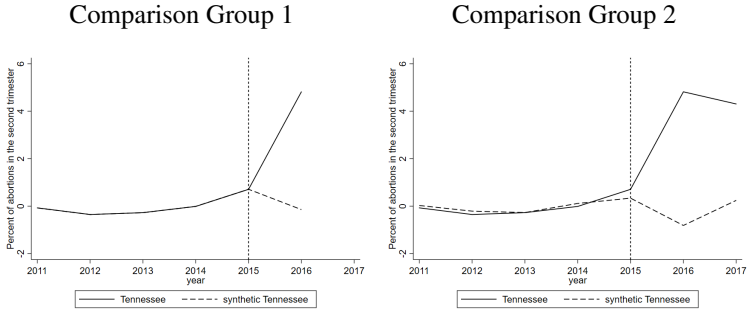
Panel C: 40% out-of-state abortions



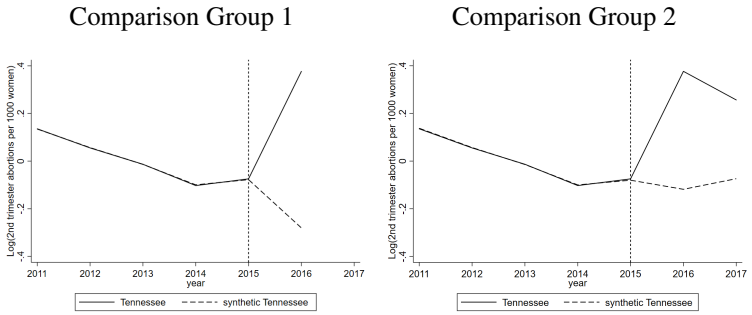
Note: Panel A, B and C compare "Refined Tennessee", which excludes the information from the Hamilton, Northeast, Southeast, and Sullivan health regions, with the states listed in columns (1), (2), and (4) of Table B.7, respectively. The overall abortion rate is the number abortions per 1,000 women (ages 15-44). See Figures B.15 and B.16 for additional information.

Figure B.20: Demeaned outcomes for Tennessee and synthetic controls, including Delaware data

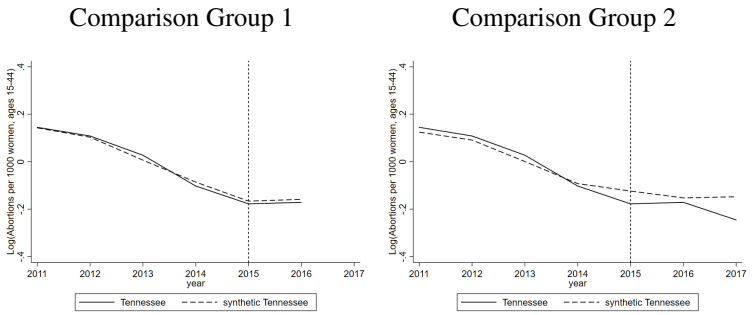
Panel A: Percent of abortions in the second trimester



Panel B: Log of the second-trimester abortions per 1,000 women (ages 15-44)



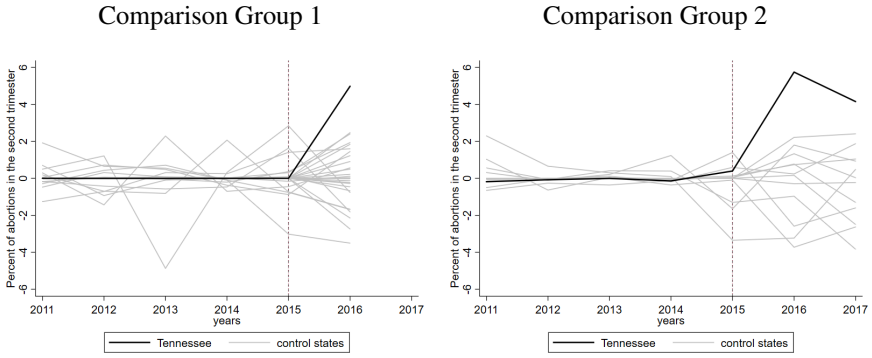
Panel C: Log of the number of abortions per 1000 women (ages 15-44)



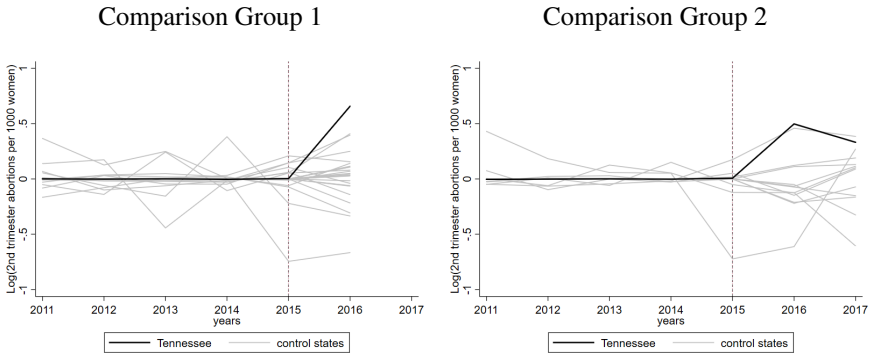
Note: Tennessee's mandatory waiting period law went into effect in May 2015. The synthetic controls were constructed following Ferman and Pinto (2017), matching on demeaned outcomes prior to 2015. These figures compare "Refined Tennessee", which excludes the information from the health regions Hamilton, Northeast, Southeast, and Sullivan, with each comparison group. See section 3.4 for more information on these health areas. Additionally to the states shown for each comparison group in Figure B.3, each one includes Delaware. The percent of abortions in the second trimester is calculated as the percent of all abortions that were obtained after 12 weeks of gestation. The second trimester abortion rate is the number abortions obtained after 12 weeks of gestation per 1,000 women (ages 15-44). The overall abortion rate is the number abortions per 1,000 women (ages 15-44). Sources: The number of abortions by gestational age were collected from states' health departments by the authors and CDC Abortion Surveillance Reports. Annual state-level population estimates were obtained from the United States Census Bureau (2017).

Figure B.21: Permutation tests associated with synthetic control estimates, including Delaware data

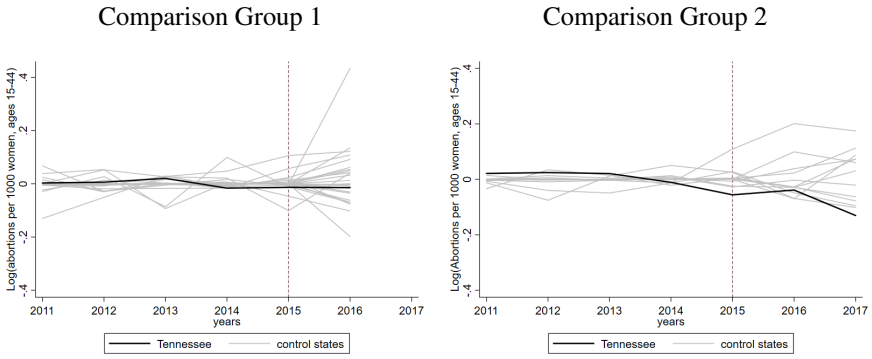
Panel A: Percent of abortions in the second trimester



Panel B: Log of the second-trimester abortions per 1,000 women (ages 15-44)



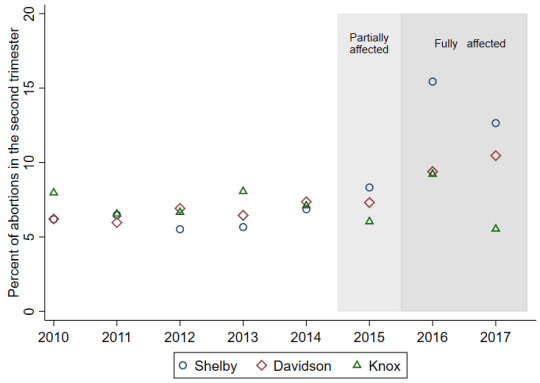
Panel C: Log of the number of abortions per 1000 women (ages 15-44)



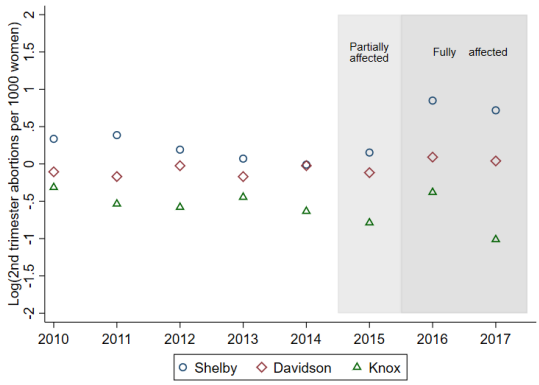
Note: The figure depicts estimates evaluating "Refined Tennessee", which excludes the information from the Hamilton, Northeast, Southeast, and Sullivan health regions, using the synthetic control design, as in Figure B.20, along with the set of placebo estimates (in gray) that can be obtained by applying the same methodology to each of the states in the comparison groups. See Figure B.20 for additional information.

Figure B.22: Outcomes for single-county health regions

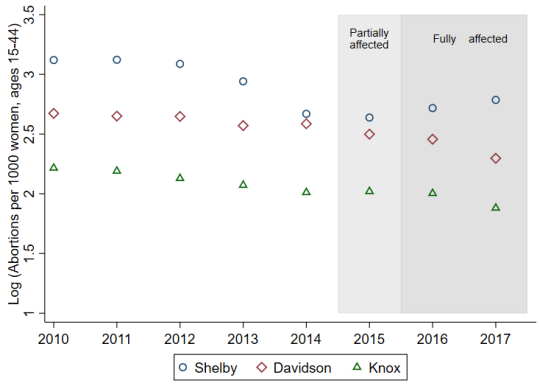
Panel A: Percent of abortions in the second trimester



Panel B: Log of the second-trimester abortions per 1,000 women (ages 15-44)



Panel C: Log of the number of abortions per 1000 women (ages 15-44)



Notes: The percent of abortions in the second trimester is calculated as the percent of all abortions that were obtained after 12 weeks of gestation. The second trimester abortion rate is the number abortions obtained after 12 weeks of gestation per 1,000 women (ages 15-44). The overall abortion rate is the number abortions per 1,000 women (ages 15-44). Sources: The number of abortions by gestational age were collected from abortion reports from the Tennessee Department of Health. Annual state-level population estimates were obtained from the United States Census Bureau (2017).

Figure B.23: Abortion prices as of October 2015

Abortion Fees

Pregnancy is measured from the first day of the last normal menstrual period (not from conception) and is verified by ultrasound.

As of October 1st the fees will be:

Medical Abortion	up to 10 weeks	$525
Surgical Abortion	6-11 weeks, 6 days	$575
	12-12 weeks, 6 days	$750
	13-13 weeks, 6 days	$850
	14-14 weeks, 6 days	$1000
	**15-16 weeks, 3 days	$1,100
Twin Pregnancy	13-15 weeks	$1,050

** Limited scheduling based on physician availability.

Abortion Fees Include:
The abortion procedure, lab work, ultrasound, counseling, medications given at the Center, take home medications, and the follow-up exam three to four weeks later.

Due to the 48 hour waiting period and imposed two clinic visits, the fee is split into two payments. The first visit is $180; the second visit is the remaining fee based on the gestational age at the time of the second visit. Full payment at the first visit is an option, if preferred.

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Note: This information was obtained using Wayback Machine. Source: Knoxville Center for Reproductive Health.

B.2 Tables

Table B.1: States reporting abortions by gestational age

	Residents	All residents	Occurrences	Occurrences and all residents
Alabama		Yes	Yes	
Alaska			Yes	
Arizona	Yes			
Arkansas			Yes	
Colorado			Yes	
Delaware	Yes*		Yes*	
Hawaii			Yes	
Idaho			Yes	
Illinois		Yes		
Indiana			Yes	
Iowa			Yes	
Kansas				Yes
Kentucky			Yes	
Louisiana			Yes	
Maine			Yes	
Michigan			Yes	
Minnesota	Yes		Yes	
Mississippi			Yes	
Missouri	Yes			Yes
Montana			Yes	
Nebraska			Yes	
Nevada			Yes	
New Mexico	Yes			
New York	Yes			
North Carolina		Yes		
North Dakota			Yes	
Ohio			Yes	
Oklahoma	Yes		Yes	
Oregon			Yes	
Pennsylvania	Yes			
South Dakota			Yes	
Tennessee		Yes		
Texas	Yes			
Utah	Yes			
Vermont			Yes	
Washington		Yes	Yes	
West Virginia			Yes	
Wisconsin	Yes		Yes	

Notes: Based on the information released in abortion and ITOP reports, we identified that these states collected data on abortions by gestational age from 2010-2017. "Residents" identifies those states that collected information on abortions by gestational age for residents that got an abortion in the state. "All residents" refers to states that collected information on abortions by gestational age for residents that got an abortion in the state and out-of-state. "Occurences" refers to states that collected information on all the abortions that occurred in the state from residents and non-residents. "Occurrences and all residents" identifies states that collected information on all the abortions that occurred in the state and the abortions of residents out-of-state. *Delaware's data is available for 2011-2017.

Table B.2: Summary statistics

	Tennessee		Comparison Group 1		Comparison Group 2	
	2010-2014	2015-2017	2010-2014	2015-2016	2010-2014	2015-2017
% of second-trimester abortions	6.50	9.91	11.08	10.83	11.55	10.75
Second-trimester abortion rate	0.66	0.77	1.16	0.99	1.24	1.02
Abortion rate	10.12	7.74	9.93	8.77	10.59	9.33
% women 15-19 years old	16.05	15.70	16.74	16.38	16.83	16.49
% women 20-24 years old	17.39	17.14	17.67	17.41	17.68	17.41
% women 25-29 years old	16.84	18.05	16.87	17.42	17.05	17.47
% women 30-34 years old	16.65	16.81	16.69	17.05	16.73	17.07
% women 35-39 years old	16.04	16.37	15.63	16.33	15.53	16.31
% women 40-44 years old	17.04	15.93	16.40	15.42	16.18	15.25
% Black women	21.94	21.95	10.19	10.44	10.34	10.54
% Hispanic women	5.75	6.19	12.38	13.59	16.74	17.66
% non-Hispanic White women	68.72	67.73	67.70	65.71	64.10	62.15
Unemployment rate	8.16	4.67	7.57	4.80	7.41	4.83

Notes: This table reports variable means in the years indicated. Tennessee's mandatory waiting period law went into effect in May 2015. See Figure B.3 for the states included in Comparison Group 1 and Comparison Group 2. % second-trimester abortions represents the percent of abortions that were obtained after 12 weeks of gestation. The second-trimester abortion rate is the number of abortions in the second trimester per 1,000 women (ages 15-44). The abortion rate is constructed similarly.

Table B.3: Difference-in-differences estimates of effects of Tennessee's mandatory waiting period

	% of abortions in 2nd trimester			log 2nd-trimester abortion rate			log overall abortion rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Comparison Group 1									
Estimated Effect	4.753	4.572	4.618	0.408	0.360	0.331	-0.120	-0.155	-0.195
two-sided *p*-value (CSEs)	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.00]	[0.00]	[0.00]
two-sided *p*-value (RI)	[0.07]	[0.04]	[0.04]	[0.15]	[0.19]	[0.19]	[0.41]	[0.22]	[0.11]
one-sided *p*-value (RI)	[0.04]	[0.04]	[0.04]	[0.07]	[0.15]	[0.15]	[0.30]	[0.07]	[0.07]
Observations	189	189	189	189	189	189	189	189	189
Panel B. Comparison Group 2									
Estimated Effect	4.872	3.222	3.551	0.428	0.201	0.231	-0.120	-0.127	-0.136
two-sided *p*-value (CSEs)	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.01]	[0.00]	[0.00]	[0.00]
two-sided *p*-value (RI)	[0.08]	[0.08]	[0.08]	[0.15]	[0.38]	[0.38]	[0.15]	[0.23]	[0.23]
one-sided *p*-value (RI)	[0.08]	[0.08]	[0.08]	[0.08]	[0.23]	[0.23]	[0.08]	[0.08]	[0.08]
Observations	104	104	104	104	104	104	104	104	104
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Unemployment rate	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Comparison with TN	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Comparison with "Refined TN"	No	No	Yes	No	No	Yes	No	No	Yes

Notes: See Figure B.3 for the states included in Comparison Group 1 and Comparison Group 2. The analyses using Comparison Group 1 use data from 2010–2016 whereas the analyses using Comparison Group 2 use data from 2010–2017. The first two columns of each outcome's analyses compare the information on all the health areas in Tennessee with each one of the comparison groups. The third column compares "Refined Tennessee", which excludes the information from the Hamilton, Northeast, Southeast, and Sullivan health areas, with each comparison group. See section 3.4 for more information on these health areas. The percent of abortions in the second trimester is calculated as the percent of all abortions that were obtained after 12 weeks of gestation. The second trimester abortion rate is the number abortions obtained after 12 weeks of gestation per 1,000 women (ages 15-44). The overall abortion rate is the number abortions per 1,000 women (ages 15-44). The treatment effect is identified based on the coefficient on the variable measuring the share of the year in which the policy was in effect for Tennessee—this variable takes the value of 7/12 for Tennessee in 2015 and one for Tennessee in 2016 and 2017. The demographic controls are the shares of women in five-year age groups (15-19, 20-24,...,40-44) and the share that are Hispanic, black, or non-Hispanic white (among women ages 15-44). The *p*-values displayed include those based on clustered standard errors at the state-level (CSEs) and those based on randomization inference. See section 3.5 for more information on the reported *p*-values. Sources: Number of abortions by gestational age were collected from states health departments by the authors and CDC Abortion Surveillance Reports.

Table B.4: Difference-in-differences estimates of effects of Tennessee's mandatory waiting period on raw abortion rates

	2nd-trimester abortion rate			overall abortion rate		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Comparison Group 1						
Estimated Effect	0.397	0.349	0.321	-0.658	-0.818	-1.562
two-sided *p*-value (CSEs)	[0.00]	[0.01]	[0.02]	[0.21]	[0.15]	[0.01]
two-sided *p*-value (RI)	[0.15]	[0.26]	[0.26]	[0.52]	[0.70]	[0.33]
one-sided *p*-value (RI)	[0.11]	[0.19]	[0.19]	[0.26]	[0.26]	[0.11]
Observations	189	189	189	189	189	189
Panel B. Comparison Group 2						
Estimated Effect	0.470	0.380	0.436	-0.188	0.206	-0.094
two-sided *p*-value (CSEs)	[0.00]	[0.00]	[0.00]	[0.79]	[0.69]	[0.86]
two-sided *p*-value (RI)	[0.23]	[0.31]	[0.15]	[0.92]	[0.85]	[0.92]
one-sided *p*-value (RI)	[0.15]	[0.15]	[0.15]	[0.23]	[0.54]	[0.38]
Observations	104	104	104	104	104	104
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	No	Yes	Yes
Unemployment rate	No	No	Yes	No	No	Yes
Comparison with TN	Yes	Yes	No	Yes	Yes	No
Comparison with "Refined TN"	No	No	Yes	No	No	Yes

Notes: See Figure B.3 for the states included in Comparison Group 1 and Comparison Group 2. The analyses using Comparison Group 1 use data from 2010–2016 whereas the analyses using Comparison Group 2 use data from 2010–2017. The first two columns of each outcome's analyses compare the information on all the health areas in Tennessee with each one of the comparison groups. The third column compares "Refined Tennessee", which excludes the information from the Hamilton, Northeast, Southeast, and Sullivan health areas, with each one of the comparison groups. See section 3.4 for more information on these health areas. The percent of abortions in the second trimester is calculated as the percent of all abortions that were obtained after 12 weeks of gestation. The second trimester abortion rate is the number abortions obtained after 12 weeks of gestation per 1,000 women (ages 15-44). The overall abortion rate is the number abortions per 1,000 women (ages 15-44). The treatment effect is identified based on the coefficient on the variable measuring the share of the year in which the policy was in effect for Tennessee—this variable takes the value of 7/12 for Tennessee in 2015 and one for Tennessee in 2016 and 2017. The demographic controls are the shares of women in five-year age groups (15-19, 20-24,...,40-44) and the share that are Hispanic, black, or non-Hispanic white (among women ages 15-44). The *p*-values displayed include those based on clustered standard errors at the state-level (CSEs) and those based on randomization inference. See section 3.5 for more information on the reported *p*-values. Sources: Number of abortions by gestational age were collected from states health departments by the authors and CDC Abortion Surveillance Reports.

Table B.5: Difference-in-differences estimates of effects of Tennessee's mandatory waiting period *by year*

	% of abortions in 2nd trimester			log 2nd-trimester abortion rate			log overall abortion rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Comparison Group 1									
1(TN in 2015)	1.207	1.394	1.368	0.003	0.012	0.018	-0.156	-0.174	-0.171
two-sided *p*-value (CSEs)	[0.00]	[0.00]	[0.00]	[0.95]	[0.79]	[0.71]	[0.00]	[0.00]	[0.00]
two-sided *p*-value (RI)	[0.59]	[0.44]	[0.48]	[1.00]	[0.96]	[0.96]	[0.15]	[0.07]	[0.07]
one-sided *p*-value (RI)	[0.41]	[0.33]	[0.37]	[0.52]	[0.59]	[0.59]	[0.11]	[0.07]	[0.07]
1(TN in 2016)	**5.476**	**5.143**	**5.215**	**0.516**	**0.448**	**0.410**	**-0.081**	**-0.117**	**-0.169**
two-sided *p*-value (CSEs)	**[0.00]**	**[0.00]**	**[0.00]**	**[0.00]**	**[0.00]**	**[0.00]**	**[0.01]**	**[0.00]**	**[0.00]**
two-sided *p*-value (RI)	**[0.04]**	**[0.04]**	**[0.04]**	**[0.07]**	**[0.15]**	**[0.15]**	**[0.63]**	**[0.33]**	**[0.19]**
one-sided *p*-value (RI)	**[0.04]**	**[0.04]**	**[0.04]**	**[0.04]**	**[0.11]**	**[0.11]**	**[0.41]**	**[0.15]**	**[0.07]**
Observations	189	189	189	189	189	189	189	189	189
Panel B. Comparison Group 2									
1(TN in 2015)	1.520	1.140	1.251	0.047	-0.003	0.026	-0.146	-0.132	-0.120
two-sided *p*-value (CSEs)	[0.00]	[0.01]	[0.00]	[0.41]	[0.95]	[0.56]	[0.00]	[0.00]	[0.00]
two-sided *p*-value (RI)	[0.38]	[0.46]	[0.46]	[0.62]	[1.00]	[1.00]	[0.08]	[0.23]	[0.31]
one-sided *p*-value (RI)	[0.23]	[0.23]	[0.23]	[0.38]	[0.38]	[0.69]	[0.08]	[0.15]	[0.15]
1(TN in 2016)	**5.478**	**4.070**	**4.291**	**0.560**	**0.355**	**0.346**	**-0.057**	**-0.066**	**-0.104**
two-sided *p*-value (CSEs)	**[0.00]**	**[0.00]**	**[0.00]**	**[0.00]**	**[0.00]**	**[0.00]**	**[0.10]**	**[0.03]**	**[0.00]**
two-sided *p*-value (RI)	**[0.08]**	**[0.08]**	**[0.08]**	**[0.15]**	**[0.15]**	**[0.15]**	**[0.54]**	**[0.54]**	**[0.38]**
one-sided *p*-value (RI)	**[0.08]**	**[0.08]**	**[0.08]**	**[0.08]**	**[0.08]**	**[0.08]**	**[0.38]**	**[0.31]**	**[0.23]**
1(TN in 2017)	4.774	2.591	3.076	0.374	0.079	0.149	-0.153	-0.172	-0.155
two-sided *p*-value (CSEs)	[0.00]	[0.00]	[0.00]	[0.00]	[0.45]	[0.15]	[0.00]	[0.00]	[0.00]
two-sided *p*-value (RI)	[0.08]	[0.23]	[0.15]	[0.15]	[0.54]	[0.54]	[0.15]	[0.23]	[0.23]
one-sided *p*-value (RI)	[0.08]	[0.08]	[0.08]	[0.08]	[0.31]	[0.31]	[0.08]	[0.08]	[0.08]
Observations	104	104	104	104	104	104	104	104	104
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Unemp. rate	No	No	Yes	No	No	Yes	No	No	Yes
Comparison with TN	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Comparison with "Refined TN"	No	No	Yes	No	No	Yes	No	No	Yes

Notes: See Figure B.3 for the states included in Comparison Group 1 and Comparison Group 2 and Table B.3 for additional details regarding the analyses.

Table B.6: States in "Comparison Group 1" with alternative thresholds based on out-of-state abortions

(1)	(2)	(3)	(4)
Selected states from CDC data based on out-of-state abortions			
less than 5%	less than 10%	less than 20%	less than 40%
Alaska	Alaska	Alaska	Alaska
Hawaii	Hawaii	Colorado	Colorado
Idaho	Idaho	Georgia	Georgia
Michigan	Michigan	Hawaii	Hawaii
South Carolina	Nevada	Idaho	Idaho
	New Jersey	Iowa	Iowa
	Ohio	Kentucky	Kentucky
	South Carolina	Michigan	Michigan
		Montana	Montana
		Nevada	Nevada
		New Jersey	New Jersey
		Ohio	North Dakota
		Oregon	Ohio
		Rhode Island	Oregon
		South Carolina	Rhode Island
		South Dakota	South Carolina
		West Virginia	South Dakota
			West Virginia
Plus selected states from comparison group 2			
Minnesota			
Missouri			
New Mexico			
New York			
North Carolina			
Oklahoma			
Utah			
Washington			
Wisconsin			

Note: The states in each column represent the states that would be included in Comparison Group 1 using different thresholds based on their average out-of-state abortions. States in the bottom panel of the table would be included regardless, because they independently report outcomes for their residents. The states in column (3) plus those in the bottom panel comprise "Comparison Group 1" used for our main results and are included in this table for comparison purposes. See section 3.4 for more information.

Table B.7: Difference-in-differences estimates of effects of Tennessee's mandatory waiting period, using alternative thresholds for states' inclusion in the analysis

	% of abortions in 2nd trimester			log 2nd-trimester abortion rate			log overall abortion rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Including states with less than 5% out-of-state abortions									
Estimated Effect	4.638	3.994	4.253	0.423	0.258	0.265	-0.123	-0.200	-0.233
two-sided *p*-value (CSEs)	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.00]	[0.00]
two-sided *p*-value (RI)	[0.13]	[0.07]	[0.07]	[0.27]	[0.47]	[0.47]	[0.20]	[0.33]	[0.07]
one-sided *p*-value (RI)	[0.07]	[0.07]	[0.07]	[0.13]	[0.33]	[0.33]	[0.07]	[0.13]	[0.07]
Observations	105	105	105	105	105	105	105	105	105
Panel B. Including states with less than 10% out-of-state abortions									
Estimated Effect	4.668	3.959	4.098	0.412	0.338	0.315	-0.124	-0.148	-0.190
two-sided *p*-value (CSEs)	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
two-sided *p*-value (RI)	[0.11]	[0.06]	[0.06]	[0.22]	[0.33]	[0.39]	[0.17]	[0.28]	[0.22]
one-sided *p*-value (RI)	[0.06]	[0.06]	[0.06]	[0.11]	[0.22]	[0.28]	[0.06]	[0.11]	[0.06]
Observations	126	126	126	126	126	126	126	126	126
Panel C. Including states with less than 20% out-of-state abortions									
Estimated Effect	4.753	4.572	4.618	0.408	0.360	0.331	-0.120	-0.155	-0.195
two-sided *p*-value (CSEs)	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]	[0.00]	[0.00]	[0.00]
two-sided *p*-value (RI)	[0.07]	[0.04]	[0.04]	[0.15]	[0.19]	[0.19]	[0.41]	[0.22]	[0.11]
one-sided *p*-value (RI)	[0.04]	[0.04]	[0.04]	[0.07]	[0.15]	[0.15]	[0.30]	[0.07]	[0.07]
Observations	189	189	189	189	189	189	189	189	189
Panel D. Including states with less than 40% out-of-state abortions									
Estimated Effect	4.742	4.578	4.628	0.406	0.362	0.334	-0.120	-0.154	-0.194
two-sided *p*-value (CSEs)	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
two-sided *p*-value (RI)	[0.07]	[0.07]	[0.07]	[0.14]	[0.21]	[0.25]	[0.39]	[0.25]	[0.11]
one-sided *p*-value (RI)	[0.04]	[0.07]	[0.07]	[0.07]	[0.18]	[0.18]	[0.29]	[0.07]	[0.07]
Observations	196	196	196	196	196	196	196	196	196
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Unemployment rate	No	No	Yes	No	No	Yes	No	No	Yes
Comparison with TN	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Comparison with "Refined TN"	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The analyses use data from 2010–2016. Panels A, B and D compare Tennessee with the states listed in columns (1), (2) and (4) of Table B.7, respectively. Panel C compares Tennessee with the states listed in column (3) of Table B.7 and contains the estimated effects for Comparison Group 1 shown in Panel A of Table B.3. This panel is included for comparison purposes. The first two columns of each outcome's analyses compare the information on all the health areas in Tennessee with each one of the comparison groups. The third column compares "Refined Tennessee", which excludes information on the Hamilton, Northeast, Southeast, and Sullivan health areas, with each comparison group. See section 3.4 for more information on these health areas. The percent of abortions in the second trimester is calculated as the percent of all abortions that were obtained after 12 weeks of gestation. The second trimester abortion rate is the number abortions obtained after 12 weeks of gestation per 1,000 women (ages 15-44). The overall abortion rate is the number abortions per 1,000 women (ages 15-44). The treatment effect is identified based on the coefficient on the variable measuring the share of the year in which the policy was in effect for Tennessee—this variable takes the value of 7/12 for Tennessee in 2015 and one for Tennessee in 2016 and 2017. The demographic controls are the shares of women in five-year age groups (15-19, 20-24,...,40-44) and the share that are Hispanic, Black, or non-Hispanic White (among women ages 15-44). The *p*-values displayed include those based on clustered standard errors at the state-level (CSEs) and those based on randomization inference. See section 3.5 for more information on the reported *p*-values. Sources: Number of abortions by gestational age were collected from states health departments by the authors and CDC Abortion Surveillance Reports.

Table B.8: Difference-in-differences estimates of effects of Tennessee's mandatory waiting period, including Delaware data 2011-2017

	% of abortions in 2nd trimester			log 2nd-trimester abortion rate			log overall abortion rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Comparison Group 1									
Estimated Effect	4.924	4.300	4.405	0.427	0.356	0.327	-0.120	-0.153	-0.194
two-sided *p*-value (CSEs)	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
two-sided *p*-value (RI)	[0.04]	[0.04]	[0.04]	[0.11]	[0.21]	[0.25]	[0.36]	[0.18]	[0.14]
one-sided *p*-value (RI)	[0.04]	[0.04]	[0.04]	[0.07]	[0.14]	[0.18]	[0.25]	[0.07]	[0.07]
Observations	168	168	168	168	168	168	168	168	168
Panel B. Comparison Group 2									
Estimated Effect	4.871	2.951	3.233	0.439	0.182	0.205	-0.114	-0.122	-0.135
two-sided *p*-value (CSEs)	[0.00]	[0.00]	[0.00]	[0.00]	[0.04]	[0.02]	[0.01]	[0.01]	[0.00]
two-sided *p*-value (RI)	[0.07]	[0.07]	[0.07]	[0.21]	[0.50]	[0.50]	[0.21]	[0.43]	[0.43]
one-sided *p*-value (RI)	[0.07]	[0.07]	[0.07]	[0.14]	[0.29]	[0.29]	[0.14]	[0.21]	[0.21]
Observations	98	98	98	98	98	98	98	98	98
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Unemployment rate	No	No	Yes	No	No	Yes	No	No	Yes
Comparison with TN	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Comparison with "Refined TN"	No	No	Yes	No	No	Yes	No	No	Yes

Notes: Additionally to the states shown for each comparison group in Figure B.3, each one also includes Delaware. The analyses using Comparison Group 1 uses data from 2011–2016 whereas the analyses using Comparison Group 2 uses data from 2011–2017. The first two columns of each outcome's analyses compare the information on all the health areas in Tennessee with each one of the comparison groups. The third column compares "Refined Tennessee", which excludes information on the Hamilton, Northeast, Southeast, and Sullivan health areas, with each comparison group. See section 3.4 for more information on these health areas. The percent of abortions in the second trimester is calculated as the percent of all abortions that were obtained after 12 weeks of gestation. The second trimester abortion rate is the number abortions obtained after 12 weeks of gestation per 1,000 women (ages 15-44). The overall abortion rate is the number abortions per 1,000 women (ages 15-44). The treatment effect is identified based on the coefficient on the variable measuring the share of the year in which the policy was in effect for Tennessee—this variable takes the value of 7/12 for Tennessee in 2015 and one for Tennessee in 2016 and 2017. The demographic controls are the shares of women in five-year age groups (15-19, 20-24,...,40-44) and the share that are Hispanic, Black, or non-Hispanic White (among women ages 15-44). The *p*-values displayed include those based on clustered standard errors at the state-level (CSEs) and those based on randomization inference. See section 3.5 for more information on the reported *p*-values. Sources: Number of abortions by gestational age were collected from states health departments by the authors and CDC Abortion Surveillance Reports.

Table B.9: State Weights for Synthetic Control Estimates

Comparison Group 1

Outcome	% second-trimester abortions	ln(second-trimester abortion rate)	ln(abortion rate)
Alaska	0.004	0	0
Colorado	0.007	0	0
Georgia	0.011	0	0
Hawaii	0.009	0	0
Idaho	0.016	0.069	0
Iowa	0.007	0	0
Kentucky	0.660	0	0.045
Michigan	0.007	0	0
Minnesota	0.008	0	0
Missouri	0.009	0	0
Montana	0.006	0	0.842
Nevada	0.015	0	0
New Jersey	0.006	0	0
New Mexico	0.005	0	0
New York	0.015	0	0
North Carolina	0.030	0.280	0
Ohio	0.008	0	0
Oklahoma	0.041	0.128	0
Oregon	0.008	0	0
Rhode Island	0.007	0	0
South Carolina	0.007	0	0
South Dakota	0.082	0.287	0
Utah	0.005	0	0
Washington	0.008	0	0
West Virginia	0.012	0.236	0.113
Wisconsin	0.006	0	0

Comparison Group 2

Outcome	% second-trimester abortions	ln(second-trimester abortion rate)	ln(abortion rate)
Arizona	0.170	0.123	0.116
Illinois	0.017	0.444	0
Minnesota	0	0	0
Missouri	0	0	0
New Mexico	0	0	0
New York	0	0.262	0
North Carolina	0.406	0.153	0
Oklahoma	0	0.018	0
Pennsylvania	0.296	0	0
Utah	0.112	0	0
Washington	0	0	0
Wisconsin	0	0	0.884

Notes: The synthetic control is constructed independently for the analyses of each outcome, and separately using states in Comparison Group 1 and Comparison Group 2 as the donor pool. See Figure B.15 for more information.

Table B.10: Descriptive statistics for Tennessee health areas

Health Area	Population (thousands)	Miles to nearest in-state clinic	Miles to nearest clinic	% non-Hispanic White	% Black	% Hispanic	Median HH income ($1,000s)	Unemp. rate	Poverty rate
Shelby	936	4.8	4.8	31.9	58.4	5.9	45.7	7.6	21.0
Davidson	663	4.4	4.4	54.8	29.7	9.7	49.3	5.4	17.8
Knox	446	4.3	4.3	81.2	10.1	4.3	49.0	5.5	15.3
Mid-Cumberland	187	31.7	31.7	77.5	11.4	6.6	61.9	5.9	11.0
Madison	98	78.8	78.8	50.5	43.3	3.9	42.8	7.2	19.3
East	67	32.8	32.8	90.6	2.2	4.7	41.3	7.8	18.2
South Central	48	52.8	62.5	85.5	6.9	5.3	42.9	7.4	17.0
Upper Cumberland	41	77.3	77.3	91.5	1.5	4.8	37.2	7.7	20.3
Southwest	35	65.4	65.4	73.1	22.3	2.7	42.8	8.8	19.2
Northwest	34	107.2	107.2	81.1	13.4	3.5	37.8	9.1	19.9

Notes: Health areas are depicted in Figure B.6. For each variable, the figures presented correspond to the population-weighted averages across the counties in each health area. The variables % non-Hispanic White, % Black, and % Hispanic are based on the population of women 15–44 years old. Distance to the nearest clinic is the population-weighted average of the distance from each county's population centroid. Distances were calculated using the "georoute" Stata command. Annual county-level population estimates were obtained from the United States Census Bureau (2017). Annual county-level unemployment rates were obtained from the Bureau of Labor Statistics (2017). The information on the location of abortion clinics was provided by Caitilin Myers. Annual county-level median household income and poverty rates were calculated using information from the Small Area Income and Poverty Estimates (SAIPE) of the United States Census Bureau (2017).

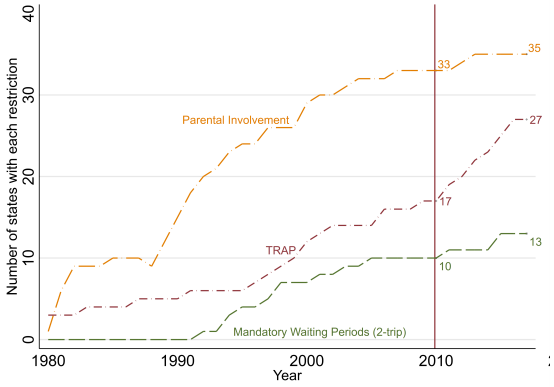
APPENDIX C

FIGURES AND TABLES FOR CHAPTER 4

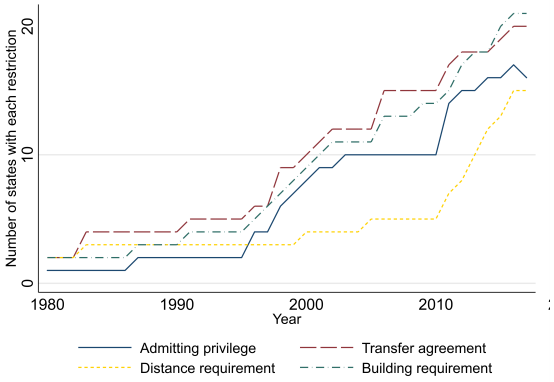
C.1 Figures

Figure C.1: Recent increases in abortion restrictions over time

(a) By restriction type

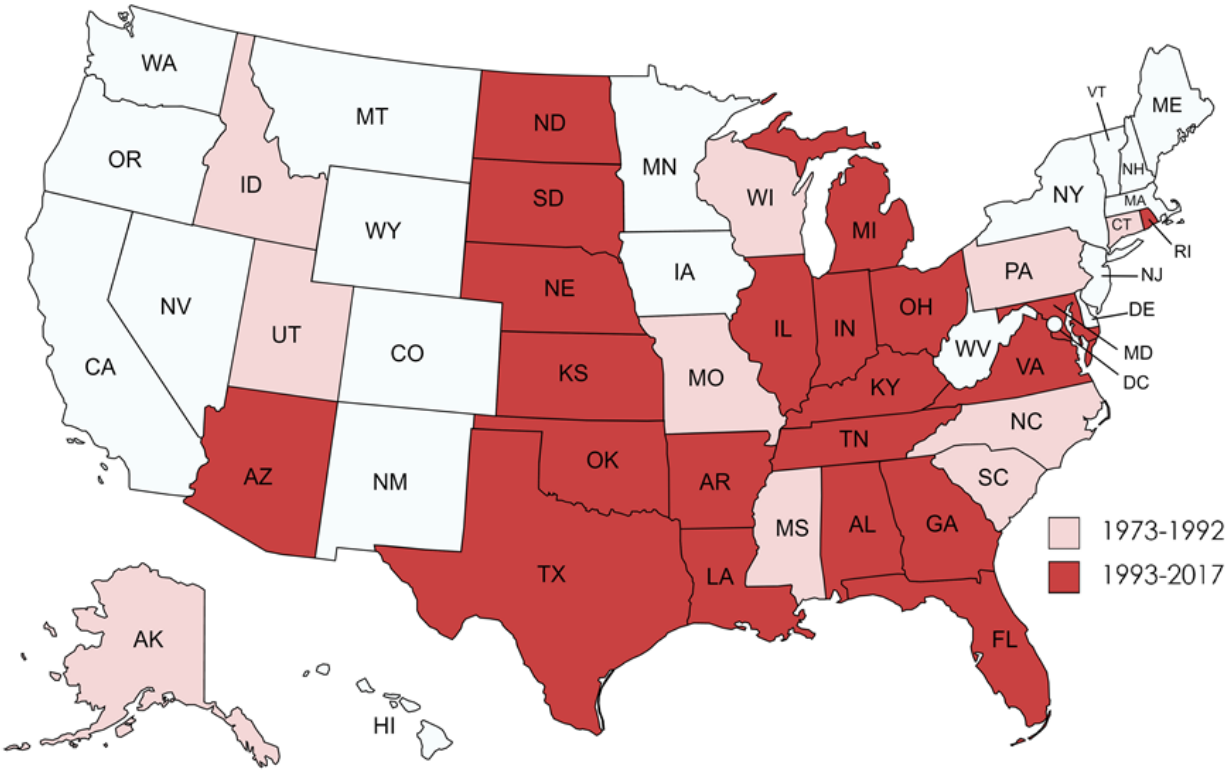


(b) By TRAP law type



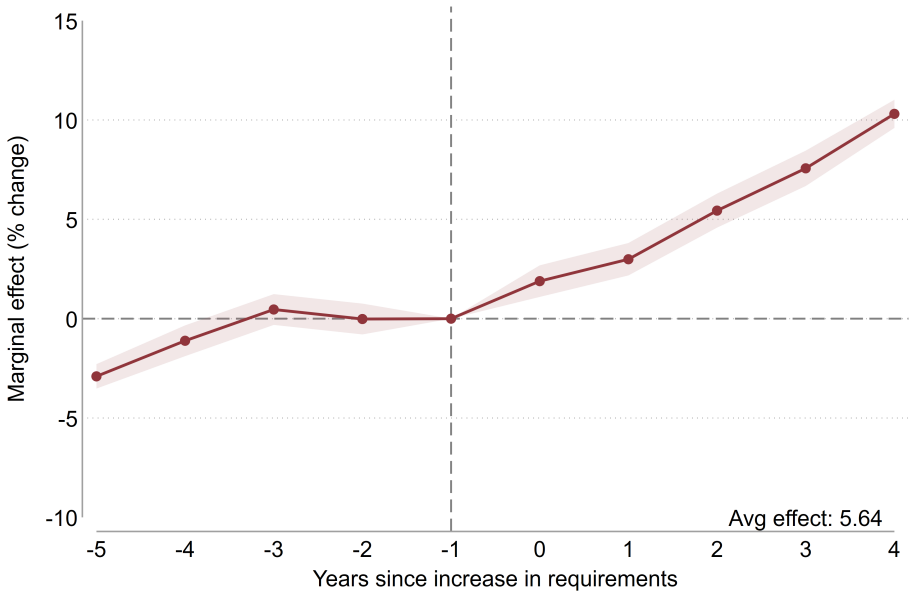
Notes: Subfigure (a) compares the evolution of the number of states implementing three of the most common abortion restriction types (parental involvement laws, two-trip mandatory waiting periods, and targeted regulations of abortion providers (TRAP laws)) from 1980-2017. See section 4.2.1 for more information on abortion restrictions. 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 (2021b). Subfigure (b) compares the number of states implementing TRAP laws by type (admitting privileges, transfer agreement, distance regulations, and building regulations) from 1980 to 2017. See section 4.3.1 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 dataset, described in detail in Appendix C.4.

Figure C.2: TRAP laws by state and era



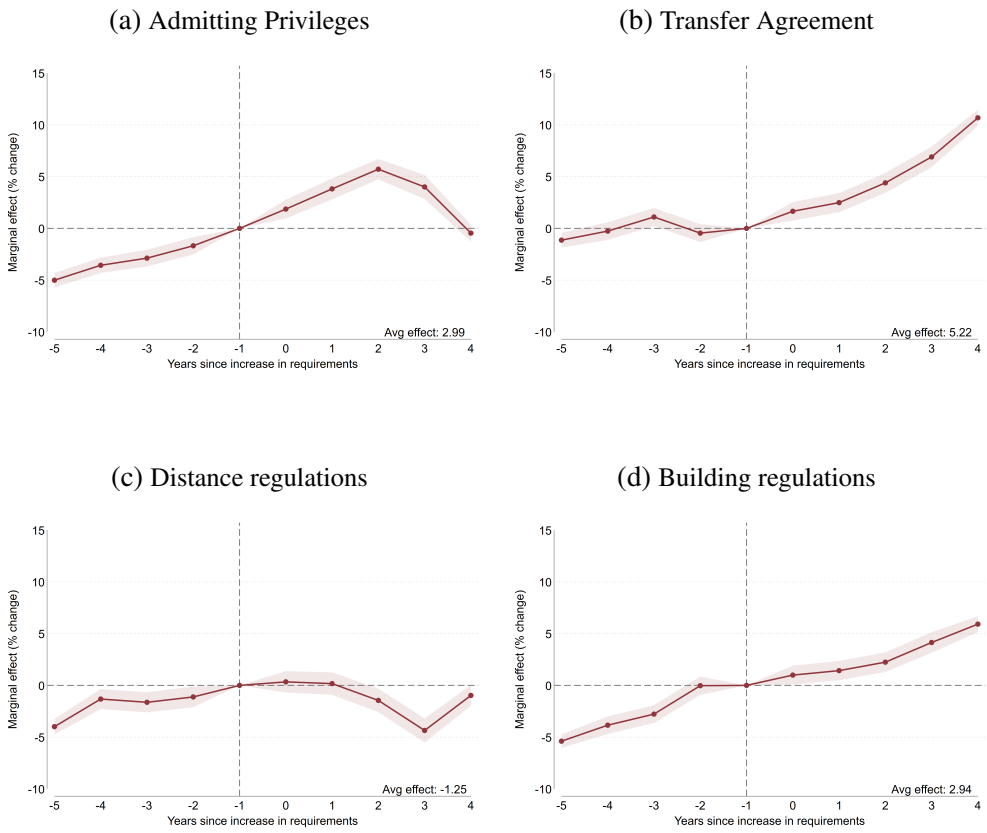
Note: This map shows the states that ever implemented a TRAP law between 1973 to 2017, by the year of first implementation. Light-colored states implemented their first TRAP law between 1973-1992. Darker-colored states implemented their first TRAP law between 1993-2017. Source: TRAP laws legal coding, described in detail in Appendix C.4.

Figure C.3: Impact of TRAP laws on White teen births



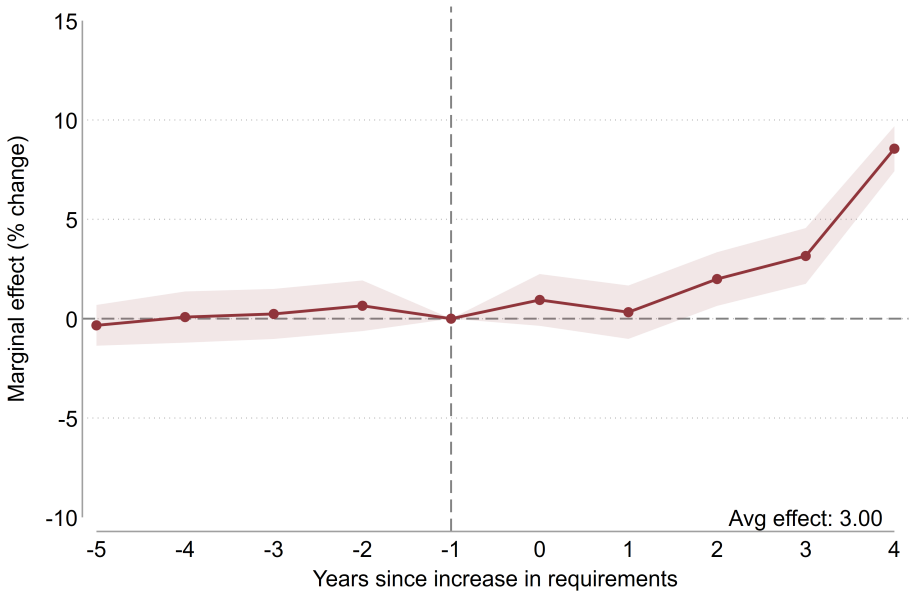
Notes: The figure presents the estimates of b_{it}^{j} from Equation 4.1. The dependent variable is the number of births of 15 to 19-year-old White women in state i and year $t+1$. Year zero indicates the implementation of either admitting privileges, transfer agreement, distance regulation, or building regulations. The omitted year is the year before the policy change, $t=-1$. The shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level. 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 on TRAP laws, as described in detail in Appendix C.4.

Figure C.4: Impact of TRAP laws on White teen births, by TRAP type



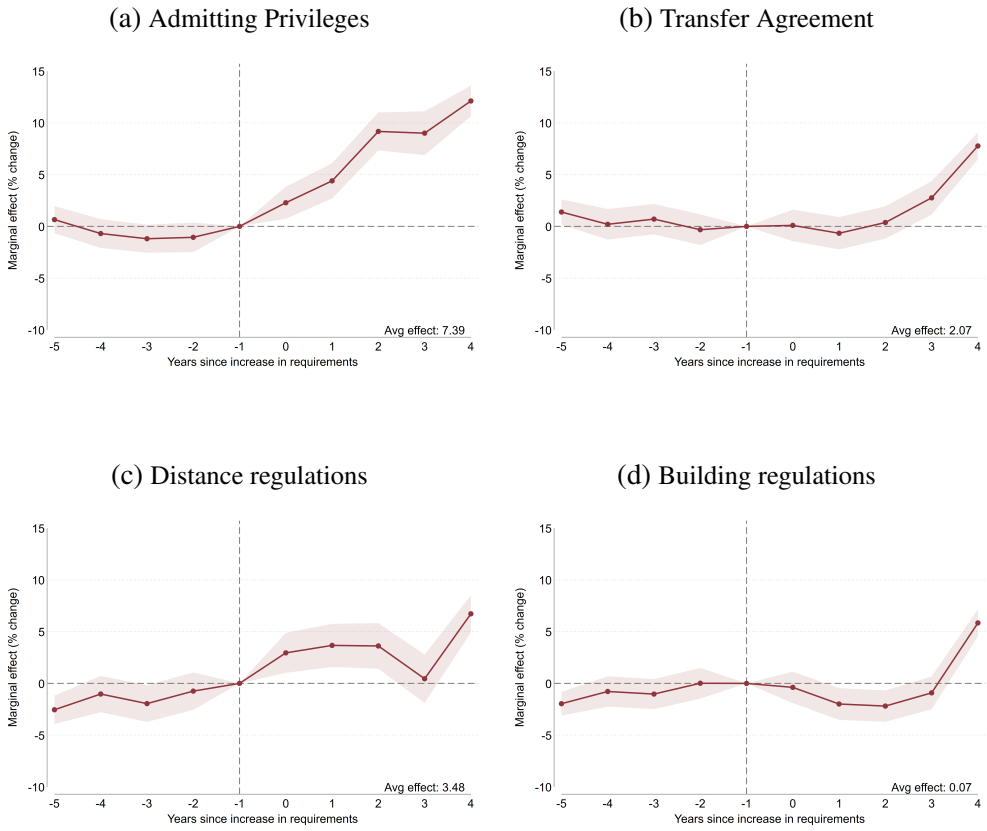
Notes: The figures present the estimates of b_{it}^{j} from Equation 4.1. The dependent variable is the number of births of 15 to 19-year-old White women in state i and year $t+1$. In each figure, year zero indicates the addition of a requirement, as follows: (a) that one or more staff members of a clinic providing abortion to have hospital admitting privileges, (b) a written transfer agreement or a plan/protocol for hospital transfer, (c) distance regulations, and (d) building regulations. The omitted year is the year before the policy change, $t = -1$. The shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level. 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 on TRAP laws, as described in detail in Appendix C.4.

Figure C.5: Impact of TRAP laws on Black teen births



Notes: The figure presents the estimates of b_{it}^{j} from Equation 4.1. The dependent variable is the number of births of 15 to 19-year-old Black women in state i and year $t+1$. Year zero indicates the implementation of either admitting privileges, transfer agreement, distance regulation, or building regulations. The omitted year is the year before the policy change, $t=-1$. The shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level. 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 on TRAP laws, as described in detail in Appendix C.4.

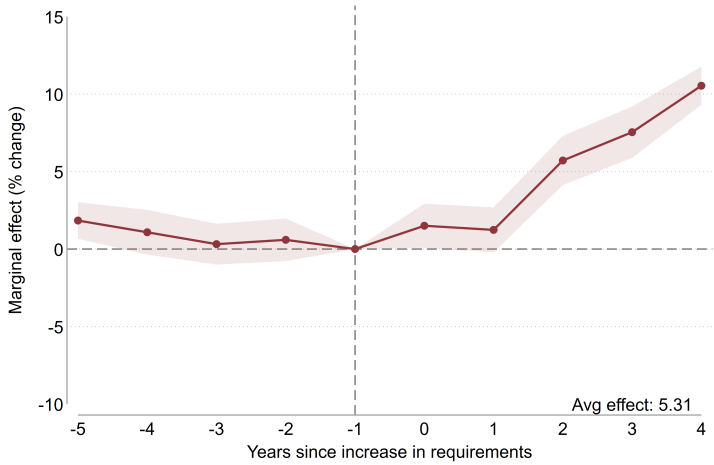
Figure C.6: Impact of TRAP laws on Black teen births, by TRAP type



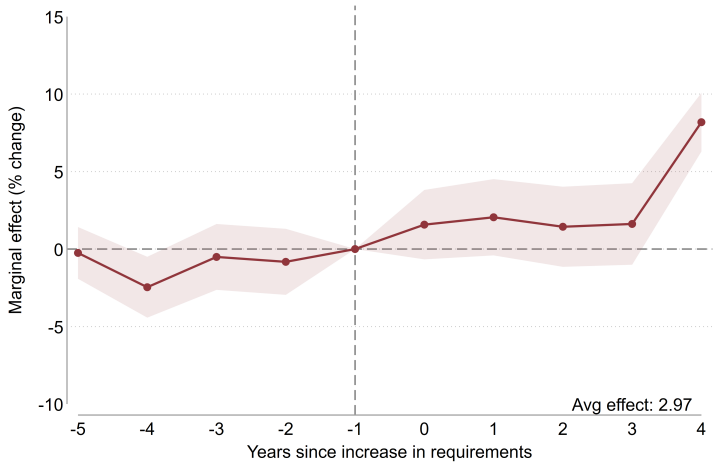
Notes: The figures present the estimates of b_{it}^{j} from Equation 4.1. The dependent variable is the number of births of 15 to 19-year-old Black women in state i and year $t+1$. In each figure, year zero indicates the addition of a requirement, as follows: (a) that one or more staff members of a clinic providing abortion to have hospital admitting privileges, (b) a written transfer agreement or a plan/protocol for hospital transfer, (c) distance regulations, and (d) building regulations. The omitted year is the year before the policy change, $t = -1$. The shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level. 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 on TRAP laws, as described in detail in Appendix C.4.

Figure C.7: Impacts of TRAP laws on Black teen births: alternative binary definitions

(a) Admitting Privileges, including least severe

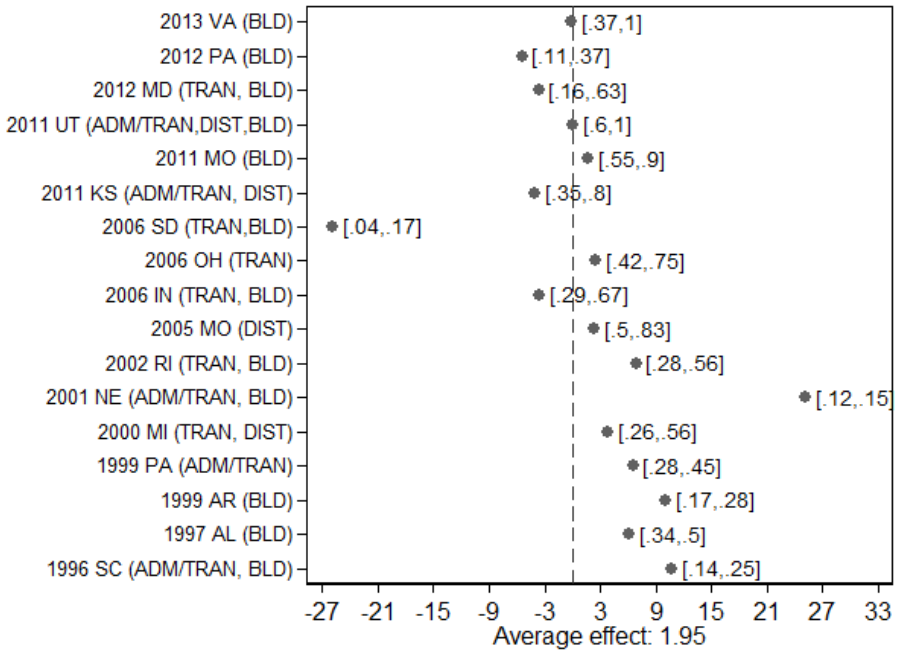


(b) Transfer Agreement, excluding least severe



Notes: The figures present the estimates of b_{it}^{j} from Equation 4.1. The dependent variable is the number of births of 15 to 19-year-old Black women in state i and year $t+1$. The omitted year is the year before the policy change, $t=-1$. The shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level. Year zero in subfigure (a) indicates states implemented any admitting privileges, including those that allow abortion facilities to have an agreement with an external physician that has these privileges. Year zero in subfigure (b) indicates states that required a formal written transfer agreements with hospitals, excluding those states that allow for a plan/protocol. 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 on TRAP laws, as described in detail in Appendix C.4.

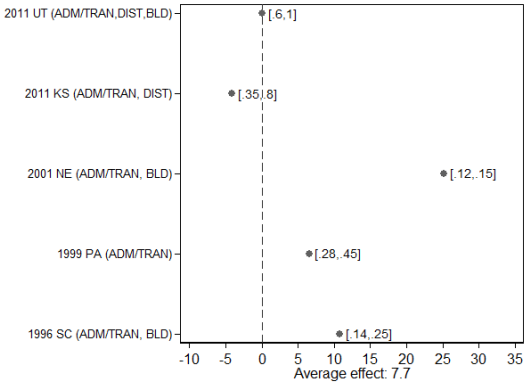
Figure C.8: Impact of TRAP laws on Black teen births. Stacked DiD



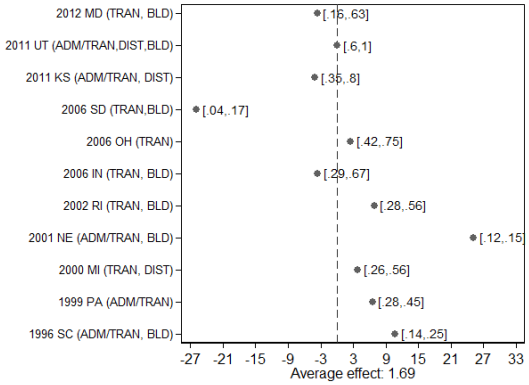
Notes: This figure displays the estimates of b_{it} from equation 4.3 for states that implemented a TRAP law in year t and did not implement any other abortion restriction within $[t-5, t+4]$. The dependent variable is the number of births of 15 to 19-year-old Black women in state i and year $t+1$. Each estimate comes from a separate regression that compares a treated state with states that never implemented an abortion restriction (never-treated) and states that implemented restrictions after period $t+4$ (future treated). The labels in the y–axis indicate the year, state, and restriction type (in parentheses) that correspond to each coefficient. ADM stands for admitting privileges laws, TRAN stands for transfer agreements, DIST stands for distance regulations and, BLD stands for building regulations. Some states implemented more than one TRAP law in a year, as indicated in the parentheses. The average of these coefficients is included at the bottom of the figure. The numbers in brackets represent the one-sided and two-sided Fisher exact p -values. See section 4.4.4 for more information on the methodology. 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 on TRAP laws, as described in detail in Appendix C.4.

Figure C.9: Impact of TRAP laws on Black teen births, by TRAP type. Stacked DiD

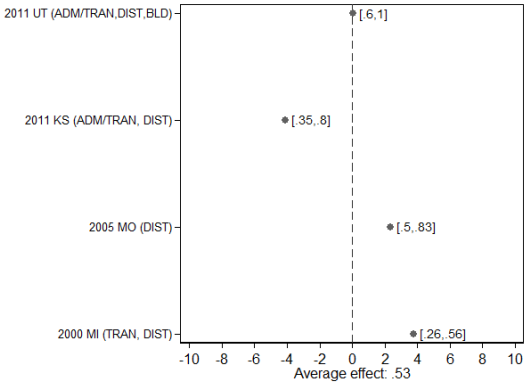
(a) Admitting Privileges



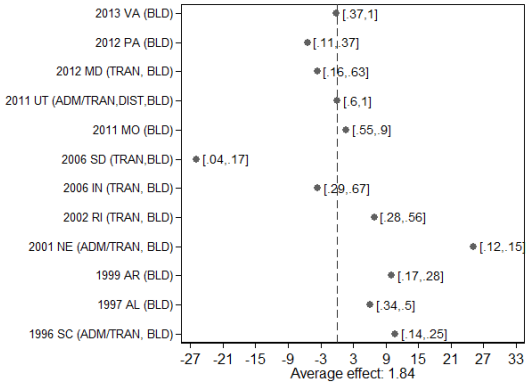
(b) Transfer Agreement



(c) Distance regulations

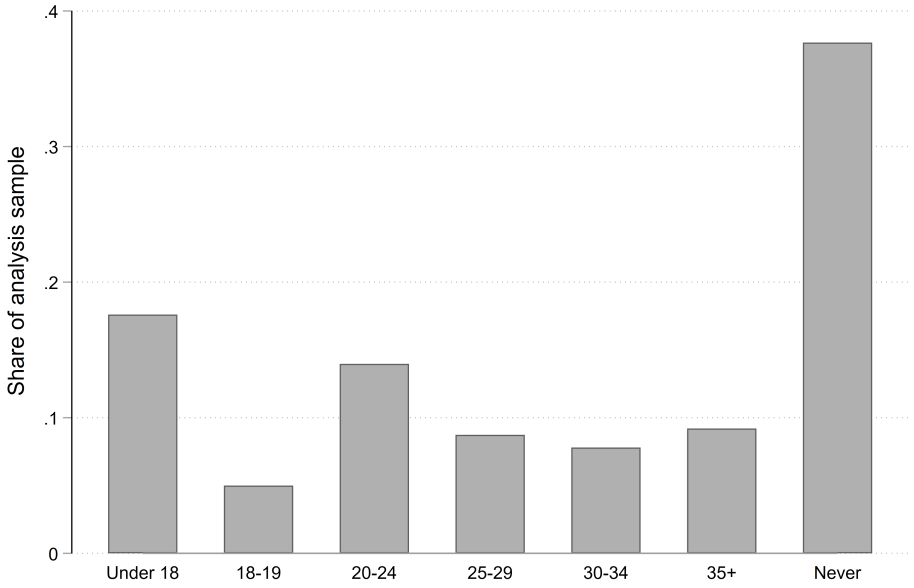


(d) Building regulations



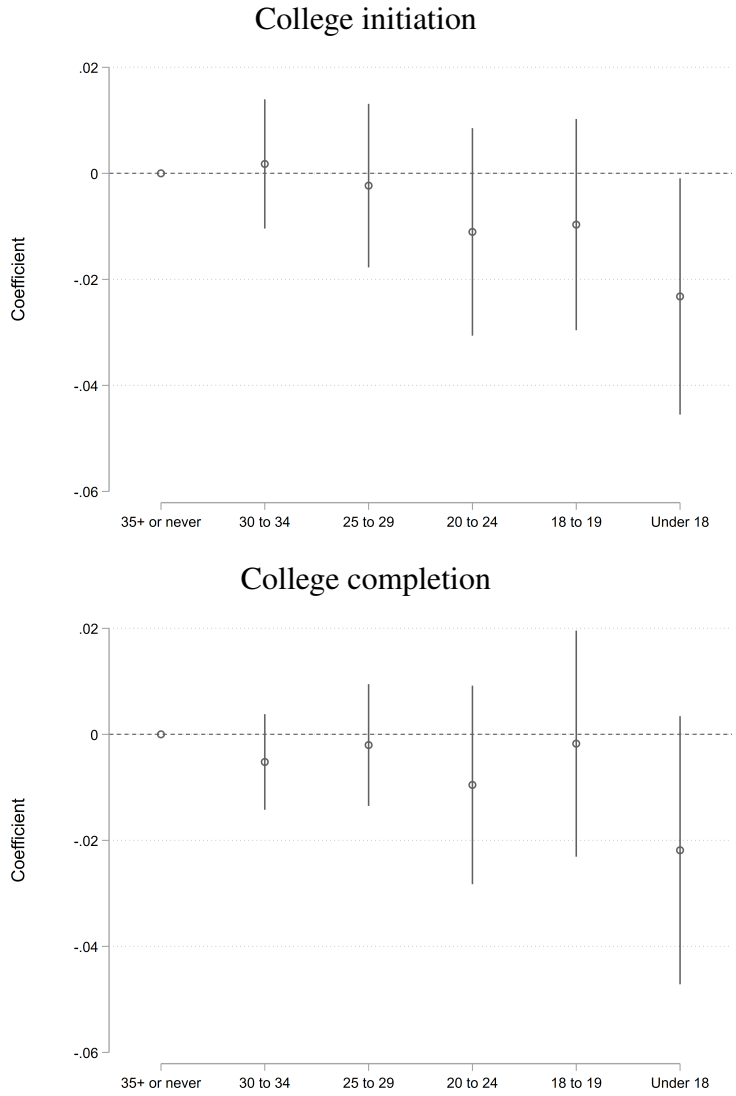
Notes: See notes in Figure C.8.

Figure C.10: Age at first TRAP exposure



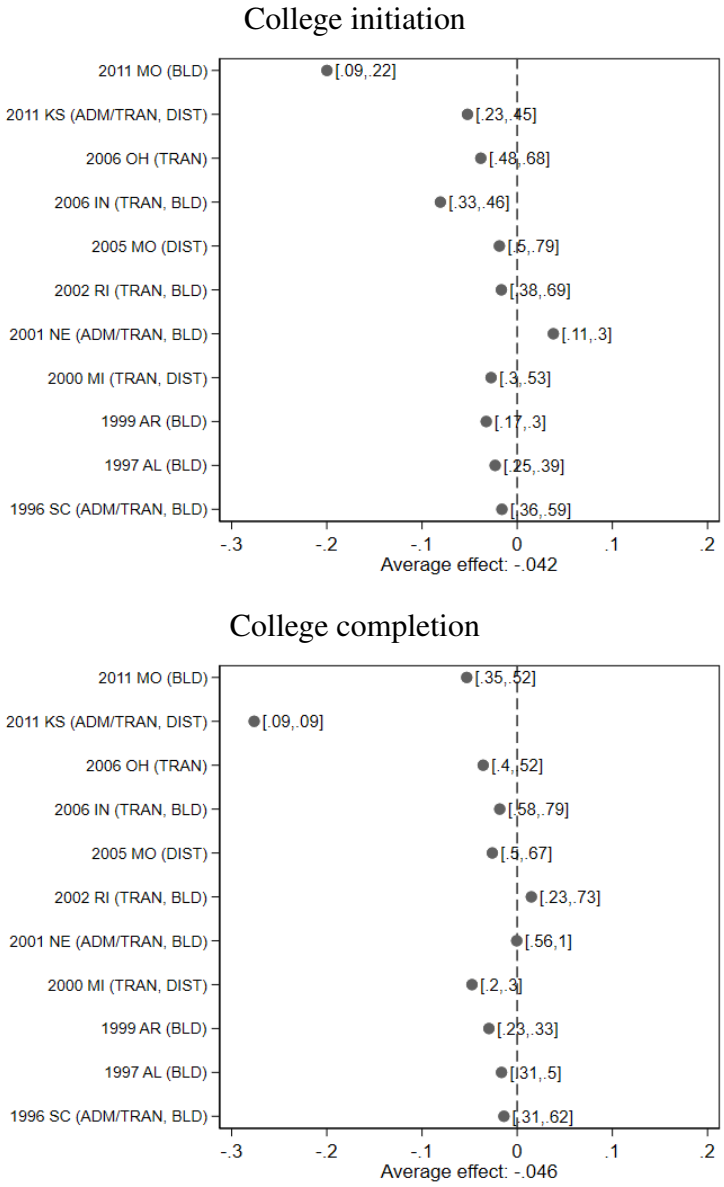
Notes: Notes: This figure shows the distribution of women by age at first exposure to a TRAP law. Source: Own calculations using ACS data from IPUMS (Ruggles et al., 2021) and the authors' legal coding of TRAP laws, as described in detail in Appendix C.4.

Figure C.11: Impacts of TRAP law on Black women's education, by age at first exposure



Notes: The figures show the estimates of a set of indicators for first exposure at various age groups, from an equation similar to 4.5.2, but replacing exp_{bs} by these indicators. College initiation indicates completing at least one year of college or more. College completion refers to completing at least a bachelor's degree. The estimations control for state, year, and age fixed effects. They additionally control for implementation of parental involvement laws, two-trip mandatory waiting periods, and other abortion, contraception, and welfare policies. These figures provide evidence of the common trends assumption for the analysis presented in section 4.5.2. Sources: ACS data from IPUMS (Ruggles et al., 2021); information on other policies from Myers and Ladd (2020), and the authors' legal coding of TRAP laws, as described in detail in Appendix C.4.

Figure C.12: Impacts of TRAP laws on Black women's education: Stacked DiD



Notes: These figures display the estimates of exp_{bs} from equation 4.5.4, which indicates exposure before age 18 to a TRAP law, which is determined by birth year b and birth state s. College initiation indicates completing at least one year of college or more. College completion refers to completing at least a bachelor's degree. The labels in the y–axis indicate the year, state, and restriction type (in parentheses) that correspond to each coefficient. Each estimate comes from a separate regression that compares individuals exposed before age 18 to TRAP laws with individuals first exposed to TRAP laws at older ages or never. ADM stands for admitting privileges laws, TRAN stands for transfer agreements, DIST stands for distance regulations and, BLD stands for building regulations. Some states implemented more than one TRAP law in a year, as indicated in the parentheses. The average of these coefficients is included at the bottom of the figure. The numbers in brackets represent the one-sided and two-sided Fisher exact p-values. See section 4.5.4 for more information on the methodology. Sources: ACS data from IPUMS (Ruggles et al., 2021); information on other policies from Myers and Ladd (2020), and the authors' legal coding of TRAP laws, as described in detail in Appendix C.4.

C.2 Tables

Table C.1: Variables averages by state's future TRAP law status. 1990 American Community Survey

	White women		Black women	
	Non-TRAP	TRAP	Non-TRAP	TRAP
Highest educational attainment				
High-school	0.33	0.35	0.27	0.29
Associate's degree	0.28	0.23	0.17	0.17
College initiation	0.48	0.41	0.36	0.35
College completion	0.20	0.17	0.12	0.11
Graduate school	0.07	0.05	0.04	0.04
Employment and income				
Labor force participation	0.57	0.54	0.61	0.60
Employed	0.55	0.51	0.55	0.54
Total family income (2012 USD)	44,395	39,808	22,025	21,843

Notes: This table shows the averages of potential determinants of unintended pregnancies by race and future TRAP status in the respondent's state of birth. Source: These averages are based on the information on the 1990 American Community Survey.

Table C.2: Implementation of TRAP laws

						Reason if excluded from analysis, by type		
Year	State	Admit	Transfer	Dist	Build	All analyses	Both Stacked DiD	ACS Stacked DiD
2013	Alabama	X					3	6
2013	North Dakota	X		X			4	6
2013	Ohio		X				4	6
2013	Texas	X		X			4	6
2013	Virginia				X			6
2012	Maryland		X		X			6
2012	Tennessee	X					3, 4	6
2012	Pennsylvania				X			6
2012	Arizona			X			5	6
2011	Indiana	X				1		
2011	Kansas	E	E	X				
2011	Missouri				X			
2011	North Dakota	X				1		
2011	Utah	E	E	X	X			7
2009	Texas				X		4	
2006	Indiana		X		X			
2006	Ohio		X					
2006	South Dakota		X		X			7
2005	Missouri			X				
2003	Alabama	X				1		
2002	Rhode Island		X		X			
2001	Nebraska	E	E		X			
2000	Arizona	X			X		5	
2000	Michigan		X	X				
1999	Arkansas				X			
1999	Pennsylvania	E	E			2		
1998	Kentucky		X				5	
1998	Oklahoma	X			X	1	5	
1998	Texas	X	X			1	5	
1997	Alabama				X			
1996	Mississippi	X				1		
1996	South Carolina	E	E		X			
1994	North Carolina				X	*		

Notes: The exclusion codes are as follows: 1=Below minimum stringency (these are included in Appendix Figure C.7); 2=Decrease in stringency; 3=Blocked within 5 years; 4=Adjacent TRAP within 5 years; 5=Adjacent PI or MWP within 5 years; 6=Too recent to be able to analyze with the ACS data; 7=Too few Black women in ACS.

Table C.3: TRAP laws excluded from analyses

		Admit	Transfer	Dist	Build
Pre-*Casey*					
1991	Mississippi		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	X			
2016	Illinois	X		X	X
2015	Louisiana				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		
2016	Florida	X			
2015	Louisiana				X
2015	South Dakota				X
2015	Tennessee		X		X
2015	Ohio			X	
2015	North Carolina		X		
2014	Louisiana	X		X	
2014	Oklahoma	X		X	
2013	Georgia	E	E		X
2012	Virginia		X		
2010	Utah	E	E		
2006	Florida				X
2005	Mississippi		X	X	X
1999	Indiana				X
1993	Indiana		X		X
1985	Utah	X	X	X	X
1976	South Carolina		X		

Table C.4: Impact of TRAP laws on Black teen births: Average effects from stacked DiD

		r = 6		*r = 8*		*r = 10*		*r = 12*	
Abortion restriction	Event-study	NT	NT+NYT	NT	NT+NYT	NT	NT+NYT	NT	NT+NYT
Any TRAP law	3.00	2.79	1.96	2.17	1.13	3.25	1.95	5.23	3.77
Admitting privileges	7.39	6.16	4.75	9.55	7.49	9.98	7.70	14.82	12.38
Transfer agreements	2.07	2.14	1.23	2.61	1.36	3.03	1.69	5.28	3.78
Distance requirement	3.48	1.86	1.50	1.57	0.67	1.71	0.53	2.63	1.30
Building requirements	0.07	2.15	1.24	1.54	0.54	3.11	1.84	6.32	4.60

Notes: This table compares the average of the post-intervention estimates in Figures C.5 and C.6, to the averages of the estimators obtained in the stacked difference-in-differences analyses. The dependent variable in all these analyses is the number of births of 15 to 19-year-old Black women in state s and year $t+1$. The first column shows the average post-TRAP effect from the event-study analyses. The subsequent columns present the average of the DiD treatment effects from the states that meet the following criteria: 1) they implemented the TRAP law listed in the first column in year t, and 2) within r years (centered at t) these states did not implement any other policy that restricted access to abortion. NT and NYT refer to the states considered in the control group. NT stands for "never treated states," which are those states that have never implemented an abortion restriction. NYT stands for "not yet treated;" these are the states that implemented an abortion restriction in a period after period $t+l$, where $l = 2, ...5$, and $r = [t-l-1, t+l]$. 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 C.4.

Table C.5: Impact of TRAP laws on abortion access and use

Panel A. Distance to nearest abortion provider	Mean	β
Geodesic distance (miles)	85.11	8.429***
		(2.569)
Travel distance (miles)	75.38	9.520***
		(3.120)
Travel time (minutes)	59.81	9.049***
		(2.988)

Panel B. Abortion use	(1)	(2)
CDC data		
All women	-3.9483	-4.3058*
	(2.6593)	(2.45)
White women	-6.2925	-0.1124
	(4.6564)	(4.8529)
Black women	-11.0096**	-4.7289
	(5.2643)	(3.5952)
Women under age 20	-5.1385	-5.515*
	(3.2589)	(2.9802)
Guttmacher Institute data		
All women	-3.5189	-4.0927
	(3.4668)	(3.8613)
Controls PI & MWP	Yes	Yes
Controls other policies	No	Yes

Notes: Panel A shows the estimates of β_3 from equation 4.4, which corresponds to an indicator for an observation from the year 2017 and from a 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 facility. Panel B shows the estimates of γ from equation 4.5, which indicates that any TRAP law was enforced in state s in year t. The dependent variables are the number of abortions for each demographic group stated in the table. Each estimate comes from a separate regression. The estimations in this panel include state and year fixed effects. The first column additionally controls for the implementation of parental involvement laws and two-trip mandatory waiting periods. The second column also includes controls for these and other abortion, contraception, and welfare policies. See section 4.4.1 for more information on these policies. The standard errors in all these estimates are clustered at the state level. Sources: The information on distance and travel was provided by Caitlin Myers. She also provided us with information on abortion counts, which was compiled from the CDC (Kortsmit, 2020). Information on abortion counts also 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 section C.4.

Table C.6: Impact of TRAP laws on teen sexual behavior

	Mean	(1)	(2)
Panel A. White teen girls			
Sexual debut	0.42	-0.0256***	-0.0231***
		(0.00826)	(0.00859)
Contraception use	0.57	0.0051	-0.0015
		(0.0114)	(0.0128)
Panel B. Black teen girls			
Sexual debut	0.54	-0.0131	-0.0127
		(0.0143)	(0.0181)
Contraception use	0.60	-0.0374**	-0.0481***
		(0.0172)	(0.0182)
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 4.6, 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. 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 4.4.1 for more information on these policies. The standard errors are clustered at the state level. 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 C.4.

Table C.7: Impact of TRAP laws on Black women's education: pooled DiD

	(1)	(2)
College initiation		
Any TRAP law	-0.012**	-0.011*
	(0.006)	(0.0057)
Admitting privileges	-0.0116**	-0.0087*
	(0.0032)	(0.0047)
Transfer agreements	-0.0083	-0.0062
	(0.0084)	(0.0078)
Distance regulations	-0.0205**	-0.0183**
	(0.0039)	(0.0042)
Building regulations	-0.0161**	-0.0166**
	(0.005)	(0.0047)
College completion		
Any TRAP law	-0.0117*	-0.0116**
	(0.0062)	(0.0056)
Admitting privileges	-0.0056	-0.002
	(0.0054)	(0.0052)
Transfer agreements	-0.0078	-0.0072
	(0.0087)	(0.0079)
Distance regulations	-0.0344**	-0.0346**
	(0.0064)	(0.0063)
Building regulations	-0.0108**	-0.0095**
	(0.0033)	(0.0034)
Controls PI & MWP	Yes	Yes
Controls other policies	No	Yes

Notes: The coefficients correspond to the value of β in Equation 4.5.2, the coefficient on the variable indicating a woman was exposed to a TRAP law before age 18, as determined by her birth year b and birth state s. 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. All the estimations control for state of birth, year of birth, and age fixed effects. Estimations in column (1) include controls for exposure during adolescence to parental involvement laws and two-trip mandatory waiting periods. Estimations in column (2) control for these and additionally for exposure to other abortion, contraception, and welfare policies. See section 4.4.1 for more information on these policies. The estimations are weighted by the person weight provided in ACS-IPUMS. The standard errors are clustered at the state of birth level. $^{*}p < 0.1$,$^{**}p < .05$. Sources: ACS data from IPUMS (Ruggles et al., 2021); information on other policies from Myers and Ladd (2020). The authors' legal coding on TRAP laws is described in detail in section C.4.

Table C.8: Impacts of TRAP laws on Black women's education, defining exposure as under age 20

	(1)	(2)
College initiation		
Any TRAP law	-0.0118**	-0.0111**
	(0.0047)	(0.0046)
Admitting privileges	-0.0116**	-0.0109**
	(0.0038)	(0.0039)
Transfer agreements	-0.0127**	-0.0116*
	(0.0062)	(0.0061)
Distance regulations	-0.0135**	-0.0125**
	(0.004)	(0.0041)
Building regulations	-0.0117**	-0.0122**
	(0.0058)	(0.0061)
College completion		
Any TRAP law	-0.0053	-0.0047
	(0.0045)	(0.0041)
Admitting privileges	-0.0009	-0.0001
	(0.0053)	(0.0057)
Transfer agreements	-0.0052	-0.004
	(0.005)	(0.0042)
Distance regulations	-0.0175**	-0.0139**
	(0.0046)	(0.0034)
Building regulations	-0.0023	-0.0027
	(0.005)	(0.0049)
Controls PI & MWP	Yes	Yes
Controls other policies	No	Yes

Notes: The coefficients correspond to the value of β in Equation 4.5.2, the coefficient on the variable indicating a woman was exposed to a TRAP law before age 20, as determined by her birth year b and birth state s. 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. All the estimations control for state of birth, year of birth, and age fixed effects. Estimations in column (1) include controls for exposure during adolescence to parental involvement laws and two-trip mandatory waiting periods. Estimations in column (2) control for these and additionally for exposure to other abortion, contraception, and welfare policies. See section 4.4.1 for more information on these policies. The estimations are weighted by the person weight provided in ACS-IPUMS. The standard errors are clustered at the state of birth level. $^{*}p < 0.1$,$^{**}p < .05$. Sources: ACS data from IPUMS (Ruggles et al., 2021); information on other policies from Myers and Ladd (2020). The authors' legal coding on TRAP laws is described in detail in section C.4.

Table C.9: Impacts of TRAP laws on Black women's education, defining exposure based on state of residence

	(1)	(2)
College initiation		
Any TRAP law	-0.0059	-0.0073
	(0.0047)	(0.0047)
Admitting privileges	-0.012*	-0.0144**
	(0.007)	(0.0067)
Transfer agreements	-0.0058	-0.007
	(0.0054)	(0.0058)
Distance regulations	-0.0136**	-0.0158**
	(0.0044)	(0.0044)
Building regulations	-0.0076	-0.0094*
	(0.0052)	(0.0055)
College completion		
Any TRAP law	-0.009	-0.0104
	(0.0071)	(0.0065)
Admitting privileges	-0.004	-0.0055
	(0.0173)	(0.0162)
Transfer agreements	-0.0051	-0.0068
	(0.0097)	(0.0092)
Distance regulations	-0.0404**	-0.0425**
	(0.0099)	(0.0108)
Building regulations	-0.0123*	-0.0128**
	(0.0063)	(0.0064)
Controls PI & MWP	Yes	Yes
Controls other policies	No	Yes

Notes: The coefficients correspond to the value of β in Equation 4.5.2, which indicates a woman was exposed to a TRAP law before age 18, determined by her birth year b and residence state s. 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. All the estimations control for state of birth, year of birth, and age fixed effects. Estimations in column (1) include controls for exposure during adolescence to parental involvement laws and two-trip mandatory waiting periods. Estimations in column (2) control for these and additionally for exposure to other abortion, contraception, and welfare policies. See section 4.4.1 for more information on these policies. The estimations are weighted by the person weight provided in ACS-IPUMS. The standard errors are clustered at the state of birth level. $^{*}p < 0.1$,$^{**}p < .05$. Sources: ACS data from IPUMS (Ruggles et al., 2021); information on other policies from Myers and Ladd (2020). The authors' legal coding on TRAP laws is described in detail in section C.4.

Table C.10: Impact of TRAP laws on Black women's education: Average effects from stacked DiD

		Unlimited cohorts		Max 10 cohorts per group	
Abortion restriction	Pooled DiD	NT	NT+NYT	NT	NT+NYT
Panel A. College initiation					
Any TRAP law	-0.0110	-0.0403	-0.0452	-0.0349	-0.0399
Admitting privileges	-0.0087	-0.0047	-0.0101	0.0031	-0.0022
Transfer agreements	-0.0062	-0.0232	-0.028	-0.0155	-0.0202
Distance regulations	-0.0183	-0.0328	-0.0399	-0.0152	-0.0218
Building regulations	-0.0166	-0.0427	-0.0474	-0.0400	-0.0449
Panel B. College completion					
Any TRAP law	-0.0116	-0.0504	-0.0481	-0.0397	-0.0383
Admitting privileges	-0.0020	-0.0990	-0.0969	-0.0858	-0.0848
Transfer agreements	-0.0072	-0.0560	-0.0541	-0.0447	-0.0436
Distance regulations	-0.0346	-0.1627	-0.1620	-0.1388	-0.1384
Building regulations	-0.0095	-0.0200	-0.0172	-0.0121	-0.0102

Notes: This Table compares the estimate in Table C.7 column (2) (shown here in column 1) to the averages of the estimates obtained in the stacked difference-in-differences analyses (shown here in subsequent columns). NT and NYT refer to the states considered in the control group. NT stands for "never treated states," which are those states that have never implemented an abortion restriction. NYT stands for "not yet treated." These are the states that implemented an abortion restriction at least five years after period t. The first row specifies the number of cohorts included in each analysis: 1) all the possible cohorts, and 2) at most ten cohorts. Sources: ACS data from IPUMS (Ruggles et al., 2021); authors' legal coding of TRAP laws, as described in detail in section C.4.

C.3 Impacts of TRAP severity

In the event study methodology presented in Section 4.4.2, 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.[1] To accommodate this change we employ the suggested reformulation of b_{it}^{j}, that is,

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

This definition of b_{it}^{j} is equivalent to that presented in Section 4.4.2, 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

[1] This modification is the reason for the reformulation of the definition of b_{it}^{j} in Equation C.1, which also follows Schmidheiny and Siegloch (2019).

In the standard case is mathematically equivalent to the more common definition:

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

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

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 4.3.1 for a list). Distance regulations are excluded from this analysis because there is no variation in their intensity.

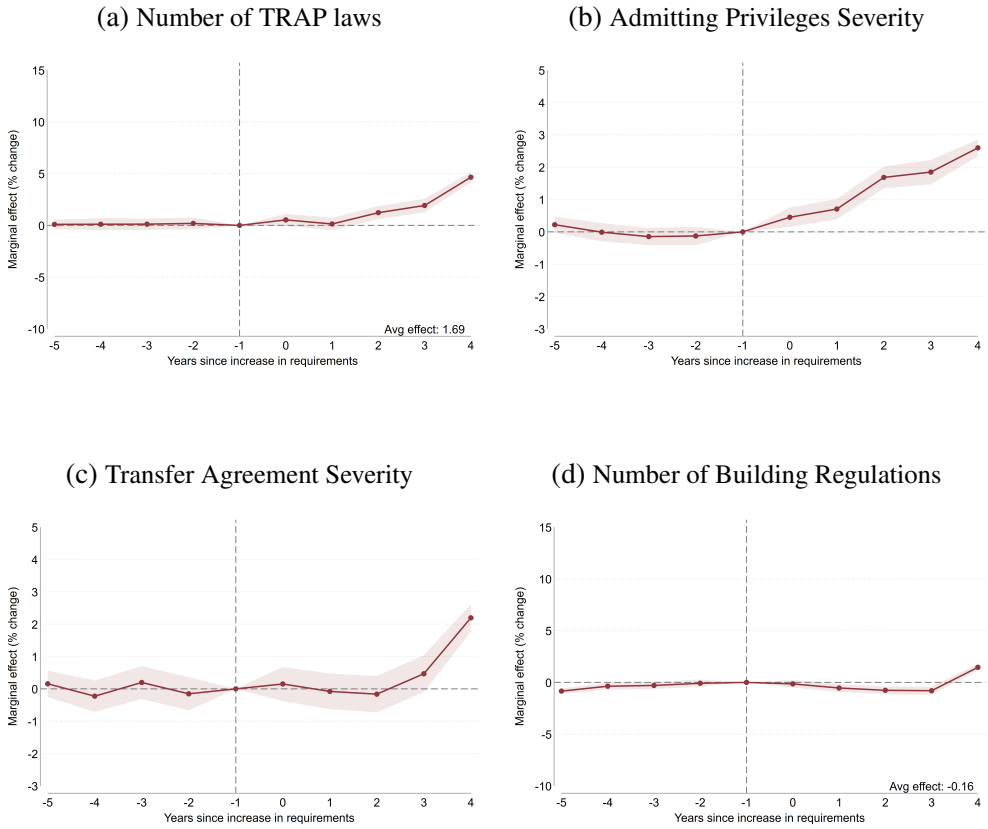
Multiple policy changes within a state are captured by the increase (or decrease) in intensity of the succeeding policy, relative to the former policy. For example, in North Dakota an admitting privileges law of level 2 turns on in 2011, and an additional admitting privileges law of level 6 turns on 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 4.4.2. The results are shown in Figure C.13. 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 4.4.2, which capture binary changes from level 0 to level 1 but also binary changes from level 0 to level 6, for example.

Figure C.13: Impacts of TRAP laws on Black teen births, by severity



Notes: Note: This figure presents the estimates of b_{it}^{j} from Equation 4.1. The dependent variable is the number of births of 15-19-year-old Black women living in state i in year $t+1$. The treatment variable reflects the intensity of the indicated regulation, as defined in section C.3. 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 shaded region indicates the 95 percent confidence interval corresponding to clustered standard errors at the state level. Sources: Figure created using the information on births recorded 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 legal coding on TRAP laws, as described in detail in Appendix C.4.

C.4 Details on legal coding of TRAP laws

Alaska

- Transfer agreement requirement: 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 analysis.

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. It is not included in the main analyses but the robustness check shown in Figure C.7.
  - 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 include 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) required 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. We coded 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.

- Distance 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 include this 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 analysis because it was implemented in the pre-Casey era.

Florida

- 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 main 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 analysis because it was implemented in the pre-*Casey* era.

Illinois

- Admitting privileges, distance 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 analysis.

- 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. Since after 1988, no law establishes all abortion facilities must operate as ASCs, we did 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 tax 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 coded this law as the less stringent version of admitting privileges laws and considered coded 2011 as its effective year. However, this law is not included in our main analyses but as part of the robustness check in Figure C.7.

- 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 distance 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 more 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.

Louisiana

- Admitting privileges and distance regulations: Effective on 9/1/2014, La. Rev. Stat. Ann. §40:1061.10 started requiring 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 to 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 assigned as the effective year the promulgation year 2015.

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 the robustness check in Figure C.7.

- COMAR 10.12.15 became effective on 7/23/2012. Both subsections 15 and 10, mentioned above, are part of it. So, we assigned 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.

- Distance 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 2012 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 distance regulations as effective since 2012, without distinction. Both laws were rescinded in 2020.

Missouri

- Building and distance 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.

- A distance 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 coded 2005 as the effective year of this law.
- In 2007, the state revised Mo. Rev. Stat. § 197.200 to require that all abortion providers operate as ASCs. This increased the physical plant requirements and changed the distance regulation to be located within 15 minutes from a hospital. Later this law was challenged in court (Drummond). This challenge implies that the same abortion provider regulations continued applying since the 2007 law was enjoined in our coding. 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, and this is our last year of data.

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 analysis.

- 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 1996 and 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 did not include it 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. We do not consider the 30 minutes travel time a distance 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 use 1996 as the effective year.

North Carolina

- Admitting privileges, distance regulations, and transfer agreement: 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 that same date, that same date, the law established some emergency back-up services requirements. This includes 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.

- 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. In Figure C.7, we exclude from the analysis this restriction.

North Dakota

- Admitting privileges and distance 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 coded the 2011 admitting privileges law as the least stringent version because it only

requires an agreement with another physician with active admitting privileges. We coded the 2013 laws as admitting privileges and distance 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 coded 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. They include specifications of examination rooms, procedure, and recovery rooms, as well as the corridor and hallway width and ventilation.

Ohio

- Distance 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 coded 2015 as the effective year of this restriction.

- Transfer agreement: The 2006 decision on the case Women's Medical Professional Corp (WMPC). v Baird mentions that ambulatory surgical facilities (ASF) were required before 1999 with attempts to enforce licensing for abortion clinics as ASFs only beginning in 1999. Note that there is no legal code requiring abortion clinics to operate as ASFs; it seems clinics were operating as ASFs have more to do with interpreting the definition of an ASF than an ASF law. 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 considered 2006 as the transfer agreement effective year. This is consistent with the fact that this TRAP law was 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 ambulatory surgical facility where non-therapeutic abortions are performed or induced. This prohibition made it harder for abortions facilities operating as ambulatory surgical facilities to comply with the transfer agreement requirement. We also coded this transfer agreement restriction as effective in 2013 to capture the increase in stringency of the law.

Oklahoma

- Admitting privileges and distance regulation: 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. We show in Figure C.7 that the event-study estimates are robust to the inclusion of these policies.

- 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 coded 1998 as the effective year.

Pennsylvania

- Admitting privileges, distance 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 assigned this year as the effective year for the transfer agreement and distance regulation.
  - 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 assigned 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, for the reasons presented above, we assigned 1983 as the effective year.
  - Effective on 6/19/2012, Dec. 22, P.L. 563, No. 122, § 2, required all facilities performing surgical abortions to operate as ASF. The ASF requirement triggers 28 Pa. Code § 571.1, which requires ASFs to comply with specific requirements according to the "Guidelines for Design and Construction of Hospital and Health Care Facilities." Therefore, we coded 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 assigned 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 main 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. Figure C.7 shows results excluding this law from the analysis.

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 analysis.
  - The 1976 version of S.C. Code Ann. Regs. 61-12. 205, required 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 analysis. 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.
  - However, the 1996 version of S.C. Code Ann. Regs. 61-12.305 required 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 coded 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 privilege. 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 coded 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 considered this law a plan/protocol since it does not require a formal written agreement with a hospital to transfer patients. Therefore, we assigned 2016 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.

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 included it in our analysis and coded 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 assigned 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 ASC T requirement was enforced in 2015. Then, we assigned 2015 as the effective year. This law was enjoined in 2018.

Texas

- Admitting privileges, distance regulation, and transfer agreement:
  - Effective on 8/13/1998, 25 Tex. Admin. Code § 139.56 required 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 coded 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.
  - 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 assigned 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 coded 2009 as the effective year for building regulations.

- The following regulations are not include in our analysis as these requirements 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 such, it is not included even in Table C.3. 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

- 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 distance regulations can be tracked down 1985 version of Utah Admin. Code r. R432-600. In the 1991 version of Utah Admin. Code r. R432-600 mentioned that clinics should follow the 1987 Guidelines for Design and Construction of Health Care Facilities, which dictate what building regulations should be implemented. However, we could not trace down the 1987 version of these guidelines. All these early regulations seem only to apply to post-first-trimester abortions. They were also implemented in the pre-Casey era. So, we did not include them in our main 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 assigned 2011 as the effective year for admitting privileges or transfer agreement, distance regulations, and building regulations. However, since we are not sure 2011 is the year of implementation of these laws, we include Figures for

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

- 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 coded 2012 as the transfer agreement effective year. However, we did not include this law in our analysis because it only targets 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 coded 2013 as the effective year of these

building regulations.

Wisconsin

- Distance 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 should make arrangements with a hospital for admission of patients needing hospital care. Such hospital shall be located sufficiently near the facility used 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.