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Notification and consent: the differential effects of parental involvement laws on teen abortion

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Abstract
US state legislation requiring parental involvement in the abortion decision of a minor has grown in prevalence since its origin in the 1970s. Today, 36 states impose a parental involvement requirement on their residents below the age of 18. These laws come in two primary categories: parental notification and parental consent. Though much research estimates the effects of these policies, limited evidence exists regarding any differential impact between parental notification and parental consent. This paper uses the synthetic control method to determine if the increased marginal cost of an abortion imposed by a parental consent statute affects the abortion rate and birth rate for minors relative to parental notification. Results indicate no evidence of a marginal effect of parental consent laws on the abortion/birth rate for minors overall, suggesting that the additional cost of a parental consent law may be small.

Keywords Abortion · Birth · Parental involvement laws

JEL classification I11 · I12 · J13 · J18

1 Introduction

In the United States, parental involvement (PI) laws are state-level policies that require the participation of a parent in the abortion decision of an unemancipated, unmarried minor (aged <18). These laws come in two broad categories: notification and consent. Parental notification laws mandate that the abortion provider make a satisfactory effort to contact and notify the parent(s) or guardian(s) of an unemancipated, unmarried minor prior to performing an abortion. Under a consent law,

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providers are required to collect various forms of parental consent, from simple verbal consent to notarized written consent.

In any single period, pregnant teens will make the decision to have an abortion based upon their marginal benefits and marginal cost. Parental involvement laws and other forms of restricted abortion legislation increase the marginal cost of an abortion. Because a parental consent law requires parental notification by necessity, a policy change from notification to consent will (at least weakly) increase the marginal cost of an abortion. Using these state-level policy changes, I test the hypothesis that, relative to parental notification, a parental consent law will decrease the abortion rate for minors (15–17). Additionally, evidence suggests that a proportion of minors who are restricted from accessing abortion carry their pregnancy to term and give birth as a result (Myers & Ladd, 2020). So, I also test the hypothesis that the policy change to parental consent will increase the birth rate for minors.

The theoretical foundation of the literature on restricted access to abortion considers abortion to be an insurance policy against negative information realized after pregnancy. Forms of restricted access (including PI laws) increase the marginal cost of the insurance policy (Kane & Staiger, 1996). Some research suggests that restricted access to abortion has long term negative consequences for women. Miller et al. (2023) survey women just before and just after the gestational limit and find that seeking but being denied an abortion results in large increases in measures of financial distress, and that this distress persists for six years after the intended abortion.

Teens are often disproportionately affected by restrictive abortion laws, as they may have limited education on reproductive options, less experience navigating the healthcare system, and fewer resources to travel to avoid restricted abortion access (Ralph & Hasselbacher, 2023). In the first six months after a 2021 ban on abortions after 6 weeks gestation in Texas, the number of abortions in the state declined by 45%, while abortions among minors declined at a much steeper rate of 66–68% (Stevenson, 2022). Following the *Dobbs v. Jackson* decision by the Supreme Court in 2022, under which 14 states have completely banned abortion procedures, teens may face additional insurmountable barriers to abortion access.

Adolescence is a particularly vulnerable time to experience an unintended pregnancy, as it is a critical stage of emotional/physical development as well as human capitalaccumulation. In a recent survey, 40% of 15–17 year-olds indicated that they would attempt to self-manage[1] their abortion in the event that they could not receive abortion care at a clinic, which suggests that the consequences of carrying an unwanted pregnancy are particularly salient to minors (Ralph et al. 2022). Jones and Pineda-Torres (2024) find that adolescent exposure to common supply-side abortion restrictions known as targeted regulations on abortion providers results in a 2.1% decline in college enrollment and a 5.8% decline in college completion among Black women. The authors do not find evidence of a similar decrease among white women, and so restricted abortion access as a teen may also contribute to existing racial disparities.

[1] Self-managed abortion refers to efforts by an individual to end a pregnancy on their own outside of the formal healthcare system.

Parental involvement laws are the most common restrictive abortion policy in the United States. As of March 2023, 36 states have a PI law in place (though many of these states have recently passed complete abortion bans following *Dobbs*). Of these states, 21 require only parental consent, 9 require only parental notification, and 6 require both notification and consent. The policies are still up for consideration in state legislatures. As recently as 2020, Florida passed a bill that changed their parental notification law to a parental consent law. Illinois had efforts to eliminate their parental notification statute appear for legislative consideration in 2019 and 2020 before finally repealing the law in 2021. Understanding how the nature of these laws interacts with their effects on an important vulnerable population is a key element of the public policy and public health discussion surrounding reproductive healthcare.

2 Background

2.1 Trends in teen abortion

Non-trivial variation across states in their teen abortion rates provides another motivation for studying topics related to teenage abortion. Figure 1 uses data from a Guttmacher Institute report detailing the pregnancy rate and abortion rate for 15–17 year-olds in all 50 states in 2013.

These graphs show significant variation in the teen abortion rate (per 1000 residents assigned female at birth) and the percent of teen pregnancies aborted. Maryland has a 15–17 abortion rate of 10, five times the abortion rate of Nebraska (teen abortion rate of 2). Minors in Maine abort their pregnancies roughly 35 percent of the time, which is nearly three times the percent of pregnancies aborted in West Virginia (12.5 percent). The variation in the percent of pregnancies abortion means that the observed variation in the teen abortion rate cannot be solely attributed to differences in pregnancy rates. This paper considers whether the type of parental involvement law contributes to the variation in the teen abortion rate.

2.2 Parental involvement laws

Figure 2 demonstrates the strong correlation between the number of enforced PI laws and the declining abortion rate among minors, and a broad literature estimates the causal effects of these policies. Generally, studies fall into two categories: a national approach to determine the effects of PI laws across the entire country (or a large part of it), and a state-specific approach analyzing a policy change in one single state.

Among national studies, Ohsfeldt and Gohmann (1994) compare states with and without a PI law over a pooled sample from 1984, 1985, and 1988. Their outcome of interest is the ratio between the abortion rate of minors (15–17) and the abortion rate of older teens (18-19). They use the abortion rate for older teens to account for overall trends in the abortion rate within a state. Their analysis implicitly assumes that the abortion rate of older teens is independent of the abortion rate for minors, and the abortion rate for older teens acts as a control group for overall statewide trends in the abortion rate. Using linear regression with controls for abortion price proxies and abortion attitude proxies, they find that parental involvement laws reduce the

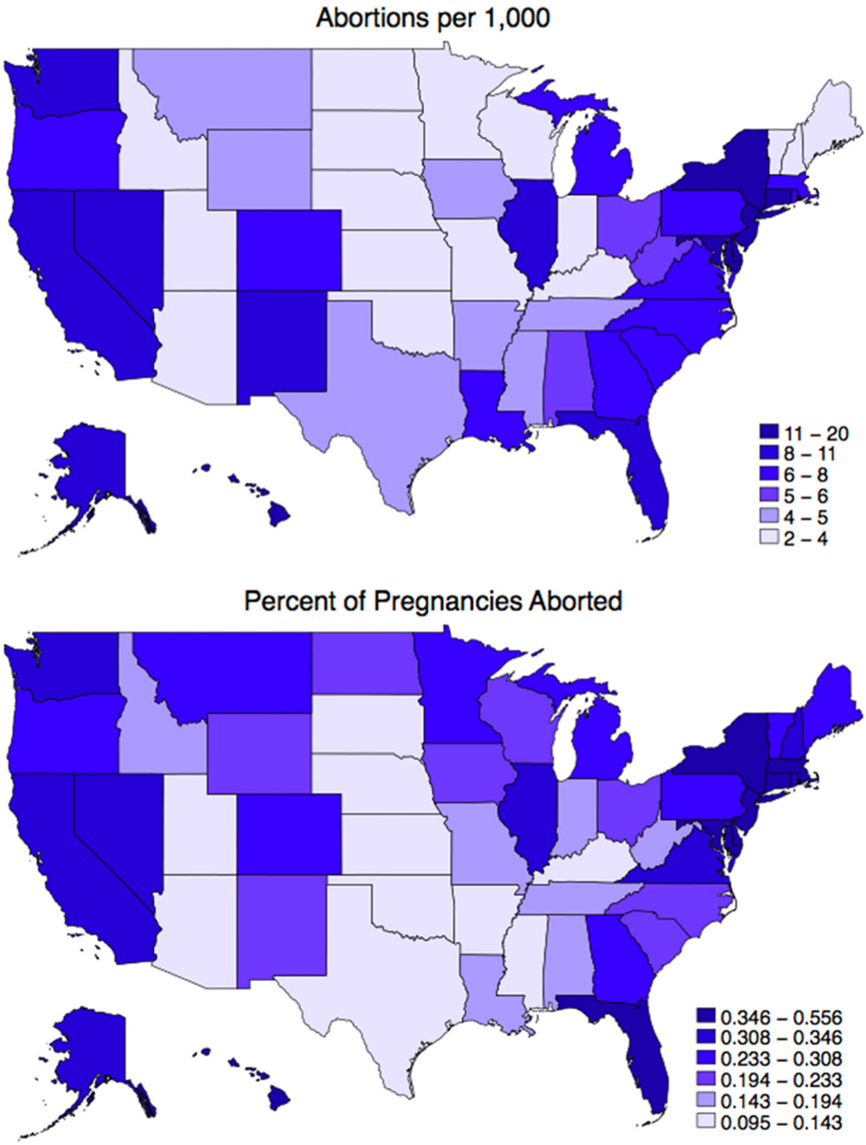


Fig. 1 Abortion rate and percent of pregnancies aborted, 2013. Source: Kost et al. (2017)

adolescent (15–17) abortion rate within a state by roughly 18 percent. In a similar study controlling for state-level characteristics such as abortion attitudes, Haas-Wilson (1996) reports a similarly sized effect of these laws on the abortion rate for teens: a reduction of 13–25 percent among 15–19 year-olds. In a later work, Levine (2003) uses difference-in-differences and triple-difference designs and reports findings consistent with the earlier papers. Both Levine (2003) and Ohsfeldt and Gohmann (1994) also consider the effect of PI laws on birth rates for minors. Studying this outcome helps distinguish between two possible adolescent behavioral

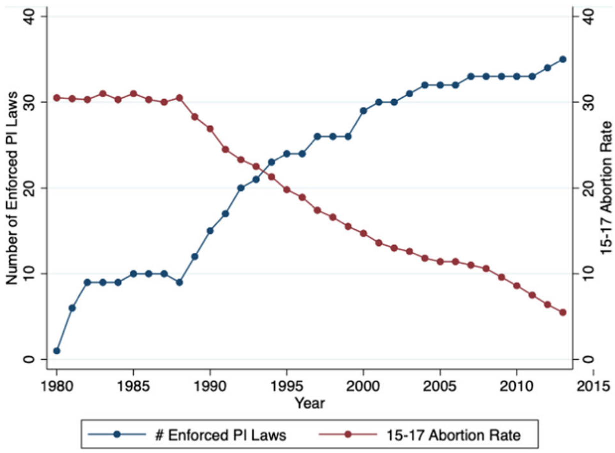


Fig. 2 PI laws and the 15–17 abortion rate over time (1980–2013). Source: Kost et al. (2017); Myers and Ladd (2020)

responses: increased use of contraception and abstinence resulting in fewer overall pregnancies and the restricted access to abortive care resulting in a greater number of births. These two early papers, however, are not in agreement about the effects of PI laws on teen birth rates. While Levine's results indicate a reduction in the abortion rate for minors without a corresponding increase in the teen birth rate, Ohsfeldt and Gohmann find that PI laws increase adolescent fertility by 10 percent.

A significant drawback to these papers using early data from the 1980s and 1990s is the inability to identify teens that travel out of state to have an abortion. The data often come from national sources and surveys such as the CDC Abortion Surveillance Summaries, which did not report abortion by state residency status until the mid-2000s. This limitation is particularly important in light of the evidence that teens do travel out-of-state to have an abortion when they are facing parental involvement law restrictions (Joyce & Kaestner, 1996; Cartoof & Klerman, 1986).

In more recent work, Myers and Ladd (2020) exploit better county-level data and a measure of distance that a minor would have to travel to avoid a PI law to determine the effect of parental involvement laws on the teen birth rate. The authors confirm Levine's earlier result that PI laws in the 1980s and 1990s were not associated with higher teen birth rates. In more recent years (1993–2016), however, they find that these laws result in an increase in teen births of around 3 percent. This difference likely arose from the increased spread of PI laws making it more difficult for a teen to travel out of state to escape the law. They write that they are unable to provide a credible estimate of any effect of PI laws on the teen abortion rate because nationally reported data from the CDC and the Guttmacher Institute is too limited.

Joyce et al. (2020) use a synthetic control method over a group of 14 states to assess the impact of parental involvement laws on the abortion rate for minors. The authors estimate separate effects for the PI law in each state. Their results indicate that some states experience a statistically significant reduction in the abortion rate of minors and other states see no meaningful effect.

State-level policy analysis is fairly consistent with the national studies. Two studies consider the implementation of a parental notification law in Texas, reporting results of a 16 percent and 25 percent decrease in the abortion rate for minors (Joyce et al. 2006; Colman et al. 2008). MacAffee et al. (2015) study the New Hampshire notification law and report a 47 percent decrease in the number of abortions performed on minors in New Hampshire, with 62 percent of this change being driven by a decrease in minors from Massachusetts traveling to New Hampshire to avoid the parental consent law there. The authors determine that the New Hampshire law resulted in a 19.3 percent decrease in the abortion rate among resident minors. Two papers also consider the parental notification law in Illinois, with one finding no apparent decrease in the abortion rate for minors compared to that of older teens, and the other reporting a small decrease in the portion of abortions performed on women under 20 (Ralph et al., 2018; Ramesh et al. 2016).

2.3 Notification and consent

A much smaller literature considers any differential effects of parental notification and parental consent laws. The basic theory underlying our understanding of parental involvement laws suggest that parental consent laws should (at least weakly) reduce the abortion rate of minors relative to parental notification laws, since a parental consent law represents a greater marginal cost of an abortion. The findings in the literature, however, are quite mixed. An early study on this topic finds a counterintuitive result – parental notification laws reduce the abortion rate for minors more than parental consent laws (Tomal, 1999). This paper has a few limitations, including a small sample of states and the inability to account for interstate travel mentioned earlier. Using data from nearly all 50 states, New (2008) determines that parental consent laws reduce the abortion rate for minors by 18.7 percent, while notification laws reduce the abortion rate by only 5 percent. Two papers also determined no significant differential effect between parental consent and parental notification. Using a 2SLS estimation of abortion demand, Medoff (2007) reports no significant different in the effects of parental consent laws and parental notification laws. Joyce (2010) exploits a natural experiment – the policy change from parental notification to parental consent in Arkansas. Using a difference-in-differences design between age groups within the state, Joyce reports no significant reduction in the abortion rate for minors compared to older teens following the policy change.

I contribute to this literature by providing an extension to the analysis of parental involvement laws in Arkansas by Joyce (2010). This paper considers the effect of a policy change from parental notification to parental consent in six states spanning the US South and Midwest. Therefore, this work contributes to the question of external validity of the natural experiment in Arkansas. In addition, this paper uses a different empirical method, the synthetic control, to estimate treatment effects. Because there is no general consensus on the marginal effect of parental consent laws, a variety of methodologies is useful to get closer to understanding any true effects.

The use of synthetic control is particularly important in this context, as the dynamic nature of fertility choice implies that older teens may effectively be treated by PI laws in years following any policy change, and this limits their credibility as a control group. The comparison between minors and older teens could potentially introduce bias in several ways. First, restricted fertility choice as a minor could spill-over into fertility decisions as an older teen. If, for instance, someone gives birth as a minor because a parental involvement law prevents them from receiving an abortion, their costs of carrying a pregnancy are altered if they become pregnant again as an older teen. Second, parental involvement laws may result in changes to contraceptive and sexual behavior among minors, and this change in behavior may continue into adulthood. Finally, pregnant minors who are close to their 18th birthday may delay their abortion to avoid the parental involvement law, decreasing the abortion rate for minors while increasing the abortion rate for older teens. These potential sources of bias motivate an empirical strategy that does not rely on comparisons between minors and older teens.

3 Data

To determine the legislative history of a state, I use the legal coding developed by Myers and Ladd (2020). I divide states into a treatment and control group based upon their legislative history. States that change their law from parental notification to parental consent make up the treatment group, while states that maintain a consistent parental involvement law serve as the control group. Table 1 provides a description of the treatment and control group.

Data on state-level abortion rates comes largely from the CDC abortion surveillance summaries. I verify[2] CDC data with state-level induced termination of pregnancy (ITOP) reports when ITOP data reports the age categories (15–17) necessary for my analysis. CDC and ITOP data are normally reported with raw numbers for abortions rather than abortion rates. Therefore, I use population estimates from the SEER database in order to impute an abortion rate (per 1000 residents assigned female at birth in age category). I use data spanning 1995–2016 in order to roughly match the analysis period and state policy variation information available in Myers and Ladd (2020).

Abortion data from the CDC surveillance has limitations. Abortion counts from the CDC come from voluntary reports from state health departments, and there have been demonstrated inconsistencies between the abortion surveillance summaries and clinic survey counts of abortion incidence from the Guttmacher Institute (Joyce et al. 2020). In particular, CDC counts are often underreported relative to Guttmacher surveys. In addition, CDC abortion surveillance data does not report abortions by state of residence. So, teens that travel out of state to avoid a parental involvement

[2] With the exception of the state of Georgia, all states who provide ITOP reports indicate abortion counts consistent with the CDC reports. Georgia consistently reports lower abortion counts in their ITOP data. In the analysis, I choose to use 15–17 abortion rates from the Georgia ITOP system. Results, available upon request, show that the results of the analysis are robust to the exclusion of Georgia from the donor pool.

Table 1 List of treatment and control states

Treatment		Control	
State	Law	State	Law
Arkansas	Notification: 1995–2004	Alabama	Consent: 1995–2016
	Consent: 2005–2016	Arizona	No Law: 1995–2002
Kansas	Notification: 1995–2010		Consent: 2003–2016
	Consent: 2011–2016	California*	No Law: 1995–2016
Nebraska	Notification: 1995–2010	Colorado	No Law: 1995–2002
	Consent: 2011–2016		Notification: 2003–2016
Ohio	Notification: 1995–2005	Georgia	Notification: 1995–2016
	Consent: 2006–2016	Iowa	Notification: 1997–2016
Texas	Notification: 2000–2004	Illinois	Notification: 1995–2016
	Consent: 2005–2016	Indiana	Notification: 1995–2016
Utah	Notification: 1995–2005	Kentucky	Consent: 1995–2016
	Consent: 2006–2016	Massachusetts	Consent: 1995–2016
Virginia	Notification: 1995–2002	Maryland*	No Law: 1995–2016
	Consent: 2003–2016	Maine	No Law: 1995–2016
		Michigan	Consent: 1995–2016
		Minnesota	Notification: 1995–2016
		Missouri	Consent: 1995–2016
		Mississippi	Consent: 1995–2016
		Montana	No law: 1995–2016
		North Carolina	Consent: 1995–2016
		New Jersey	No Law: 1995–2016
		New Mexico	No Law: 1995–2016
		Nevada	No Law: 1995–2016
		New York	No Law: 1995–2016
		Oregon	No Law: 1995–2016
		Pennsylvania	Consent: 1995–2016
		South Carolina	Consent: 1995–2016
		South Dakota	Notification: 1995–2016
		Tennessee	Consent: 2000–2016
		Vermont	No Law: 1995–2016
		Washington	No Law: 1995–2016
		Wisconsin	Consent: 1995–2016
		West Virginia	Notification: 1995–2016
		Wyoming*	Consent: 1995–2016

Source: Myers and Ladd (2020). States that only report birth data and do not report abortion data are indicated with an*

law may not be properly represented. To compensate for the limitations of abortion count data available, I also estimate effects of the policy change on birth rates among minors. The birth data from the National Vital Statistics System Natality Reports contain more credible reports of birth counts by individual age, and therefore may be better suited to measuring the fertility effects of a parental consent law. In the primary analysis, I aggregate individual-age birth counts to measure births to people who are 15–17 years old.

4 Methods

4.1 The synthetic control

The synthetic control method (SCM) is an empirical strategy that is often used in comparative case study frameworks with a potentially small sample of data. Synthetic control allows researchers to identify the effects of policy interventions at the state/regional level when a control group for the area is not obvious. Instead of comparing one treated unit to one untreated control unit, the treated state is compared to a weighted average of several potential control states.

Following Abadie et al. (2010), the method can be thought of as a generalization of the difference-in-differences method commonly used in linear panel data settings. Define $\alpha_{\{it\}} = Y_{it}^{I} - Y_{it}^{N}$ to be the treatment effect for unit i at time t. Y_{it}^{I} is the outcome of interest in the presence of intervention, and Y_{it}^{N} is the outcome of interest absent intervention – the counterfactual.

Since Y_{it}^{N} is not observed, it is estimated through a pre-treatment period matching process. I select a relevant set of matching characteristics and outcomes for both the treated unit and the set of controls. Then, a set of weights W is generated such that any differences between the treated unit and the weighted control are minimized, only considering the pre-intervention period. Following the work of Klößner and Pfeifer (2018), I use only pre-treatment outcomes (abortion rates and birth rates) in order to construct the weights,

$$W_1 = argmin_{w_j^1 \epsilon [0,1]} \sum_{t=t_0-n}^{t_0-1} \left(Y_{1t} - \sum_{j=2}^{J+1} w_j^1 Y_{jt} \right)^2$$

where unit 1 is the treated unit and n pre-treatment time periods are used. The central idea is that this weighted average of the control states is close to identical to the treated unit. Therefore, it will serve as a good estimate of the counterfactual. This leads to the treatment effect estimator presented in Abadie et al. (2010)

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}$$

In this analysis, I match a treated state to a synthetic control group using pre-treatment outcomes across a limited analysis window in order to improve the pre-period fit between the treated unit and its synthetic control group, with the goal of creating a more credible post-treatment counterfactual. Table 2 describes the outcomes used to match each state to its synthetic control group.

Ferman et al. (2020) demonstrate that the selection of the matching characteristics for synthetic control is not arbitrary, and the resulting treatment effects can be quite sensitive to alternative matching characteristics in certain applications. In Appendix C, I provide results using five alternative matching criteria discussed in Ferman et al. (2020), and I show that the treatment effects are robust to a variety of commonly-used matching criteria.

Figure 3a, b shows the visual results from the synthetic control for the six treated states for both the 15–17 abortion rate and birth rate. The figures provide

Table 2 Details on synthetic control matching criteria

State	Matching variables
Abortions	
AR	Abortion rate 1998–2004
VA	Abortion rate 1998–2002
TX	Abortion rate 2000–2004
OH	Abortion rate 1998–2005
KS	Abortion rate 2005–2010
NE	Abortion rate 2005–2010
Births	
AR	Birth rate 1998–2004
VA	Birth rate 1998–2002
TX	Birth rate 2000–2004
OH	Birth rate 1998–2005
KS	Birth rate 2005–2010
NE	Birth rate 2005–2010

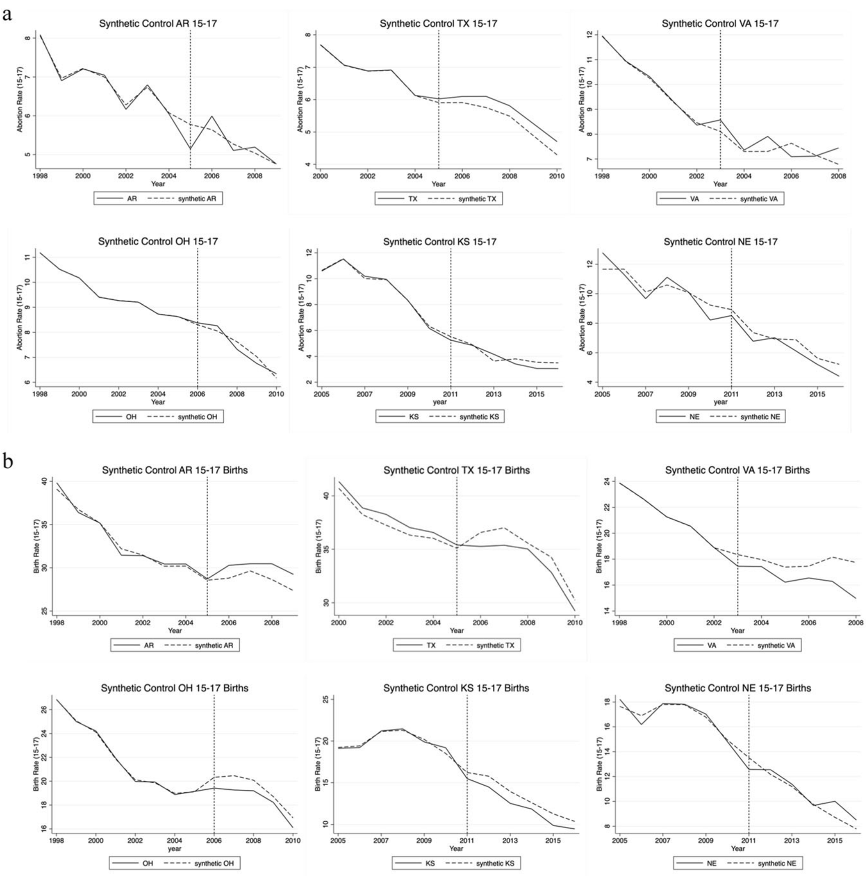


Fig. 3 **a** Synthetic control for the abortion rate of minors (15–17). **b** Synthetic control for the birth rate of minors (15–17)

information about the quality of the synthetic control match and the general direction of the treatment effects. In the pre-period, the abortion/birth rate trends for the treated states and their synthetic control group appear similar, and this supports the assumption that the synthetic control group estimates a counterfactual in the post-period. Post-period differences in the abortion rates for the treated states and their synthetic control group represent treatment effects $\hat{\alpha}_{it}$. Post-period trends in the abortion rate for minors in Fig. 3a generally do not indicate that there are substantial differences between a treated state and its synthetic control group. The largest treatment effect, $\alpha = 0.29$, of a parental consent law on the abortion rate for minors occurs in Texas represents a small 3% increase from the pre-period rate. Generally, effect sizes range from 0.2 to 3% changes from the pre-period, and the direction of the treatment effect is heterogeneous across states. A similar pattern exists for effects of the parental consent law on the birth rate for minors – effect sizes range from a 1 to 4% change from the pre-period average with no consistent direction. The results for the abortion and birth rate for minors taken together suggests that the marginal effect of a parental consent law is limited. A full description of the make up of the synthetic control group for each outcome and treated state is presented in the appendix.

A notable requirement for developing a synthetic control group is that the outcomes in the treated state that are used in the matching process must lie in the convex hull of the control state outcomes. In other words, the trends in the donor pool of control states must contain values that are above and below the trend in the treated state. If this condition is not met, a good synthetic control match using the standard method cannot be attained. Although the state of Utah qualifies as a treated state, because they changed their parental notification law to a parental consent law in 2006, the abortion rate for minors in the pre-period (the characteristics used to match) does not sit in the convex hull of the abortion rate for minors in the control states. For this reason, I exclude Utah from the analysis.

4.2 Inference

Standard in the synthetic control method, I use placebo tests for permutation inference. For each treated state, I generate a set of placebo effects by repeating the SCM procedure on the pool of control states as if they were treated at the time of the policy change. From this permutation inference, I can view the effect size of the policy in the treated state relative to a state chosen at random. Figure 4a, b presents the placebo tests for the abortion/birth rate of minors. These graphs present the difference between the abortion or birth rate in a given state and its synthetic control group. When the synthetic control match in the pre-period is poor for one of my placebo states (RMSPE[3] > 0.5), it is eliminated from the graph and the individual analysis. If the synthetic control match for a control state is poor in the pre-period, its trend in the post-period (the placebo effect) is not very informative.

To determine the statistical significance of any effect, it is common to use a percentile rank statistic that has a similar interpretation to the parametric *p*-value used

[3] RMSPE = root mean-square prediction error.

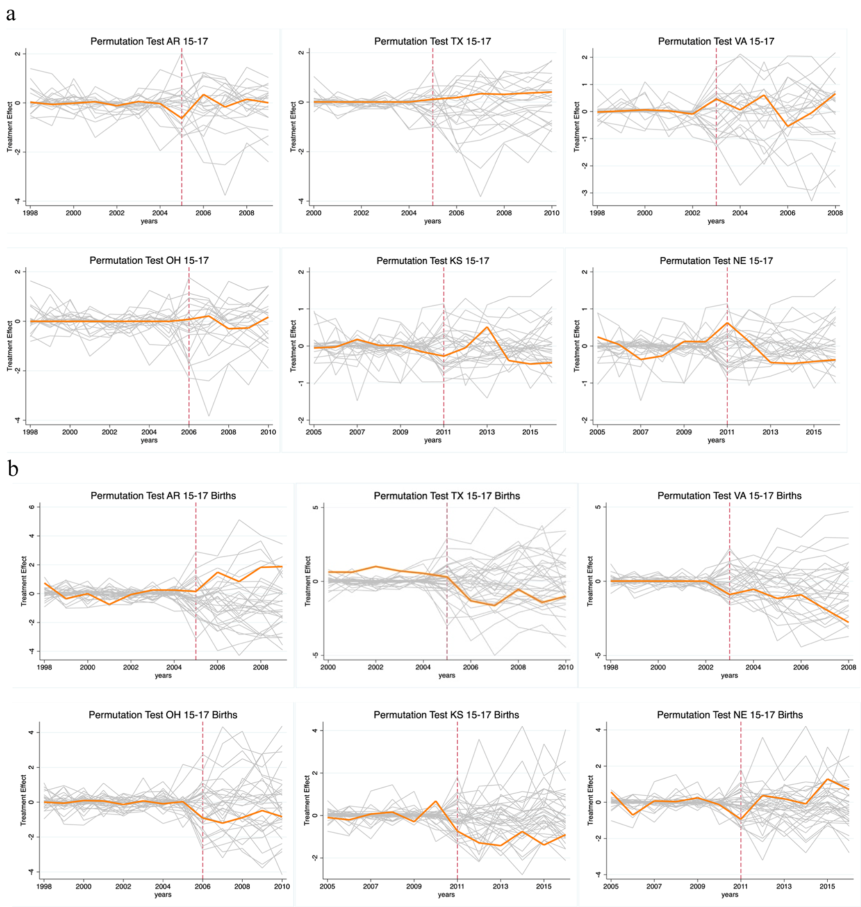


Fig. 4 **a** Permutation tests for the abortion rate of minors (15–17). **b** Permutation tests for the birth rate of minors (15–17)

in regression analysis. I calculate the percentile rank statistic based upon the average treatment effect in the post period $\overline{\alpha}_1 = \frac{1}{s}\sum_{t=t_0}^{t_0+s}\alpha_{1t}$. The percentile rank statistic will be $p_1 = \hat{F}(\overline{\alpha}_1)$, where $\hat{F}$ is the empirical CDF of the average placebo effects $\overline{\alpha}_j$ from the control group[4]. Percentile rank statistics around 0.5 indicate that the treatment effect lies near the middle of the distribution of placebo effects, as is the case for the permutation test for the abortion rate of minors in Ohio pictured in Fig. 4a ($p = 0.53$). This may be evidence that whatever treatment effect we observe in that state could be due to random variation in the abortion rate. Small percentile rank statistics indicate that the treatment effect lies toward the extreme values of the placebo distribution. This is the case in the permutation test for the birth rate of minors in Arkansas pictured in Fig. 4b ($p = 0.07$). A full summary of treatment effects and percentile rank statistics is presented in the Results section in Tables 3 and 4.

[4] Following the method described by Dube and Zipperer, I also use the Weibull-Grumbel rule: $p_1 = \frac{r_1}{N+1}$, where r_1 describes the rank of the treatment effect, and N is the number of control states.

To aggregate information from multiple treated units, I use the pooling method presented by Dube and Zipperer (2015). The pooling method first requires that permutation tests be performed and the percentile rank statistics of each treated state be calculated. Under the null hypothesis that the policy intervention has no effect, these percentile ranks should be random draws from the Uniform [0,1] distribution. So, while the null hypothesis may not be rejected in any treated state individually, we could consider whether or not these percentile ranks from several treated units reasonably represent consecutive random draws from the uniform distribution. To do this, the percentile rank statistics from the treated units are pooled together into a simple average $\bar{p}$. Then, I use the Irwin-Hall distribution of the sum of independent uniform random variables to test the hypothesis that $\bar{p}$ is distributed with mean 0.5.

5 Results

I select two possible groupings for pooling analysis. In one grouping, I pool all of the treated states together to get an overall sense of the effect of the policy intervention. Following the observations in Joyce et al. (2020), my second grouping is based on the timing of the policy. Joyce observes that states that pass their PI law earlier see a larger effect size. So, I divide my states into early treatment (2003–2006) and late treatment (2011) to see if my results are consistent with this observation.

Tables 3 and 4 report the average treatment effect and percentile rank statistics from the placebo tests for minors and older teens. The treatment effect is the simple average of the difference between the abortion rate in the state and its synthetic control group in the post-treatment period. The percentile rank corresponds to the alternative hypothesis for the group. The rank for a state when considering the abortion rate for minors describes the proportion of placebo effects that are at or below the treatment effect (because the alternate hypothesis is that the treatment *reduces* the abortion rate for minors), while the rank considering the birth rate for minors describes the proportion of placebo effects that are at or above the treatment effect (because the alternate hypothesis is that the treatment *increases* the birth rate for minors).

Simply from the treatment effects and percentile ranks, it does not appear that the implementation of a consent law has a very large effect on the abortion rate for minors. The treatment effects also do not operate in a consistent direction across states. Arkansas, Ohio, Kansas, and Nebraska have negative treatment effects, indicating that the policy change may reduce the abortion rate for minors. But, there is not evidence that any of these effects are statistically different from zero. While Texas and Virginia have surprising positive treatment effects, the effect sizes are small (3.1% and 1.7% change from the pre-period average respectively) and still lie toward the center of the distribution of placebo effects.

The effects of the policy change to parental consent on the birth rate for minors exhibit a similar pattern. Treatment effects are generally small, not statistically significant from placebo inference, and do not operate in any consistent direction. It is interesting to note that the direction of the treatment effects on birth rates do not directly correspond to the direction of the effects on abortion rate. We may expect a policy that decreases the abortion rate for minors will increase the birth rate and

Table 3 Treatment effect for abortion rate of minors (15–17)

	Treatment effect	Pre-period average	p
Early states:			
Arkansas	−0.06	7.46	0.44
Texas	0.29	9.36	0.64
Virginia	0.20	11.83	0.72
Ohio	−0.02	10.09	0.53
Late states:			
Kansas	−0.19	13.21	0.43
Nebraska	−0.16	6.89	0.43

Table 4 Treatment effect for birth rate of minors (15–17)

	Treatment effect	Pre-period average	p
Early states:			
Arkansas	1.23	36.70	0.07
Texas	−0.93	42.31	0.70
Virginia	−1.39	23.81	0.80
Ohio	−0.86	24.24	0.68
Late states:			
Kansas	−1.08	22.53	0.91
Nebraska	0.26	18.75	0.29

vice-versa, but this is not the case in the analysis presented. This could be further evidence that post-period differences in the abortion rate between the treated states and their synthetic control group are due to random variation unrelated to the policy change. Overall, results from the synthetic control on individual states do not support a conclusion that the marginal cost of a parental consent law has large fertility effects for minors. To observe average effects across all treated units, I use the pooling analysis described in the previous section.

5.1 Pooling inference

Tables 5 and 6 describe the results from pooling. The average treatment effect here is the simple average of effects for the group in question – a kind of average of averages. The value for $\overline{p}$ comes from the simple average of percentile rank statistics within the group. The "p-value" comes from testing the hypothesis that the values for p within the group are n independent random draws from U[0,1] using the Irwin-Hall statistic.

Results of the pooling analysis are consistent with the observations made from the state-level treatment effects and percentile rank statistics. There is no evidence of a significant negative effect of the policy change among minors. Effects on the abortion rate for minors in Table 5 are different across early and late adopting states. For early adopting states, the average difference between the

Table 5 Pooling results for the abortion rate of minors (15–17)

	Average treatment effect	$\overline{p}$	p-value
Early States ($n = 4$)	0.103	0.553	0.637
Late States ($n = 2$)	−0.175	0.380	0.259
All States ($n = 6$)	0.010	0.495	0.483

Table 6 Pooling results for the birth rate of minors (15–17)

	Average treatment effect	$\overline{p}$	p-value
Early States ($n = 4$)	−0.488	0.563	0.662
Late States ($n = 2$)	−0.410	0.610	0.696
All States ($n = 6$)	−0.427	0.587	0.764

treated unit and the synthetic control group is equivalent to 0.103 additional abortions per 1000 AFAB[5] residents per year. For later states, the average difference is 0.175 fewer abortions per 1000 AFAB residents per year. Neither of these treatment effects, however, are statistically different from zero. For the birth rates in Table 6, effects across early and late states are similar in direction and magnitude.

6 Discussion

A straightforward interpretation of the results would suppose that there is no marginal effect of a parental consent law on fertility outcomes for minors because the additional cost of a parental consent law is small. In this sense, the barriers to abortion access are driven by broad parental involvement and not dependent on the specific nature of the PI law. I propose an additional potential mechanism behind these null effects where an institutional feature of parental involvement, the judicial bypass options, mitigates barriers to abortion access.

6.1 The judicial bypass

The judicial bypass option allows minors to petition the court at no financial cost for access to an abortion without meeting the parental involvement requirement. Joyce (2010) describes the relative importance of the judicial bypass option for minors seeking abortion care. In Arkansas, roughly 10% of minors who received an abortion di so using the judicial bypass. The statutory standards for a judicial bypass are fairly consistent across states. A judge may grant a minor access to an abortion without parental involvement if one of the following criteria are met:

1. The judge determines that the minor is mature enough to make their own reproductive choices.
2. The judge determines that the minor may be in immediate danger by seeking to satisfy the parental involvement requirement.

[5] AFAB = assigned female at birth.

3. The judge determines that the abortion would be in the best interest of the minor.

Note that the set of criteria is quite subjective. Particularly the first and third item, which require the judge presiding to use their own personal judgment to assess the case. The subjective nature of the judicial criteria, however, implies that the generosity of the judicial bypass may change in response to a more restrictive parental involvement law. Judges who believe that a law is too restrictive have the ability to grant additional judicial bypass waivers.

Data regarding the judicial bypass is difficult to come by. Generally, the records for such court proceedings are sealed by law. The best evidence to describe the generosity of the judicial bypass comes from state-level non-profit organizations that assist minors in seeking the option. One such organization is Jane's Due Process (JDP). Based on Texas, JDP collects their own data on the number of cases judicial bypass cases that they refer to an attorney, and how many of these cases result in a judicial bypass waiver (Fig. 5).

Though this is just observational data, a much smaller percentage of JDP judicial bypass cases were denied following the change from parental notification to parental consent in Texas in 2005. Additionally, the JDP was sending a larger number of judicial bypass cases to the courts after 2005. This evidence, though limited, demonstrates the plausibility that the judicial bypass option became more generous in Texas in response to the parental consent law.

If this trend in the generosity of the judicial bypass procedure exists broadly following parental consent legislation, it may mitigate additional barriers to abortion access imposed by the more stringent PI requirement. While some minors may be prevented from accessing an abortion due to the new parental consent law, other minors may benefit from the additional generosity of the judicial bypass. These effects together may help explain the null effects of the policy change on the abortion and birth rate for minors. Further research into the generosity of the judicial bypass

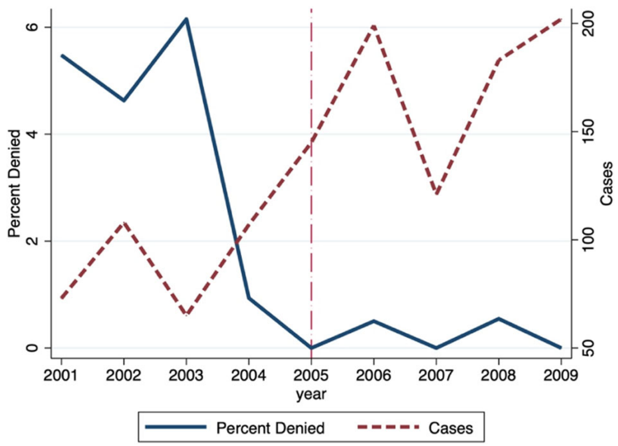


Fig. 5 JDP cases and denials in Texas, 2001–2009. Source: Stevenson et al. (2020)

across states and the nature of judicial bypass recipients is needed to confirm the presence of this treatment mechanism.

7 Conclusion

Overall, this research suggests that there is not evidence to support a differential effect between parental notification and parental consent laws on the abortion rate (and birth rate) for minors (15–17). The evidence supports a conclusion that legislative shifts from parental notification to parental consent are unlikely to be a primary driving force behind the wide variation in the abortion rate for minors across the United States.

This study also provides information regarding the external validity of the effect of parental consent in Arkansas presented in Joyce (2010). In this paper, I study the effect of a policy change from notification to consent in six states across the US South and Midwest, and I observe results consistent with Joyce's finding that there is no evidence of a substantial marginal effect of a parental consent law on the abortion rate for minors. I use an empirical methodology that does not rely on comparisons between minors and older teens (18-19), limiting the potential bias introduced due to the dynamic nature of fertility choice. In addition, I provide some descriptive evidence that the generosity of the judicial bypass procedure may be affected by strict parental involvement requirements. This may be an important mitigating factor in explaining the null effect of a parental consent law.

This finding may impact policymakers and reproductive health advocates in multiple ways. The most straightforward takeaway is that the impact of a parental consent law relative to a parental notification law is small, and efforts to reduce barriers to reproductive healthcare access among minors by shifting to a less-stringent PI law may not be very effective. However, it is worth noting that the null effects in this paper do not imply that no one is affected by parental consent laws, only that a policy change from parental notification to parental consent does not have a large enough fertility effect to be detectable in population averages. There is still an important consideration of who might be impacted by these laws. If it is truly the case that the judicial bypass procedure mitigates the burden of a consent law, then there should be substantial equity concerns within this consideration. Indeed, advocates are already aware that the presence of the judicial bypass option is a cause for concern, as this option may disproportionately benefit minors with the resources available to properly navigate a complex judicial system. Following a parental consent law, if only the most advantaged minors benefit from increased generosity of the judicial bypass, then a parental consent law clearly does impose an additional burden on minors even when the effects are not observable on average.

The primary limitation of this study is the quality of the abortion count data. Systematic changes in reporting behavior across states could potentially mask real effects of the policy change, resulting in a false null effect. To address this limitation, I provide complementary analysis of the effects of a shift from notification to consent on the birth rate for minors. I find consistent results that demonstrate a lack of evidence to support the conclusion that the policy change has strong effects for birth rates as well as abortion rates, but understanding birth effects does not entirely compensate for the limitations in estimating abortion effects. True effects on the abortion rate may be too small to cause significant birth effects, and changing contraceptive and sexual behavior among minors following the policy change may diminish upward pressure on the birth rate driven by restricted abortion access. Multiple initiatives currently exist to collect regular high-quality data on abortion counts across the US, including the "#WeCount Project" from the Society of Family Planning (Society of Family Planning, 2022). As more of this information becomes available, higher quality estimates of the effects of public policy on abortion rates becomes possible.

Author contributions G.G. prepared all elements of the manuscripts.

Compliance with ethical standards

Conflict of interest The author declares no competing interests.

8 Appendix A: Data sources

Demographics

Surveillance, Epidemiology, and End Results (SEER) Program Populations (1969–2018)

CDC abortion data

Koonin and Smith (1998), Koonin et al. (1999), (2000), Herndon et al. (2002), Elam-Evans et al. (2002), Elam-Evans et al. (2003), Strauss et al. (2004), Strauss et al. (2005), Strauss et al. (2006), Strauss et al. (2007), Gamble et al. (2008), Pazol et al. (2009), Pazol et al. (2011), Pazol et al. (2011), Pazol et al. (2012), Pazol et al. (2013), Pazol et al. (2014), Pazol et al. (2015), Jatlaoui et al. (2016), Jatlaoui et al. (2017), Jatlaoui et al. (2018), and Jatlaoui et al. (2019)

ITOP data

Arkansas Department of Health Statistics (2000–2016), Georgia Department of Public Health Online Analytical Statistical Information System (1995–2016), Iowa Department of Health (2005–2016), Minnesota Department of Health (2009–2016), South Dakota Department of Health (2008–2016), and Utah Office of Vital Records and Statistics (1998–2016)

9 Appendix B: Synthetic control details

Arkansas

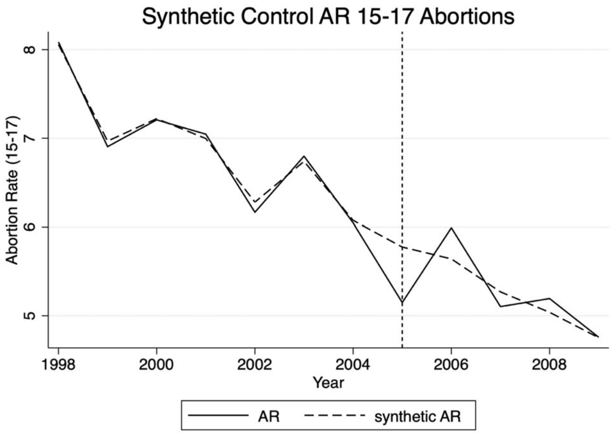


Table 7

Table 7 Arkansas – synthetic control group for abortion rate of minors

State	Weight
MI	0.146
NE	0.028
NM	0.085
OR	0.038
WI	0.704

Table 8

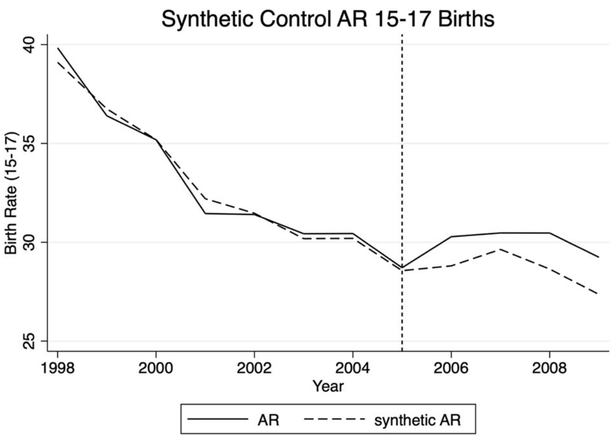


Table 8 Arkansas abortions – states excluded from donor pool

Excluded state	Reason for exclusion	Excluded state	Reason for exclusion
CA	Birth data only	MD	Birth data only
WY	Birth data only	AZ	Policy change in 2003
ID	Consent law enjoined 2005–2006	TX	Policy change in 2005
VA	Policy change in 2003	WV	Missing abortion data 2003, 2004
DE	Missing abortion data 1995, 1996, 2009	KY	Missing abortion data 1999, 2002
LA	Missing abortion data 2005, 2006, 2008, 2009	ND	Missing abortion data 2004, 2005
PA	Missing abortion data 2009	RI	Missing abortion data 2007
VT	Missing abortion data 2009	OH	Policy change in 2006
UT	Policy change in 2006	TN	Policy change in 2000
IA	Missing abortion data 1995–1999	CO	Policy change in 2003

Tables 9, 10

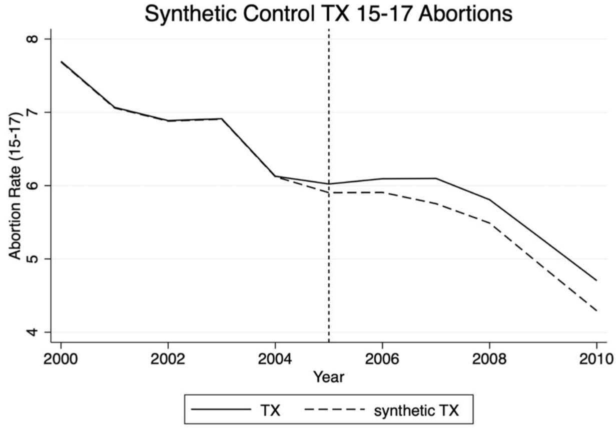
Table 9 Arkansas – synthetic control group for birth rate of minors

State	Weight
AL	0.478
CA	0.116
NM	0.352
WY	0.054

Table 10 Arkansas births – states excluded from donor pool

Excluded state	Reason for exclusion	Excluded state	Reason for exclusion
AZ	Policy change in 2003	ID	Consent law enjoined 2005, 2006
OH	Policy change in 2006	TN	Policy change in 2000
TX	Policy change in 2005	UT	Policy change in 2006
VA	Policy change in 2003	CO	Policy change in 2003

Texas



Tables 11, 12

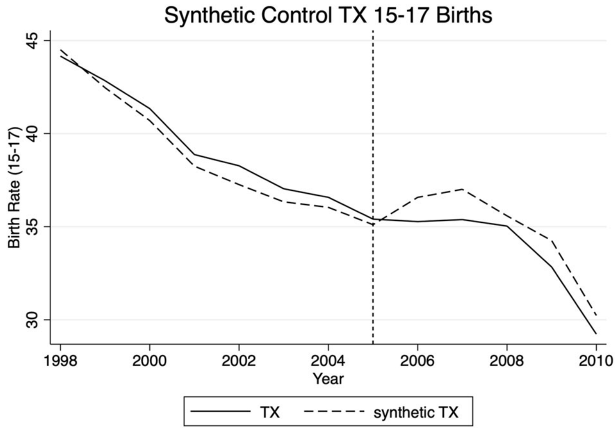


Table 11 Texas – synthetic control group for abortion rate of minors

State	Weight	State	Weight
AL	0.02	NC	0.019
GA	0.033	NE	0.028
IA	0.027	NJ	0.031
IL	0.018	NM	0.024
IN	0.025	NV	0.014
KS	0.018	NY	0.01
MA	0.088	OR	0.04
ME	0.019	SC	0.025
MI	0.037	SD	0.037
MN	0.025	TN	0.024
MO	0.051	WA	0.018
MS	0.32	WI	0.025
MT	0.023		

Table 12 Texas abortions – states excluded from donor pool

Excluded state	Reason for exclusion	Excluded state	Reason for exclusion
CA	Birth data only	MD	Birth data only
WY	Birth data only	AZ	Policy change in 2003
ID	Consent law enjoined 2005–2006	AR	Policy change in 2005
VA	Policy change in 2003	WV	Missing abortion data 2003, 2004
DE	Missing abortion data 1995, 1996, 2009	KY	Missing abortion data 1999, 2002
LA	Missing abortion data 2005, 2006, 2008, 2009	ND	Missing abortion data 2004, 2005
PA	Missing abortion data 2009	RI	Missing abortion data 2007
VT	Missing abortion data 2009	OH	Policy change in 2006
UT	Policy change in 2006	CO	Policy change in 2003

Tables 13, 14

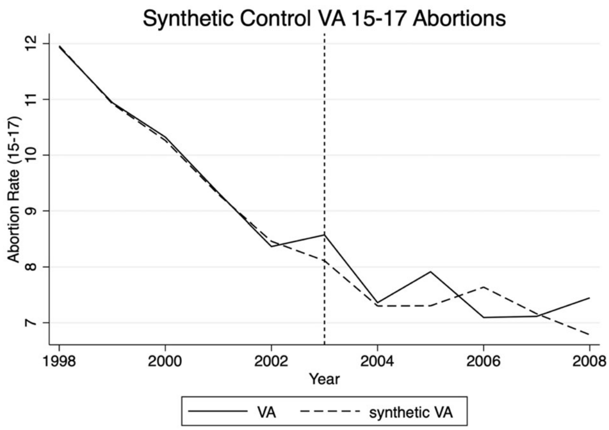
Table 13 Texas – synthetic control group for birth rate of minors

State	Weight
MS	0.319
NM	0.681

Table 14 Texas births – states excluded from donor pool

Excluded state	Reason for exclusion	Excluded state	Reason for exclusion
AZ	Policy change in 2003	ID	Consent law enjoined 2005, 2006
OH	Policy change in 2006	TN	Policy change in 2000
AR	Policy change in 2005	UT	Policy change in 2006
VA	Policy change in 2003	CO	Policy change in 2003

Virginia



Tables 15, 16

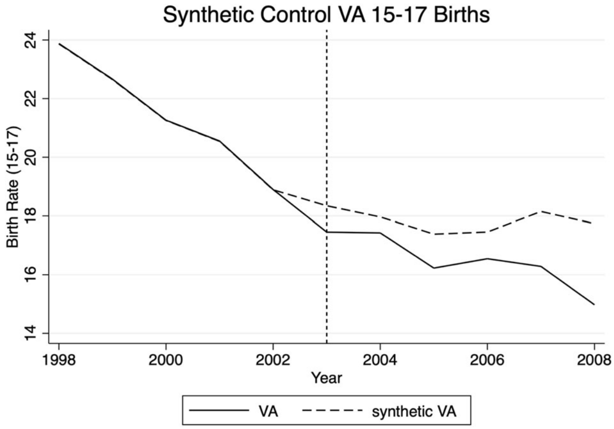


Table 15 Virginia – synthetic control group for abortion rate of minors

State	Weight
AL	0.484
MS	0.078
NE	0.029
OR	0.33
WI	0.079

Table 16 Virginia abortions – states excluded from donor pool

Excluded state	Reason for exclusion	Excluded state	Reason for exclusion
CA	Birth data only	MD	Birth data only
WY	Birth data only	AZ	Policy change in 2003
ID	Consent law enjoined 2005–2006	TX	Policy change in 2005
AR	Policy change in 2005	WV	Missing abortion data 2003, 2004
LA	Missing abortion data 2005, 2006, 2008, 2009	KY	Missing abortion data 1999, 2002
UT	Policy change in 2006	ND	Missing abortion data 2004, 2005
IA	Missing abortion data 1995–1999	RI	Missing abortion data 2007
TN	Policy change in 2000	OH	Policy change in 2006
		CO	Policy change in 2003

Tables 17, 18

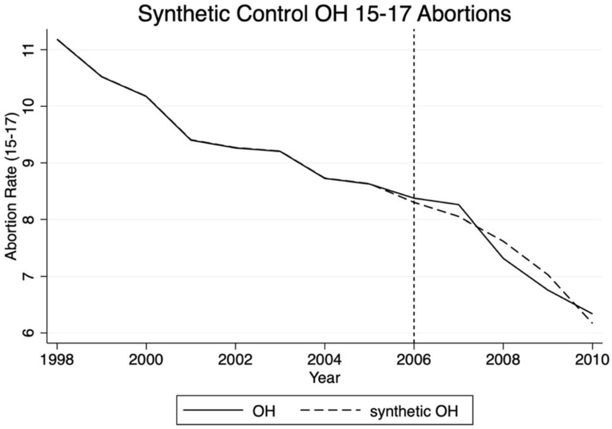
Table 17 Virginia – synthetic control group for birth rate of minors

State	Weight	State	Weight
AL	0.005	MT	0.004
CA	0.01	NC	0.007
DE	0.099	ND	0.023
GA	0.007	NE	0.172
IA	0.011	NJ	0.016
IL	0.01	NM	0.007
IN	0.008	NV	0.007
KS	0.04	NY	0.016
KY	0.007	OR	0.011
LA	0.007	PA	0.011
MA	0.02	RI	0.218
MD	0.009	SC	0.007
ME	0.012	SD	0.007
MI	0.011	VT	0.049
MN	0.011	WA	0.009
MO	0.007	WI	0.065
MS	0.004	WV	0.025
		WY	0.066

Table 18 Virginia births – states excluded from donor pool

Excluded state	Reason for exclusion	Excluded state	Reason for exclusion
AZ	Policy change in 2003	ID	Consent law enjoined 2005, 2006
OH	Policy change in 2006	TN	Policy change in 2000
AR	Policy change in 2005	UT	Policy change in 2006
TX	Policy change in 2005	CO	Policy change in 2003

Ohio



Tables 19, 20

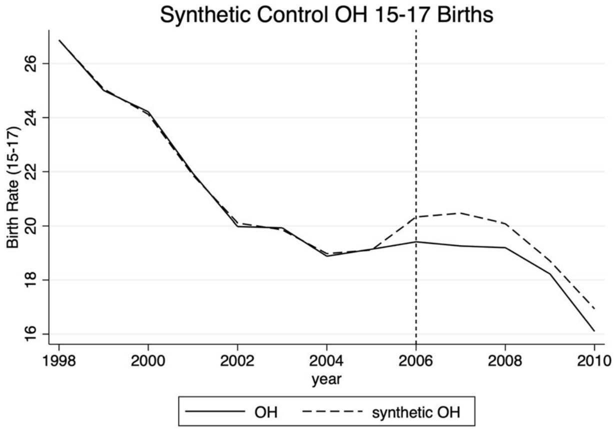


Table 19 Ohio – synthetic control group for abortion rate of minors

State	Weight	State	Weight
AL	0.007	NC	0.006
GA	0.065	NE	0.005
IL	0.007	NJ	0.022
IN	0.007	NM	0.189
KS	0.011	NV	0.017
MA	0.01	NY	0.118
ME	0.008	OR	0.005
MI	0.023	SC	0.006
MN	0.012	SD	0.037
MO	0.007	WA	0.009
MS	0.275	WI	0.015
MT	0.14		

Table 20 Ohio abortions – states excluded from donor pool

Excluded state	Reason for exclusion	Excluded state	Reason for exclusion
CA	Birth data only	MD	Birth data only
WY	Birth data only	AZ	Policy change in 2003
ID	Consent law enjoined 2005–2006	TX	Policy change in 2005
VA	Policy change in 2003	WV	Missing abortion data 2003, 2004
DE	Missing abortion data 1995, 1996, 2009	KY	Missing abortion data 1999, 2002
LA	Missing abortion data 2005, 2006, 2008, 2009	ND	Missing abortion data 2004, 2005
PA	Missing abortion data 2009	RI	Missing abortion data 2007
VT	Missing abortion data 2009	AR	Policy change in 2005
UT	Policy change in 2006	TN	Policy change in 2000
IA	Missing abortion data 1995–1999	CO	Policy change in 2003

Tables 21, 22

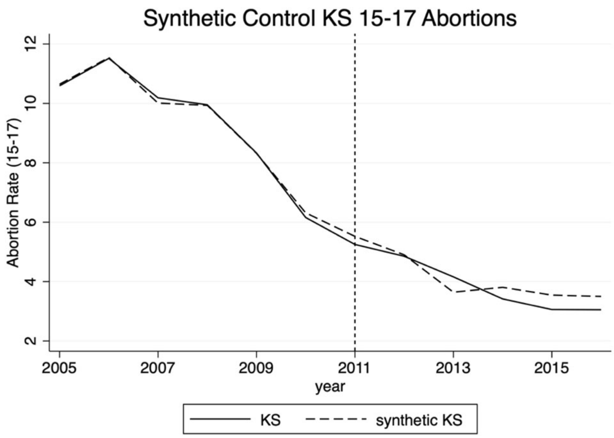
Table 21 Ohio – synthetic control group for birth rate of minors

State	Weight
ME	0.078
MI	0.112
MS	0.125
ND	0.204
OR	0.187
RI	0.04
SC	0.158
SD	0.096

Table 22 Ohio births – states excluded from donor pool

Excluded state	Reason for exclusion	Excluded state	Reason for exclusion
AZ	Policy change in 2003	ID	Consent law enjoined 2005, 2006
AR	Policy change in 2005	TN	Policy change in 2000
TX	Policy change in 2005	UT	Policy change in 2006
VA	Policy change in 2003	CO	Policy change in 2003

Kansas



Tables 23, 24

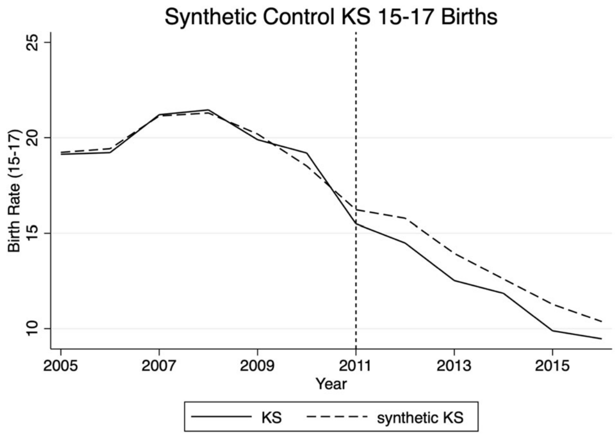


Table 23 Kansas – synthetic control group for abortion rate of minors

State	Weight
MN	0.249
NV	0.606
SC	0.013
WA	0.132

Table 24 Kansas abortions – states excluded from donor pool

Excluded state	Reason for exclusion	Excluded state	Reason for exclusion
CA	Birth data only	MD	Birth data only
WY	Birth data only	TX	Policy change in 2005
DE	Missing abortion data 1995, 1996, 2009	ND	Missing abortion data 2004, 2005
LA	Missing abortion data 2005, 2006, 2008, 2009	RI	Missing abortion data 2007
PA	Missing abortion data 2009	OH	Policy change in 2006
VT	Missing abortion data 2009	ID	Consent law enjoined 2005, 2006
UT	Policy change in 2006	MA	Missing abortion data 2014–2016
NE	Policy change in 2011	ME	Missing abortion data 2011, 2012

Tables 25, 26

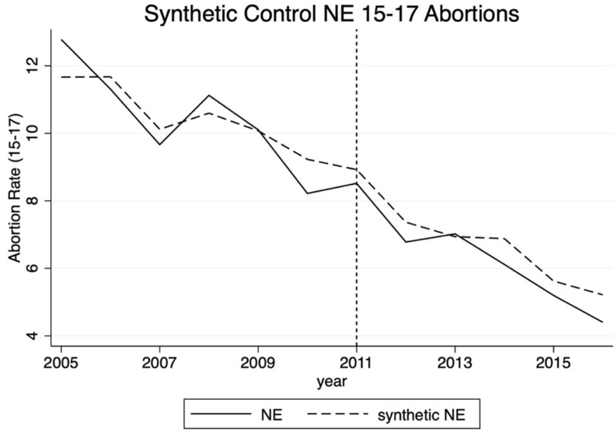
Table 25 Kansas – synthetic control group for birth rate of minors

State	Weight
MS	0.073
ND	0.333
NM	0.054
WV	0.243
WY	0.296

Table 26 Kansas births – states excluded from donor pool

Excluded state	Reason for exclusion	Excluded state	Reason for exclusion
ID	Consent law enjoined 2005, 2006	NE	Policy change in 2011
OH	Policy change in 2006	UT	Policy change in 2006

Nebraska



Tables 27, 28

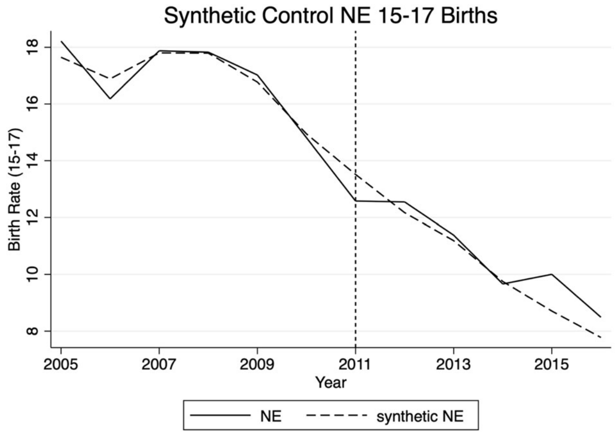


Table 27 Nebraska – synthetic control group for abortion rate of minors

State	Weight
KY	0.025
MS	0.157
MT	0.128
WI	0.427
WV	0.263

Table 28 Nebraska abortions – states excluded from donor pool

Excluded state	Reason for exclusion	Excluded state	Reason for exclusion
CA	Birth data only	MD	Birth data only
WY	Birth data only	TX	Policy change in 2005
DE	Missing abortion data 1995, 1996, 2009	ND	Missing abortion data 2004, 2005
LA	Missing abortion data 2005, 2006, 2008, 2009	RI	Missing abortion data 2007
PA	Missing abortion data 2009	OH	Policy change in 2006
VT	Missing abortion data 2009	ID	Consent law enjoined 2005, 2006
UT	Policy change in 2006	MA	Missing abortion data 2014–2016
KS	Policy change in 2011	ME	Missing abortion data 2011, 2012

Tables 29, 30

Table 29 Nebraska – synthetic control group for birth rate of minors

State	Weight
KY	0.025
MS	0.157
MT	0.128
WI	0.427
WV	0.263

Table 30 Nebraska births – states excluded from donor pool

Excluded state	Reason for exclusion	Excluded state	Reason for exclusion
ID	Consent law enjoined 2005, 2006	KS	Policy change in 2011
OH	Policy change in 2006	UT	Policy change in 2006

10 Appendix C: Alternative specifications

Figures 6, 7

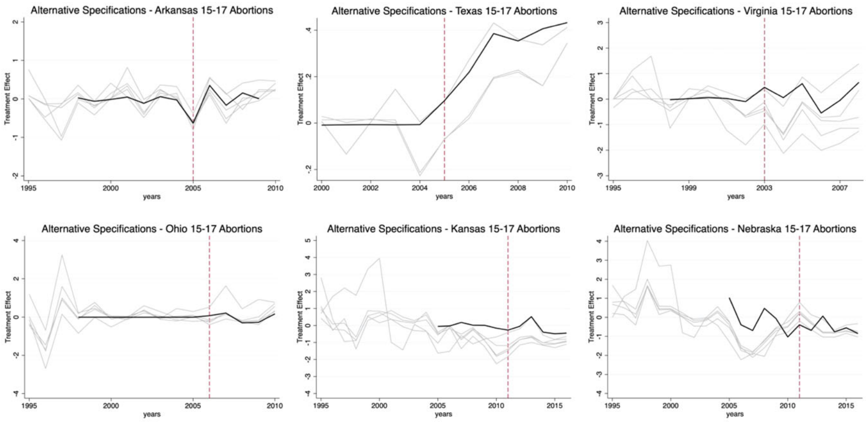


Fig. 6 Alternative specifications - abortions

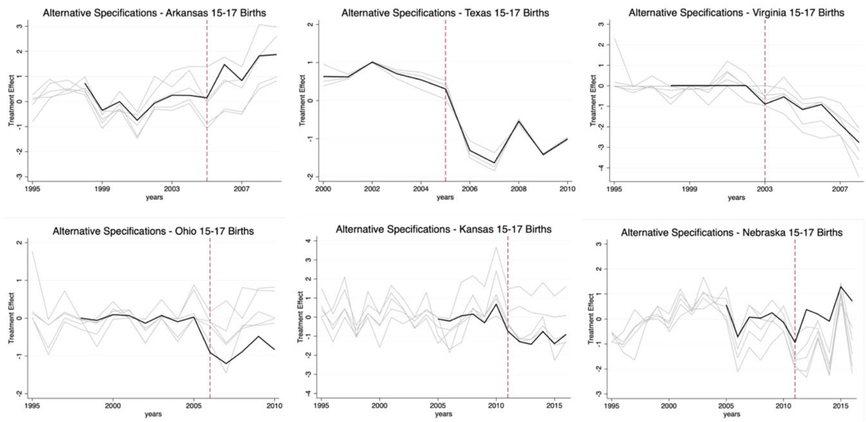


Fig. 7 Alternative Specifications - births

One potential concern with the synthetic control method is the opportunity for researchers to present results that are the product of "specification searching," described in Ferman et al. (2020). The synthetic controls results are often sensitive to the selection of the matching criteria used to generate the synthetic control group. The primary analysis in the paper uses only pre-treatment outcomes within a limited analysis window to generate a control group to improve the pre-treatment fit. In this section, I present treatment effects for five alternative common matching criteria and compare these treatment effects to the primary specification. The five alternative matching criteria are:

1. All pre-treatment outcomes
2. Even pre-treatment outcomes
3. Odd pre-treatment outcomes
4. The first, middle, and last pre-treatment outcomes
5. The first 75% of pre-treatment outcomes

Figures 6 and 7 plot the treatment effects from the primary specification using the limited analysis window (represented by a bold black line) alongside the treatment effects from the five alternative specifications (gray lines) for the 15–17 abortion rate and the 15–17 birth rate, respectively. This comparison demonstrates that treatment effects in the primary specification generally lie within the distribution of treatment effects from the alternative specifications, and the main results of the paper are not a product of specification searching to achieve a particular result.

11 Appendix D: Heterogeneity in birth effects by age

Tables 31, 32

Table 31 Synthetic control estimates of the birth rate of minors by single age

15 year-olds			16 year-olds			17 year-olds		
State	Treatment effect	p	State	Treatment effect	p	State	Treatment effect	p
AR	0.91	0.083	AR	2.90	0.043	AR	−0.007	0.375
VA	−0.867	0.897	VA	−2.21	0.926	VA	−2.164	0.842
TX	−0.548	0.833	TX	−0.381	0.636	TX	−0.743	0.438
OH	0.042	0.435	OH	0.241	0.381	OH	−0.229	0.583
KS	−0.053	0.517	KS	−0.858	0.862	KS	−1.169	0.80
NE	0.094	0.345	NE	0.589	0.276	NE	0.351	0.32

Table 32 Pooling results of the birth rate of minors by single age

	Average treatment effect	$\overline{p}$	p-value
15 year-olds			
Early States ($n = 4$)	−0.116	0.562	0.660
Last States ($n = 2$)	0.021	0.431	0.371
All States ($n = 6$)	−0.070	0.518	0.560
16 year-olds			
Early States ($n = 4$)	0.138	0.497	0.490
Last States ($n = 2$)	−0.135	0.569	0.628
All States ($n = 6$)	0.047	0.521	0.568
17 year-olds			
Early States ($n = 4$)	−0.786	0.560	0.654
Last States ($n = 2$)	−0.205	0.560	0.613
All States ($n = 6$)	−0.660	0.560	0.689

We might expect that effects from parental involvement laws are heterogeneous across maternal age. It could be the case that younger people are affected more than 17 year-olds by these restrictive policies, but the effect on the average hides these effects because they are aggregated to minors 15–17. Here, I provide an analysis of the birth effects from the policy change from parental notification to parental consent using single-year birth rates for 15, 16, and 17 year-olds.

The synthetic control and pooling inference procedure is equivalent to that described in Section 4, but the outcome of interest (and matching criteria) are single-year birth rates. Table 31 provides a description of the average treatment effect and percentile rank statistic for each treated state across maternal age. There is no discernable pattern of differential treatment effects across maternal age. For VA and KS, treatment effects are the largest among 15 year-olds, but the estimates are still negative. For AR, TX, OH, and NE, treatment effects are largest among 16 year-olds.

Table 32 provides the results of the pooling inference procedure by maternal age. From this analysis, there is not enough evidence to conclude that a policy change from notification to consent has a significant effect on the birth rate for any of these age groups.

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