Notification vs 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 of 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, 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 (weakly) increase the marginal cost of an abortion. Using these

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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 and 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 (include PI laws) increase the marginal cost of the insurance policy (Kane and Staiger, 1996). Some research suggests that restricted access to abortion has long term negative consequences for women. In a current working paper, Miller, Wherry, and Foster (2020) 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.

Using sibling fixed effects, Johansen, Nielson, and Verner (2019) show that those in Denmark who give birth under the age of 21 obtain fewer years of schooling, experience a lower employment rate, and have lower earnings at age 35. The authors make special note that these effects exist within a robust welfare state in Denmark, and would likely be exacerbated in nations with fewer support programs for young parents, such as the United States.

For teens specifically, Maynard and Hoffman (2008) show that motherhood is associated with negative educational, financial, and health outcomes for both the mother and child compared to peers who are not parents. In their book *Kids having kids: Economic costs and social consequences of teenage pregnancy*, Maynard and Hoffman also report that teen births cost taxpayers between $9.4 and $28 billion every year through public assistance, foster care, and criminal justice services. The potential consequences for the teen mother, the child, and the state motivate a discussion surrounding any policies that could be exacerbating these issues by restricting teen access to abortion.

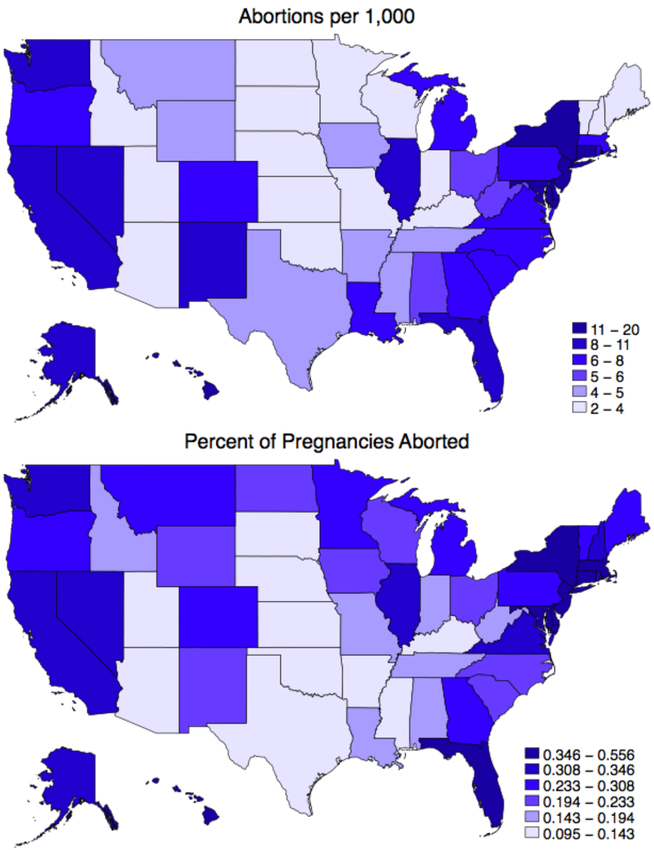
2 Background

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 aborted 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.

Figure 1: Abortion Rate and Percent of Pregnancies Aborted, 2013



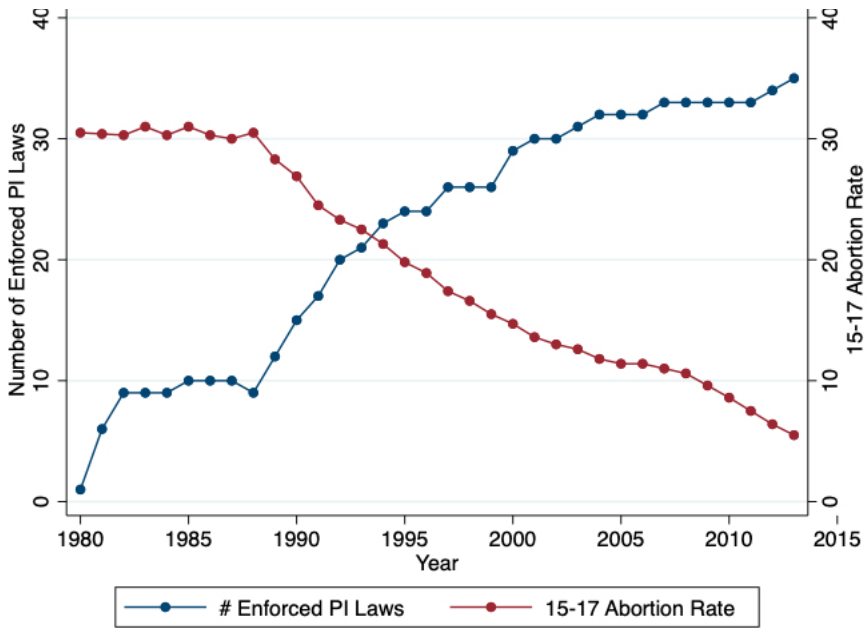
Source: Kost et al. (2017)

2.2 Parental Involvement Laws

Utah passed the first parental involvement law in 1974. Since then, their prevalence has grown tremendously. 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.

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

Figure 2: PI Laws and the 15-17 Abortion Rate Over Time (1980-2013)



Source: Kost et al. (2017); Myers and Ladd (2020)

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 for older teens is independent of the abortion rate for minors, and the abortion rate for older teens acts as a control for overall statewide trends in the abortion rate. Using a linear regression with controls for abortion price proxies and abortion attitude proxies, they find that parental involvement laws reduce the 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 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 the state of *residence* until the mid-2000s. This limitation is particularly important in light of evidence that teens do travel out-of-state to have an abortion when they are facing parental involvement law restrictions (Cartoof and Klerman, 1986; Joyce and Kaestner, 1996).

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, 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., 2006a; Colman et al., 2008). In 2015, MacAfee et al. (2015) studied the New Hampshire notification law and reported 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 suggests 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 difference 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 consent in Arkansas by Joyce (2010). This paper considers the effect of a policy change from parental notification to consent in seven states spanning the US South, Midwest, and West. 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 effects 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.

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. 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 supplement 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 1,000 residents assigned female at birth in age category).

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. 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 age, and therefore may be better suited to measuring the fertility effects of a parental consent law.

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	Consent: 2003-2016
Kansas	Notification: 1995-2010	Colorado	Notification: 2003-2016
	Consent: 2011-2016	Georgia	Notification: 1995-2016
Nebraska	Notification: 1995-2010	Iowa	Notification: 1995-2016
	Consent: 2011-2016	Illinois	Notification: 1995-2016
Ohio	Notification: 1995-2005	Indiana	Notification: 1995-2016
	Consent: 2006-2016	Kentucky	Consent: 1995-2016
Texas	Notification: 2000-2004	Massachusetts	Consent: 1995-2016
	Consent: 2005-2016	Maine	No Law: 1995-2016
Utah	Notification: 1995-2005	Michigan	Consent: 1995-2016
	Consent: 2006-2016	Minnesota	Notification: 1995-2016
Virginia	Notification: 1995-2002	Missouri	Consent: 1995-2016
	Consent: 2003-2016	Mississippi	Consent: 1995-2016
		Montana	No Law: 1995-2016
		North Carolina	Consent: 1996-2016
		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

Source: Myers and Ladd (2020)

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 an 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. Then, the observed outcome for unit i at time t may be written as

$$Y_{it} = Y_{it}^{N} + \alpha_{it} D_{it}$$

where D_{it} is an indicator for the policy intervention. Since the counterfactual outcome Y_{it}^{N} is never observed when $D_{it} = 1$, suppose that it can be represented by a factor model

$$Y_{it}^{N} = \delta_t + \theta_t Z_i + \lambda_t \mu_i + \epsilon_{it}.$$

Here, δ_t is an unknown common factor, Z_i is an observed set of covariates, θ_t is a vector of unknown parameters, λ_t is a set of unobserved common factors, and μ_i is an unknown vector of factor loadings. The $\lambda_t \mu_i$ term separates synthetic control from the usual difference-in-differences. While difference-in-differences assumes that unobserved confounders are constant across time, this method does not. So, synthetic control allows for unobserved *time-varying* confounders to exist.

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 controls are minimized, only considering the pre-intervention period. Following the work of Klößner and Pfeifer (2018), I use only lagged dependent variables in order to construct the weights,

$$W_1 = \text{argmin}_{w_j^1 \epsilon [0,1]} \sum_{t=t_0-5}^{t_o-1} (Y_{1t} - \sum_{j=2}^{J+1} w_j^1 Y_{jt})^2,$$

where unit 1 is the treated unit and five 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)

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

Figures 3a and 3b show the visual results from the synthetic control for the six treated states for both the 15-17 abortion rate and birth rate.

Figure 3a: Synthetic Control for the Abortion Rate of Minors (15-17)

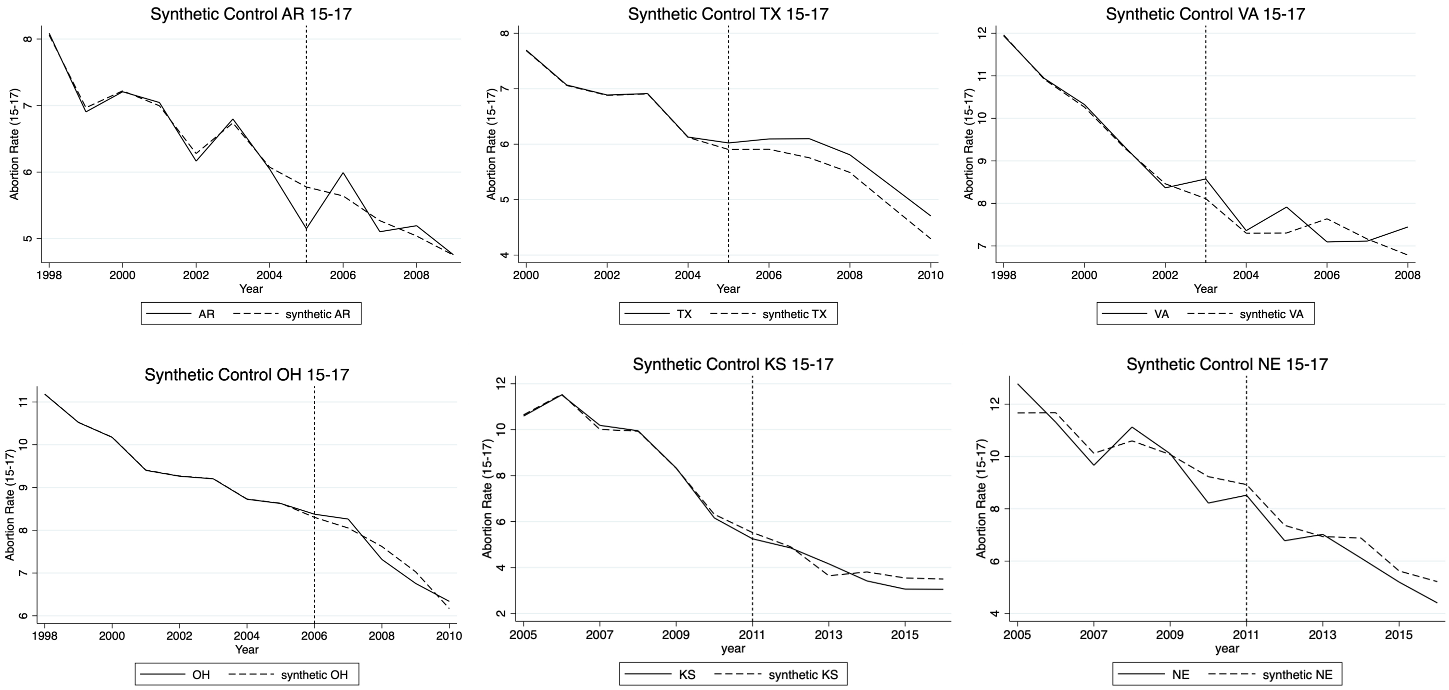
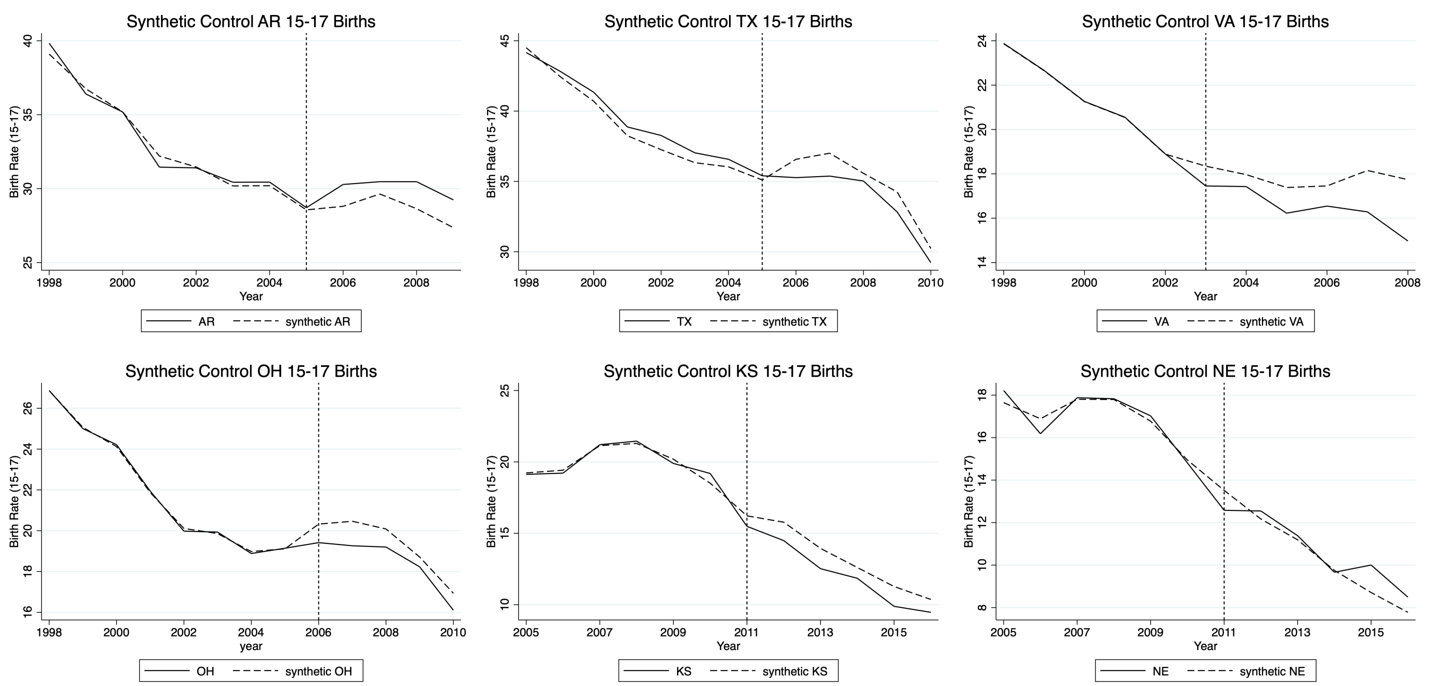


Figure 3b: Synthetic Control for the Birth Rate of Minors (15-17)



Figures 3a and 3b provide 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 Figure 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, and 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 of 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. Figures 4a and 4b present 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, it is eliminated from the graph and 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.

Figure 4a: Permutation Tests for the Abortion Rate of Minors (15-17)

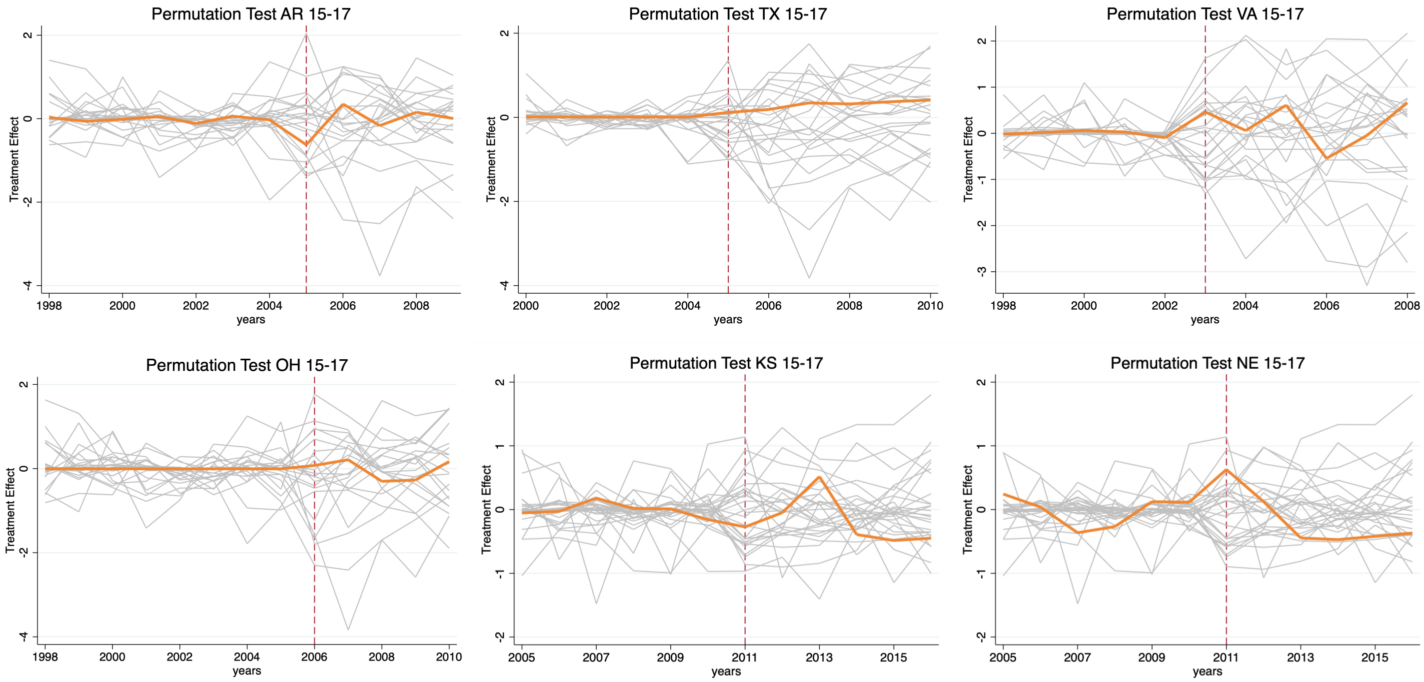
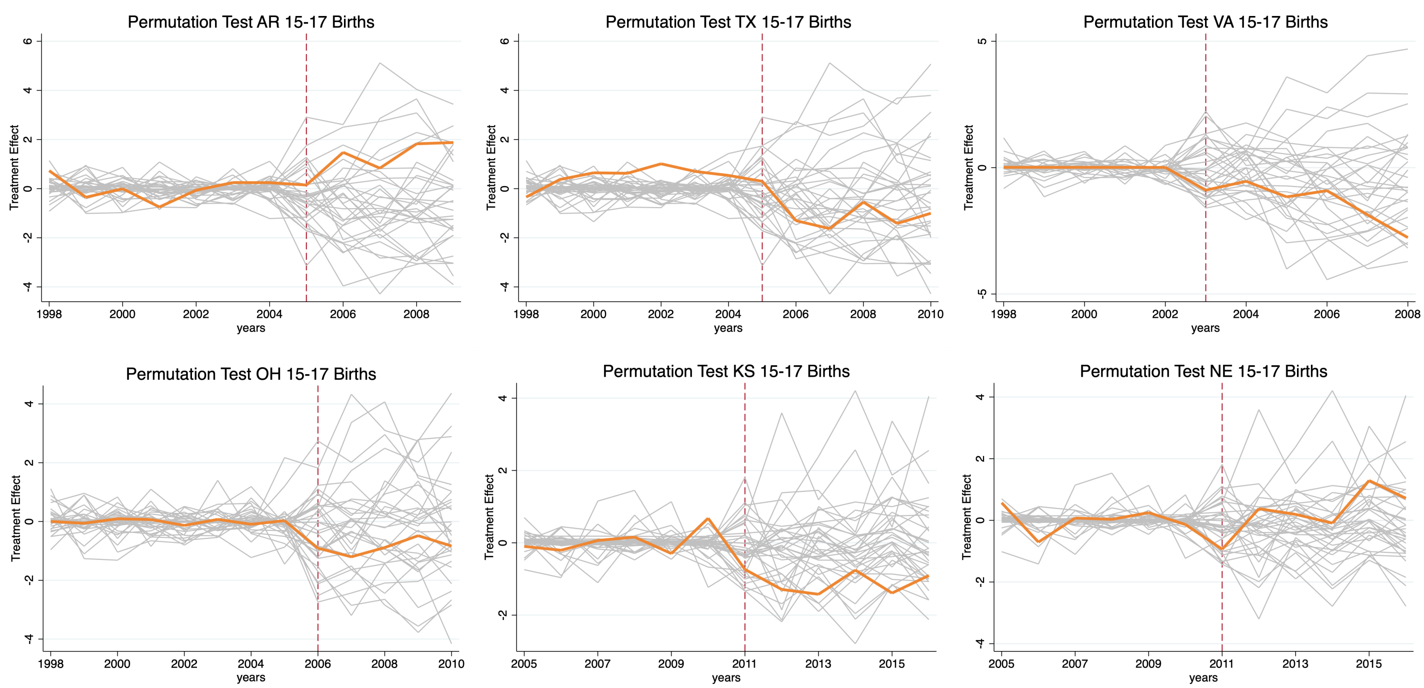


Figure 4b: Permutation Tests for the Birth Rate of Minors (15-17)



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 in regression analysis. I calculate the percentile rank statistic based upon the average treatment effect in the post-period $\bar{\alpha}_1 = \frac{1}{s}\sum_{t=t_0}^{t_0+s} \alpha_{1t}$. The percentile rank statistic will be $p_1 = \hat{F}(\bar{\alpha}_1)$, where $\hat{F}$ is the empirical CDF of the average placebo effects $\bar{\alpha}_j$ from the control group[1]. 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 Figure 4a ($p = 0.5$). 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 Figure 4b ($p = 0.16$). A full summary of treatment effects and percentile rank statistics is presented in the Results section in Tables 3 and 4.

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 (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 also consistent with this observation.

Tables 2 and 3 report the average treatment effect and percentile ranks 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 alternate hypotheses 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).

[1] 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.

Table 2: Treatment Effect for Abortion Rate of Minors (15-17)

	Treatment Effect	Pre-Period Average	p
Early States:			
Arkansas	-0.06	7.46	0.41
Texas	0.29	9.36	0.63
Virginia	0.20	11.83	0.67
Ohio	-0.02	10.09	0.50
Late States:			
Kansas	-0.19	13.21	0.39
Nebraska	-0.16	6.89	0.37

Table 3: Treatment Effect for Birth Rate of Minors (15-17)

	Treatment Effect	Pre-Period Average	p
Early States:			
Arkansas	1.50	36.70	0.16
Texas	-0.93	42.31	0.64
Virginia	-1.45	23.81	0.74
Ohio	-0.86	24.24	0.66
Late States:			
Kansas	-1.08	22.53	0.91
Nebraska	0.26	18.75	0.31

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 of 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 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 4 and 5 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 $\bar{p}$ comes from the simple average of percentile ranks 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.

Table 4: Pooling Results for the Abortion Rate of Minors (15-17)

	Average Treatment Effect	$\bar{p}$	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 5: Pooling Results for the Birth Rate of Minors (18-19)

	Average Treatment Effect	$\bar{p}$	p-value
Early States (n=4)	-0.435	0.550	0.631
Late States (n=2)	-0.410	0.610	0.696
All States (n=6)	-0.427	0.570	0.719

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 4 are different across early and late adopting states. For early adopting states, average differences between the treated unit and its synthetic control group is equivalent to 0.103 additional abortions per 1,000 AFAB[2] residents per year. For later states, average differences are 0.175 fewer abortions per 1,000 AFAB residents per year. Neither of these treatment effects, however, are statistically different from zero. For the birth rates in Table 5, effects across early and late states are nearly indistinguishable.

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 parental consent 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 option, mitigates barriers to abortion access.

[2] AFAB = assigned female at birth

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 juidical bypass option for minors seeking abortion care. In Arkansas, roughly 10% of minors who received an abortion did 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.

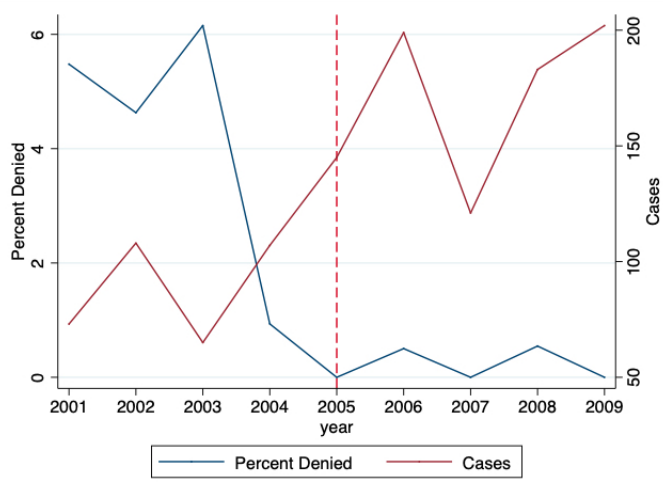
2. The judge determines that the minor may be in immediate danger by seeking to satisfy the parental involvement requirement.

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

Note that this set of criteria is quite subjective. Particularly the first and third item, which require the judge presiding to use their 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 in 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.

Figure 7: JDP Cases and Denials 2001-2009



Source: Stevenson et al. (2020)

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 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 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 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.

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 (SFP, 2022). As more of this information becomes available, higher quality estimates of the effects of public policy on abortion rates become possible.

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Appendix A: Data Sources

Demographics

Surveillance, Epidemiology, and End Results (SEER) Program Populations (1969-2018) (www.seer.cancer.gov/popdata), National Cancer Institute, DCCPS, Surveillance Research Program, released December 2019.

CDC Abortion Data

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Strauss LT, Gamble SB, Parker WY et al. Abortion Surveillance – United States 2003. MMWR Surveillance Summ 2006;55(SS11):1-32

Strauss LT, Gamble SB, Parker WY et al. Abortion Surveillance – United States 2004. MMWR Surveillance Summ 2007;56(SS09):1-33

Gamble SB, Strauss LT, Parker WY et al. Abortion Surveillance – United States 2005. MMWR Surveillance Summ 2008;57(SS13):1-32

Pazol K, Gamble SB, Parker WY et al. Abortion Surveillance – United States 2006. MMWR Surveillance Summ 2009;58(SS08):1-35

Pazol K, Zane SB, Parker WY et al. Abortion Surveillance – United States 2007. MMWR Surveillance Summ 2011;60(SS01):1-39

Pazol K, Zane SB, Parker WY et al. Abortion Surveillance – United States 2008. MMWR Surveillance Summ 2011;60(SS15):1-41

Pazol K, Creanga AA, Zane SB et al. Abortion Surveillance – United States 2009. MMWR Surveillance Summ 2012;61(SS08):1-44

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Surveillance Summ 2013;62(SS08):1-44

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Jatlaoui TC, Eckhaus L, Mandel MG et al. Abortion Surveillance – United States 2016. MMWR Surveillance Summ 2019;68(SS11):1-41

ITOP Data

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Appendix B: Synthetic Control Details

Arkansas

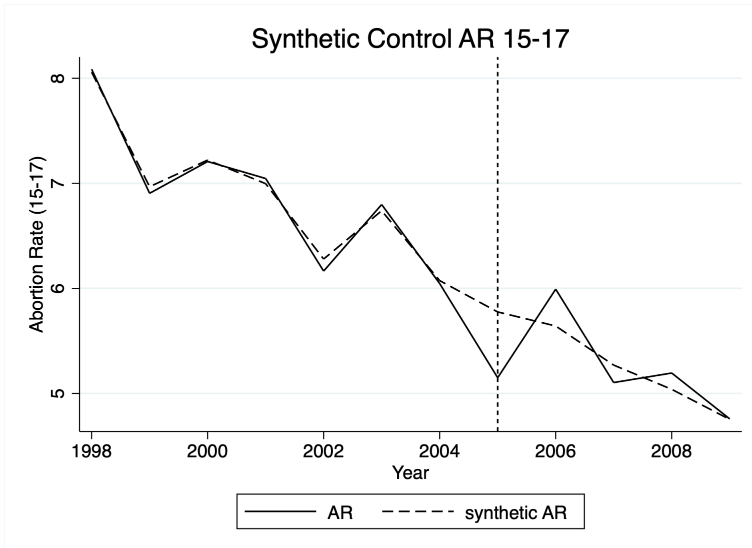

Table B1: Arkansas - Synthetic Control Group for Abortion Rate of Minors

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

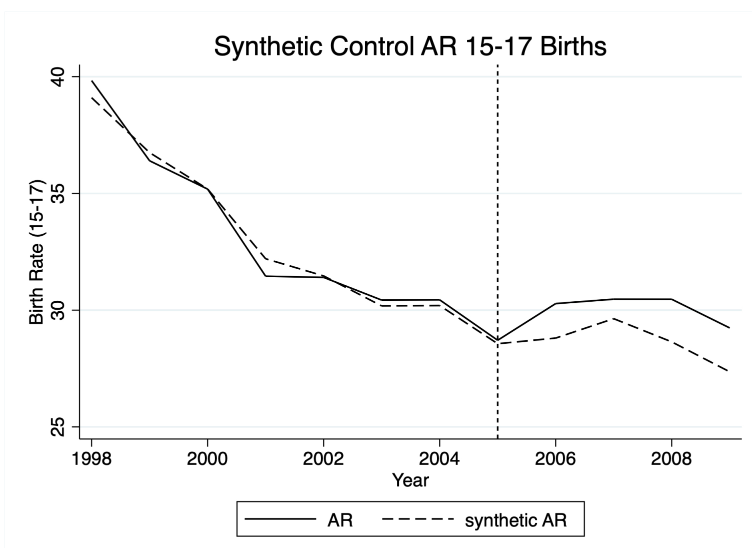

Table B2: Arkansas - Synthetic Control Group for Birth Rate of Minors

State	Weight
AL	0.478
CA	0.116
NM	0.352
WY	0.054

Texas

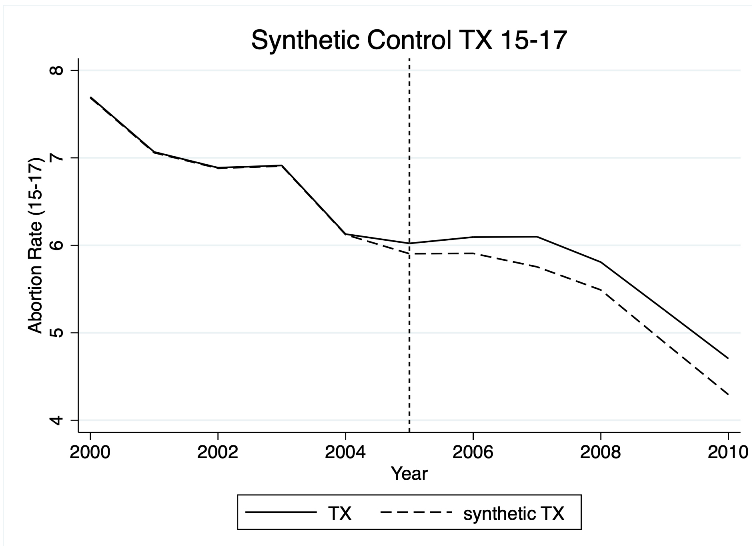

Table B3: Texas - Synthetic Control Group for Abortion Rate of Minors

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		

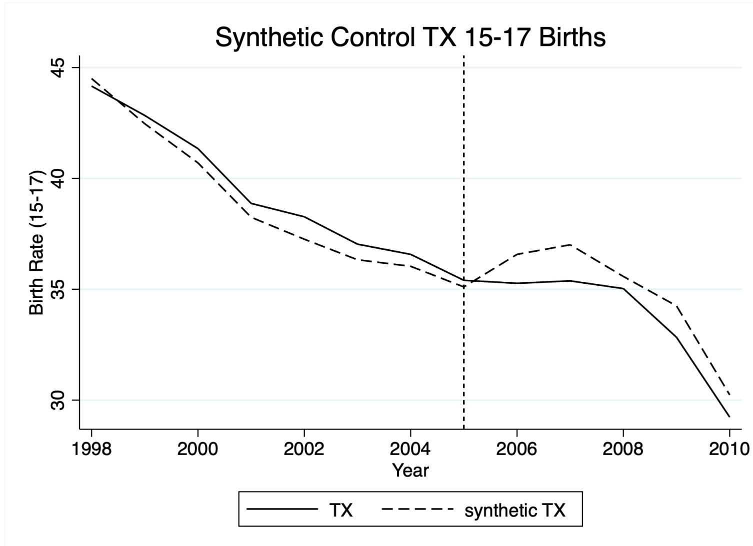

Table B4: Texas - Synthetic Control Group for Birth Rate of Minors

State	Weight
MS	0.319
NM	0.681

Virginia

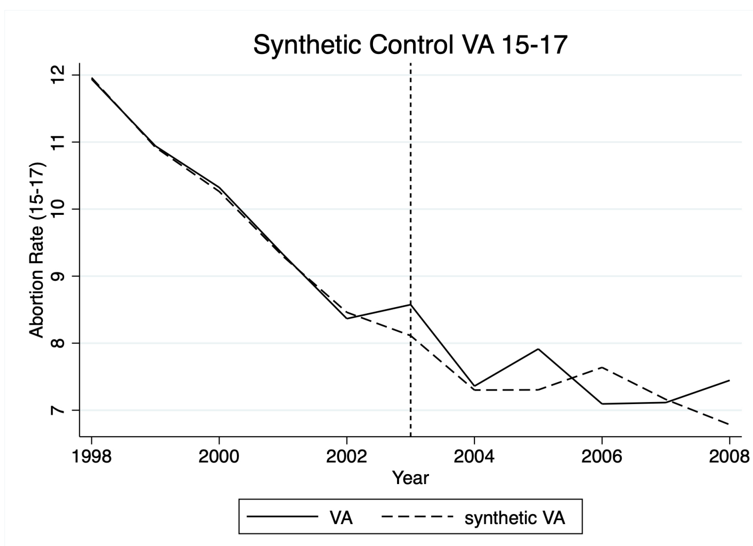

Table B5: Virginia - Synthetic Control Group for Abortion Rate of Minors

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

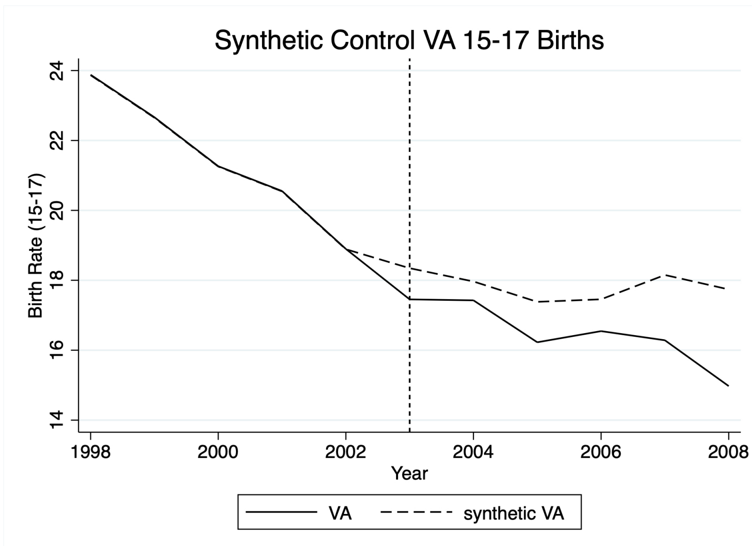

Table B6: Virginia - Synthetic Control Group for Birth Rate of Minors

State	Weight	State	Weight
AL	0.005	MT	0.005
CA	0.01	NC	0.007
CO	0.005	ND	0.021
DE	0.111	NE	0.154
GA	0.007	NJ	0.016
IL	0.01	NM	0.007
IN	0.008	NV	0.007
KS	0.06	NY	0.016
KY	0.007	OR	0.011
LA	0.007	PA	0.011
MA	0.019	RI	0.236
MD	0.009	SC	0.006
ME	0.013	SD	0.009
MI	0.011	VT	0.066
MN	0.011	WA	0.009
MO	0.007	WI	0.046
MS	0.004	WY	0.068

Ohio

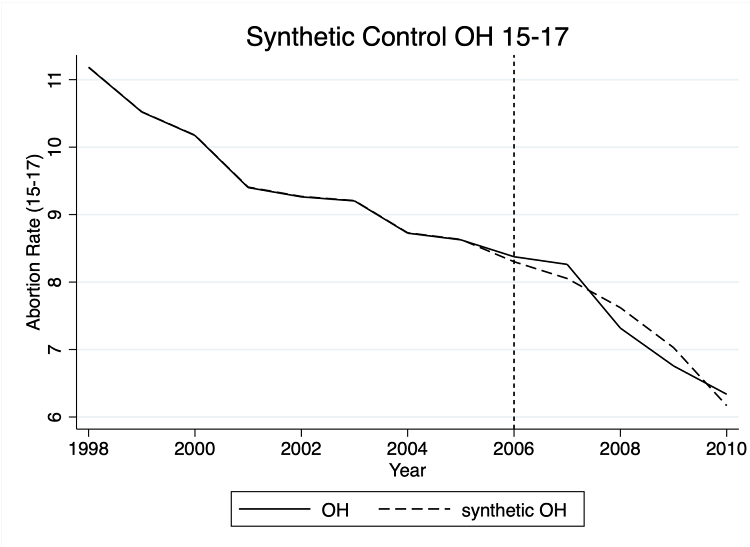

Table B7: Ohio - Synthetic Control Group for Abortion Rate of Minors

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		

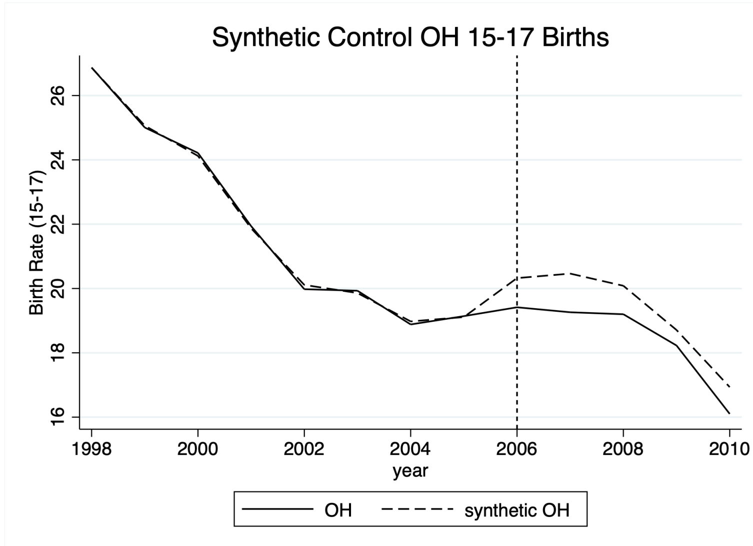

Table B8: Ohio - Synthetic Control Group for Birth Rate of Minors

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

Kansas

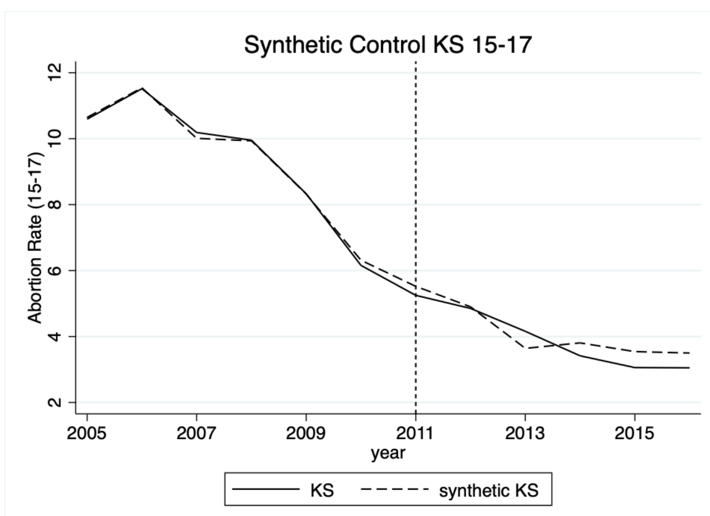

Table B9: Kansas - Synthetic Control Group for Abortion Rate of Minors

State	Weight
MN	0.249
NV	0.606
SC	0.013
WA	0.132

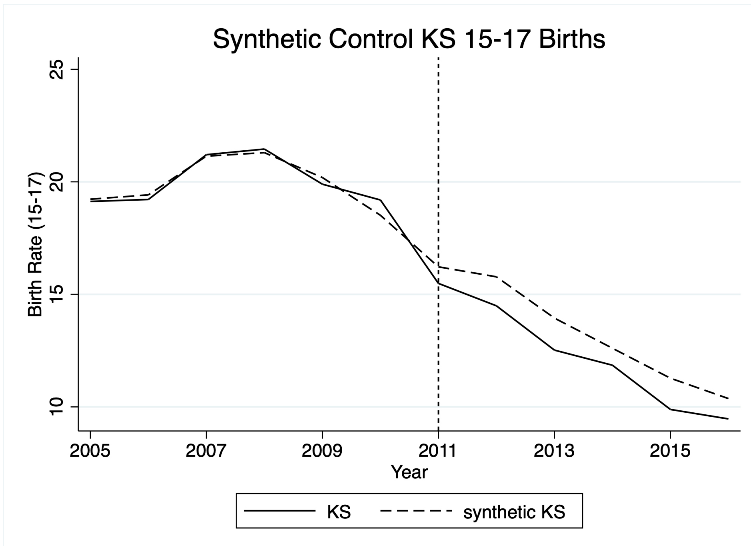

Table B10: Kansas - Synthetic Control Group for Birth Rate of Minors

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

Nebraska

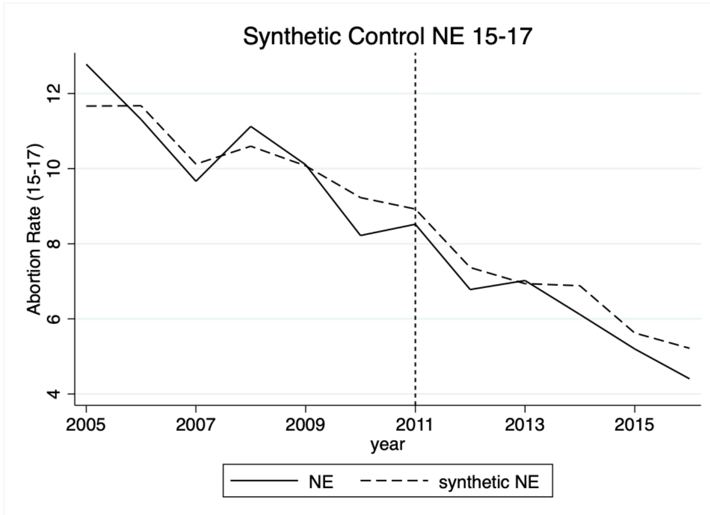

Table B11: Nebraska - Synthetic Control Group for Abortion Rate of Minors

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

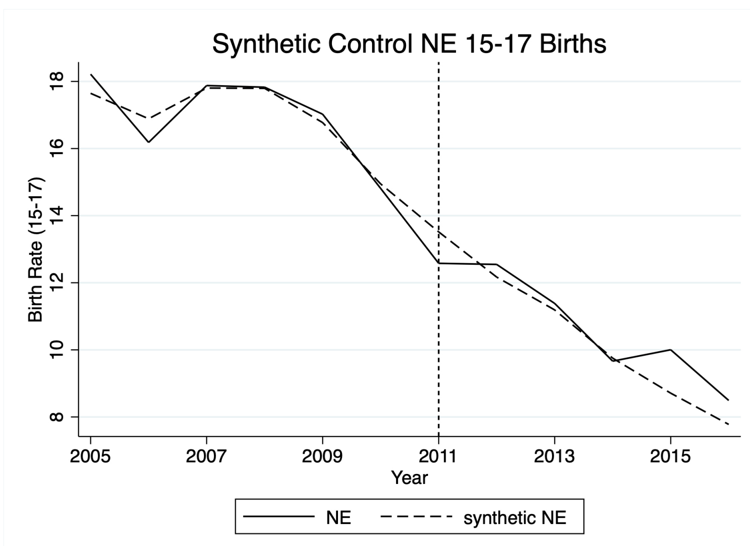

Table B12: Nebraska - Synthetic Control Group for Birth Rate of Minors

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