

Notification vs Consent: The Differential Effects of Parental Involvement Laws on Teen Abortion

Graham Gardner

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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, 37 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, little is known about 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 reduces the abortion rate for minors relative to parental notification. Additionally, this paper is the first to consider the potential for spill-over effects of this policy change on older teens (18-19). Results indicate no evidence of a marginal effect of parental consent laws on the abortion rate of minors. For older teens, however, results suggest that the policy change increases the abortion rate by about 12%.

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. So, I test the hypothesis that, relative to parental notification, a parental consent law will reduce the abortion rate for minors (15-17).

But, fertility choice is also a dynamic process, and the dynamic models of fertility include the fact that fertility decisions in any period are dependent on past fertility choices (Arroyo and Zhang, 1997). This implies that parental involvement legislation could influence the abortion choices of age groups beyond minors. I consider the potential for these spill-over

effects in my analysis as well. In particular, I estimate the differential effects of a parental consent law on the abortion rate for older teens (18-19).

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

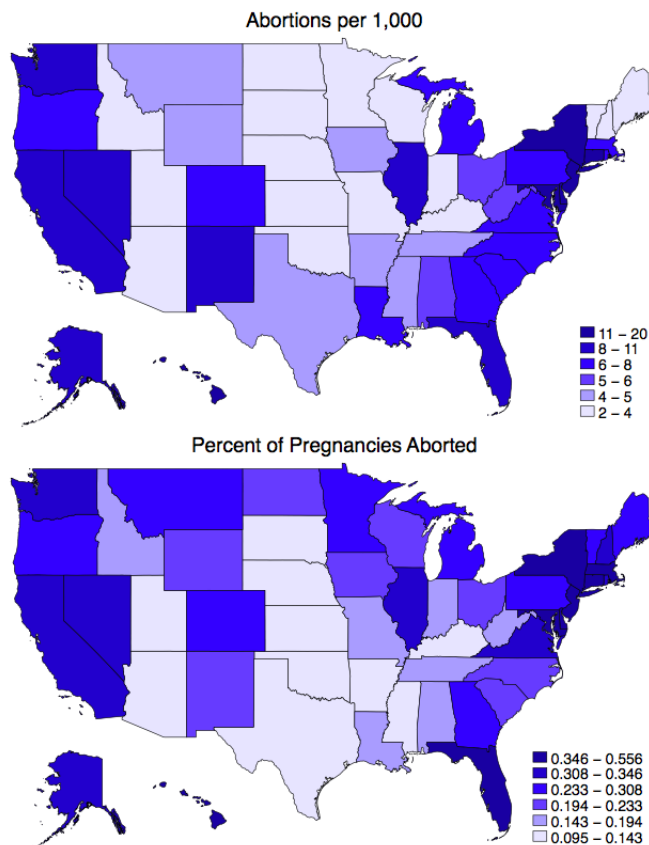
Non-trivial variation across states in their teen abortion rates provides another motivation for studying topics related to teenage abortion. Figure 1 uses data from a Guttmacher Institute report detailing the pregnancy rate and abortion rate for 15-17 year-olds in all 50 states in 2013.

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.

2.2 Parental Involvement Laws

Utah passed the first parental involvement law in 1974. Since then, their prevalence has grown tremendously. As of December 2020, 37 states have a PI law in place. Of these states, 21 require only parental consent, 10 require only parental notification, and 6 require

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



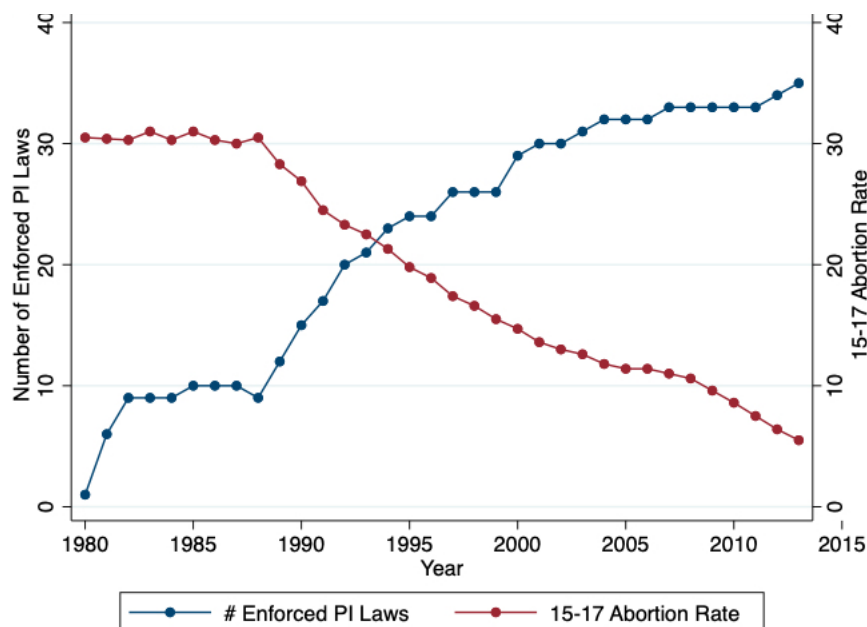
Source: Kost et al. (2017)

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.

A broad literature estimates the effects of parental involvement laws. 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 for older teens is independent of the abortion rate for minors, and the abortion rate for older

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



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

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 in two ways. First, I provide a new empirical exploration of any differential effects of parental consent laws on the abortion rate for minors. 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. Secondly, I provide the first estimates of spillover effects of these parental involvement policies on older teens. In this way, the paper has implications for the literature on parental involvement laws defined broadly, which has not considered empirically the potential for older teens to be treated by these policies.

Table 1: Summary of Enforced Parental Involvement Laws 1980 - 2017

State	Years	State	Years
Alabama	1987 - present	Montana	
Alaska	2010 - present	Nebraska	1981-1983; 1991 - present
Arizona	1982 - 1987; 2003 - present	Nevada	
Arkansas	1989 - present	New Hampshire	2012 - present
California		New Jersey	
Colorado	2003 - Present	New Mexico	
Connecticut		New York	
Delaware		North Carolina	1985 - present
D.C.		North Dakota	1981 - present
Florida	2005 - present	Ohio	1990 - present
Georgia	1991 - present	Oklahoma	2001-2002; 2004 - present
Hawaii		Oregon	
Idaho	2000-2004; 2007 - present	Pennsylvania	1994 - present
Illinois	2013 - present	Rhode Island	1982 - present
Indiana	1982 - present	South Carolina	1990 - present
Iowa	1997 - present	South Dakota	1997 - present
Kansas	1992 - present	Tennessee	1992-1996; 2000 - present
Kentucky	1989; 1994 - present	Texas	2000 - present
Louisiana	1981 - present	Utah	1980 - present
Maine		Vermont	
Maryland		Virginia	1997 - present
Massachusetts	1981 - present	Washington	
Michigan	1991 - present	West Virginia	1984 - present
Minnesota	1981-1986; 1990 - present	Wisconsin	1992 - present
Mississippi	1993 - present	Wyoming	1989 - present
Missouri	1985 - present		

Source: Myers (2017)

3 Data

To determine the legislative history of a state, I use the legal coding developed by Myers (2017). Table 1 summarizes the legal coding. 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 notification law serve as the control. Table 2 provides a description of the treatment and control group.

Data on state-level abortion rates comes largely from the induced termination of pregnancy (ITOP) portion of the state’s vital statistics report. I supplement ITOP data with the CDC abortion surveillance summaries when ITOP data does not report the age categories (15-17, 18-19) 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). I also collect data on birth rates (per 1,000 AFAB residents) from the CDC Wonder database.

Table 2: List of Treatment and Control States

Treatment		Control	
State	Law	State	Law
Arkansas	Notif: 1989-2004 Cons: 2005-2017	Georgia	Notif: 1991-2017
Texas	Notif: 2000-2004 Cons: 2005-2017	Colorado	Notif: 2003-2017
Virginia	Notif: 1997-2002 Cons: 2003-2017	Iowa	Notif: 1997-2017
Utah	Notif: 1980-2010 Cons: 2011-2017	Ohio	Notif: 1990-2017
Nebraska	Notif: 1991-2009 Cons: 2010-2017	Minnesota	Notif: 1990-2017
		South Dakota	Notif: 1997-2017
		West Virginia	Notif: 1984-2017

Source: Myers (2017)

Historically, abortion data was very limited. Researchers often ran into issues such as voluntary reporting, evidence of misreporting, and no reporting at all. Fortunately, at the same time that parental involvement laws were gaining popularity, so were state-level mandatory abortion reporting requirements. Many states actually have a reporting requirement written into their PI law specifically. All states in my sample have an abortion reporting requirement during the sample years, which mandate that abortion providers report various information about the services they perform to the state (Saul, 1998). Consequences for failing to abide by the reporting requirement vary, but generally include a disciplinary hearing by a health department or medical association.

I collect state-level economic and demographic information from the Federal Reserve Economic Data (FRED). These variables include median household income, the unemployment rate, the percent of residents receiving SNAP benefits, the homeownership rate, and

the percent of residents who have a Bachelor’s degree or higher. I also collect racial demographic measures from the Kaiser Family Foundation reports, which utilize Census data.

Table 3 compares treatment and control states. Differences along most of these measures are very small, with the exception of racial composition. Treatment states in my sample are much less White and more Hispanic.

Table 3: Economic and Demographic Features of Sample

	Treatment	Control
Median Household Income	60,314.61	59,770.82
Unemployment Rate	4.72	5.13
Homeownership Rate (%)	68.75	70.99
SNAP Benefits (%)	9.19	10.03
Bachelor’s Degree (%)	28.14	27.62
White (%)	68.98	79.25
Black (%)	10.13	8.32
Hispanic (%)	15.09	6.64
Abortion Rate (Minors)	5.66	5.72
Abortion Rate (Older Teens)	16.34	15.37
Birth Rate (Minors)	22.44	19.31

Source: US Census Bureau, Bureau of Labor Statistics, Kaiser Family Foundation, CDC Abortion Surveillance Summaries, SEER, CDC Wonder.

Notes: Values presented are averages across the maximum years of data available between 1995 and 2016. Data on Median Household Income, Unemployment, Homeownership, and the Abortion Rate are averaged over 1995-2016. Data on SNAP recipients are averaged over 1997-2016. Data on Bachelor’s Degree recipients are averaged over 2006-2016. Data on Racial Demographics are averaged over 2008-2016.

4 Methods

4.1 Event Study

To provide a visual description of the relationship between abortion rates in my treatment and control group over time, I use the following event study specification:

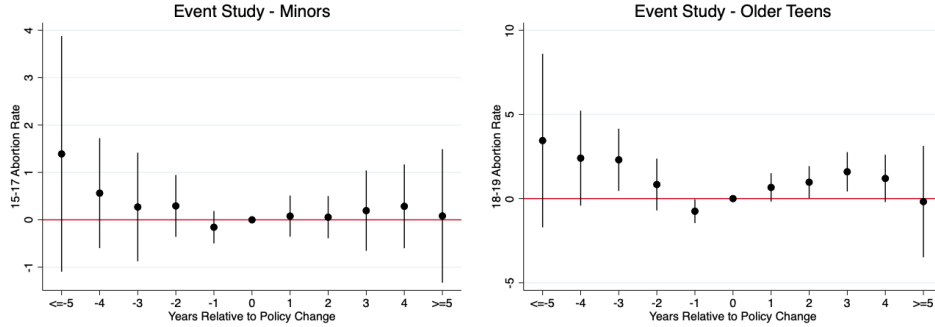
$$Y_{it} = \alpha_i + \delta_t + \sum_{k=-5}^5 \gamma_k 1(t - \tau_i = k) + \epsilon_{it}.$$

Here, Y_{it} is the outcome of interest (the abortion rates for minors and older teens), τ_i represents the year of the policy change from parental notification to parental consent in state i , and coefficients γ_k measure the average abortion rate for minors and older teens k years relative to the policy change. Values of $k < 0$ correspond to pre-trends. The year of policy change $k = 0$ is excluded, and time periods of more than 5 years outside policy change are collapsed into values “ ≤ -5 ” and “ ≥ 5 ”.

Because the institution of a parental consent statute increases the marginal cost of an abortion, I expect the sign of γ_k to be negative for minors in the post-period. It is unclear ex-ante how effects of this policy change may spill over to older teens. It could be the case that restricted access to abortion among minors incentivizes better contraceptive habits that carry over to early adulthood, resulting in fewer pregnancies among older teens, and reducing the abortion rate in this population. However, policies that induce birth effects among minors could also put upward pressure on the abortion rate for older teens. If the policy change results in more births among minors, then this fundamentally changes the set of constraints for those minors should they become pregnant again as an older teen. The social, economic, and health effects of a previous birth may incentivize the older teen to have an abortion.

Figure 3 plots OLS estimates of γ_k and the associated 90 percent confidence intervals. Estimates in the figure provide evidence of the reliability of the parallel trends assumption imposed in a standard difference-in-differences design. The parallel trends assumption states that there should be no relationship between outcomes in the treatment and control group before the policy intervention. Under this assumption, the OLS estimates for γ_k should be roughly zero in the pre-period, which does not appear to be the case for minors or older teens. The lack of evidence to support the parallel trends assumption, along with my small sample size, lead me to rely on the synthetic control method, rather than standard difference-in-differences, as my primary empirical strategy.

Figure 3: Event Study for Minors and Older Teens



Notes: Data on the abortion rate for minors and older teens are imputed using raw abortion counts from the CDC Abortion Surveillance Summaries and population estimates from SEER.

4.2 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. 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,

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

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 4a and 4b show the visual results from the synthetic control for the five treated states for both the 15-17 abortion rate and the 18-19 abortion rate.

Figure 4a: Synthetic Control for the Abortion Rate of Minors (15-17)

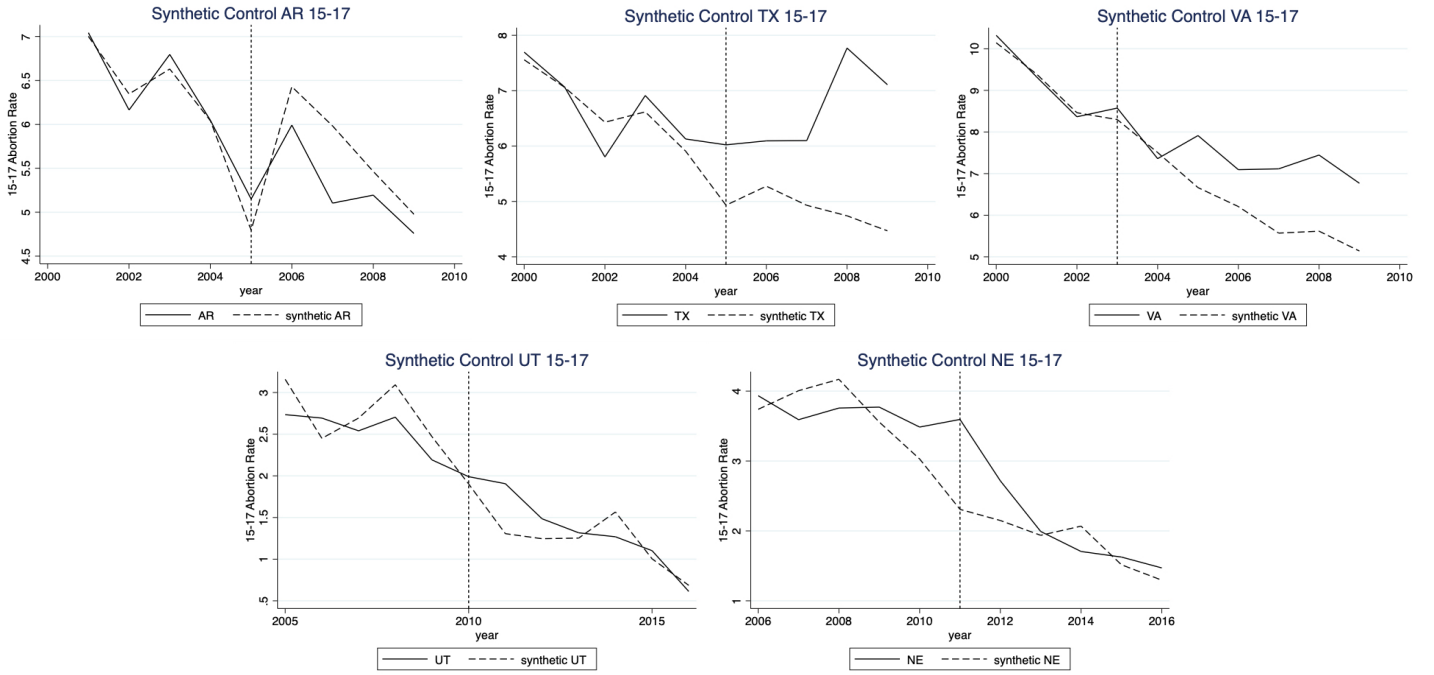
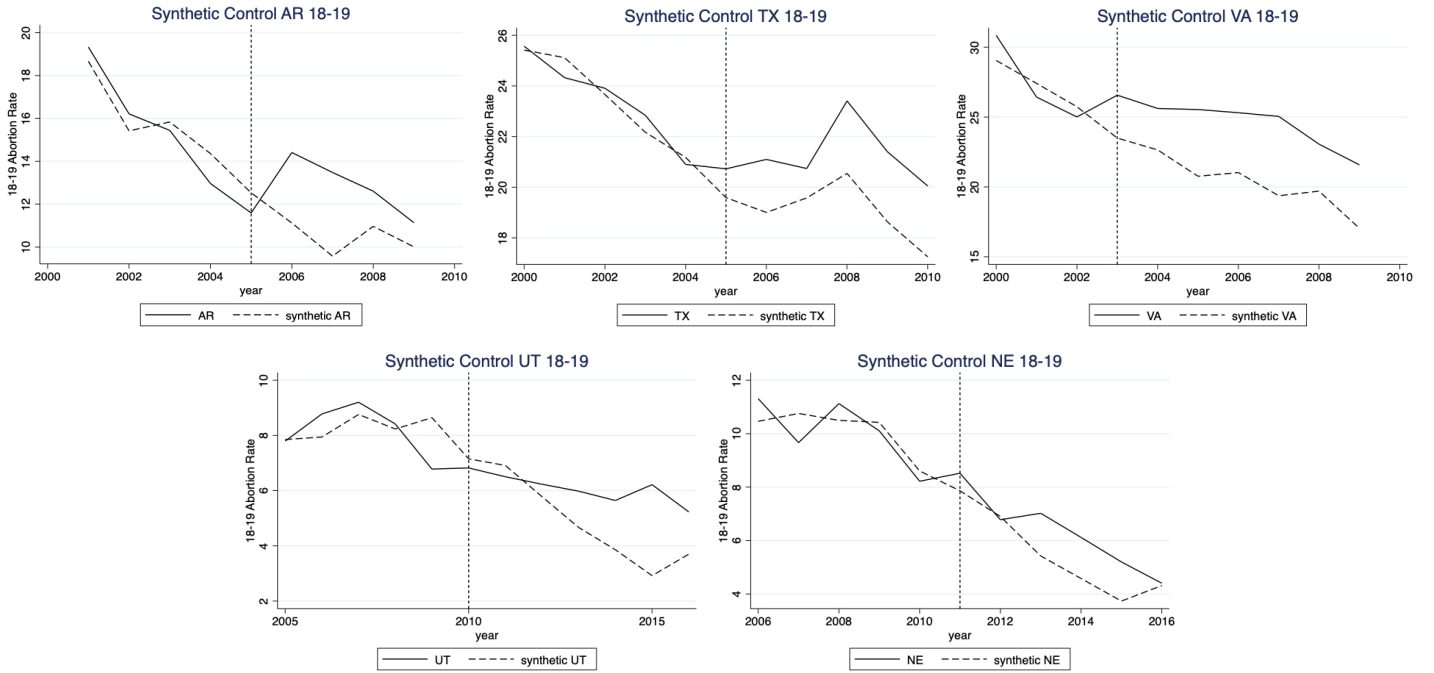


Figure 4b: Synthetic Control for the Abortion Rate of Older Teens (18-19)



Figures 4a and 4b provide information about the quality of the synthetic control match and the general direction of the treatment effects. In the pre-period, the abortion 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 $\hat{\alpha}_{it}$. Among older teens (Figure 4b), these treatment effects are consistently positive. In each treated state, the post-treatment trend in older teen abortion sits above its synthetic control group. This descriptive evidence motivates an alternate hypothesis in my analysis: the marginal effect of a parental consent law is an increase in the abortion rate for older teens. Among minors (Figure 4a), the direction of the treatment effect is not as clear. In Arkansas, the treatment effect is negative. But, in Virginia and Texas, the treatment effect is positive - the opposite of what would be expected from restrictive abortion legislation. Treatment effects in Utah and Nebraska appear close to zero. A formal description of these treatment effect estimates is presented in the Results section.

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 Kansas qualifies as a treated state, because they changed their parental notification law to a parental consent law in 2011, the abortion rate for minors in the pre-period (the characteristics I am using to match) does not sit in the convex hull of the abortion rate for minors in the control states. For this reason, I exclude Kansas from the analysis.

For proper identification, synthetic control assumes that there are no unobserved confounders in the post-period that do not appear in the pre-period. Although the lack of any confounders can't be determined with certainty, I explore how reasonable this assumption is in my context by looking for post-treatment changes in abortion access, contraceptive access, and sex education programs. I did not find anything that would lead me to eliminate any of the treatment or control states from my analysis.

4.3 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 5A and 5B present the placebo tests for the abortion rate of minors and older teens. These graphs present the difference between the abortion 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 5a: Permutation Tests for the Abortion Rate of Minors (15-17)

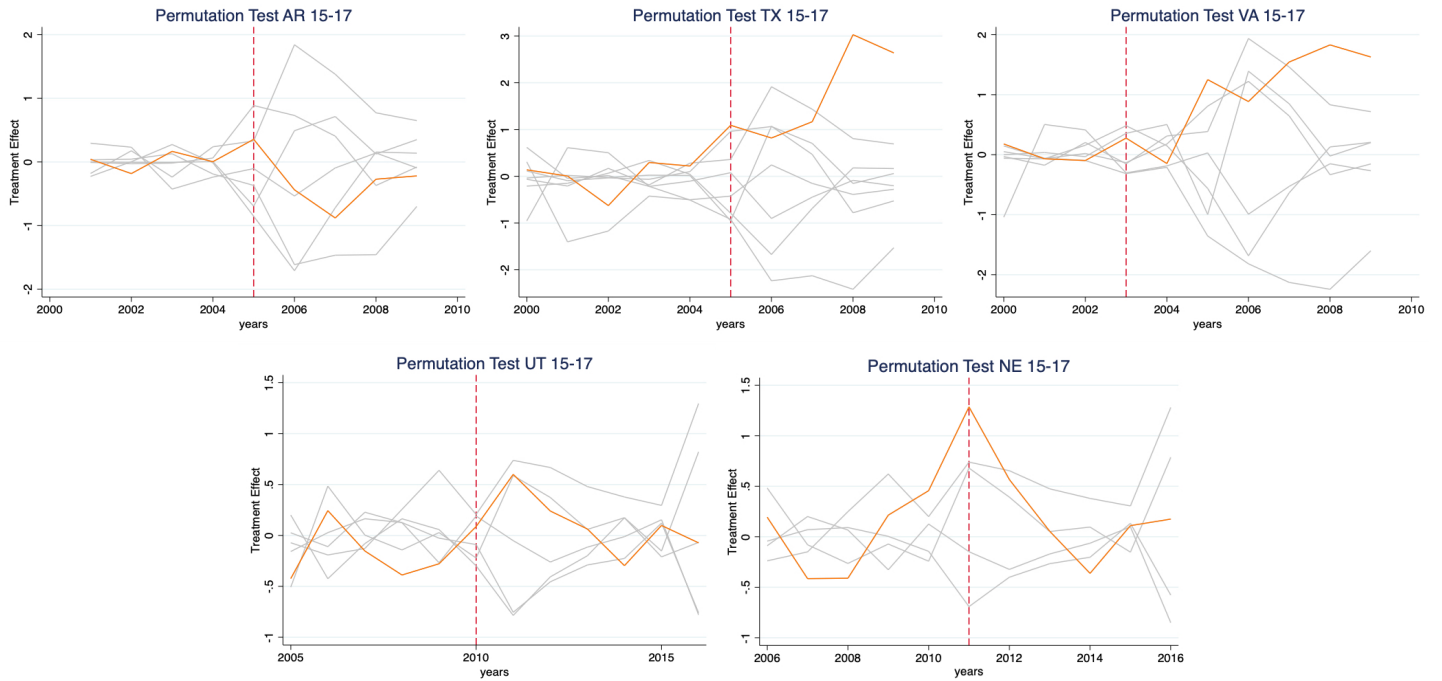
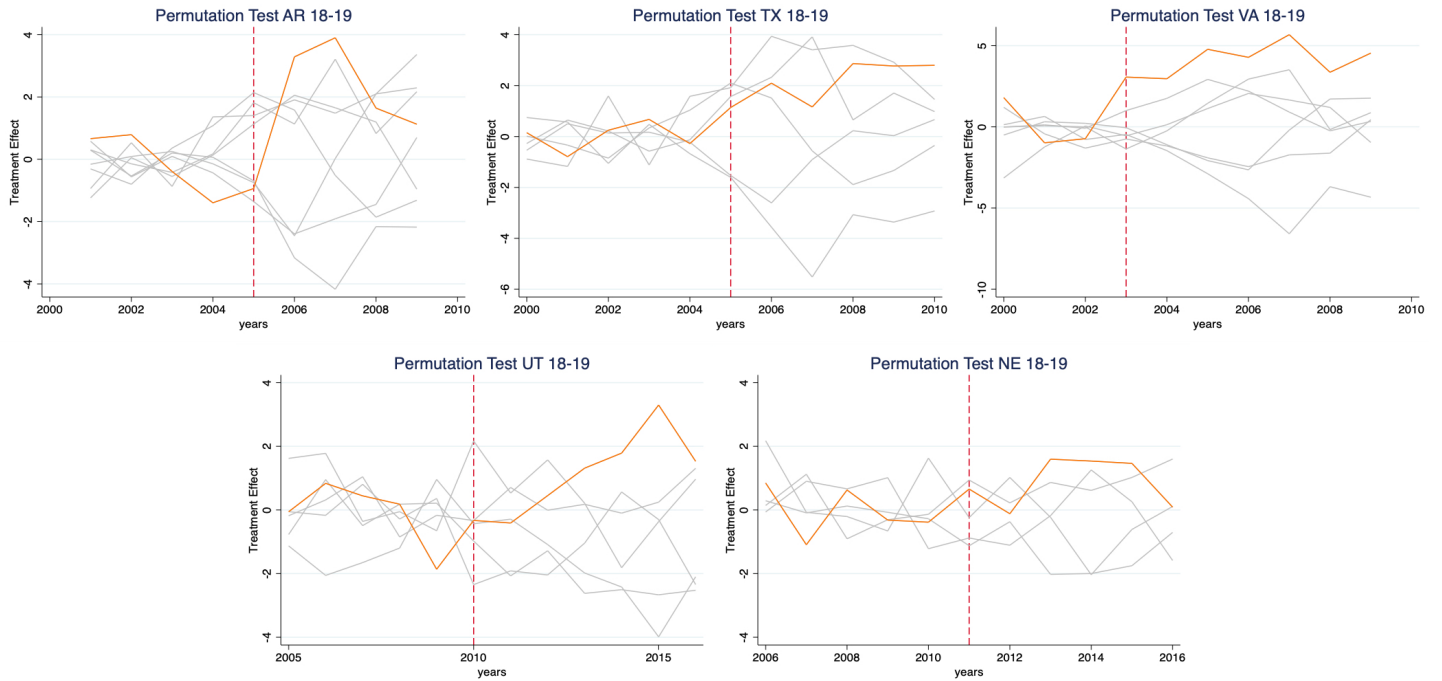


Figure 5b: Permutation Tests for the Abortion Rate of Older Teens (18-19)



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 group¹. 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 Arkansas pictured in Figure 5a ($p = 0.43$). 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 abortion rate of older teens in Virginia pictured in Figure 5b ($p = 0.14$). A full summary of treatment effects and percentile rank statistics is presented in the Results section in Tables 5 and 6.

In studies with many control states, this percentile rank statistic may be sufficient to achieve statistical significance. For instance, in a situation with one treated unit and 50 control units, it is possible to achieve a percentile rank statistic that is below the general p-value threshold for statistical significance, $p = 0.05$.

In this project, however, I have relatively few control units for each of my state-level policy analyses. Using a SCM with one treated unit and 8 control units, no matter how large the effect size of the treated unit is relative to the placebo effects, the minimum percentile rank is 0.11². To aggregate information from multiple treated units when the number of controls is small, 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 there may be too few control states to reject the null hypothesis 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 (2003-2005) and late treatment (2010-2011) to see if my results are also consistent with this observation.

¹Following 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.

²Calculation: minimum percentile rank occurs with the smallest possible rank value $r = 1$. Using the Weibull-Grumbel rule described earlier $p = \frac{r}{N+1}$. Therefore, minimum $p = \frac{1}{9} = 0.11$.

Table 4: Treatment/Control in Early vs Late Groups

Early Treatment		Late Treatment	
Treatment	Control	Treatment	Control
Arkansas	Georgia	Utah	Colorado
Virginia	Iowa	Nebraska	Georgia
Texas	Ohio		Iowa
	Kansas		Minnesota
	Minnesota		South Dakota
	Nebraska		West Virginia
	South Dakota		
	Utah		

Tables 5 and 6 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 abortion rate for older teens describes the proportion of placebo effects that are at or above the treatment effect (because the alternate hypothesis is that the treatment *increases* the abortion rate for older teens).

Table 5: Treatment Effect for Minors (15-17)

	Treatment Effect	p
Early States:		
Arkansas	-0.32	0.43
Texas	1.75	1.00
Virginia	1.04	1.00
Late States:		
Utah	0.10	0.67
Nebraska	0.30	0.60

Simply from the treatment effects and percentile ranks, it does not appear that the implementation of a consent law has a very large (or even consistently negative) effect on the abortion rate for minors. Arkansas is the only state with a negative treatment effect of 0.32 abortions per 1,000 resident minors assigned female at birth (AFAB). And, there is not statistically significant evidence that this treatment effect is due to the policy change ($p = 0.43$). Virginia and Texas both have positive treatment effects above 1 abortion per 1,000 AFAB resident minors, and Utah and Nebraska have small insignificant positive treatment effects.

When considering the impact of the policy change on the abortion rate of older teens, however, the results demonstrate a consistently positive treatment effect, with relatively small percentile rank statistics. The small percentile rank statistics ($p = 0.14$ in Virginia,

Table 6: Treatment Effect for Older Teens (18-19)

	Treatment Effect	p
Early States:		
Arkansas	1.80	0.25
Texas	2.04	0.33
Virginia	4.09	0.14
Late States:		
Utah	1.33	0.17
Nebraska	0.87	0.40

$p = 0.17$ in Utah, $p = 0.25$ in Arkansas) suggest that these values may not be independent random draws from $U[0,1]$. In this case, we may reject the null hypothesis that the policy change from parental notification to parental consent has no effect on the abortion rate for older teens. To determine the probability that these percentile rank statistics are independent draws from the uniform distribution, I use the pooling method described earlier.

5.1 Pooling Inference

Tables 7 and 8 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 $U[0,1]$.

In addition, I provide weighted average treatment effects, percentile rank statistics, and the corresponding p-values. I assign each state within the group a weight according to their pre-period inverse root mean square prediction error (RMSPE). Because the RMSPE in the pre-period denotes the quality of the match, this process weights the treatment effects and percentile rank statistics by the reliability of the post-period treatment effect estimate. Weighted percentile rank averages and their Irwin-Hall p-values are denoted with a w .

Table 7: Pooling Results for Minors (15-17)

	Average Treatment Effect	\bar{p}	p-value	Weighted Average Treatment Effect	\bar{p}_w	p-value _w
Early States (n=3)	0.823	0.810	0.969	0.583	0.761	0.939
Late States (n=2)	0.204	0.634	0.731	0.198	0.636	0.734
All States (n=5)	0.575	0.739	0.969	0.494	0.737	0.967

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. But, the results suggest a strong positive effect of the change from parental notification to parental consent for older teens. In early treatment states, the policy change results in an increase of 2.64 abortions/1,000 AFAB residents per year among older teens, an increase of 1.101 abortions/1,000 AFAB

Table 8: Pooling Results for Older Teens (18-19)

	Average Treatment Effect	\bar{p}	p-value	Weighted Average Treatment Effect	\bar{p}_w	p-value _w
Early States (n=3)	2.643	0.242	0.064*	2.427	0.266	0.085*
Late States (n=2)	1.101	0.284	0.161	1.069	0.301	0.180
All States (n=5)	2.082	0.259	0.030**	1.879	0.281	0.045**

** $p < 0.05$, * $p < 0.10$

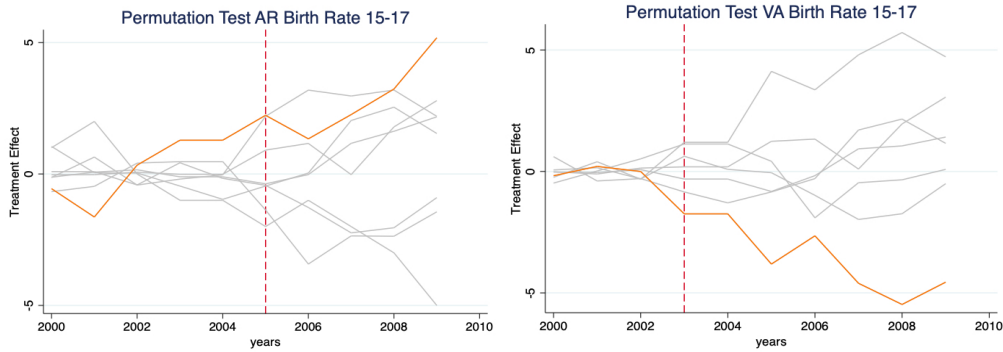
residents per year among older teens in late treatment states, and an overall increase of 2.082 abortions/1,000 AFAB residents per year. When just considering the size of the average treatment effect, these results appear consistent with the findings of Joyce (2020) that the effect size varies by the timing of the policy. However, when you consider the effect in terms of a percent change, all of these changes to the abortion rate represent a roughly 12% increase from pre-treatment average abortion rates. These results do not appear to be sensitive to weighting on the pre-period RMSPE.

5.2 Birth Rate Analysis

Birth effects are a potential consequence of policies that restrict abortion access. Because giving birth is one alternative to an abortion, the higher marginal cost of an abortion imposed by a parental consent law could result in more minors choosing to give birth. At the same time, restrictive abortion legislation could result in greater take-up of contraception and abstinence behaviors among minors, decreasing the birth rate.

As in my analysis for the abortion rate, I match a treated state to a synthetic control group using only pre-period values of the outcome variable, and I drop any placebo effect from the analysis when the pre-period match is poor. Permutation tests are presented in Figure 6. Note that Texas is not included in the birth rate analysis. This is due to a violation of the synthetic control assumption that the birth rate outcomes in Texas during the pre-period sit in the convex hull of the outcomes in the control states. I perform inference by pooling all treated states together in the manner described earlier. Results from pooling are presented in Table 9.

Figure 6: Permutation Tests for the Birth Rate of Minors



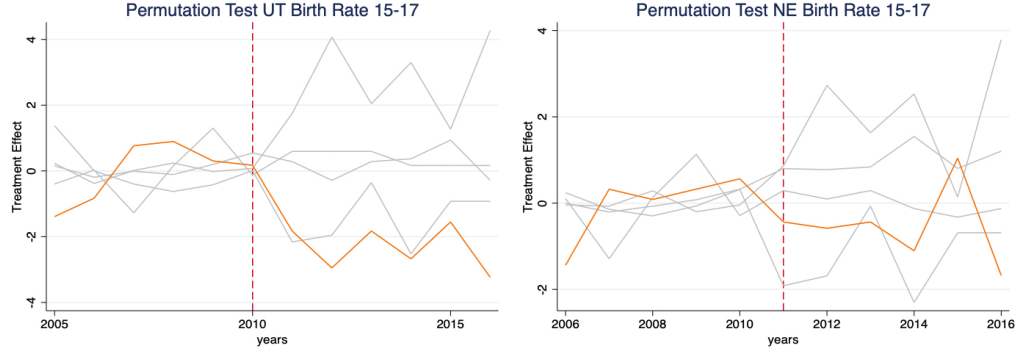


Table 9: Pooling Results for the Birth Rate of Minors

	Average Treatment Effect	\bar{p}	p-value
All States (n=4)	-0.796	0.731	0.944

The permutation tests from Figure 6 do not appear to demonstrate a consistent direction of any treatment effect. The treatment effect of the policy change is positive in Arkansas, negative in Virginia and Utah, and close to zero in Nebraska. Results from pooling suggest no significant marginal effect of a parental consent law on the birth rate of minors.

These results can act as a kind of “check” on my results for the abortion rate for minors. While abortion rate effects can’t always be tied to equivalent birth rate effects due to the potential availability of foreign abortifacients and other forms of self-induced abortion, it would be surprising to observe effects of the policy change on the birth rate for minors without observing an effect on the abortion rate. To that end, these null effects of the switch from parental notification to parental consent on the birth rate for minors support my results of a null effect on the abortion rate for minors.

6 Discussion

The lack of any differential effects of parental consent laws on the abortion rate for minors is consistent with much of the existing evidence, but the large spill-over effects of the policy change on the abortion rate for older teens are surprising. The dynamic nature of fertility choice implies that laws which restrict access to abortion among minors – and therefore induce birth effects among minors – could have effects on the abortion rate for later age groups. But, my results do not indicate any marginal effect of a parental consent law on the abortion rate for minors nor the birth rate for minors. So, these results require reconciliation.

I propose a mechanism that describes how these results can exist in tandem which depends on an institutional feature of parental involvement laws – the judicial bypass option.

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. The statutory stan-

dards 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:

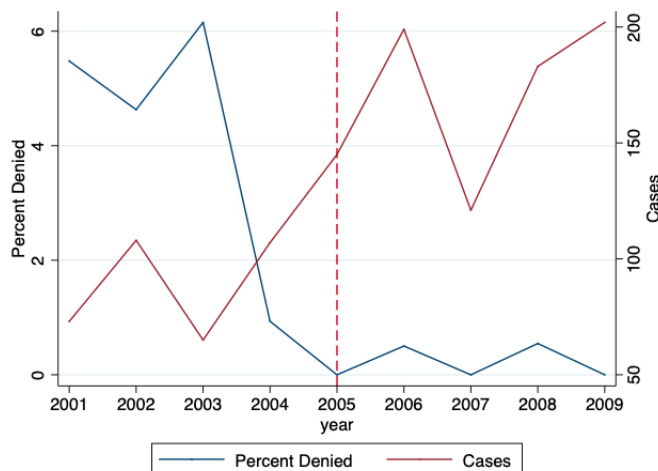
1. The judge determines that the minor is mature enough to make their own reproductive choices.
2. The judge determines that the minor may be in immediate danger by seeking to satisfy the parental involvement requirement.
3. The judge determines that the abortion would be in the best interest of the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

It is not possible to identify the effect of the judicial bypass in the empirical analysis because there is no variation in the presence of the judicial bypass option within my sample. All states in my sample offer the judicial bypass option in each year of my analysis. 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.

6.2 Reconciliation Mechanism

Suppose that the judicial bypass option does become more generous in response to the policy change from parental notification to parental consent. Then, consider the following mechanism:

- Treatment Group A
 - There exists a set of minors who have access to an abortion under a parental notification law, but do not have access to an abortion under a consent law. These minors are responding to the increase in the marginal cost of an abortion imposed by the parental consent statute by having fewer abortions.
- Treatment Group B
 - There exists a set of minors who did not have access to an abortion under the parental notification law, but now benefit from the extra generosity in the judicial bypass procedure under the consent law and are able to receive their desired abortion.

If these two groups are similar in size, then this could explain the observed lack of any differential effect of parental consent on the abortion rate of minors. Some minors are experiencing the increased marginal cost imposed by the consent law, and others are benefitting from the increase in judicial bypass generosity. Though no comprehensive data on these two populations exists, it is reasonable to suspect that they may differ along several dimensions. Minors who are able to access and navigate the court proceedings required to obtain a judicial bypass waiver might differ economically, socially, and geographically from the set of minors who do not have such access.

These differences in treatment groups A and B could be used to reconcile the results for minors and older teens. Suppose that minors in treatment group A are more likely to become pregnant again as an older teen. Then, this could explain the observed spill-over effects on older teens. If a minor in treatment group A chooses to give birth in response to the policy change, then this has implications for their fertility choices later in life. If that minor becomes pregnant again as an older teen, the financial, social, and health constraints imposed by the prior birth may incentivize them to have an abortion. Since these minors make up a larger proportion of those who become pregnant as an older teen, this puts upward pressure on the abortion rate for older teens in response to the policy change.

Of course, this mechanism rationalizing the observed results rests on a set of unsubstantiated assumptions. More research into the judicial bypass procedure – and who has access to the procedure – is needed to substantiate these claims.

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). At the same time, results indicate that a parental consent law increases the abortion rate for older teens (18-19) by about 12% relative to a parental notification law. The policy implication being that these laws have spill-over effects that are likely unintended by policy-makers.

Additionally, the empirical evidence of spill-over effects in this discussion of a differential effect from parental consent has implications for the broader PI law literature. Because these spill-over effects appear during a policy change from notification to consent, it is reasonable to suspect they may also appear when any parental involvement law is passed. While the research into PI laws considered as one category generally finds that these laws reduce the abortion rate for minors by about 15%, it would be worthwhile to explore the potential for spill-over effects in these contexts.

The exact mechanism through which these spill-over effects exist is still largely unclear. While this paper proposes a process through which spill-over effects can exist while a direct effect of the policy change on minors is absent, the argument relies on a series of assumptions and a small amount of limited observational data. Further research on issues related to the judicial bypass procedure may be a key element of understanding the effects of parental involvement laws on both minors and older teens.

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Appendix A: Data Sources

Economic Info

Arkansas

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Colorado

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Georgia

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Ohio

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South Dakota

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Texas

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