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The Impact of Parental Involvement Laws on the Abortion Rate of Minors

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Abstract

In this article, we conduct a comprehensive analysis of the effect of parental involvement (PI) laws on the incidence of abortions to minors in the United States. We contribute to the extant literature in several ways. First, we explore differences in estimates of the effect of PI laws across time that may result from changes in contraception, the composition of pregnant minors, abortion access in nearby states, and differences in how these laws are enforced. We find that PI laws enacted before the mid-1990s are associated with a 15% to 20% reduction in abortions to minors, but PI laws enacted after this time are not associated with declines in abortions to minors. Second, we assess the role of out-of-state travel by minors and find that it is not a significant factor moderating the effect of PI laws. Third, we use a synthetic control approach to explore state-level heterogeneity in the effect of PI laws and find large differences in the effect of PI laws on abortions to minors by state that appear unrelated to the type of PI law or whether contiguous states have enacted PI laws. Finally, we show that estimates of the effect of PI laws using data from either the Centers for Disease Control or the Guttmacher Institute do not differ qualitatively once differences in the states and years available across these data are harmonized.

Keywords

Abortion; Public policy; Women's health

Introduction

State regulation of fertility control methods are widespread and have the potential to significantly affect the timing and number of children born. The effect of such regulations on

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minors is particularly important because of the theoretical and empirical linkages between abortion and unintended childbearing. If state laws reduce abortion among minors, the birth rates of minors are likely to rise, and most of these additional births will be unintended. And despite debate over the consequences of teen births, it is likely that unintended births—births that would have been aborted in the absence of a state restriction—adversely affect teens' current and future well-being because they impose a significant constraint on a teen's choices related to schooling, work, and household formation (Kane et al. 2013).

A common state regulation is parental involvement (PI) laws. These statutes require that an abortion provider notify the parent(s) of a minor's request for an abortion or that the parent(s) provide written consent before a procedure can be performed. Several studies have examined the effect of such laws on abortion for minors, but most of these studies are from an earlier period, and there remain several gaps in the evidence that warrant additional research.[1]

One of the most important unanswered questions is whether the effect of PI laws differs by geography and time. There are three plausible reasons to suggest that the effect of PI laws on abortions to minors would vary along these dimensions. First, the prevalence of PI laws has increased over time. Currently, 37 states enforce PI laws, and the widespread geographic coverage of current PI laws may make it more difficult for teens to avoid compliance by traveling to another state. For example, Myers and Ladd (2017) reported that the average distance to avoid a PI law increased from 55 miles in 1992 to 454 miles in 2017; thus, the effect of PI laws on abortion may have increased over time. Second, methods of contraception have changed over time, and the use of different methods varies by region. The increased use of long-acting reversible contraception is most notable (Daniels et al. 2013; Kavanaugh and Jerman 2018). Temporal and geographic variation in contraception will alter the demographic characteristics of teens seeking an abortion and, therefore, the effect of PI laws on abortion. Another factor that may alter the effect of PI laws is the variation across states in the use of judicial bypass for minors seeking an abortion without parental consent (Altindag and Joyce 2017). Finally, family structure (e.g., cohabitation) and parental characteristics (e.g., age and education of mother) differ significantly by geography and time, and these differences may affect child-parent relationships and the composition of teens at risk of being affected by PI laws.[2] Overall, there are ample reasons to suggest that the effect of PI laws will differ by geography and time. However, no research has assessed this issue systematically.

In this article, we explore potential heterogeneity in the effect of PI laws by time and geography. We examine the effect of PI laws on abortion rates of minors from 1985 to 2013,

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[1]The literature consists of state-specific studies (Cartoof and Klerman 1986; Colman et al. 2008; Ellertson 1997; Henshaw 1995; Joyce and Kaestner 1996, 2001; Joyce et al. 2006; MacAfee et al. 2015; Ralph et al. 2018; Ramesh et al. 2016; Rogers et al. 1991) and analyses using a broader set of states (Haas-Wilson 1996; Levine 2003; Myers and Ladd 2017; New 2011; Ohsfeldt and Gohmann 1994). After reviewing this literature, we conclude that results from these studies indicate that PI laws decreased abortions among minors and that an effect size of 15% is typical, although there is meaningful variation across studies.

[2]Analysis of the U.S. Census (1990, 2000) and American Community Survey (five-year 2013) data reveals significant changes over time and across states in the characteristics of mothers/families with teenage children. For example, the share of mothers that are married was 78% in 1990 and 73% in 2013. The share of married mothers also differed across states by up to 12 percentage points. The share of mothers with a BA or higher was 14% in 1990 and 27% in 2013, and the share with BA degree or more differed by up to 11 percentage points across states. The average age of mothers increased over time and differed across states by one to two years.

allowing effects to vary by period and state. Our investigation of heterogeneous effects includes several state-specific case studies using a synthetic control approach, which has not been previously used in this context. These case studies differ by year, type of PI policy (e.g., consent vs. notification), and proximity to a confidential provider.

Another important consideration in the analysis of PI laws has been the extent to which minors travel to other states for an abortion without parental involvement (Dennis et al. 2009). Although it is often argued that out-of-state travel by teens is significant and that such travel reduces the impact of PI laws, the evidence to support this argument is surprisingly sparse. For example, Cartoof and Klerman (1986) is a widely cited study of the effect of Massachusetts PI law on the abortion rates of minors. The authors reported that abortions to minors by state of occurrence fell from 5,113 in 1980, one year before the law, to 2,802 in 1982, one year afterward. However, because so many Massachusetts minors went to neighboring states for an abortion, the authors concluded that, “the parental consent law had little effect on adolescent’s pregnancy-resolution behavior” (Cartoof and Klerman 1986:397). Their conclusion is incorrect. Using Cartoof and Klerman’s published data, we find that resident abortions to minors in Massachusetts fell 20% after the law.[3] Other evidence of teen travel to obtain an abortion is limited.[4]

Moreover, the conventional wisdom that teens often travel to avoid the effect of a PI law stands in contrast to a broader literature documenting that distance to an abortion provider presents a substantial barrier to obtaining an abortion. Recent studies by Colman and Joyce (2011), Lindo et al. (2018), and Fischer et al. (2018) reported large declines in abortions in response to closures of abortion clinics in Texas. These studies, which apply to all women, suggest that travel would be an even larger barrier for minors, who lack the resources and capabilities of adult women, and raise questions about whether out-of-state travel is an easily exercised option for teens.

In this article, we address the distance issue explicitly by analyzing whether the distance necessary to evade a PI law affects abortion rates among minors (and older teens). We also assess whether the effect of PI laws has changed over time as distance to a confidential abortion provider has increased markedly. Third, we take advantage of individual-level data on abortions in three states to provide more detailed information on travel by minors before and after enactment of a PI law in each state.

[3]Because the Cartoof and Klerman (1986) study is frequently cited as the seminal example of travel by minors, we detail our claim. The Massachusetts law went into effect May 1981. In 1980, 5,113 abortions to minors were performed in Massachusetts; 6.3% of all abortions in the state were to nonresidents, and 3% of Massachusetts residents obtained abortions in other states. If we assume that 6.3% of abortions to minors were also nonresidents and that 3% of resident minors went out of state in 1980, there were 4,935 abortions to resident minors in 1980. In 1982, there were 3,942 abortions to resident minors: 2,802 were performed in Massachusetts and 1,140 were performed in other states. Accordingly, abortions to resident minors fell by 20.1% from 1980 to 1982 (4,935 – 3,942) / 4,935).

[4]MacAfee et al. (2015) and Ralph et al. (2018) showed that abortions fell more among out-of-state minors following PI laws in New Hampshire and Illinois than among resident minors of each state. However, their results pertain to one clinic, or a few clinics, in close proximity to states without a PI law. Ellertson’s (1997) simulation analysis is speculative and used data on travel four years *after* the implementation of the PI law. Henshaw (1995) argued that Mississippi’s PI law had no effect on the abortion rate of minors because most resident minors went out of state for an abortion. Henshaw’s estimates, however, are potentially confounded by Mississippi’s mandatory delay law enforced eight months prior to its PI law, which also induced many nonminors to leave the state for an abortion.

Finally, we provide an assessment of whether the two primary data sources used in abortion research yield different estimates of the effect of PI laws and, if so, what may explain these differences. Abortions by age, year, and state of occurrence are published by the Centers for Disease Control and Prevention (CDC). The other major data source is the Alan Guttmacher Institute (GI), which uses its periodic survey of abortion providers to estimate abortions by age and state of residence. State-year analyses of PI laws based on abortion rates by state of occurrence will overstate the impact of the law if resident minors leave the state for a confidential abortion and nonresident minors stop coming into the state with a PI law. Abortions by state of residence would resolve the issue, but such data are unavailable nationally, and they are estimated by Guttmacher Institute. The GI data do not account for travel by minors in response to a law, as we describe later. Another complication is that the states and years included in the CDC and GI data differ (see Table A1, online appendix). We harmonize these data and report estimates using a common set of states and years to illuminate the source of differences, if any, in estimates obtained using the two data sources.[5]

Results from our analyses indicate that abortions to minors declined by approximately 15% to 20% in states that adopted PI laws prior to the mid-1990s. After this time, we find little evidence that, on average, PI laws reduce abortions to minors. Notably, we find no effect of PI laws on the abortion rate of 18- or 19-year-olds, which supports interpreting the effects of PI laws on minors as causal. Second, results indicate that the effect of a PI law does not differ significantly by the distance to a state without a PI law. This suggests that teens' ability to travel out of state to avoid compliance with a PI law is limited and is consistent with evidence from three states for which we have individual-level data on abortions to resident and nonresident teens, and the broader literature demonstrating that distance to a provider is a significant barrier to abortion access.

Third, we find that results are qualitatively similar regardless of the source of abortion data, whether from the CDC or GI. Additionally, once we harmonize these data sources to have the same states and years, estimates are quantitatively very similar. The heterogeneity of estimates when different states and years are used in analysis underscores the heterogeneity in effects that we hypothesize. Fourth, state-specific case studies suggest that there is substantial heterogeneity in the effects of state PI laws on abortions to minors and that this heterogeneity does not appear to be related to the type of law or whether surrounding states have enacted PI laws. The state-specific analyses, however, are underpowered, which necessitates caution in interpreting these results.

Empirical Approach

We use two empirical approaches—difference-in-differences and synthetic control—to obtain estimates of the effect of PI laws on abortions to teens aged 15–17.

[5]We use both data sets for two reasons. The GI data are available only every three to four years, and these gaps make the GI data unusable for our event study and synthetic control analyses. Second, GI data are considered a more accurate count of abortion, which is important because the decline in the abortion rate associated with PI laws sets the likely upper limit for effects of PI laws on birth rates.

Difference-in-Differences

The difference-in-differences (DiD) research design measures changes in abortions to minors (aged 15–17) pre- and post-adoption of a PI law in states that did and states that did not adopt a law. The DiD is implemented using regression methods and the following model specification:

$$A_{jt} = \alpha_j + \delta_t + \beta PI_{jt} + \lambda \mathbf{Z}_{jt} + u_{jt} \, . \tag{1}$$

In Eq. (1), the abortion rate (A), or log of the abortion rate, of teens aged 15–17 in state j and year t depends on state fixed effects (α_j), year fixed effects (δ_t), and an indicator of whether any PI law is enforced in that state and year. The model also includes state-specific, time-varying covariates ($\mathbf{Z}_{jt}$): unemployment rate; median wage for women aged 25–44; and shares of women aged 15–19 who are Black, non-Hispanic, and White Hispanic (non-Hispanic Whites and Other are the reference group). The coefficient, β, is the DiD estimate under the assumption of a homogeneous treatment effect. We estimate the model using weighted least squares, where the weight is the state population of women aged 15–17. We also estimate unweighted regressions and report results in Table A3 in the online appendix. Standard errors are constructed using a robust-cluster method that allows for nonindependence of observations within state.[6]

The primary threat to the validity of the DiD approach is from unmeasured confounders that vary by state and year. To assess whether this is the case, we estimate an event-study specification in which we allow the effect of the PI law to differ by year relative to year of implementation:

$$A_{jt} = \alpha_j + \delta_t + \sum_{m=-6}^{m=5} B_m (PI_j \times \delta_{m=t-t^*}) + \lambda \mathbf{Z}_{jt} + u_{jt} \, . \tag{2}$$

PI_j is an indicator that the state adopted a law, and t^* is the year the law was enforced. Therefore, $\delta_{m=t-t^*}$ is 1 if $m = t - t^*$ and 0 otherwise; $m = -6$ includes all years more than five years prior to implementation of a PI law; and $m = 5$ includes all years more than four years after implementation of a PI law. The test of the validity of the DiD approach under a homogeneous treatment effect assumption is that estimates of B_m in periods prior to the implementation of the law are 0.[7]

We also address potential threats from confounding factors by estimating a modified version of Eq. (1) that allows the effect of state-specific, time-varying covariates ($\mathbf{Z}_{jt}$) to differ by year:

$$A_{jt} = \alpha_j + \delta_t + \beta PI_{jt} + \lambda \mathbf{Z}_{jt} + \pi_{kt} (\mathbf{Z}_{jkt} \times \delta_t) + u_{jt} \, . \tag{3}$$

The inclusion of the covariates ($\mathbf{Z}_{jt}$)-by-year effects is a way to control for potential confounding (Jaeger et al. 2018). We use the subscript k to index covariates. An alternative

[6] We also use an alternative method of inference: randomization inference. Results (see Table A4, online appendix) from this alternative are very similar to those from the robust cluster method.

[7] The estimates presented for the event study analyses include covariate-by-year interactions as in Eq. (3). Analogous estimates from the basic, two-way fixed-effect model (Eq. (2)) do not qualitatively differ from those shown in Fig. 1.

approach to control for state-specific, time-varying factors is to include state-specific linear time trends, as follows:[8]

$$A_{jt} = \alpha_j + \delta_t + \beta PI_{jt} + \lambda \mathbf{Z}_{jt} + \pi_{kt}(\alpha_j \times TREND_t) + u_{jt}\,. \tag{4}$$

Finally, to assess whether the effect of a PI law differs by the distance to the nearest non–PI law state, we estimate a model that allows the effect of the PI law to differ by the distance to the nearest state without a PI law:

$$A_{jt} = \alpha_j + \delta_t + \beta PI_{jt} + \beta_2(PI_{jt} \times DIST_{100-199}) + \beta_3(PI_{jt} \times DIST_{200-299}) + \beta_4(PI_{jt} \times DIST_{300-399}) + \beta_5(PI_{jt} \times DIST_{400+}) + \lambda_1 \mathbf{Z}_{jt} + \boldsymbol{\pi}_j(\alpha_j \times TREND_t) + u_{jt}\,. \tag{5}$$

Equation (5) includes interactions between the PI indicator and the distance to the nearest state without a PI law. *Distance* is measured as linear miles from the most populous city in the state with a PI law to the most populous city of the nearest state without a PI law.[9] Distance is 0 when a state does not have a PI law. We divide states that adopted a PI law into five distance groups: <100, 100–199, 200–299, 300–399, and 400+. The distance to a state without a PI law is updated as states adopt laws. The coefficients β_2 to β_5 measure whether the effect of a PI law at a specific distance differs from the reference category (main effect) of less than 100 miles.

We also test whether the effect of PI laws has changed over time. One motivation for this choice is that distance to the nearest confidential provider has been growing over time as states adopt PI laws. If the effectiveness of PI laws increases in a manner proportional to distance, then all else equal (e.g., homogenous treatment effect), the effect of PI laws on resident abortion rates of minors should increase over time as minors have to travel further to avoid parental involvement.[10] Other reasons to explore the heterogeneity of effects by year are for those previously stated related to changes over time in contraception practices and family structure. We estimate Eq. (3) for five periods: 1985–1996, 1988–2000, 1992–2005, 1996–2008, and 2000–2013. In addition, we estimate Eq. (3) in 10-year rolling panels using the annual CDC data so as to trace out the evolving impact of PI laws over the 28-year study period.

Finally, we repeat the analyses just described but using the abortion rate of teens aged 18–19. This group is almost completely unaffected by PI laws, and the analysis of these teens is a falsification test that provides further evidence on the validity of the DiD approach. The results of these falsification analyses, discussed in detail later, bolster our claim that the estimated effects of PI laws on the abortion rate of minors is causal.

[8]In analyses not reported, we replaced state-specific trends with “timing-group” trends as defined by Goodman-Bacon (2018). Results were similar, as expected and shown to be analytically the case by Goodman-Bacon (2018).

[9]We also used distance from the population centroid of the state with a PI law to the population centroid of the nearest non–PI state. Results, shown in Table A5 of the online appendix, did not differ meaningfully from those reported here.

[10]If abortions are measured by state of occurrence, the relationship between the effect of a PI law and distance (all else equal) is ambiguous and depends on how distance affects baseline rates of abortion by state of occurrence.

Synthetic Control Approach

We conduct several state-specific case studies using the synthetic control method of Abadie et al. (2010). The synthetic control estimate of the effect of a PI law on abortions among minors is obtained by taking the difference in mean abortion rates in the post-adoption period between the treated state and a weighted average of control states (i.e., synthetic control). The key decision in implementing the synthetic control approach is the method to select the weights used to construct the counterfactual outcome from a pool of potential "donor states." In general, the weights are selected to minimize the difference between the pre-period characteristics (e.g., the dependent variable) of the treated and control states.

Such weights can be selected in a variety of ways, with no generally accepted best method. Thus, we use two methods. The first uses the value of the dependent variable in each of the pre-period years to derive weights for control states. The second uses the value of the dependent variable in every other pre-period year, every pre-period year of the state unemployment rate, and the pre-period average of the state median wage; state share of 15- to 19-year-old Black women; and state share of 15- to 19-year-old Hispanic women. It is common practice to rely on lagged values of dependent variable to select weights because of the quality of the match that results from this choice, despite some potential for bias (Kaul et al. 2015). We also conduct synthetic control analyses for teens aged 18–19. We expect to find no effect of PI laws on teens in this age group.

Data

Individual-Level Data on Abortions

We obtain individual-level data on abortion that included abortions to residents performed in-state and out-of-state as well as to nonresidents performed in-state. The three states with data covering the years pre– to post–PI law are Mississippi (1993), North Carolina (1995), and Texas (2000). There is no mandatory reporting agreement between states in which induced abortions to residents in state A that occur in state B are reported to state A. However, states that collect data on abortions as part of the vital statistics surveillance often report a nonresident's state of residence. In the case of North Carolina, for example, we use data from South Carolina and Virginia to track the change in abortions to minors of North Carolina before and after the law. In the case of Texas, we have data on abortions to Texas residents performed in Arkansas, New Mexico, and Oklahoma. In Mississippi, the state collected data on induced abortion to residents of Mississippi that occurred in other surrounding states except for Florida and Louisiana. These data are used to illustrate the extent of travel by minors and older teens in response to PI laws.

State-Year Aggregate Data

We use data from two sources. The first is from the CDC and provides information on annual abortions by age and state of occurrence. The data span 1985 to 2013, but a number of states did not report abortions in every year (see Table A1, online appendix). We drop information from a small number of states because of either direct knowledge of fundamental changes in data reporting (Washington, DC), or because of the presence of extreme outlier observations (year-to-year changes in abortion of more than 3 standard deviations). The state-years excluded are all years for Washington, DC; Delaware prior to 1999; Kentucky prior to 1988; Texas prior to 1988; and West Virginia prior to 1992.

The second source of abortion data is the Guttmacher Institute, whose data on abortion are considered a more accurate estimate of total abortions than CDC data. The mean value of the ratio of abortions among minors reported by the CDC to abortions among minors reported by the GI among states and years present in both data sets is 0.86. GI data are available for all states and the years 1985, 1988, 1992, 1996, 2000, 2005, 2008, 2010, 2011, and 2013. Notably, GI does not collect information on the age or the state of residence of a person. Estimates of the total number of abortions by age and state of residence are constructed from their total counts of abortion by state of occurrence using the age distribution and information on residence collected by the CDC and state agencies. In states and years when there is no CDC- or state-provided information, abortions to minors in a state are estimated using age distributions from either surrounding states or the national average. Therefore, in these cases, the GI data will not accurately reflect the effect of PI laws on abortions to minors because the share of abortions to minors in other states will not accurately reflect the adoption of a PI law in the state of interest. As such, the Guttmacher Institute notes that its resident abortion rates account for general patterns of out-of-state travel but not for potentially differential out-of-state travel by minors in response to PI laws (Kost et al. 2017).

To assess whether the source of data affects estimates of the effect of PI laws, we conduct analyses on the CDC data, the GI data, and harmonized data, which consists of the overlap between CDC and GI data (i.e., CDC states and GI years). As we noted earlier, examining the effect of PI laws using CDC and GI data offers a way to reveal whether there are heterogeneous effects by state and year because of the differences in the composition of states and years available in each data set.

Parental Involvement Laws

Information on PI laws is obtained from several sources, but we rely primarily on Myers (2017).[11] For the DiD analyses, we create an indicator of whether a PI law was in effect in a state and year. For states that adopted a PI law midyear, we use the fraction of the year the law was in effect. We do not distinguish between the types of law—whether it required parental notification or parental consent. For the event study and synthetic control analyses, we use a binary indicator equal to 1 in the year of adoption if the law was adopted before July 1 and 0 if it was adopted later in the year (with the subsequent year coded to 1 instead). Similar results were obtained from DiD analyses when we relied on the binary classification used in event-study and synthetic control analyses.

[11]Our classification follows Myers (2017) with a few exceptions. We assign Utah as having a PI law from 1974, whereas Myers used 2006. All previous analyses of PI laws have used 1974 (e.g., Levine 2003; Merz et al. 1996; the NARAL Foundation 2017). We treat Maryland as enforcing a PI law since 1992, whereas Myers did not. The ambiguity arises because in Maryland physicians do not have to notify a parent if they believe it is in the best interest of the minor. Following Myers (2017) does not affect our results meaningfully.

Results

Analysis of Out-of-State Travel in Three States

In Table 1, we show the number for abortions one year before and after a PI law went into effect in North Carolina (1996) and Texas (2000). For Mississippi (1993), we use the number of abortions two years prior to implementation of the law as the pre-law period because Mississippi imposed a mandatory delay law in August 1992 that required two in-person visits to the provider: one for the counseling and a second for the procedure. The mandatory delay law had a large impact on travel out of state for an abortion (Joyce et al. 1997). Data in Table 1 is presented by age, state of occurrence, and state of residence. The simple DiD analysis in Table 1 is intended to highlight changes in abortions by state of residence and occurrence as well as the role of out-of-state travel.

In Mississippi, there were large absolute and relative changes (post-/pre-law) in total resident abortions to minors (−180, −25.2%) and teens aged 18–19 (−226, −25.7%). The total change in resident abortion consists of 148 more minors leaving the state and 328 fewer resident minors receiving abortions in-state. Similar changes in travel are observed for older teens aged 18–19, although the change in out-of-state travel were smaller. Thus, abortions by state of occurrence fell by 501 (89.0%) among minors and 471 (64.7%) among older teens, suggesting a difference-in-difference estimate in relative terms of 24.3% (89.0% – 64.7%). In contrast, abortions by state of residence suggest no relative changes because of the greater travel out-of-state by minors.[12]

Changes in abortions to minors pre- to post-law in North Carolina and Texas indicate that resident abortions fell 17.2% more among minors relative to older teens in North Carolina (23.7% vs. 6.5%) and 12.5% more in Texas (21.4% vs. 8.9%). Similar estimates are obtained using occurrence data. The lack of difference in the estimates obtained with resident and occurrence data is due to the small amount of documented out-of-state travel in North Carolina and Texas. For example, Virginia, which borders North Carolina, did not have a law in 1995, and yet few minors travelled there. With respect to Texas, there were no PI laws in New Mexico and Oklahoma in 2000, but travel to these two states was limited, likely because of the large distances necessary to travel.

Another way to assess travel is to analyze the change in the number of abortions to nonresident minors from states that border Mississippi, North Carolina, and Texas when the border state imposes a PI law. As we show in Table A2 in the online appendix, very few minors travelled from Alabama or Arkansas to Mississippi after enforcement of a PI law in 1987 for Alabama and 1989 for Arkansas. The same absence of travel is true for states that border North Carolina and Texas. Overall, the results in Table 1 and Table A2 (online appendix) suggest that out-of-state travel by minors seeking confidential abortion is not extensive.

[12]There may be a reporting issue in Mississippi because a provider who performed 60% to 70% of all abortions lost his medical license (Nossiter 1994). Some of the abortions the provider performed may not have been reported. The similar decline in abortions for teens aged 18–19 in Mississippi is consistent with this hypothesis.

Event-Study Estimates

We begin the presentation of results with estimates from the event-study specification of the DiD model. We use CDC data and show results for both minors and teens aged 18–19 in Fig. 1. If the DiD design is valid, then estimates of the effect of adopting a PI law should be 0 in periods prior to implementation. This is exactly the pattern found in Fig. 1 in panels a (abortion rate, ages 15–17) and c (log of abortion rate, ages 15–17). *F* tests for these analyses all fail to reject the null hypothesis that all pre-adoption estimates of the coefficients on the interaction terms between the PI indicator and year are jointly equal to 0. Estimates also suggest a negative effect of PI laws on abortion rates of minors after implementation of approximately 20% (e.g., minus 0.2 log points, panel a). It is also evident that the effect of the PI law appears to remain relatively constant and persists for several years after implementation, which is counter to arguments that teens adapt to the law (e.g., Kane and Staiger 1996).

In contrast, estimates in panels b and d of Fig. 1 indicate that the adoption of a PI law had no effect on the abortion rate of teens aged 18–19. Estimates are statistically insignificant and small (<5%). These results provide evidence supportive of a causal interpretation of the estimated effect on minors.

Difference-in-Differences Estimates

Table 2 presents estimates of the various DiD models described by Eqs. (1), (3), and (5).[13] For each, we use two measures of abortions: the abortion rate per 1,000 women, and the natural logarithm of this abortion rate.[14] We refer to estimates from Eq. (1) as Model A, estimates from Eq. (3) as Model B, estimates from Eq. (4) as Model C, and estimates from Eq. (5) as Model D. For each model, we present estimates using CDC, GI, and harmonized data (CDC data limited to the years available in the GI data, and GI data limited to state-years present in the CDC data). To focus on models that we think are preferred on statistical grounds, we limit discussion of results to estimates from Models B–D. It is clear from the variation in estimates and standard errors that the addition of controls for state-specific confounders in Models B and C matters. However, there is general consistency among estimates obtained from Models B and C, suggesting that the way we adjust for state-specific confounding is not particularly important.

Beginning with results obtained using the natural logarithm (log) of the abortion rate, estimates from Models B and C indicate that adoption of a PI law is associated with between a 12% and 24% decrease in the abortion rate of minors, and all estimates are statistically significant. Estimates using the unharmonized CDC and GI data are around −13% to −18%. Estimates that use the CDC data limited to the years in the GI data and the GI data limited to

[13]We present estimates from unweighted regression models in Table A3 in the online appendix. With one exception, results are qualitatively similar. For Model D (distance), when log abortion rate is used, the pattern of estimates differs from that in Table 2 but still does not suggest an important effect of distance. The differences between weighted and unweighted estimates is consistent with the heterogeneous effects that we hypothesize.

[14]We use two measures of abortion because previous studies have used both, and there is no theoretical justification for one or the other. The two measures assign different weights to similar changes in the dependent variable. Last, absolute changes in the abortion rate of minors matter because the decline in the abortion rate sets the likely upper limit for the possible increase in birth rates.

state-years in the CDC are larger, indicating that PI laws decrease abortion rates by an average of approximately 22%.

The larger (more negative) estimates found when the GI data are limited to the CDC states and years are consistent with how the GI data are constructed. In years with no CDC data, GI uses the share of abortions to minors in surrounding states (or uses the national average) to estimate the age-specific abortion rate. In these cases, the abortion rate for minors will not reflect any effect of the PI law in a state that adopted a law. In control states, using the share of minors from states with PI laws will also result in measurement error. The measurement error will attenuate estimates, which is what we observe. After we account for this issue, the data source (CDC or GI) does not affect the estimates in a qualitatively meaningful way, as evidenced by the similarity of estimates using Models B and C in the last two columns of each panel in Table 2.

Finally, estimates from Model D of Table 2 indicate that the effect of PI laws does not differ statistically or quantitatively by distance to a non-PI state. None of the estimates of the effect of a PI law when distance to a nearest provider is greater than 100 miles are statistically significant, and most are quite small. There is also no pattern indicating that the effect of PI laws increases or decreases with distance. These findings suggest that minors do not travel much in response to the adoption of a PI law. If they did, we would expect the effect of a PI law to be changing, arguably getting larger, in states that are far from another state that does not have a PI law. The estimates in Model D do not support this expectation or the existence of any meaningful variation in the effect of PI laws as distance increases. In Table A5 in the online appendix, we estimate Model D with distance measured as distance between population centroids and find similar results (i.e., no moderating effect of distance).

Results from models that use the abortion rate are generally consistent with those that use the log rate. In this case, however, there is a bit more variability of estimates between Models B and C. Nevertheless, all estimates from Models B and C are negative and generally similar in magnitude, and those from Model C are statistically significant and larger. Estimates from Model B indicate that adoption of a PI law is associated with between a 10% (1.59 /15.4) and 17% (1.81 / 10.9) decrease in abortions among minors. From Model C, estimated effects are between −12% and −29% and are, as noted, statistically significant. Estimates from Model D suggest, again, that the effect of PI laws does not differ by distance to the nearest non-PI law state. All estimates of the effect of a PI law at distances greater than 100 miles are not significant.

Overall, estimates in Table 2 provide consistent evidence that PI laws are associated with a decrease in abortion rates to teens aged 15–17. A simple average of estimates from Models B (across all eight combinations of samples and outcomes) and C yields −16.0% and −19.6%, respectively. Second, distance to the nearest non-PI state does not seem to affect estimates and is consistent with the relative lack of cross-state travel by minors seeking confidential abortions shown in Table 1 and Table A2 in the online appendix. This finding suggests that travel by minors is not a particularly important behavioral response to the adoption of a PI law. Third, estimates obtained with harmonized CDC or GI data are similar.

The convergence of estimates when the same states and years are used also suggests heterogeneity by state and year, an issue we turn to shortly.[15]

To further assess the validity of the DiD research design, we reestimate the models in Table 2, using a sample of teens aged 18–19. These women were largely unaffected by PI laws. Estimates for this sample are presented in Table A6 in the online appendix. None of the estimates are statistically significant, and there is no systematic evidence of a negative effect or any other consistent pattern. Effect sizes are also quite small; estimates from Models B and C in log abortion rates models are between −0.006 and 0.059. In sum, these estimates provide further evidence that the estimated effects of the PI laws for minors are likely causal.

Period-Specific Estimates

We next assess whether the effects of the PI law differ by period. The motivations for this analysis are (1) the fact that distance to the nearest confidential provider has been growing over time as more states adopt PI laws, and (2) changes over time in contraception practices and family structure. We conduct two exercises. First, we estimate effect of PI laws using the CDC data and 10-year rolling periods. Second, using both the CDC and GI data, we obtain estimates of the effect of PI laws for five periods, selected on the basis of GI data availability: 1985–1996, 1988–2000, 1992–2005, 1996–2008, and 2000–2013. In both cases, estimates are obtained using the specification in Model B.

Figure 2 shows estimated effects of a PI law for 10-year rolling periods. As shown in panel a for 15- to 17-year-olds, all estimates from the earlier periods are negative, statistically significant, and increasing in magnitude through the period 1991–2000. The largest effect size during these periods was approximately −25%. After the 1991–2000 period, there is a clear monotonic decline in the effect size, and estimates become statistically insignificant after 1995–2004. The same analysis for 18- to 19-year-olds (panel b of Fig.2) shows no statistically significant effect of PI laws on the abortion rates in any period, and most estimates are below 5%.

Table A7 in the online appendix presents estimates from the analysis that uses five, partially overlapping periods. The general pattern of the estimates is the same as shown in Fig. 2: estimates are more negative and significant in earlier period than later periods. We conduct the same analysis using a sample of teens aged 18–19 (Table A8, online appendix). This group is unaffected by PI laws, and we expect estimates to be 0, on average, if the research design is valid. This is what we find.

It is clear from estimates in Fig. 2 and Table A7 (online appendix) that the effect of PI laws declined over time. This period-specific heterogeneity may stem from heterogeneous effects across states that adopt PI laws at different times and/or because of changing contraception, family structure, and distance to the nearest confidential abortion provider. In the next section, we use the synthetic control method to explore potential heterogeneity in the effect of PI laws by state.

[15]We also conduct the decomposition of the basic two-way fixed-effect estimate in Table 2 (e.g., log rates regressed on state and year fixed effects) suggested by Goodman-Bacon (2018). The results (Fig. A1, online appendix) are consistent with heterogeneous treatment effects, as we hypothesize and show evidence of shortly.

Synthetic Control Estimates

We conduct a series of case studies using synthetic control methods to examine the effect of PI laws in 14 states: Arizona, Georgia, Idaho, Maryland, Minnesota, Mississippi, Nebraska, North Carolina, Pennsylvania, South Carolina, Tennessee, Texas, Virginia, and Wyoming. These states are those that we can feasibly examine given data limitations. We require that there be data for five years prior to adoption of the PI law and three years subsequent to adoption of the law.[16] We limit the analysis to states in the CDC data because the synthetic control approach requires several pre-period observations, a requirement that is infeasible using the GI data. We present results for only 12 of 14 potential states because for two states, Idaho and Wyoming, the pre-period match between the treated state and synthetic control is poor (see the online appendix, Fig. A8 and Table A21).

In our discussion of results, we group states by whether they had a PI law before or after 1997, which is motivated by evidence in Fig. 2. The early group of states and the type of law are Georgia (1992, notice), Maryland (1992, notice), Minnesota (1990, notice), Mississippi (1993, consent), Nebraska (1991, notice), North Carolina (1995, notice), Pennsylvania (1994, consent), South Carolina (1990, consent), and Tennessee (1992, notice). The late-adopting states and type of law are Arizona (2003, consent), Texas (2000, notice), and Virginia (1997, notice). In addition, in all but one case (except as noted in Idaho and Wyoming), the match of pre-period outcomes between the treated state and the counterfactual is quite good (visually) and is always better when weights are selected using all pre-period values of the dependent variable to construct the counterfactual outcome. Therefore, we focus our discussion on results obtained using all pre-period values of the dependent variable to select weights. Results for the other method of selecting weights are presented, and magnitudes of estimates are often noticeably affected despite the largely similar quality of the match between the pre-period outcomes for the treated and synthetic control. Finally, the synthetic control analyses sometimes lack adequate statistical power, and the method of inference often alters the level of statistical significance of estimates.

Among the early adopters of PI laws, the laws have clear, substantial, and marginally statistically significant effects on the abortion rates of minors in Mississippi, North Carolina, and Pennsylvania (Figs. 3 and 4; and Tables A12 and A13, online appendix). Effects sizes are substantial. Estimates of the average post-law decline in abortions in Mississippi are 39% (abortion rate) and 53% (log rate), with *p* values ranging from .05 to .32, depending how they are constructed. In North Carolina, estimates of the average post-law decrease is 25% (abortion rate) and 29% (log abortion rate), with *p* values ranging from .09 to .26. Similarly, in Pennsylvania, estimates of the average post-law effect are 31% (abortion rate) and 39% (log abortion rate), with all *p* values equal to .11. The effect size for Mississippi is substantially larger than that reported by Henshaw (1995) and Joyce and Kaestner (2000). The large decline in abortions to minors may be related to the closing of a major abortion provider (see footnote 12), although we do not see as large of a decline for teens aged 18–19 (online appendix, Fig. A5 and Table A18). In terms of out-of-state travel, Virginia (which borders North Carolina) did not have a PI law; in the case of Pennsylvania, both nearby New

[16]For Minnesota, only four years of pre-period data were available.

York and New Jersey did not have PI laws. Estimates in Pennsylvania indicate a growing effect of the law over time, but estimates are relatively constant in North Carolina. These increasing or persistent effects of PI laws are inconsistent with the hypothesis that minors adapt to the law (Kane and Staiger 1996). Finally, Pennsylvania required parental consent, whereas North Carolina required parental notification.

Four of the other early adopting states experienced declines in abortion to minors post-law that were smaller and not statistically significant. In Tennessee (Fig. 5; and Table A11, online appendix), the average post-law difference is −4% (abortion rate) and −10% (log abortion rate); in Minnesota (Fig. 6; Table A9, online appendix), the average post-law decline is approximately 10% for both measures of the abortion rate. In South Carolina (Fig. 6; Table A9, online appendix), the average post-law decline was about 11% for both measures of abortion rate; and in Nebraska (Fig. 7; Table A10, online appendix), the analogous estimates are −19% (abortion rate) and −23% (log abortion rate). The effect size reported for Minnesota is similar to that reported by Ellertson (1997), and the effect size for South Carolina is similar to that reported by Joyce and Kaestner (2001). In these states, there are differences in the type of law and differences in the distance to a nearest confidential provider, but little systematic relationship between the magnitude of the effects and these potential moderators of the effect of PI laws. For example, Nebraska experiences a relatively large decline in abortions after the law, but it has a less stringent parental notification law.

Among early adopting states, only Georgia (Fig. 7; Table A10, online appendix) and Maryland (Fig. 5; Table A11, online appendix) do not experience a decline in abortions to minors relative to the counterfactual. The absence of an effect in Maryland is consistent with the nature of the law that allowed a physician to bypass the notification requirement if the minor is deemed mature. Minors in Maryland also had easy access to abortion services in Washington, DC, Pennsylvania, and Delaware at the time.

For later-adopting states, there is evidence of a decline in the abortion rates of minors in all three states (Virginia, Texas, and Arizona), but estimates are smaller than for most early adopters and are not statistically significant. In Virginia (Fig. 4; and Table A13, online appendix), estimates indicate a post-adoption decline of 8% for both abortion measures and p values of estimates range from .05 to .55. Adoption of a PI law in Texas (Fig. 8; and Table A14, online appendix) is associated with a 16% decline in abortions, and adoption of a law in Arizona (Fig. 8 and Table 14, online appendix) is associated with an 11% decline; in both cases, though, estimated p values are quite large. The effect size reported here for Texas is consistent with that reported by Joyce et al. (2006).

We also conduct the same state-specific analyses for women aged 18–19. These results are presented in Figures A2–A7 in the online appendix (and in Tables A15–A20). The great majority of the estimated effects for teens ages 18–19 are small, and estimates tend to be positive and not close to being statistically significant. However, there is one exception. In Texas (Fig. A7 and Table A20, online appendix), we observe a decline in abortions among those aged 18–19 that is similar to that among minors. The lack of a clear null result for older teens suggests that the results for minors in Texas may be confounded by unobservable factors.

Conclusion

State parental involvement laws that limit the ability of minors to obtain an abortion have potentially serious consequences. Besides the effect of such laws on abortion, these laws may affect unintended births, contraception use, and (through changes in these outcomes) the human capital investments of young women. In this article, we examined the effect of the PI laws on the abortion rates of minors. We added to the existing literature in several ways, most notably by assessing whether the effect of PI laws vary significantly across states and over time.

Results from our study indicated that PI laws adopted prior to the mid-1990s reduced abortion among minors but that after this time, the adoption of PI laws had little effect, on average, on abortions to minors. Estimates suggest that earlier PI laws reduced abortions to minors by between 15% and 20%. We also found little relationship between the effect of PI laws and the distance to a state without a PI law. This finding suggests that the ability to travel out of state to avoid complying with a PI law is not sufficient to offset the constraint such laws impose on abortion to minors. Third, we found evidence to suggest substantial state heterogeneity in the effect of PI laws, although the variation in effects did not appear to be related to the type of law or whether surrounding states had laws that may have made it more difficult for minors to travel out of state to avoid compliance. In addition, our state-specific case studies were underpowered to reliably detect small to moderate effect sizes, and therefore we view these results as more suggestive than definitive.

The finding of substantial heterogeneity in the effects of PI laws by year and state is an interesting result. We were unable to identify the causes of these heterogeneous effects, although we provided evidence that this heterogeneity is not likely related to the ability to travel out of state or the type of law. One possibility is that the heterogeneity across states and over time is related to differential changes in the composition of teens at risk of abortion resulting from differences in the use of contraceptive technology, access to medication abortion, and changes in parent-child relationships. Differences in the availability of judicial and physician bypass may also play an important role in some settings. Other causes are also possible. This is clearly an important area for future research to investigate.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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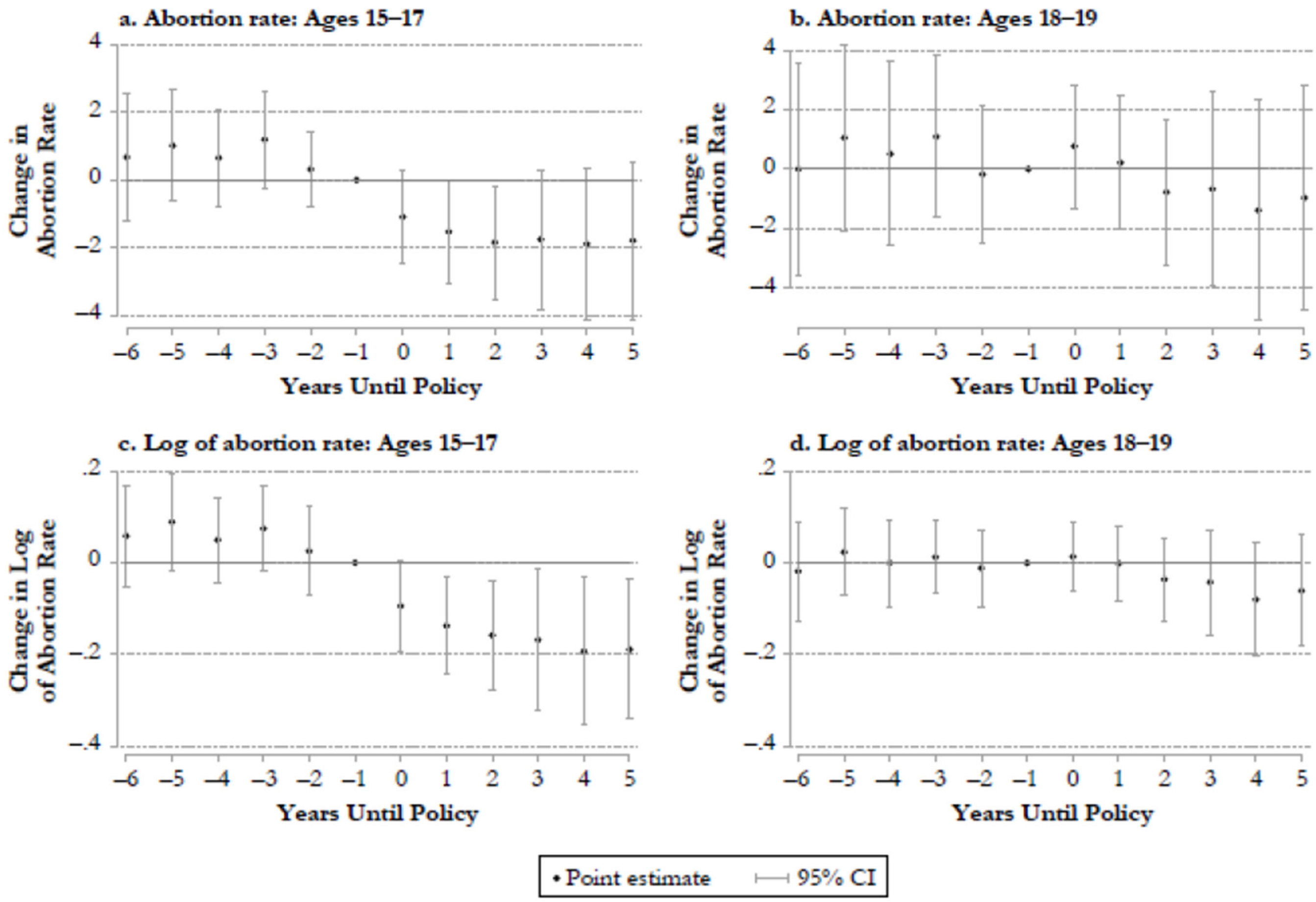

Fig. 1.

Event study estimates of effect of PI law on abortions. The regression model controls for state and year fixed effects; state unemployment rate; state median wage; state share female aged 15–19 who are Black, non-Hispanic; and state share female aged 15–19 who are White, Hispanic; as well as each of these controls interacted with year indicators. Regressions are weighted by state population of females of the indicated age. Standard errors are clustered at the state level.

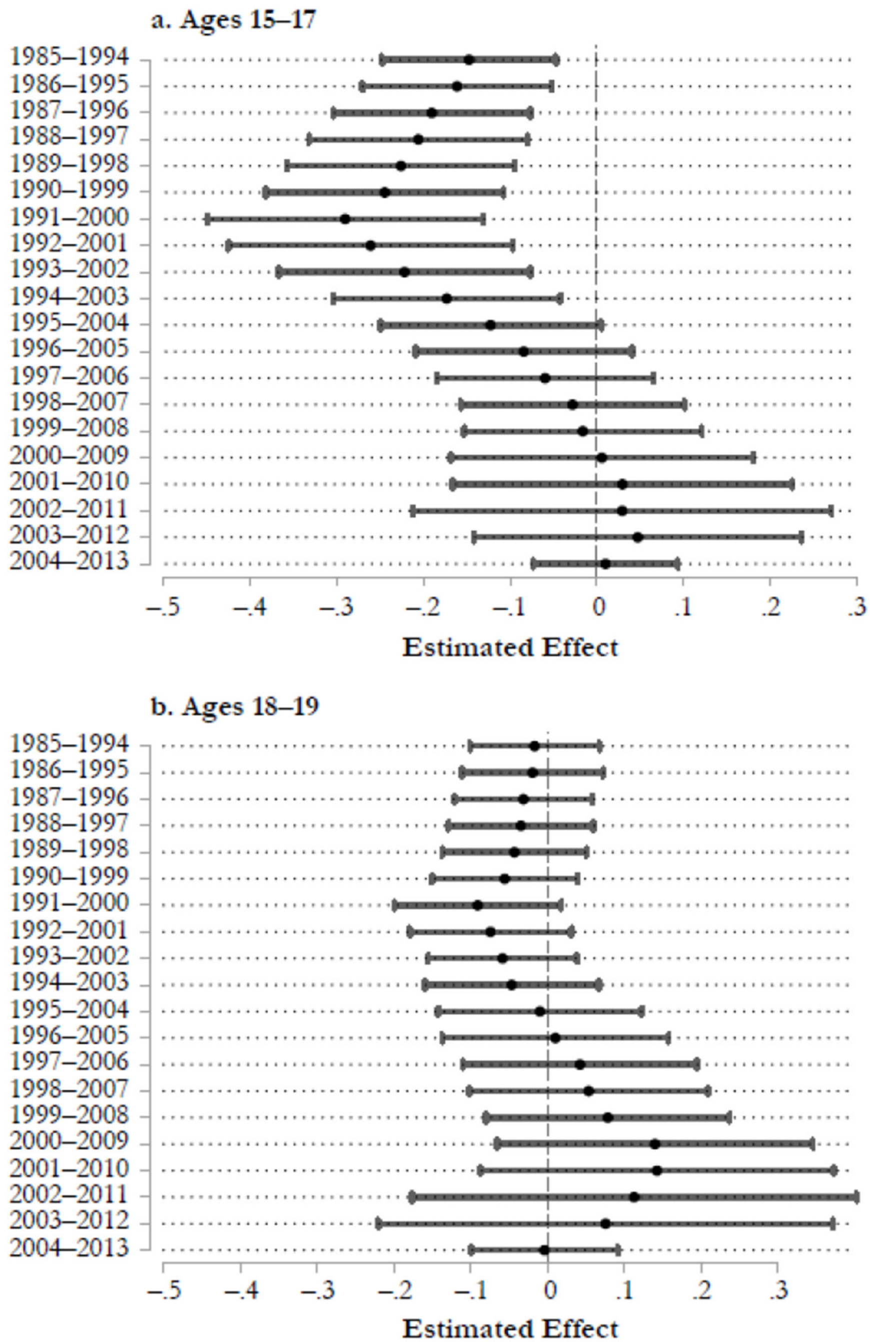

Fig. 2.

Estimated effect of PI laws on log of abortion rate using a 10-year moving average. Estimates are generated using a 10-year window of data incremented by one year for each estimate as indicated on the *y*-axis. Estimates are generated using Model B, which includes state and year fixed effects; state unemployment rate; state median wage; state share female aged 15–19 who are Black, non-Hispanic; and state share female aged 15–19 who are White, Hispanic; and interactions between these covariates and year fixed effects. Regressions are weighted by state population of females of the indicated age. Standard errors are clustered at the state level. *Source:* CDC Abortion Surveillance Reports 1985–2013.

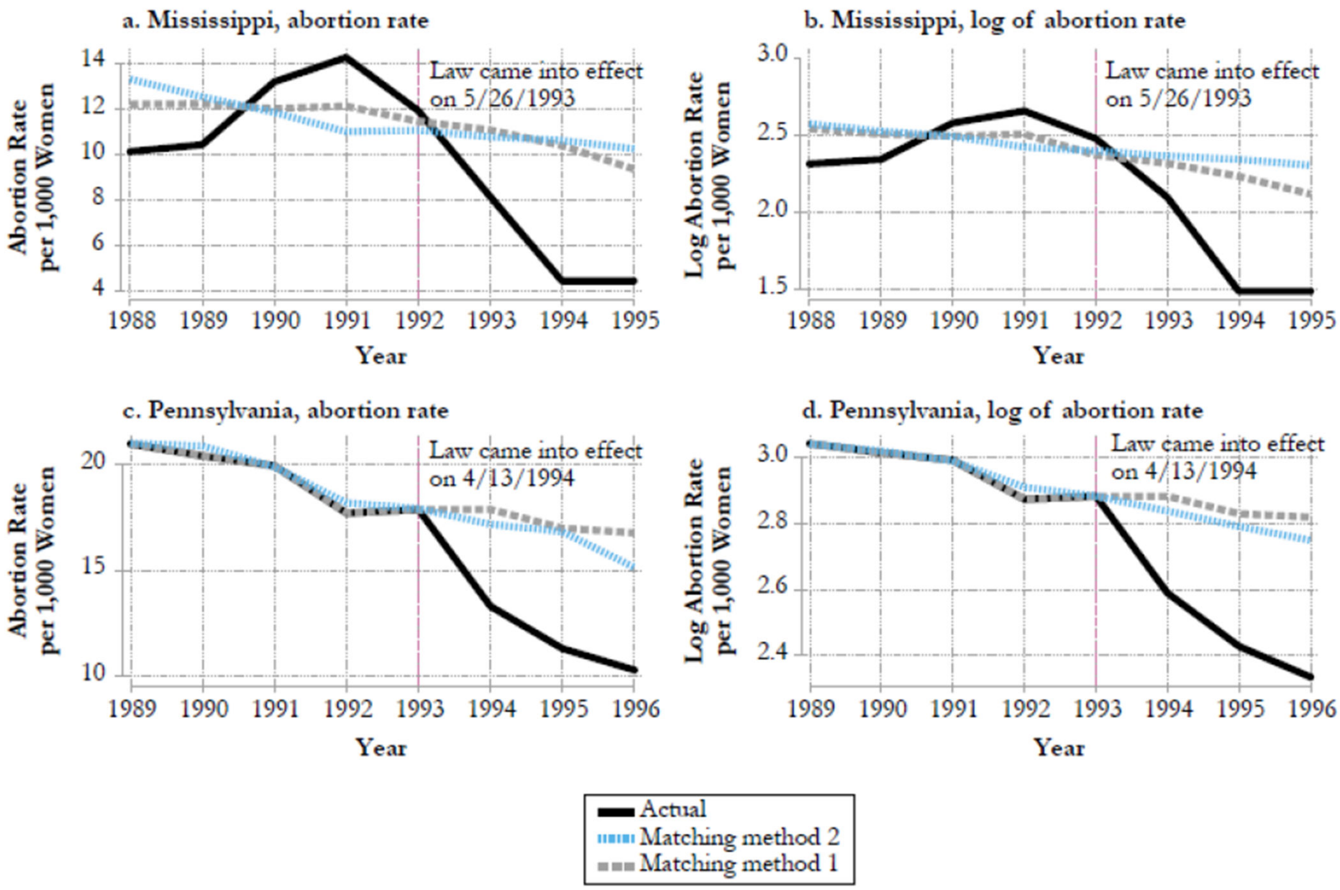

Fig. 3.

Synthetic control estimates of effect of PI laws on abortion to minors: States adopting in 1993–1994. The figure shows the path of the log of the abortion rate per 1,000 females for the state "treated" with a PI law and the counterfactual path of the outcome derived from two different synthetic control specifications as indicated. Method 1 uses values of the dependent variable in all pre-period years to match and derive weights for control states. Method 2 uses the values of the dependent variable in odd pre-period years (i.e., $t-1$, $t-3$, $t-5$); each pre-period value of the unemployment rate; and the average value of state median wage and shares of Black, non-Hispanic and White, Hispanic females ages 15–19. Dashed line indicates year before policy began ($t-1$). When a PI law went into effect later than June 30 of a calendar year, the following year is used as t_0.

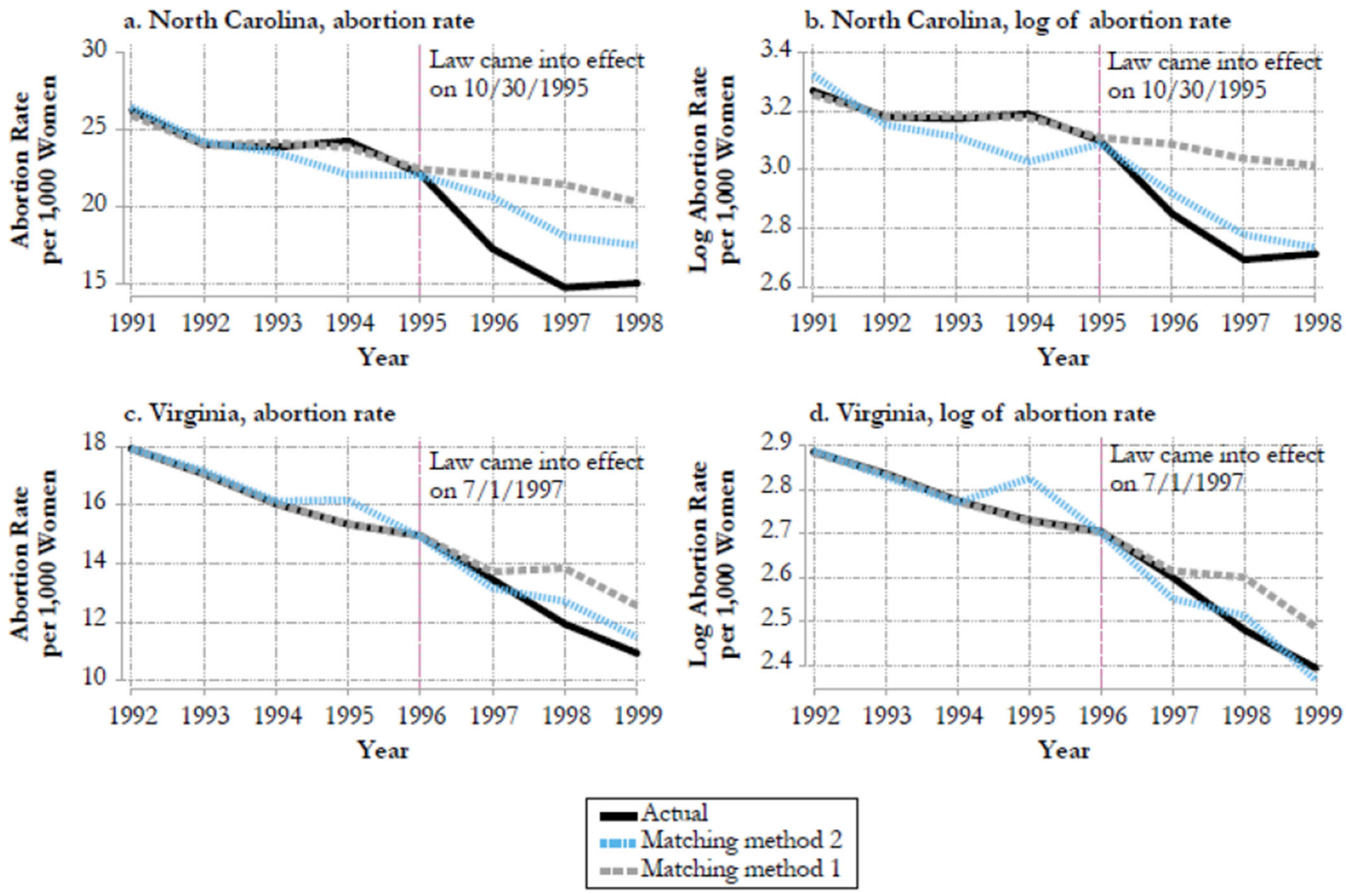

Fig. 4.

Synthetic control estimates of effect of PI laws on abortion to minors: States adopting in 1995–1997. See notes to Fig. 3.

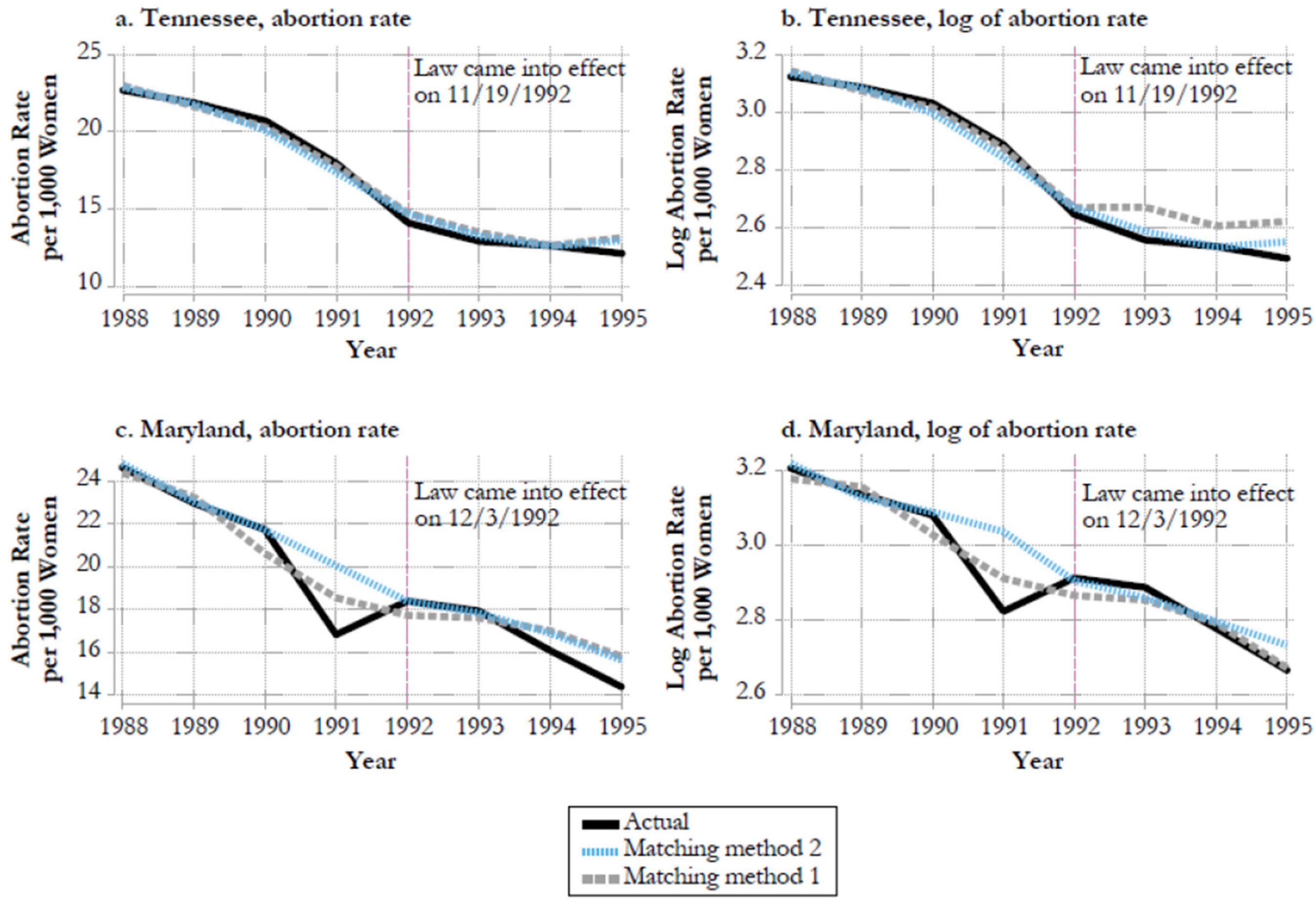

Fig. 5.

Synthetic control estimates of effect of PI Laws on abortion to minors: States adopting in 1992. See notes to Fig. 3.

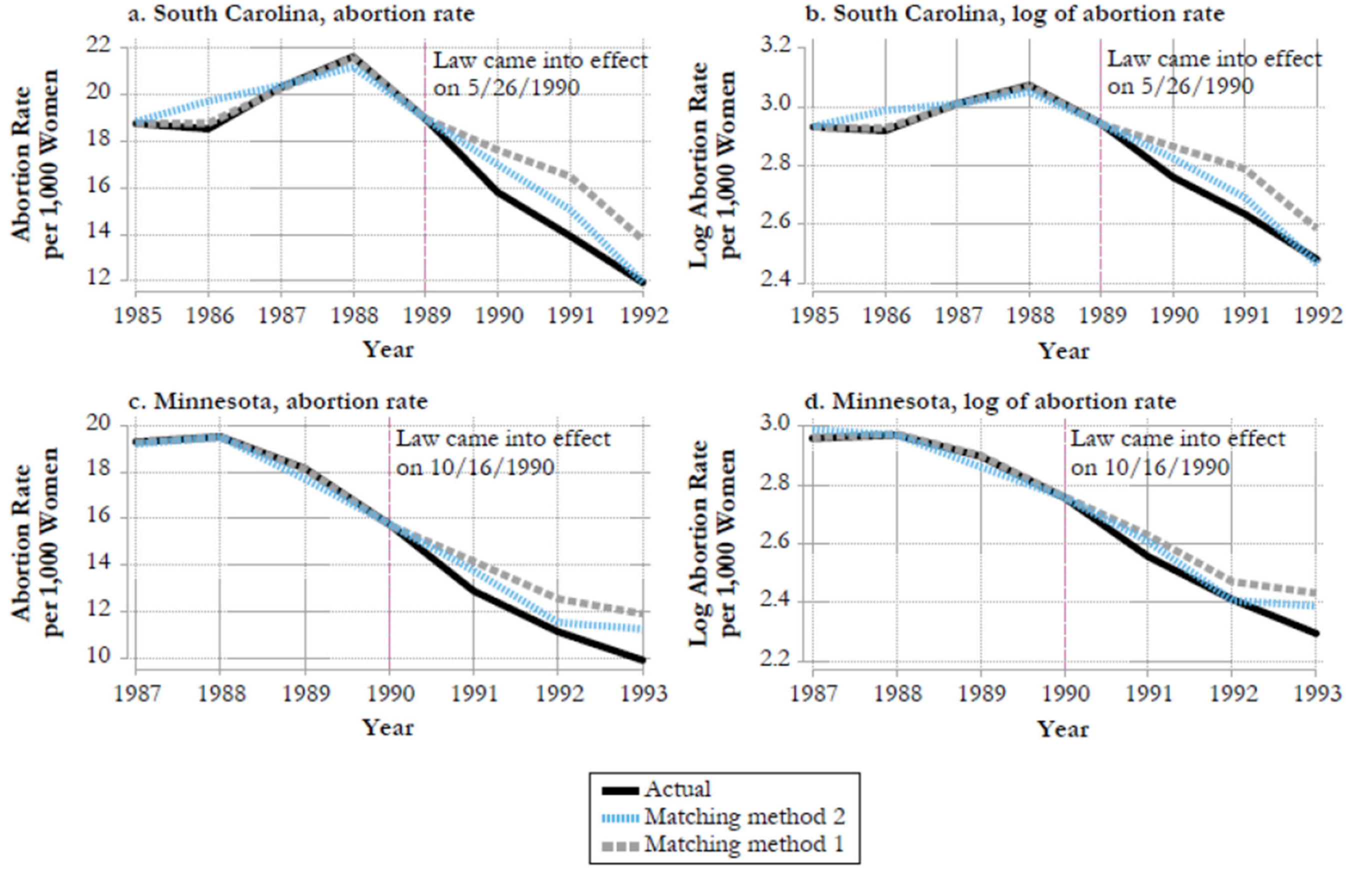

Fig. 6.

Synthetic control estimates of effect of PI laws on abortion to minors: States adopting in 1990. See notes to Fig. 3.

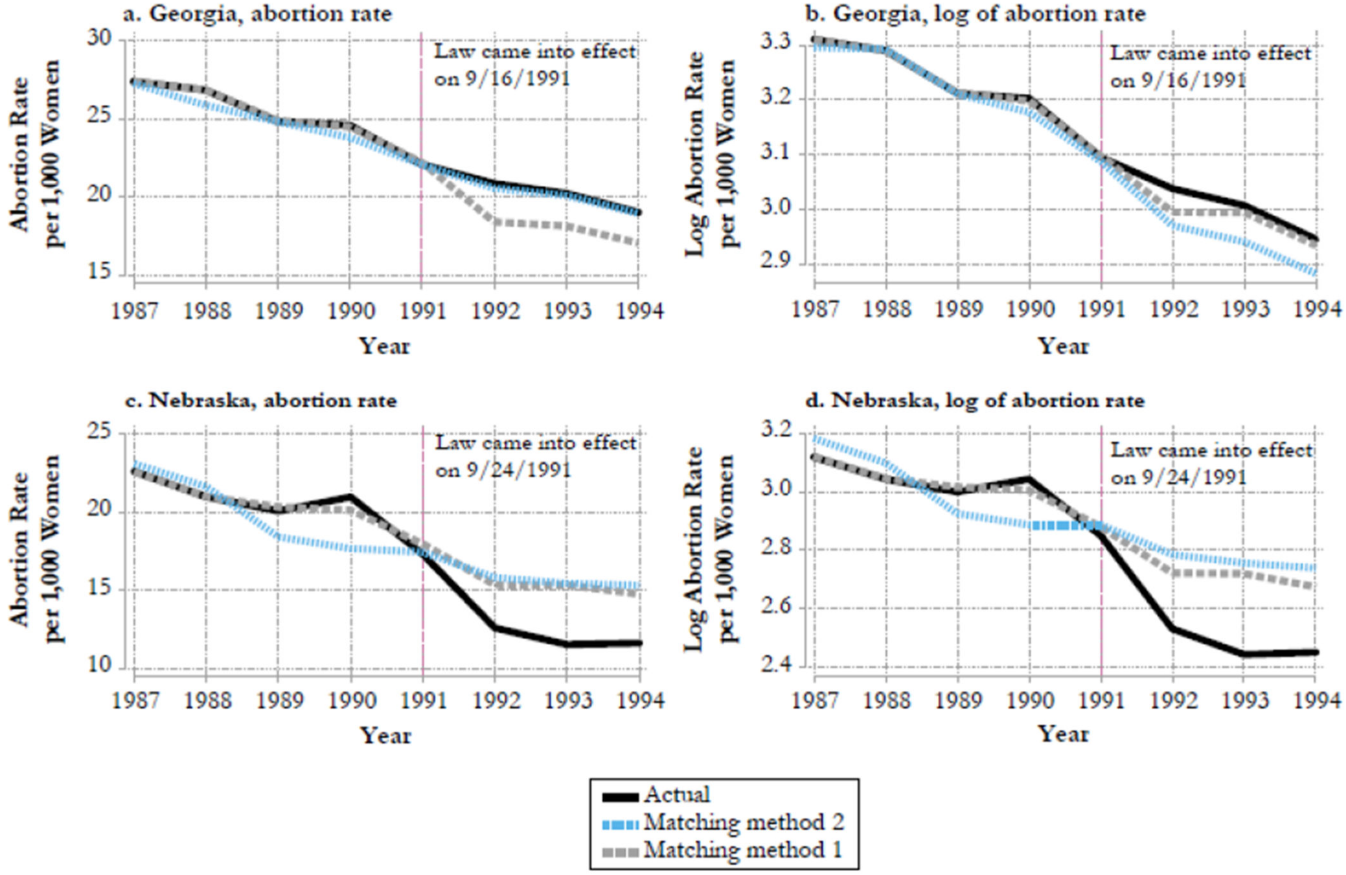

Fig. 7.
Synthetic control estimates of effect of PI laws on abortion to minors: States adopting in 1991. See notes to Fig. 3.

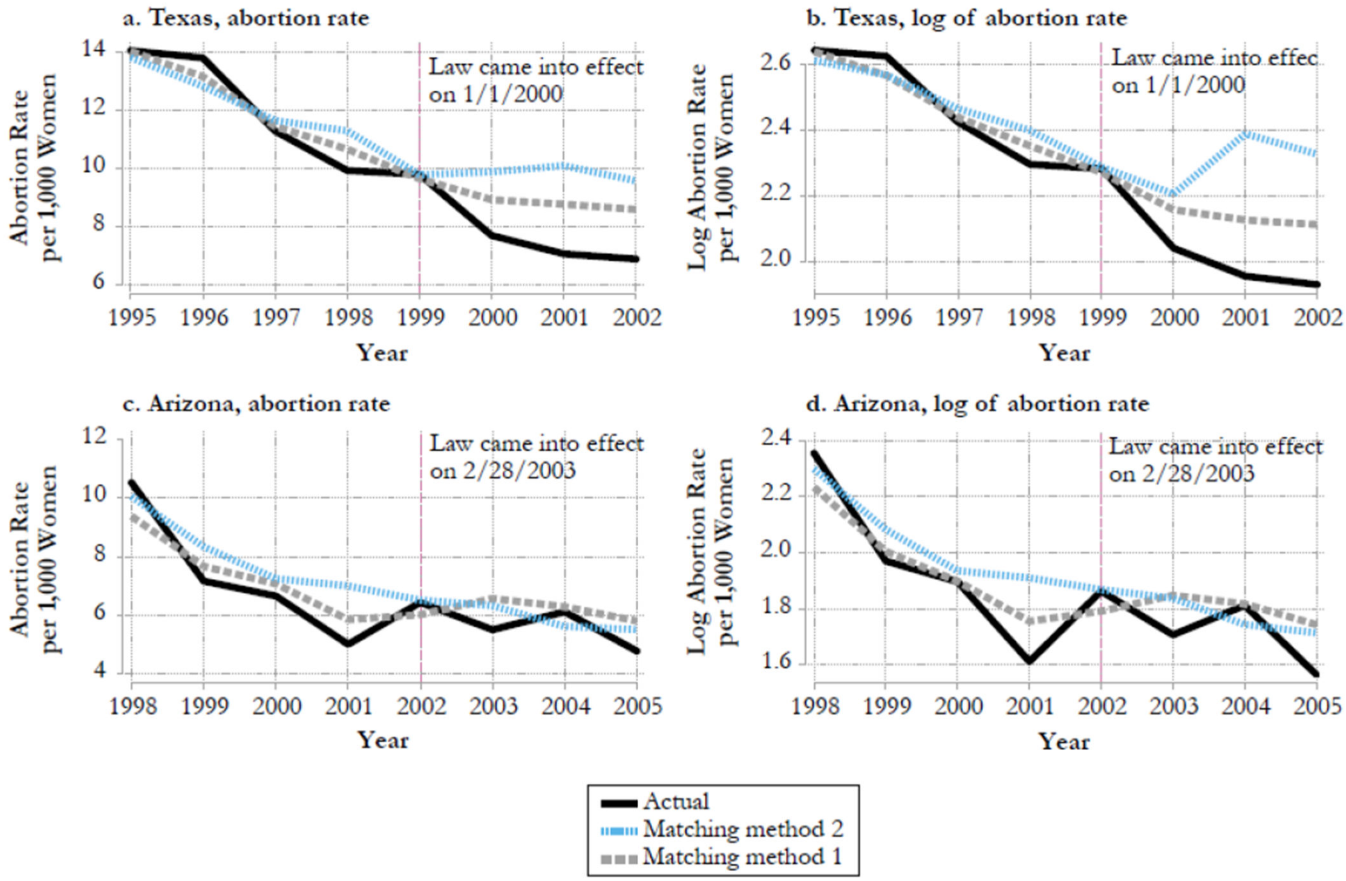

Fig. 8.

Synthetic control estimates of effect of PI laws on abortion to minors: States adopting in 2000–2003. See notes to Fig. 3.

Table 1

Number of abortions to minors and older teens in three states pre- and post-PI law by state of residence and occurrence

	Mississippi		North Carolina		Texas	
	Minors	**Teens Aged 18–19**	**Minors**	**Teens Aged 18–19**	**Minors**	**Teens Aged 18–19**
In-State, Resident						
Pre-PI law	663	780	3,146	3,860	4,769	8,578
Post-PI law	335	491	2,431	3,619	3,829	7,835
Δ	−328	−289	−715	−241	−940	−743
(%Δ)	(−68.2)	(−46.2)	(−25.8)	(−6.4)	(−22.0)	(−9.1)
In-State, Nonresident						
Pre-PI law	187	209	544	501	173	343
Post-PI law	14	27	330	484	149	281
Δ	−173	−182	−214	−17	−24	−62
(%Δ)	(−259.2)	(−204.6)	(−50.0)	(−3.5)	(0.7)	(−9.6)
Out-of-State Resident						
Pre-PI law	145	217	90	76	6	8
Post-PI law	293	280	121	71	28	19
Δ	148	63	31	−5	22	11
(%Δ)	(70.3)	(25.4)	(29.6)	(−6.8)	(154)	(86.5)
Total Resident						
Pre-PI law	808	997	3,236	3,936	4,775	8,586
Post-PI law	628	771	2,552	3,690	3,857	7,854
Δ	−180	−226	−684	−246	−918	−732
(%Δ)	(−25.4)	(−25.7)	(−23.7)	(−6.5)	(−21.4)	(−8.9)
Total Occurrence						
Pre-PI law	850	989	3692	4361	4942	8921
Post-PI law	349	518	2761	4107	3978	8116
Δ	−501	−471	−931	−254	−964	−805
(%Δ)	(−89.0)	(−64.7)	(−29.1)	(−6.0)	(−21.7)	(−9.5)

Notes: Abortion is categorized in five ways: (1) abortions obtained in state by residents of the state, (2) abortions obtained in state by nonresidents, (3) abortions to residents obtained out of state, (4) total abortions to residents, and (5) total abortions that occurred in state irrespective of residency. Pre–PI law in Mississippi, North Carolina, and Texas is 1991, 1994, and 1999, respectively; post–PI law for these states is, respectively, 1994, 1996, and 2000.

Sources: Abortions to residents and nonresidents that occurred in Mississippi, North Carolina, and Texas are from state health departments in the three states. Data on residents of Mississippi, North Carolina, and Texas performed in other states came from individual level records in Alabama, Arkansas, Colorado, Georgia, Kansas, Missouri, Oklahoma, South Carolina, Tennessee, Utah, and Virginia. Data from New Mexico are from Joyce et al. (2006). Percentage changes are calculated as ln(pre−PI law) − ln(post−PI law).

Table 2

Estimates of the effect of parental involvement laws on abortions among minors, 1985 to 2013

	Abortion Rate				Log of Abortion Rate			
1985 to 2013	CDC Occurrence	GI Residence	CDC Occurrence GI Years	GI Residence CDC State-Years	CDC Occurrence	GI Residence	CDC Occurrence GI Years	GI Residence CDC State-Years
Model A	−1.907*	1.240	−1.895*	−0.742	−0.241**	−0.159*	−0.316**	−0.259**
	(0.803)	(1.802)	(0.857)	(1.010)	(0.067)	(0.071)	(0.084)	(0.070)
Model B	−1.916*	−1.592	−1.807*	−1.744*	−0.181**	−0.129*	−0.220**	−0.209**
	(0.846)	(1.389)	(0.827)	(0.736)	(0.058)	(0.055)	(0.059)	(0.049)
Model C	−2.305**	−1.830*	−3.187**	−3.345**	−0.162**	−0.120**	−0.240**	−0.225**
	(0.615)	(0.787)	(0.741)	(0.619)	(0.048)	(0.044)	(0.066)	(0.047)
Model D								
PI law main effect (<100 miles)	−3.185**	−1.840	−3.810**	−3.403**	−0.213*	−0.104	−0.267*	−0.229*
	(0.975)	(1.280)	(0.996)	(0.936)	(0.102)	(0.110)	(0.110)	(0.096)
Law × Distance (100–199 miles)	0.766	−0.124	0.431	0.380	0.065	−0.015	0.019	0.040
	(0.940)	(1.270)	(0.965)	(1.154)	(0.102)	(0.120)	(0.121)	(0.117)
Law × Distance (200–299 miles)	1.624	−0.558	1.028	0.188	0.093	−0.021	0.032	0.021
	(1.201)	(1.121)	(1.168)	(0.814)	(0.118)	(0.106)	(0.131)	(0.096)
Law × Distance (300–399 miles)	1.002	−0.078	0.721	0.151	0.084	−0.027	0.020	−0.004
	(1.120)	(1.155)	(1.047)	(0.928)	(0.103)	(0.111)	(0.117)	(0.102)
Law × Distance (400+ miles)	1.594	0.491	1.056	0.742	0.076	−0.020	0.016	0.019
	(1.022)	(1.078)	(0.844)	(0.820)	(0.102)	(0.105)	(0.105)	(0.100)
Mean of Dependent Variable	11.9	15.4	10.9	15.4	11.9	15.4	10.9	15.4
Number of Observations	1,114	463	386	386	1,114	463	386	386

Notes: Model A includes state and year fixed effects; state unemployment rate; state median wage; state share of females aged 15–19 who are Black, non-Hispanic; and state share of females aged 15–19 who are White, Hispanic. Model B adds interactions between covariates and year fixed effects to Model A. Model C adds a state-specific linear time trend to Model A. Model D is Model C but with the effect of PI law allowed to differ by distance to nearest state without a PI law. Distance is measured between the most populous city in each state. Regressions weighted by state population of females aged 15–17. Mean of dependent variable is average across sample period. Standard errors, clustered at the state level, are shown in parentheses.

Source: CDC Abortion Surveillance Reports 1985–2013 and Guttmacher Institute estimates of number of abortions by state residents for years 1985, 1988, 1992, 1996, 2000, 2005, 2008, 2010, 2011, 2013.

* $p < .05$

** $p < .01$