



## The Impact of Laws Requiring Parental Involvement for Abortion: A Literature Review

Amanda Dennis, Stanley K. Henshaw, Theodore J. Joyce, Lawrence B. Finer and Kelly Blanchard

### HIGHLIGHTS

- In 2008, 34 states had laws in effect that mandated parental involvement in minors' abortions.
- A literature search identified 29 studies of the impact of these laws on a range of outcomes.
- The clearest documented impact of parental involvement laws is an increase in the number of minors traveling outside their home states to obtain abortion services in states that do not mandate parental involvement or that have less restrictive laws.
- Many studies reported a decline in minors' abortion rate associated with parental involvement laws. However, most of these studies did not measure abortions among minors who leave the state, or stop coming into the state, because of the law. Studies in Mississippi and Massachusetts, which incorporated data on minors traveling out of state, found no effect on the abortion rate, while one in Texas suggested that parental involvement laws lower abortion rates and raise birthrates if minors must travel long distances to access providers in states without such laws.
- Several state studies found no short-term impact on pregnancy rates.
- Many studies had serious limitations, including incomplete data, inadequate controls for factors other than the imposition of the law and lack of statistical power because they measured outcomes among all women or teenagers rather than minors. Several reported anomalous findings that indicate confounding by uncontrolled variables.
- Three studies reported large impacts of parental involvement laws on infant and child health. These findings are implausible, given the small or undocumented increase in unintended childbearing and the limited data on infant and child well-being.
- Future research should incorporate straightforward designs with minor-specific data. Researchers should document prelaw trends in outcomes among those exposed and unexposed to the laws. They also should clearly discuss expected outcomes, statistical power and the plausibility of their findings.



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# Background

Approximately 7% of abortions in the United States are provided to minors.\*<sup>1</sup> A majority of pregnant minors who seek abortion indicate that their parents are aware that they are doing so. Nonetheless, some of these parents do not approve of their daughters' decisions, and some minors are unwilling to tell their parents.<sup>2-4</sup> Since the Supreme Court's decisions in *Planned Parenthood of Central Missouri v. Danforth* in 1976 and *Bellotti v. Baird* in 1979, it has been constitutional for states to require minors seeking abortions to obtain parental consent or to notify their parents, provided that there is an alternative approval mechanism such as a court bypass procedure. Over the years, an increasing number of states have instituted such requirements; in 2008, a total of 34 states<sup>†</sup> had parental involvement laws in effect. An additional seven states had laws whose enforcement had been enjoined (i.e., that were legally prohibited from taking effect) owing to conflicts with state constitutions or the federal constitution.<sup>5</sup>

Parental involvement laws include both parental consent and parental notification requirements. The laws take varied forms in different states, although many require the consent or notification of just one parent, usually at least 24 or 48 hours before the procedure. A handful of states require the involvement of both parents, and six states allow certain other adult relatives (such as grandparents) to approve an abortion.

Proponents of parental involvement laws argue that excluding parents from their children's contraceptive and pregnancy decisions will harm minors seeking terminations or infringe on the rights of parents. They also claim the laws produce other benefits, including better family

communication and reduced pregnancy rates. An implicit purpose of the laws is to prevent abortions by encouraging minors to continue their unwanted pregnancies. Opponents of parental involvement laws, on the other hand, argue that these laws may limit teenagers' access to abortion services, and that requiring parental involvement may, for at least some teenagers, lead to family violence, force minors to continue unwanted pregnancies or delay abortions, thereby increasing the risk of medical complications related to the procedure.

***Since the Supreme Court's decisions... in 1976 and... 1979, it has been constitutional for states to require minors seeking abortions to obtain parental consent or to notify their parents, provided that there is an alternative approval mechanism such as a court bypass procedure.***

As a matter of U.S. constitutional law, if a state requires parental involvement in a minor's abortion, it must also provide her with the opportunity to go directly to a court to waive the parental notice or consent requirements, and authorize the abortion. Such a waiver is permissible if the court finds that the minor is mature and well informed enough to make the abortion decision on her own, or that having the procedure would be in her best interest.<sup>6</sup> The availability of judicial bypasses varies by state. In some states, streamlined systems make it easier for teenagers to obtain a waiver, although even in these states, some rural minors have to travel a long distance to find a judge willing to hear their case.<sup>7</sup> Older minors (i.e., 16–17-year-olds) are more likely to seek a bypass than to consult their parents, whereas the opposite is the case for younger minors.<sup>7</sup>

However, many teenagers are unaware of the need for parental consent or the availability of a bypass procedure.<sup>8</sup> The laws' influence on minors' behavior depends in part on minors' awareness of the requirements. Some assume that parental consent is required even though their state has no such requirement, and some believe that abortion is illegal except under special circumstances.<sup>9</sup> Teenagers often learn of the requirements and the possibility of judicial bypass when they contact an abortion clinic.

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\*In general, minors are individuals younger than age 18; however, the specific age-group affected by parental involvement laws varies by state. In all but a few states, parental involvement laws pertain to unemancipated women younger than 18 years of age. In South Carolina, the law pertains to women younger than 17 years of age, and in Delaware, to women younger than 16 years of age. In a few states, there is no exception for emancipated minors.

†Alabama, Arizona, Arkansas, Colorado, Delaware, Florida, Georgia, Idaho, Indiana, Iowa, Kansas, Kentucky, Louisiana, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Nebraska, North Carolina, North Dakota, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, West Virginia, Wisconsin and Wyoming.

### **DEFINITIONS OF REPRODUCTIVE OUTCOMES USED IN STUDIES OF THE IMPACT OF PARENTAL INVOLVEMENT LAWS**

- **Abortion rate:** The number of abortions per 1,000 women in the age-group.
- **Abortion ratio:** The number of abortions per 1,000 pregnancies in the age-group.
- **Birthrate:** The number of births per 1,000 women in the age-group.
- **Pregnancy rate:** The sum of induced abortions and live births divided by the age-specific population of women.
- **Second-trimester abortion rate:** The number of second-trimester abortions per 1,000 women. In this review, second-trimester abortions are defined as those occurring after 12 weeks of gestation, measured from the last menstrual period.

used in studies are given in the box.) We have considered abortion restrictions as the predictor variable and the outcomes assessed as the dependent variables. The aim of this paper, beyond synthesis of the available literature, is to critically review the methodology used in current research and the outcomes addressed in order to highlight effective research designs and identify the data needed to accurately assess the impact of these restrictions in the future.

Some minors travel to other states with no, or at least less restrictive, parental involvement laws in order to obtain an abortion. To travel out of state, a minor must have access to transportation and must be within a reasonable distance of a state with less restrictive laws. The degree to which minors exercise this option varies by age, socioeconomic status and access to public transportation. The ability to travel out of state is an important factor in the studies in this literature review because if a large number of minors travel to states without parental involvement laws, it becomes important to know the number of minors who obtain abortions in those states—information that is generally unavailable.

In this literature review, we summarize the results of studies that have evaluated the impact of parental involvement laws on a multitude of outcomes, including sexual and reproductive behaviors of minors (sexual activity, contraceptive use, abortion rate and ratio, second-trimester abortion rate, birthrate and pregnancy rate), the number of female-headed households and the health of infants and children. (Definitions of reproductive outcomes

# Methods

We identified studies on the impact of parental involvement laws using four search engines: Google Scholar, PubMed, Popline and Web of Science. The search term combinations used were “abortion AND parental notification,” “abortion AND parental involvement” and “abortion AND parental consent.” We searched for all articles published after 1900.

We scanned the titles of the articles returned from the database searches and eliminated ones that were obviously not relevant. We then collected and reviewed abstracts of the remaining articles to identify those that were eligible for inclusion in the review. We selected articles that were published in English, focused on the United States, constituted original research and provided details on the impact of parental involvement laws on reproductive behavior or other outcomes. We examined the citations in the articles selected to identify additional papers to be considered for inclusion in the review. We also consulted with experts in the field and gathered articles that received media attention during the time of our review (September 2007 through December 2008). In all, 29 studies were selected and are reviewed below and summarized in Table 1 (page 31).

To frame the discussion, we first present an overview of the methodological challenges facing researchers who analyze the impact of parental involvement laws. This includes a discussion of the expected outcomes, strengths and limitations of the data, and efforts to reduce the effect of unknown or uncontrolled influences. We then review the literature by first looking at studies that evaluate reproductive behaviors and outcomes from a national perspective—that is, those based on data from large numbers of states or national population samples. Next, we look at literature that examines groups of states, then at studies on single states and cities. We then review the literature that focuses on female-headed households and infant and child health outcomes. Within each of these sets, we examine the literature chronologically. All descriptions of parental involvement laws in this review are presented in the past tense, as they represent the formulation of the law at the time of the research and as reported by the researcher. Because such laws are sometimes revised, even the more recent papers may not reflect the current state of parental involvement laws.

# Methodological Challenges in Assessing the Impact of Parental Involvement Laws

Randomized social experiments are often considered the gold standard in social science research. In such studies, participants are randomly assigned to a treatment group or a control group, guaranteeing that the groups will be similar to each other. Studies that evaluate parental involvement laws are unable to randomize in this manner and often rely on a “natural experiment.” Researchers must therefore be creative about sources and methods of data collection and analysis, and must determine the best techniques for establishing appropriate comparison groups. Because of these limitations, the estimated impact of parental involvement laws on abortion rates and birthrates of minors varies substantially across studies, as researchers use various ways to overcome inherent limits of the research. In this section, we outline the methodological challenges that confront researchers in the evaluation of parental involvement laws.

## Conceptual Issues

A high-quality study will contain, at a minimum, a clear theoretical underpinning, an evaluation of the statistical power of the design in relation to the size of the outcome expected and an assessment of the real-world likelihood of the research findings. Beginning with a discussion of relevant conceptual issues enables the researcher to generate a clear set of testable hypotheses. For example, a commonly hypothesized effect of a parental involvement law is an initial decline in the abortion rate of minors. Whether this would translate to a rise in birthrates depends on whether analysts believe minors would continue more unintended pregnancies or make greater efforts to avoid pregnancy.

Researchers should also discuss the magnitude, or power, of the outcomes they expect to find. For instance, approximately 60% of minors say their parents know about their pregnancy and desire to have an abortion, even in states without parental involvement laws.<sup>3</sup> Thus, if a parental involvement law is associated with a decline in the abortion rate of all minors of, say, 3.0 per 1,000, then the abortion rate must have fallen by 7.5 among those who did not involve a parent (3.0/[1 – 0.60]). The plausibility of such a decline should be defended in light of the circumstances associated with the law. For instance, did minors have access to abortion services in nearby states

without parental involvement laws? Was there a change among minors in other outcomes, such as second-trimester abortion rates and birthrates? And what proportion of minors used a judicial bypass?

In addition, racial and ethnic differences in parental involvement can be used to assess the credibility of the results. Younger minors and black minors are more likely to involve their parents than are older minors and white minors.<sup>3,10,11</sup> The effect of parental involvement laws, therefore, will likely be greater among 17-year-olds than 15-year-olds, and greater among whites than blacks.

## Data Issues

Arguably the biggest difficulty in evaluating parental involvement laws is the lack of population-based data on abortions.\* The available data come from three main sources: the Guttmacher Institute, the Centers for Disease Control and Prevention (CDC) and state health departments.

The Guttmacher Institute’s periodic survey of abortion providers yields the most widely accepted estimate of the number of abortions by state of occurrence.<sup>12</sup> These data have two important limitations for the evaluation of parental involvement laws: Abortions are tallied according to the state in which they occur and not according to the state in which a woman resides, and data are not available by age. To overcome these limitations, Guttmacher researchers have applied the distribution of abortions by state and age as reported by the CDC to estimate the number of abortions among minors based on their survey. They also use information from the CDC on the proportion of abortions provided to nonresidents in a state along with other sources to estimate abortions by state of residence. However, the Guttmacher analysts caution against using these age-specific resident abortion rates to evaluate parental involvement laws, since the algorithm does not

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\*Almost all studies of parental involvement laws use one of the three sources of data described in this section. A few have used data from the National Longitudinal Survey of Youth (NLSY) or the National Survey of Family Growth (NSFG). However, the NLSY survey samples include few pregnancies among minors, and abortions are greatly underreported in both surveys (source: Jones EF and Forrest JD, Underreporting of abortions in surveys of U.S. women: 1976 to 1988, *Demography*, 1992, 29(1):113–126).

account for minors' travel out of state for an abortion in response to these laws. This is an important drawback, which is often ignored.

The CDC collects data from state health departments and reports abortions by state, year and several demographic factors: age, race, marital status, gestational age, type of procedure, parity and previous induced abortions. There are two advantages to the CDC data. First, abortions are available by single year of age for teenagers 15–19 years old for a majority of states. Second, data are available annually, whereas the Guttmacher Institute reports data periodically. As with data from the Guttmacher Institute, however, the CDC reports abortions by state of occurrence. In addition, the total number of abortions as reported by the CDC is approximately 15% lower than that reported by the Guttmacher Institute, and the degree of undercounting varies substantially by state.<sup>13</sup> Further, not all states report abortions by age to the CDC; California and Florida are two populous and notable exceptions. Finally, the limited cross-tabulation of the data prevents analyses by two characteristics, such as race and age or race and gestational age.

The third major source of data is state health departments. The CDC uses these same data in its surveillance reports. The major advantage of obtaining them directly from the state is that some states make available to researchers individual-level data on induced abortions, which allows for a more refined aggregation of data than what is available from the CDC. This can substantially improve the internal and external validity of an analysis (the ability to measure what one sets out to measure).<sup>\*</sup> The two major drawbacks to these data are similar to those stated above: Completeness of reporting varies by state, and residents who leave their state for an abortion are rarely counted by the state in which they reside.<sup>†</sup> However, the latter draw-

back can be overcome if researchers are able to secure data from neighboring states.

The lack of data by state of residence is a major limitation. Studies of parental involvement laws based on data by state of occurrence will overestimate the decline in abortions associated with the law, not only because of resident minors' leaving the state for an abortion, but because of declines in nonresident minors' entering the state for an abortion. Studies of parental involvement laws in the 1980s and early 1990s were particularly vulnerable to this source of bias, since only 13 states had such laws in 1988. This made travel outside one's state of residence to another state without a law feasible. More recent evaluations are less vulnerable to this source of bias because 34 states, including almost all states in the South and Midwest, now have parental involvement laws. This makes traveling to a state without a law very challenging.

Finally, an additional source of data uncertainty is the classification of state parental involvement laws. Some states allow certain adult relatives other than parents to satisfy the requirements, and it is not clear how researchers have classified these states or how the states should be classified in an analysis. For example, at least one researcher has classified Connecticut as requiring parental involvement, although the law specifies only that counselors must discuss with women younger than age 16 the possibility of involving their parents.<sup>14</sup> As of 2008, three states allowed health care professionals to waive parental involvement under certain circumstances. Published articles rarely specify how these states have been categorized. In addition, it is difficult for researchers to know when the laws went into effect in cases in which enforcement was temporarily enjoined.

## Research Design

Given that researchers typically have access only to secondary data on abortion, the most effective designs use a pre-post analysis with a comparison group.<sup>‡</sup> These designs are also referred to as difference-in-differences estimators. The change in minors' abortion rate from before to after a parental involvement law went into effect in a state (the experimental state) is compared with the change in the rate among minors in other states (comparison states) or among older teenagers within the same state who are unaffected by the law. The comparison is an effort to ensure that any variation in the abortion rate of minors associated with the law does not include ongoing trends in

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<sup>\*</sup>For instance, Joyce et al. successfully petitioned the Institutional Review Board of the Texas Department of State Health Services to obtain individual-level data on induced terminations in that state (source: reference 41). The file included the woman's exact date of birth, which enabled the researchers to more accurately categorize teenagers' exposure to the state's parental notification law.

<sup>†</sup>Most states have no reciprocal agreement for reporting induced abortions, as they do for births. For instance, if a minor from Mississippi has a birth in Louisiana, the birth certificate information is returned to Mississippi. If the same minor terminates a pregnancy in Louisiana, however, the report of the termination is not sent back to Mississippi's health department. A few southern and midwestern states have shared induced termination reports, but there is no consistent policy, and the practice of sharing has varied over time. Sharing of reports has enabled some researchers to analyze travel by minors outside their state of residence in response to a parental involvement law (sources: references 28 and 29).

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<sup>‡</sup>Some cross-sectional studies have compared the abortion rate of teenagers between states with and states without a parental involvement law (sources: references 24, 25, 31, 33 and 38). This design leaves the estimated effect of the law vulnerable to influence from unmeasured differences between states.

the abortion rate of all minor or older teenagers. There are numerous permutations of this basic design. The comparison group may consist of minors in a single neighboring state without a law, minors in all states in which the law is not changing or older teenagers in states with and without parental involvement laws.

As straightforward as this design appears, it rests on the credibility of the comparison group. Both the prelaw trend and the abortion rate among minors, as well as the characteristics of the minors themselves, in experimental and comparison states should be as similar as possible. Differences in both rate and trend would suggest potential confounding factors. However, few studies present plots of trends in the experimental and comparison groups. Instead, researchers typically rely on regression analysis

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to control for observable differences between states. Yet common factors analyzed—such as race, per capita income and even the number of abortion providers—tend to change relatively slowly. These factors may be correlated with differences in minors' abortion rate between states, but they often have little ability to explain changes over short periods of time. A common regression technique is to include a dichotomous indicator for each state in the sample. Because there are 50 states, the researcher would include 49 dummy variables. This set of state indicators is referred to as state fixed effects. The inclusion of state fixed effects removes any variation between states in the abortion rate and the included covariates that is fixed over time. For example, if the difference in abortion rates between California and Utah partly reflects permanent differences in religiosity, then the state fixed effect will remove this source of between-state variation in the abortion rate. As a result, the association between the parental involvement law and minors' abortion rate is based solely on changes in the dependent and independent variables within each state. Similarly, the inclusion of year fixed-effects variables (i.e., a dummy variable for each year) adjusts for variation over time in the dependent and independent variables that is common to all groups and states.

State and year fixed-effects models are now commonly used, particularly by economists, in evaluating parental involvement laws. This approach is a powerful way to reduce confounding from hard-to-measure variables. A major advantage is that researchers can use all 50 states to maximize the number of parental involvement "experiments." However, when doing so, researchers implicitly assume that variation in the abortion rate of minors in, say, California, New York and Illinois is a good counterfactual for changes in Utah, South Carolina and Mississippi. This is a dubious assumption, since state and year fixed effects do not control for differences in trends in the abortion rate or other outcomes *within* states. Thus, some researchers go beyond state and year fixed effects, and include a linear time trend for each state. The problem with this solution is that it adds 49 variables to the model and may "overfit" the data. In addition, linear trends may not fit the data well, especially over long periods. For example, the trend in the abortion rate since 1973 is clearly concave (i.e., an inverted U-shaped curve), with a peak around 1983. Adding state-specific squared trend terms to the linear ones adds 49 parameters. In other words, a regression with more than 50 dummy variables and almost 100 trend terms can "soak up" so much variation in the abortion rate that there is little variation left for the law to explain.

Distinguishing short-term from longer-term effects of a law is another challenge. A parental involvement law may cause an initial fall in abortion rates and a rise in birthrates. However, as more minors become aware of the law, they may protect themselves against pregnancy more effectively. Thus, the longer-term effects of the law could be reductions in both abortion and pregnancy rates. Empirically, it is very difficult to attribute longer-term declines in abortion or pregnancy rates to enforcement of a parental involvement law, given ongoing changes in contraceptive technology, welfare reform, changing labor markets and more. For instance, the abortion and pregnancy rates of 15–19-year-olds fell 47% and 24%, respectively, between 1988 and 2000 at the national level.<sup>15</sup>(Table 3.3) The change, however, was evident in states with differing legal climates. In California, Illinois, New Jersey and New York, four large states without enforced parental involvement laws, the teenage abortion and pregnancy rates fell, on average, 34% and 25%, respectively. Clearly, other forces have had a major impact on teenagers' reproductive choices. Distinguishing the longer-term impact of parental involvement laws from these other factors may be asking too much of available data and methods.

In the end, the best research designs are the most transparent. Prelaw trends in the abortion rate of minors in experimental and comparison states are key factors to review. If a parental involvement law has a substantial effect

on abortion rates, there should be obvious discontinuity in the time series (plot of rates over time). Future researchers should consider plotting abortion rates for groups of states that changed their laws in the same or nearly the same year, and comparing them with plots of states with similar abortion rates but whose laws did not change. This approach would also allow readers to assess differences in levels and trends of abortion rates between experimental and comparison states. If the natural experiment afforded by the law is truly exogenous (i.e., the change in the law is not associated with other state-level factors that may also affect the outcome of interest), and changes in the abortion rate among the comparison groups effectively capture ongoing trends, then estimated effects based on simple difference-in-differences estimators should not change when additional factors are added to the model. As in randomized designs, adjustment for other factors will be needed in such models to improve the precision of the estimates and not to control for confounding.

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Future researchers may also consider more extensive use of falsification tests in which they estimate the association between a parental involvement law and changes in the abortion rates of groups unaffected by the law. Thus, such a law should have no effect on the abortion rate of 18–19-year-olds. Nor should a law whose enforcement is enjoined have any effect on minors' abortion rate. Similarly, a law that is enacted in a given year should have no impact on minors' abortion rate in the previous year. If an association does emerge from such tests, it points to likely confounding. Finally, future researchers should discuss the plausibility of the magnitude of estimated associations they discover. The focus is too often on statistical significance. Recent econometric studies have shown that researchers tend to underestimate the variance of estimated regression coefficients when evaluating the impact of state policies.<sup>16,17</sup> As a result, analysts will reject the null hypothesis of no association too often and incorrectly conclude there exists a statistically significant association between parental involvement laws and reproductive outcomes. The combination of simple plots, robust regression analyses and clear specification checks can greatly enhance the credibility and validity of study findings.

# National-Level Studies of Reproductive Behaviors

Seventeen of the 29 articles in this review evaluated the impact of parental involvement laws on minor and adult women's reproductive health behaviors using data from most or all states. Most of these studies compared outcomes of women living in states with and without such laws in place. Many also compared the behaviors of minors with those of older women. The outcomes of interest included the abortion rate and ratio, gestational age at the time of an abortion, the pregnancy rate and the birthrate.

National studies can be classified as those that analyzed the general determinants of abortion and those that focused primarily on parental involvement laws. The former treated the laws as one of several state policies that could affect access to abortion. The dependent variable in these studies was the abortion rate of all women of childbearing age in a state, regardless of age; however, these studies tended to lack statistical power. In 1990, abortions provided to minors accounted for fewer than 9% of all abortions for which age was known.<sup>18</sup> Thus, even if parental involvement laws were associated with a 20% decrease in minors' abortion rate, they would result in a fall of less than 2% in the abortion rate of all women of childbearing age (9% $\times$ 20%). As we show below, many studies were not adequately powered to reliably detect such a small change. Nevertheless, it is important to review them, as they sought to provide a global perspective on the determinants of abortion and are widely cited in the literature.

## Studies of Impact Among All Women of Childbearing Age

Four of the studies we reviewed assessed the impact of parental involvement laws among women of reproductive age as a whole. Hence, they included both minors and adults.

Blank et al.<sup>19</sup> analyzed the determinants of state abortion rates of women aged 15–44 from 1974 to 1988 (excluding 1983 and 1986). They used abortion rates published by the Guttmacher Institute and the CDC for 49 states and the District of Columbia. They applied multiple regression techniques to evaluate the effect of Medicaid financing of abortions and parental involvement laws while holding constant the availability of abortion providers,

social and demographic factors, and a full set of state and year fixed effects. They found no statistically significant association with parental involvement laws, but because the study examined all women of reproductive age rather than focusing on minors, only a very large effect on minors could have been detected.

Meier et al.<sup>20</sup> used a pooled time-series design with data from 1982–1992 to estimate the effect of parental involvement laws on the abortion rate of all women aged 15–44. The law was one of 23 policies related to abortion that were included simultaneously in the analysis. Results indicated that parental involvement laws had no impact on abortion rate.

This study was unique in that the researchers eschewed state fixed effects and instead included the previous year's abortion rate to control for hard-to-measure determinants of abortion. In theory, the lagged abortion rate was an effective way to control for these determinants between states. However, the coefficient on this rate was close to 1.0 (0.94). In essence, the authors were trying to correlate one-year changes in the abortion rate with the level of the other covariates.\* A more appropriate approach would have been to regress changes in the abortion rate on changes in the covariates and policy variables. Any association using this approach could have been interpreted as the effect of a change in an abortion policy on the change in the abortion rate.

Another problem is that the year-to-year variation in the total abortion rate at the state level was probably insufficient to identify the effect of each of 23 policies. The fact that parental involvement laws affect fewer than 10% of women aged 15–44 exacerbated problems of statistical power. Unfortunately, the authors did not show the standard errors of the coefficients on the policy variables; thus, more specific comments on statistical power are not possible. In sum, the finding that parental involvement

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\*For example, assume that the coefficient on the abortion rate lagged one year was exactly 1.0. The regression model could then be written as  $A_t = A_{t-1} + \beta X_t + e_t$ , where  $A_t$  is the abortion rate in year  $t$ ,  $A_{t-1}$  is the abortion rate in the previous year,  $X_t$  represents other covariates in year  $t$ ,  $\beta$  is the coefficient on  $X_t$ , and  $e_t$  is the residual. However, this model can be rewritten as  $A_t - A_{t-1} = \beta X_t + e_t$ , where  $A_t - A_{t-1}$  is the first difference, or annual change, in the abortion rate.

laws had no effect on the state abortion rate could be due to the weak design of the study.

Matthews et al.<sup>21</sup> used state-level data from the Guttmacher Institute and national vital statistics for the years 1978–1988 to analyze the social and economic determinants of abortion rates and birthrates of women aged 15–44. They included parental involvement laws as one policy variable, and examined abortion rates and birthrates both before and after the implementation of the law. The study had a number of strengths. The authors used abortions by state of residence instead of by state of occurrence. They computed birthrates as well. And they presented estimates from regressions that included state-specific linear time trends in addition to state and year fixed effects. Although the authors risked overfitting the data with the trend terms, the reader was able to assess the sensitivity of the results to their inclusion.

The authors found that parental involvement laws were associated with a 3% decline in state abortion rates ( $p < .05$ ) and a 2% decline in state birthrates ( $p < .05$ ). These results, however, were not empirically consistent. For instance, a 2% decline in the birthrate represents a fall of 1.3 births per 1,000 women aged 15–44. But the 3% decline in the abortion rate would result in a decline of only 0.9 abortions per 1,000 women this age. In other words, the fall in births exceeded the fall in abortions, a highly unlikely result. This underscores the need to analyze both absolute and relative changes in rates of abortions and births. To the authors' credit, however, they showed the same models with state linear time trends added. Here, they found a statistically nonsignificant decline of 1% in the abortion rate and less than a 1% increase in the birthrate. They concluded that parental involvement laws were not associated with these outcomes.

Bitler and Zavodny<sup>22</sup> used a pooled time-series analysis of CDC data from all 50 states for 1974–1997 to assess the effect of policies that restrict access to abortion services on the timing of abortions and the abortion rate among women aged 15–44. One policy they analyzed was mandatory parental involvement. They used regression analysis to control for demographic characteristics and state economic conditions; they also included proxies for the political climate, as well as state and year fixed effects.

The authors found that enforced parental involvement laws were associated with a one–percentage point increase in the proportion of abortions occurring after 12 weeks' gestation and a 5.5% decrease in the abortion rate. More striking was the finding that parental involvement laws whose enforcement was enjoined were associated with a 1.6–percentage point increase in the share of second-trimester abortions (relative to a mean of 11%)

and an increase of 41% in the rate of abortions after 12 weeks. These findings changed modestly when adjusted for state-specific trends.

The credibility of these estimates is questionable for several reasons. First, the finding that nonenforced parental involvement laws had a larger effect on abortion timing than enforced ones points to confounding or misspecification. Second, these effects are extremely large, given that they are driven by minors, who made up fewer than 10% of all abortion patients in 1990.<sup>18</sup> For instance, the observed 5.5% decrease in the abortion rate of all women suggests that the abortion rate of minors fell by 55% (5.5/0.10). Moreover, parental involvement laws affect about 40% of minors, or fewer than 4% of all abortion patients, making these estimates even more implausible. Third, the effects of these laws exceeded the impact of Medicaid financing restrictions for abortions. This, too, appears questionable, given that about one-fourth of abortion patients were Medicaid-eligible in 1987.<sup>23</sup>

These four national studies of state abortion rates focused mainly on the statistical significance of the coefficient on the parental involvement law variable, at the expense of fully examining the magnitude of the purported effects. Since their publication, new research has shown that many analysts did not appropriately adjust the standard errors of the policy variables.<sup>16</sup> The technical aspects go beyond the scope of this review, but the essence is that in most large panels of states over time, there are usually no more than 50 “experiments,” since each state's parental involvement law goes from unenforced to enforced. In other words, there are often no more than 50 degrees of freedom, not the 500 or so assumed in the analysis (e.g., 50 states over 10 years). As a result, researchers have underestimated the standard errors on the coefficient of the parental involvement variable and concluded incorrectly that they had uncovered statistically significant effects. Careful attention paid to the magnitude of the associations, rather than to statistical significance, might have made the researchers more cautious in their conclusions. In any case, it is unlikely that a law affecting only minors would influence the abortion rate of all women to a detectable extent, and any effects found would probably be spurious.

### **Studies of Impact Among Minors Only**

The next set of studies at the national level focus explicitly on the association between parental involvement laws and the abortion rate of minors (or, in one case, of 15–19-year-olds). This is a much more appropriate approach, since only minors are directly affected by the laws. This design also allows for falsification tests: Parental involvement laws should have no appreciable effect on

the abortion rate of 18–19-year-olds or of adults. However, an important limitation in these studies is the use of abortion rates by state of occurrence, since they can be very sensitive to the number of resident minors who leave the state for an abortion and of nonresident minors who stop coming into the state once a parental involvement law is enforced.

Haas-Wilson<sup>24</sup> examined data for 36 states to determine the effect that parental involvement laws had on minor women's abortion rates and the availability of abortion providers. She used CDC data to calculate 1987 abortion rates for minors; she apparently did not have access to population estimates for women aged 15–17, so she calculated the rates of abortions for minors per 1,000 women aged 15–19, for whom she did have estimates. She found rates averaging 13.2 abortions for minors per 1,000 among women aged 15–19 living in states without parental involvement laws and 9.9 abortions per 1,000 among same-aged women living in states with these laws.

***An important limitation in national studies is the use of abortion rates by state of occurrence, since they can be very sensitive to the number of resident minors who leave the state for an abortion and of nonresident minors who stop coming into the state once a parental involvement law is enforced.***

Further, the author found that minors obtained an average of 12% of all abortions in states without parental involvement laws, compared with 9% of all abortions in states with such laws in place. Additionally, using data from the Guttmacher Institute, she determined that states with restrictions on minors had notably fewer abortion providers than those without such restrictions (0.21 vs. 0.54 per 10,000 women of childbearing age). She concluded that parental involvement laws decreased the availability of abortion services, not only for minors, but for all women in the state.

A limitation of the study, as acknowledged by the author, was the assumption that state parental involvement laws were the *cause* of the identified differences in the minor abortion rate. Differences in abortion rates between restrictive and nonrestrictive states may reflect unmeasured characteristics that are correlated with the law. Cross-sectional designs such as this are especially vulnerable to this source of bias, and the study did not control for potential confounders. An additional weakness was the use of CDC data, which record abortions only by state of occurrence. Differences between restrictive and nonrestrictive states may be spurious if minors in restric-

tive states leave their state for abortions and nonresident minors stop traveling to these states.

Ohsfeldt and Gohmann<sup>25</sup> used data from 35 states to examine differences in minor abortion and pregnancy rates between states with and states without parental involvement laws. The authors argued that increasing the total psychological and travel cost of accessing abortion would induce minors to practice contraception, leading to a fall in abortions and pregnancies. However, among minors who conceive unintentionally, the law raises the costs of an abortion and should result in an increase in births. Which of these responses dominates is unclear.

In the empirical analysis, the authors calculated the ratio of the rates of 15–17-year-olds to those of 18–19- and 20–44-year-olds. They used resident abortion rates from the Guttmacher Institute for the years 1984, 1985 and 1988. The research design was cross-sectional, but the use of abortion rates of older teenagers and adults adjusted for unmeasured factors affecting these rates. On the basis of their best estimates, the authors concluded that the laws were associated with a reduction in the ratio of adolescent to older teenage abortion rates of 18% and pregnancy rates of 8%. All results presented were statistically significant.

The study has several noteworthy weaknesses. First, the authors assumed that they had 103 independent observations, when in fact they had only 34. As a result, they underestimated their standard errors and increased the probability of falsely concluding that their results were significant. Second, the estimated effects of the law were large and may have reflected inadequate control for travel by minors to states without laws.\* Third, the authors concluded that "a parental involvement law is associated with about an 18% reduction in the adolescent abortion rate and an 8% reduction in the adolescent pregnancy rate, thereby increasing adolescent fertility by about 10%."<sup>25(p. 74)</sup> This conclusion is inconsistent and reflects confusion between relative and absolute changes. Given the means reported in the study's appendix, an 18% decrease in the abortion rate represents a decline of 4.0 abortions per 1,000 minors, while a 10% increase in the birthrate represents a rise of 7.9 per 1,000. Hence, the increase in the birthrate is twice as large as the decline in the abortion rate; this could occur only if parental involvement laws caused an increase in the pregnancy rate. But the authors concluded that the pregnancy rate fell by 8% or by 8.1 per 1,000. The inconsistency of these results undermines the ability to draw meaningful conclusions. Finally, the study was essentially

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\*The estimated abortion rates by state of residence assumed that minors crossed state lines in the same proportions as adults, which would not be true in states with parental involvement laws.

a cross-sectional analysis without controls for the many ways that states with and without parental involvement laws may differ.

In 1996, Haas-Wilson<sup>26</sup> used CDC data on minors' abortions during 1978–1990 from varying numbers of states, depending on the year, to estimate the effect of parental involvement laws. She assessed two outcomes among minors: the abortion rate (defined as abortions to minors younger than 18 years of age per 1,000 teenagers 15–19 years of age) and the ratio of abortions to births among minors. In a pooled time-series analysis with state and year fixed effects, she found that enforced parental involvement laws were associated with a 17% reduction in the abortion rate and a 13% reduction in the ratio of abortions to births among minors, while laws whose enforcement was enjoined were associated with a 9% increase in the abortion rate ( $p < .05$ ). These estimates were slightly smaller in absolute value in models that lacked fixed effects but included proxies for state sentiment toward abortion.

This study, which became the prototype for numerous national evaluations, had three strengths. First, Haas-Wilson used abortion rate among minors as the dependent variable, a major improvement over studies that used the abortion rate of all women or even of all teenagers. Second, she included state and year fixed effects to eliminate confounding from time-invariant factors between states. Third, she used parental involvement laws that were not enforced as a proxy for sentiment in the state for such restrictions.

However, the research was still hampered by the use of abortions by state of occurrence. By the time this study was published, other research had demonstrated that many minors would leave their state of residence for an abortion to avoid complying with a parental involvement law.<sup>27–29</sup> Indeed, Cartoof and Klerman<sup>27</sup> showed that a 43% decline in abortions provided to minors in Massachusetts after enforcement of that state's consent law was spurious, the result of having measured abortions by state of occurrence instead of by state of residence. Haas-Wilson, like other researchers who followed, tried to mitigate the bias associated with occurrence data by including a variable for the number of border states that enforced a parental involvement law as a control for cross-state travel by minors. However, there was no evidence that such an adjustment controls in any meaningful way for cross-state travel.\*

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\*Models with state fixed effects remove all time-invariant factors. If the parental involvement laws of border states do not change over the study period or change rarely, they will have little ability to explain cross-state travel. This is consistent with Haas-Wilson's findings that the coefficients on the border-state variables were very small and never statistically significant.

Another limitation was that the author did not analyze birthrates to verify the large drop in abortion rates. Births, unlike abortions, are measured by state of residence. If the 17% decline in the abortion rate was real and minors did not increase their contraceptive use in response to the law, birthrates should have risen modestly. Finally, Haas-Wilson found that parental involvement laws that were not enforced were associated with a 9% increase in minors' abortion rate. This result was counter to the author's expectations and raises questions as to the credibility of her other findings.

Kane and Staiger<sup>30</sup> used county-level data derived from national statistics for women aged 15–29 to estimate the effect of parental involvement laws on teenage births. These authors were the first to propose that relatively modest increases in the overall cost of abortion might result in a decrease not only in abortions but in births as well. As a test, they used parental involvement laws and changes in the availability of abortion providers as proxies for the changes in the costs of abortion. They assessed associations of each measure with county teenage birthrates during 1973–1988, excluding 1983 and 1986. They had more than 40,000 observations, using county-years as the units of analysis. Their regression models included demographic and economic characteristics, as well as county- and state-by-year fixed effects.

The authors found that among white women, parental involvement laws were associated with a 3% lower birthrate of women aged 15–17, but also with a 2% lower birthrate of those aged 18–19 and a 1% lower birthrate of women aged 20–29. Furthermore, the absolute reduction in birthrate was greater among older teenagers than among minors. Both results were inconsistent with a causal effect of the law on the birthrates of minors. The authors concluded, "Overall, these results provide no strong evidence that parental consent laws influenced teenage birth rates."<sup>30(p. 496)</sup>

The models of Kane and Staiger rest on the unsupported assumption that teenagers were aware of changes in the location of abortion clinics and used this knowledge in making reproductive choices. Focus groups and surveys of teenagers reveal a severe lack of knowledge about laws that regulate abortion—even those that affect teenagers directly, such as parental notification statutes.<sup>8,9</sup> In addition, the study may not have been able to control for all confounding factors, and the authors should have omitted counties with small populations.

Levine<sup>14</sup> analyzed the effect of parental involvement laws on the birthrates and abortion and pregnancy rates of minors and older women between 1985 and 1996. The study represented the most comprehensive approach to the analysis of these laws that can be achieved with state

aggregates on a national scale. At the same time, the paper revealed the limits of this approach. Levine, like Kane and Staiger, argued that parental involvement laws represented a modest rise in the cost of abortion, broadly defined to include both financial and nonfinancial costs associated with accessing the service. Nevertheless, he hypothesized that forward-looking minors would view the law as an increase in the cost of pregnancy and modify either their sexual activity or their contraceptive use. Thus, the model generated three predictions: Parental involvement laws should lower minors' abortion rates, have little impact on birthrates and thus lower pregnancy rates, and cause minors to reduce their exposure to an unintended pregnancy.

To test these predictions, Levine used state birth statistics as reported to the National Center for Health Statistics (NCHS) and abortion data by age from the Guttmacher Institute as dependent variables in a regression model with state and year fixed effects. The model also controlled for demographic, economic and other policy variables. He found that parental involvement laws were associated with a 22% reduction in the abortion rate of minors aged 15–17 (15% when state-specific trends were included), but had little effect on birthrates. The laws were also associated with reduced pregnancy rates for both 15–17-year-olds and 18–19-year-olds in models without state-specific trends, but not in models controlling for these trends. Second, Levine found through an analysis of the 1988 and 1995 National Surveys of Family Growth (NSFG) that parental involvement laws were associated with a 6% decrease in unprotected sexual activity (not quite statistically significant) among women aged 15–18, resulting mainly from an increase in contraceptive use at last intercourse. However, this analysis also produced the anomalous finding that the laws were associated with a 12% increase in the proportion of women aged 19–24 who were sexually active.

Although this study used the best available measure of the abortion rate of the target group, these data are inherently limited for such a study. The Guttmacher Institute estimates teenage abortion rates by state of residence. For 12 states in 1988, the proportion of abortions occurring among minors was estimated from the proportions in neighboring states. The estimated proportions would not have reflected any effects of parental involvement laws. The resident abortion rates took into account interstate travel by women of all ages, but would not have accounted for teenagers who went out of state to avoid parental involvement requirements. As with similar studies, Levine used a control for whether a border state enforced a parental involvement law, but it had no effect on his estimates.

Another limitation was that Guttmacher data were available for only four of the 11 years covered. Moreover, most states implemented laws between 1988 and 1996, but Levine had only one data point between those years, making it difficult to distinguish changes associated with laws from ongoing declines in abortion rates.\* His estimates were also relatively sensitive to the inclusion of state-specific trends. Indeed, one could read Levine's results adjusted for state trends as indicating that parental involvement laws caused abortion rates to fall by 15%, birthrates to rise by 3% ( $p=.11$ ) and pregnancy rates to remain unchanged.

In addition, states that enacted parental involvement laws differed greatly from states that did not. Since the regression analyses were weighted by population, the comparison states were dominated by California, Florida, New York and Texas, the four most populous states, all of which have large Hispanic populations. Whether they were good controls for Georgia, Kentucky, Kansas, Maine, Mississippi, South Carolina and North Carolina—states that implemented parental involvement laws during this period—merited greater scrutiny. For instance, simple plots of abortion rates between 1985 and 1996 in the states with laws and the comparison states would have offered some insight as to their comparability.

Data limitations were even more relevant to the analysis of the NSFG. There were only 711 teenagers 15–18 years of age in his regression analysis of contraception use at last intercourse from two NSFG surveys in 1988 and 1995. It is unclear how these teenagers were distributed over states and time, but this sample yields approximately seven teenagers per state and year if we divide the number of observations by two years and 50 states. Regardless of how the sample is distributed, we question whether there is sufficient statistical power to detect credible effects at the state level with such a small sample of individuals (see Donald and Lang<sup>17</sup>). Moreover, 18-year-olds are likely to be disproportionately represented in his sample, and they were unaffected by the law. In sum, we are not convinced that Levine's analysis of sexual activity and parental involvement laws adds appreciably to the study.

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\*To understand this better, assume Levine had had only two data points. Because these two points can yield only a linear trend, it is not possible to distinguish the change in the law from a continuation of a downward trend in abortion rates. The addition of comparison states would provide a second line, but this raises the crucial question as to whether the change in these "control" states was a good counterfactual for the change in the abortion rate of minors in the "experimental" states. Econometrically, Levine has only modest variation with which to distinguish changes in the abortion rate of minors associated with the law from the downward trend in the abortion rate of all minors after 1988.

Medoff<sup>31</sup> analyzed the determinants of the abortion ratio. He pooled state-level data on the abortion ratio from the Guttmacher Institute for the years 1982, 1992 and 2000. He focused on three abortion policies: parental involvement laws, Medicaid financing of abortions and mandatory waiting periods. He included the price of abortion, as well as other socioeconomic characteristics of the state. In the regression analysis of the abortion ratio for all women of childbearing age, parental notification laws were associated with a decline of 28 abortions per 1,000 pregnancies, or 13%. Among minors (ages 15–17), parental involvement laws were similarly associated with a decline of 52.6 abortions per 1,000 pregnancies, a reduction of 14%.

Medoff's study suffers from numerous methodological weaknesses. First, like Ohsfeldt and Gohmann,<sup>25</sup> Medoff relied on variation between states to identify effects of parental involvement laws. In essence, he compared abortion ratios between states with these laws (e.g., Alabama, Georgia, Mississippi and South Carolina) and states without them (e.g., California, Illinois, New Jersey and New York). Regardless of the controls, the potential for confounding in a cross-sectional design is great, as illustrated by the implausibility of his results, such as the nearly identical reduction in the abortion ratio for all women and for minors (13% vs. 14%). This makes little sense. Minors accounted for only 7–11% of all abortions in the years studied.<sup>13,18</sup> If parental involvement laws were associated with a 13% reduction in the abortion ratio of all women of childbearing age, then the laws must have lowered the abortion ratio of minors by more than 100% ( $-13.0/0.11$ ), since no more than 11% of the sample were exposed to the law.

New,<sup>32</sup> following Haas-Wilson<sup>26</sup> and Levine,<sup>14</sup> analyzed the effect of "prolife" policies on minors' abortion rates. He used abortion rates from the CDC for 1985–1999 as the dependent variable, and his regression models included controls for demographic factors as well as state and year fixed effects. The author found that parental involvement laws were associated with a reduction in the abortion rate by an average of 1.67 abortions per 1,000 females aged 13–17 (or approximately 16%). Further, abortion rates were lower (by 1.94 per 1,000 females aged 13–17) in states with enforced parental involvement laws than in states with nonenforced ones.

However, the statistically significant decline among minors (1.67 per 1,000 women) was only modestly greater in absolute value than the statistically significant decline of 1.05 abortions per 1,000 women of childbearing age associated with the law. Indeed, given the standard errors reported in the paper, the difference between the impact on all women and on minors most likely was statistically

nonsignificant. But New did not present this basic test and simply asserted that the difference in the size of the coefficients "provides evidence that parental involvement laws, not broad value shifts, are affecting minors' decisions."<sup>32(p. 7)</sup> A more plausible interpretation is that other influences accounted for part or possibly all of the apparent impact of the laws on minors.

The most important limitation of New's work was the use of abortions by state of occurrence instead of by state of residence. He did not correct for minors' interstate travel, which could account for some of the observed decreases in abortion rates. Second, New's claims to have overcome the "endogeneity problem" are overstated. He correctly noted that state parental involvement laws are not randomly assigned: States that enact such laws tend to be more conservative than states that do not. Thus, an observed decline in the abortion rate of minors in the wake of a parental involvement law may simply confirm an ongoing trend in states that tend to discourage abortion more generally. New's solution was to compare the effect of laws that had been enjoined with the effect of those that were enforced. He incorrectly interpreted this as a correction for the nonrandom distribution of parental involvement laws. The approach is best described as a falsification test that can signal potential confounding, but it is not a correction for "endogeneity."<sup>\*22,26</sup>

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\*An example of a falsification test would be to regress the abortion rate of women aged 20–44 on an indicator of a parental involvement law. A statistically significant association would make no sense and would suggest problems with the research design, since older women are not affected by the law.

# National-Level Studies of Sexual Activity

Two of the studies reviewed focused on the relationship between parental involvement laws and teenage sexual activity. They merit comment because an important unresolved issue, as mentioned earlier, is whether minors respond to parental involvement laws by modifying their pre-pregnancy behavior. The laws' ability to influence minors' sexual and contraceptive behaviors before a pregnancy depends in large part on their awareness of the requirements of the law. Many teenagers are unaware of the laws and often learn of the requirements and the possibility of judicial bypass when they contact an abortion clinic.<sup>8</sup> Therefore, we are skeptical that minors would alter their sexual behavior shortly after a parental involvement law went into effect. Teenage sexual behavior may change over time as these laws are implemented, but also as other state restrictions on abortion start to affect norms. However, the empirical challenges of identifying effects of a law beyond a few years after its enforcement are formidable, given changes in other factors that affect reproductive choice.

Averett et al.<sup>33</sup> assessed the association of parental involvement laws with the probabilities that 15–19-year-old women were sexually active and had used contraceptives at last intercourse. They used national data from the 1995 NSFG. The authors found no association between the laws and the outcomes assessed, but the study lacked statistical power. The standard error for the parental involvement law variable suggested that the authors would be unlikely to detect a change in sexual activity of less than 20 percentage points or a decline in contraceptive use of less than 32 percentage points. These would be huge effects, since minors, who would be the only ones affected by the laws, represented only a subset of women aged 15–19. In addition, the effect of the parental involvement was ascertained from cross-state variation in policies, which is vulnerable to confounding because of the difficulty of controlling for unobserved characteristics that are correlated across outcomes.

Klick and Stratmann<sup>34</sup> presented a more sophisticated test of whether parental involvement laws affect teenage sexual behavior. They examined the association between these laws and gonorrhea rates of women younger than 20 years of age and compared them with those of adult women aged 20 years and older using data from the CDC

for all states from 1981 to 1998. Their regression models included state and year fixed effects, and linear state-specific trends; as an additional control, they included the gonorrhea rates of the adult women. They developed separate models by race/ethnicity.

The authors found that enforced parental involvement laws were associated with a decline of 9.5 cases of gonorrhea per 100,000 white females younger than 20 years of age and 12 cases per 100,000 Hispanic females of the same age. These represented average reductions of 12% and 21%, respectively. They found no association for black females or males in this age-group. As a specification check, they regressed gonorrhea rates on parental involvement laws that were not in force and found no effect. They also regressed adult gonorrhea rates on these laws and saw no association. The authors concluded that raising the cost of abortion induces a substantial proportion of teenagers to avoid risky sex.

***Two of the studies reviewed focused on the relationship between parental involvement laws and teenage sexual activity. They merit comment because an important unresolved issue...is whether minors respond to parental involvement laws by modifying their pre-pregnancy behavior.***

As presented, the study by Klick and Stratmann appears convincing. However, a number of questions remain unanswered. First, it was not clear why the authors compared women younger than 20 years of age with all women aged 20 or older rather than with women aged 20–24. The rate of gonorrhea in 1998 was 780 per 100,000 among women aged 15–19 and 646 among women aged 20–24, but it was much lower, at 133, among all women of childbearing age. National time-series plots by age would have shown not only that differences by age were huge, but also that women aged 15–19 and women aged 20–24 have tracked each other very closely since 1981.<sup>35</sup> In other words, the latter group was the most obvious comparison group for the former.

Another important point is that gonorrhea rates are higher for 18- and 19-year-olds than for minors; these older teenagers were included with minors but were not

exposed to the law. Klick and Stratmann acknowledge this limitation, but suggest that the teenage group is only “slightly over-inclusive.” However, minors account for only 40% of all teenage pregnancies, a percentage that is likely a good estimate of their share of gonorrhea rates as well. We also know that only 40% of minors do not involve their parents in their decision to have an abortion.<sup>3</sup> Thus, the 21% decline in gonorrhea rates among Hispanic teenagers associated with parental involvement laws is driven by only 16% of those who were exposed to the law and were sexually active ( $0.40 \times 0.40$ ). If we inflate the estimated declines reported above, we would conclude that gonorrhea rates fell 131% among Hispanic minors and 75% among white minors affected by the law. These estimates are too large to be credible. In addition, the authors provide no rationale for why restrictions would have an effect on female but not male teenagers.

# State-Level Studies of Reproductive Behaviors

Five of the articles we examined used data from groups of states (2–11 states) to evaluate women’s reproductive behavior after parental involvement laws were put into place. Another seven focused on single states or cities.

## Multistate Studies

In a before-and-after review of public health statistics from the state health departments in Minnesota, Massachusetts and Rhode Island, Donovan<sup>7</sup> illustrated the impact of parental involvement laws on abortion rates and birth rates. She also described the court bypass procedures in the states based on interviews with judicial officials. In Minnesota, notification of both parents of a minor’s decision to terminate was mandated (even if the parents were divorced, unless the noncustodial parent could not be located). In Massachusetts, the consent of both parents was required, unless a minor’s parents were divorced (in which case the custodial parent’s consent sufficed). In Rhode Island, the consent of one parent was required before a minor’s termination of pregnancy.

Donovan found that in Minnesota, between 1980 (the last full year before implementation of the parental notification law) and 1982 (the first full year during which the law was in effect), the number of abortions that minors obtained in the state decreased by 33%. The decline was greatest (35%) for those aged 16–17. Birth data for Minnesota had not yet been released at the time of publication, so the impact on birthrates was not assessed.

In Massachusetts, the parental consent law took effect in April 1981. From 1980 to 1981, the number of abortions minors obtained in the state dropped by 34%. Again, the decline was greatest for those aged 16–17. In addition, one hospital in Boston reported that the number of minors scheduling second-trimester abortions rose after the law went into effect. During the same period, the number of resident minors who gave birth was unchanged. On the basis of anecdotal information from Planned Parenthood of Massachusetts, Donovan speculated that the decrease in the number of minors who obtained abortions in the state was “substantially offset” by the large number who terminated pregnancies in neighboring states.

In Rhode Island, the parental consent law was not put into place until September 1982. Hence, the analysis took place before its effects could be ascertained.

In sum, this study provides a good description of the way three states implemented the court bypass provisions. A study limitation was the anecdotal description of the law’s impact in place of a more rigorous evaluation.

Joyce and Kaestner<sup>29</sup> used individual-level data on births and abortions from South Carolina, Tennessee and Virginia for 1986–1991 to examine the effect of parental involvement on the probability that a pregnancy would be terminated. Study data were obtained from state vital statistics agencies. The study states have fairly complete reporting of abortions to residents and nonresidents obtained within the state. The parental consent law in South Carolina went into effect in May 1990 and required that a parent or grandparent of an unemancipated minor younger than age 17 provide written consent before a termination. Tennessee’s law went into effect in November 1989 and required that at least one parent of an unmarried minor be notified of the minor’s intention to terminate at least two days before an abortion. Virginia, with no parental involvement law in place at the time of the study, was used as a control state because the authors considered it similar to the other states in size, region, demographic characteristics and completeness of abortion reports.

Changes in the probability of an abortion given pregnancy in South Carolina and Tennessee were compared with those in Virginia. The changes were broken down by age, race, marital status and date of conception. Additionally, within-state and out-of-state control groups were identified. \* Specifically, unmarried females aged 18 or younger in Tennessee and 17 or younger in South Carolina were compared with unmarried women aged 19–20 in all three study states.

The authors found that the probability of women having an abortion fell in all study states, including Virginia, suggesting an overall decline for all age and state cohorts. However, after conducting a difference-in-differences-in-differences analysis as well as a regression analysis, they found that the parental involvement laws had no impact

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\*This study design allowed for an examination of state and regional characteristics that vary over time.

on the abortion probability in either state enacting these laws in any age-group or racial group, except nonblack 16-year-olds in South Carolina, whose probability of abortion declined by 10 percentage points.

Although this study involved only two states with parental involvement laws and one control state, it had the benefit of having accurate data on abortions occurring out of state. At the same time, the study had three limitations. First, the authors included 17-year-olds in South Carolina and 18-year-olds in Tennessee among those exposed to the law, although most of these women would not have been exposed. Their inclusion in the exposed group would tend to bias the study estimates toward no effect. Second, the authors focused primarily on pregnancy resolution, and not birthrates and abortion rates. Third, birthrates and abortion rates of older women were much greater than those of minors; this makes the results sensitive to whether changes are measured in absolute or proportional terms.

Ellertson<sup>36</sup> used individual-level data from state vital statistics offices to examine the effect of parental involvement laws on birthrates, in-state abortion rates, the odds of interstate travel and the odds of late-term abortion for minors in Minnesota, Missouri and Indiana. In 1981, as noted above, the Minnesota parental notification law went into effect; the law's enforcement was enjoined in 1986, but four years later, the law was again in force. A 1985 Missouri law required the consent of at least one parent before a minor's termination. Indiana's parental consent law took effect in 1982, although its enforcement was enjoined in 1983. The law was amended the following year and went into effect again in 1985. The author performed regression analyses to compare outcomes among 15–17-year-olds, 18–19-year-olds and 20–24-year-olds before and after the laws were enforced.

Ellertson found no increase in birthrates for minors compared with older women in any study state. In-state abortion rates for minors decreased 16–26%; information on abortions obtained out of state was unavailable for Indiana and Minnesota, and for Missouri, it was unavailable for two neighboring states (Iowa and Illinois). In Missouri, the parental involvement law was associated with an increase of more than 50% in the odds of a minor's traveling out of state for an abortion, even without information from the two missing states. Older teenagers and women in their early 20s also experienced increases, but these were smaller (13% and 18%, respectively).

Many residents of the St. Louis area obtain abortions in nearby Granite City, Illinois, and Ellertson suggested that resident minors who traveled out of state could have accounted for the entire observed decline in the in-state abortion rate in Missouri. The author could not determine

the number of resident minors who may have traveled in other study states, so the study could draw no conclusions about the effect of parental involvement laws on the abortion rate of minors in those states.

Evidence concerning the effects of parental involvement laws on minors' decisions to delay their abortions was mixed; in Minnesota, for example, the odds of having an abortion later than eight weeks of gestation increased significantly, by 10%, for minors compared with older women when the law was in place. In Minnesota and Indiana, the odds of delay past 12 weeks among minors were elevated relative to those among older women, but not significantly so. The author noted that interstate travel and evidence of delayed abortions were inextricably linked.

This study's analysis of the trend in birthrates would have benefited from a comparison with trends in similar neighboring states. In addition, the data on abortions in Missouri differ inexplicably from the data published in an analysis by the officer of the Missouri Department of Public Health responsible for collecting these data.<sup>37</sup>

***Five of the articles we examined used data from groups of states (2–11 states) to evaluate women's reproductive behavior after parental involvement laws were put into place. Another seven focused on single states or cities.***

Tomal<sup>38</sup> conducted a cross-sectional regression analysis of 1995 county abortion rates and birthrates in 11 states—Arizona, Arkansas, Idaho, Montana, New York, North Carolina, South Carolina, Oregon, Utah, Virginia and Washington—using tabulations provided by the state vital statistics offices. Study states were selected because induced abortion and live birth statistics were available by the woman's county of residence and age. Several regional and demographic control variables were used in the regression analysis.

The author found that parental involvement laws were associated with lower abortion rates and higher birthrates among both younger and older teenagers. By type, parental consent laws were associated with abortion rates that were 16% lower among women aged 15–17 and 31% lower among those aged 18–19; parental notification laws, with rates that were 34% and 15% lower, respectively. The laws were associated with much higher birthrates: consent laws, with rates that were 42% and 26% higher for the two age-groups, and notice laws with rates that were 39% and 44% higher.

It is highly implausible that the study's findings reflect an effect of the laws, since they should have had little, if any, impact on the abortion rates of the older teenagers.

In addition, the hypothetical rise in birthrates swamps the fall in abortion rates. For example, a 34% decline in minors' abortion rate, evaluated at the mean of 13.0 per 1,000 minors, represents a decline of 4.3 abortions per 1,000 minors; however, a 42% increase in the birthrate represents a rise of 16 births per 1,000 minors. The same inconsistency pertains to older teenagers. The implausible findings underscore the limitations of a cross-sectional design when comparing rates between counties in states with and without parental involvement laws.

Using individual-level data, Joyce and Kaestner<sup>39</sup> compared abortion measures among minors with those among older teenagers and young women living in Mississippi and South Carolina from August 1989 to March 1994. The 1990 South Carolina law mandated the written consent of one parent or grandparent for minors younger than age 17. The 1993 Mississippi law required the written consent of both parents for unemancipated women younger than age 18 unless the parents were separated or living apart or otherwise not available. In this case, only one signature was required.

***The Cartoof and Klerman analysis is a seminal study because it was the first to demonstrate that many teenagers would leave their state of residence for an abortion in response to a parental involvement law. Subsequent studies based on a small number of states uncovered similar behavior.***

The researchers analyzed data on the abortions obtained by women in their state of residence and in border states. However, the number of residents obtaining abortions in Louisiana was unavailable, which may have affected the results. The authors compared changes in gestational age at abortion and out-of-state travel with enactment of the laws between the teenagers subject to the law (those younger than age 17 in South Carolina and younger than age 18 in Mississippi) and teenagers and young women not subject to the law (women aged 18–20 in South Carolina and aged 19–20 in Mississippi). They then compared teenagers whose nearest provider was in state with teenagers whose nearest provider was out of state. They conducted multivariate analyses, controlling for race, marital status, previous live births, previous induced abortions and distance to the nearest provider.

In Mississippi, the results suggested that minors traveled out of the state to obtain abortions: The number of out-of-state abortions in this group increased 17% after enactment of the law. Meanwhile, the number of abortions fell by 58% among nonresident minors and by 39% among nonresidents 19 and 20 years of age in the state. Also, the

probability of a minor's obtaining a second-trimester abortion increased 2.9 percentage points, but this finding was not significant. However, the mean gestational age at abortion among minors in this state increased significantly, by more than half a week. In contrast, in South Carolina, the authors found no effect of the parental involvement law, possibly, they concluded, because the law was less severe and the age-group affected was younger.

As in the earlier study by Joyce and Kaestner,<sup>29</sup> the abortion rates of the older teenagers were twice those of minors in the prelaw period. This raises questions as to the validity of the comparison group.

## **Massachusetts**

Cartoof and Klerman<sup>27</sup> analyzed monthly abortion and birth data for Massachusetts minors before and after the April 1981 implementation of the state's parental involvement law (described previously). They performed a time-series analysis of data from August 1977 through December 1982. Data for resident women younger than age 18 were obtained from the health departments of the state (number of in-state abortions) and neighboring states (number of out-of-state abortions during 1980–1982).

The authors found that the number of resident minors obtaining in-state abortions fell by 43% in the 20 months after the law went into effect, but the number obtaining out-of-state abortions increased by 300%. On the basis of the trend in the total number of abortions for minors, including those obtained out of state, they concluded that the law had little effect on minors' abortion rate.

Evidence of an effect on birthrates after the implementation of the law was less clear, although the number of births to resident minors in the year after enforcement seemed to increase by approximately 50–100. In interviews with the authors, abortion clinic counselors and administrators estimated that 25% of minors who remained in state to terminate their pregnancies, or 50 minors a month, opted for a judicial bypass.

This is a seminal study because it was the first to demonstrate that many teenagers would leave their state of residence for an abortion in response to a parental involvement law. Subsequent studies based on a small number of states uncovered similar behavior. This became a critical finding, for it undermined seemingly more comprehensive studies with data from upward of 40–45 states based on abortions by state of occurrence as published by the CDC. Inverse associations between parental involvement laws and the abortion rates of minors that did not take into account the movement by minors from restrictive to nonrestrictive states were not convincing. The only limitation of the study of Cartoof and Klerman was that it pertained to a single state on the East Coast in which

minors had easy access to states without parental involvement laws. Whether this finding would hold in larger western states in which distance between states was much greater remained unknown.

## Minnesota

Rogers et al.<sup>40</sup> examined trends in birthrates and in-state abortion rates of Minnesota residents from 1975 to 1987 to assess the effect of the state's 1981 parental notification law (described previously). Using special tabulations of Minnesota vital statistics records and a regression model that included age and year effects, and age and year interactions, they compared reproductive health outcomes of minors (women aged 15–17) with those of older teenagers (women aged 18–19) and adults (women aged 20–44). They found that the abortion rate increased by 5% among the adult women; meanwhile, it decreased by 9% among the older teenagers and by an even greater extent, 28%, among minors. Additionally, the proportion of abortions that were performed in the second trimester (after 12 weeks' gestation) increased more for minors (25%) than for the adult women (6%), although the rate of second-trimester abortions fell among minors. Finally, the authors reported no impact on minors' birthrate. Hence, they concluded that the law resulted in "pregnancy avoidance" behaviors.

As in several other studies, the researchers were unable to account for abortions obtained out of state. They asserted that travel out of Minnesota is difficult for minors because of the distances involved, but some parents likely took their daughters to other states to avoid involving the other parent or going to court. The authors presented no data supporting increased contraceptive use or reductions in unprotected sex. There were no comparisons with other states, where similar trends among younger and older teenagers might have occurred. Furthermore, Rogers et al. measured age at the time of the birth and not at the time of conception. This distinction is significant in analyses that use 18-year-olds within the same state as the comparison group, since approximately three-fourths of 17-year-olds who conceive as minors will give birth when they are 18 years of age.<sup>41,42</sup> Consequently, some births among 18-year-olds in Minnesota may have been to minors who conceived as 17-year-olds. This would bias the results toward finding no effect of the law on birthrates. Thus, the authors' conclusion that Minnesota's parental notification law had no effect on birthrates may be the result of misclassification of births to minors as births to older teenagers. This underscores the importance of demonstrating increased contraceptive use or less unprotected sex associated with the law before concluding that the law is associated with a fall in pregnancy rates.

Finally, 18- and 19-year-olds are an imperfect comparison group for minors, as their trends might differ from those of minors.

Rogers and Miller<sup>6</sup> later examined a rise in inner-city birthrates that appeared to have occurred after the enactment of Minnesota's parental notification law. Using state vital statistics, they found a sharp increase in minors' birthrates in Minneapolis, especially from 1983 to 1987. The increase was concentrated among nonwhites, especially Asians, during a period when there was an influx of Hmong immigrants, who are "known to bear children at early ages."<sup>6</sup> The researchers concluded that the rise in birthrates was not related to parental notification laws, but rather to the city's changing demographic profile.

## Mississippi

Using tabulations provided by the Mississippi State Department of Health, Henshaw<sup>28</sup> compared the abortion rates of minors younger than 18 years of age in Mississippi five months before and six months after the state's 1993 consent law (described previously) went into effect. All of the four surrounding states had parental involvement laws at the time, but theirs were less restrictive than Mississippi's. In the analysis, all women aged 18 or older were used for comparison rather than only those aged 18–19, to increase the sample size in the comparison group and increase the chance of identifying small effects of the law. To control for seasonal\* and other influences that affect women of all ages, the author examined the ratio of the number of abortions among minors to the number among adult women.

The author found that the ratio of minors to adults who obtained an abortion in Mississippi was 16% lower in the six months after the law was put into place compared with before the law, in part because of a 28% decrease in the ratio of out-of-state minors to adults. Among Mississippi residents, including those who had abortions elsewhere, the ratio of minors to women older than age 18 having an abortion fell by a statistically nonsignificant 3%. Among Mississippi residents who had abortions in the state, the ratio of resident minors to adults fell by 13%, a decrease offset by a 32% increase in the ratio of minors to adults obtaining abortions out of state.

Taking the potential difference in seasonality between minors' and adult women's abortions into account, the decrease was 2% greater, or 5% in total. However, the author could not assess whether some minors traveled to Louisiana to avoid the parental consent requirement,

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\*A statistical check on the number of abortions reported by 14 states in 1988 was performed to confirm that seasonal patterns of abortions for minors differed only slightly from those of women older than age 18.

as this state does not collect data on the state of residence of women having abortions (in contrast to Alabama, Arkansas and Tennessee, the three other border states).<sup>\*</sup> If minors traveled to Louisiana, the decrease in abortions due to the new law was likely less than 5%.

The mean gestational age at abortion in Mississippi increased among minors and decreased slightly among adult women. Procedures among minors were delayed by approximately three days, a marginally significant delay. Among minors having abortions, the proportion after 12 weeks' gestation increased from 22% to 25%; among older women, this proportion fell from 13% to 12%. After the law, there was a marginally significant 19% increase in the ratio of minors to adults who obtained abortions after 12 weeks of gestation. Although not statistically significant, the results suggested that minors may have been about 10–20% more likely to have procedures after 12 weeks.

In sum, Henshaw's findings provide little evidence that the law reduced minors' abortion rate, although the author acknowledged that the study was somewhat limited by the relatively small number of cases in the sample and the lack of data on abortions in Louisiana. In addition, the comparison group was not ideal (women aged 19 or older would have been best), and trends were not compared with those of other states. The 2001 study by Joyce and Kaestner<sup>39</sup> provides a more extensive analysis of the effect of the Mississippi law.

## Missouri

Pierson<sup>37</sup> analyzed the impact of the 1985 Missouri consent law (described previously) using data on live births, fetal deaths (deaths at more than 19 weeks' gestation or of infants with a birth weight greater than 350 g) and induced abortions for 1980–1992. The analysis was based on individual records of abortions performed on Missouri residents in Missouri and neighboring states other than Illinois and Iowa. The author found that women aged 15–17 experienced a decline in pregnancy and abortion rates, and in the proportion of pregnancies ended by abortion relative to women aged 18–19. Pierson concluded that the law was inducing more minors to continue their pregnancies. Only 3% of infants born to minors were adopted, a proportion that was unchanged after the law took effect. Between 1984 and 1986, the proportion of abortions occurring past 12 weeks increased from 19% to

22% among minors, whereas it decreased from 16% to 15% among the older teenagers.

Although the law appeared to decrease rates of abortion and pregnancy among minors, it is difficult to draw definitive conclusions because of the lack of data from Iowa and Illinois. First, the analysis is completely descriptive: The author did not test statistically for differences in pregnancy outcomes before and after Missouri's consent law took effect. Second, the largest clinic in St. Louis referred minors seeking to avoid parental involvement to the previously described clinic in Granite City, Illinois. Evidence that the law led to an increase in the gestational age at the time of minors' abortions is stronger; moreover, the 21% increase in the proportion of abortions occurring past 12 weeks among minors (when compared with older teenagers) is consistent with findings in Minnesota and Mississippi.<sup>28</sup> This finding could also be misleading, however, if Missouri minors' abortions in Granite City were disproportionately in the first trimester.

## Texas

Joyce et al.<sup>41</sup> assessed the impact of a law that took effect in Texas on January 1, 2000, and requires notification of the parents of minors younger than age 18 at least two days before the procedure. The authors were able to obtain more detailed information than has been available for other state case studies—specifically, individual-level birth and abortion data from the state that included exact dates of pregnancy outcomes. Pregnancies could therefore be tabulated according to the date of conception and the woman's exact age at the time. Comparison of data for the two years before the law took effect with data for the two years after showed that the abortion rate fell among 18-year-olds, but it decreased by 11% more among 15-year-olds, 20% more among 16-year-olds and 16% more among 17-year-olds. The birthrates of women aged 15 and 16 fell 5% more than the rate for women aged 18. The rate for 17-year-olds also decreased.

Since trends for older and younger teenagers might differ in the absence of parental involvement restrictions, the authors also compared the trend among women aged 17.5–17.75 at the time of conception with the trend among women aged 18.0–18.2, two groups that are very close in age but only the younger of which is affected by the law. They found a 16% decrease in abortion rate along with a 4% increase in the birthrate among the younger women relative to their slightly older counterparts. The effect was significant among whites and Hispanics, but not among blacks. In addition, the proportion of abortions performed past 12 weeks of gestation was comparatively elevated among the women who conceived just before age 18, evidently because they waited until they turned

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<sup>\*</sup>It is unlikely that many minors went to Arkansas, since in 1991, only nine Mississippi residents of any age had abortions there (source: Joyce T, Henshaw SK and Skatrud JD, The impact of Mississippi's mandatory delay law on abortions and births, *Journal of the American Medical Association*, 1997, 278(8):653–658).

18 to have the procedure. Data from neighboring states indicated that few minors left Texas to have abortions.

The authors contend that this study provides strong evidence that in states where distances make it difficult for minors to seek abortion services in other states, parental involvement laws cause some minors to continue pregnancies that would otherwise end in abortion. The most credible results, however, were limited to the subgroup of minors who were 17.5–17.75 years of age at conception. The authors had less confidence in the results for 15- and 16-year-olds because they differ so much from 18–19-year-olds in terms of maturity, sexual activity, completed schooling and previous pregnancies. One limitation of the study is that the results will be difficult to replicate. Texas is one of only a few states that record the woman's date of birth on both birth and abortion certificates. In addition, the large number of minors in Texas provided the sample size necessary to analyze the outcomes of a narrow subgroup. For example, neither Florida nor California, two other populous states, collect data on the date of birth of women having induced terminations.

Given these outcomes found in Texas, Colman et al.<sup>42</sup> evaluated potential misclassification bias in previous analyses of the effect of parental involvement laws on minors' reproductive outcomes when exposure to laws was measured by age at the time of pregnancy resolution instead of at the time of conception. Most such previous analyses used the adolescent's age at the time of birth or abortion, largely because data on the age at conception are not generally available. The authors contended that this led some researchers to overestimate the effect of parental involvement laws. Instead, Colman et al. compared pregnancy outcomes between 17- and 19-year-olds on the basis of age at the time of pregnancy resolution (abortion or birth) and on the basis of age at conception.

The authors used data from Texas abortion and birth certificates for 1998–2001; thus, the analysis included one full year before and one year after the implementation of the state's notification law. They calculated the difference in differences in levels and in natural logarithms, where the difference in logs equals the relative rate ratio. They found that on the basis of age at the time of pregnancy resolution, the Texas law was associated with reductions in abortion rate, birthrate and pregnancy rate of 26%, 7% and 11%, respectively, of 17-year-olds relative to 18-year-olds. However, in analyses based on age at the time of conception, the abortion rate fell 15%, the birthrate rose 2% and the pregnancy rate was unchanged.

Colman et al. concluded that previous studies of parental involvement laws should be interpreted with caution, as their methodologies may have overestimated the fall in abortions and underestimated the rise in births.

A strength of the study is that most minors likely did not cross into the border states of Texas for an abortion, because Louisiana and Arkansas enforced parental involvement laws in 2000, and Oklahoma's statute was being contested in the courts. Previous studies on parental involvement laws were primarily conducted in the 1980s and 1990s, when fewer states enforced parental involvement laws and, hence, minors were more likely to cross state borders for an abortion.

# Studies of Other Outcomes

The remaining studies that we reviewed assessed somewhat less direct measures of the impact of parental involvement laws. One evaluated the impact of such laws on the proportion of households headed by single women. Another three examined the impact on measures of infant and child health and well-being.

## Female-Headed Households

Lichter et al.<sup>43</sup> estimated the impact of parental involvement laws on the proportion of women in a population who were single heads of households with children younger than age 18. They hypothesized that an unintended and unanticipated effect of restrictions on abortion access, such as parental involvement laws, may be an increase in the number of nonmarital births, which, in turn, would increase the proportion of unmarried women heading households. The analysis used matched sets of cross-sectional county records from the 1980 and 1990 censuses. Alaska and Hawaii were excluded because their demographic characteristics were not representative of the rest of the country. Multiple regression analyses were performed and included state, county or state-by-year fixed effects to control for unobserved variables.\*

The authors found that parental consent or notification laws were associated with statistically significant increases in the proportion of women in a county who were heads of households. For every year the law was in effect, the proportion would be expected to increase by 0.05–0.06 percentage points—or by less than 1%, given a mean of approximately 7%—over the proportion for the preceding five years. It increased by 0.05 points for whites, 0.06 points for blacks and 0.09 points for Hispanics, but only the change for white women was significant. The authors concluded that the introduction of parental involvement laws in the 1980s contributed “modestly” to the rise in the proportion of single white women heading households.

We are skeptical that the authors uncovered a meaningful association. There is no evidence that parental involvement laws caused a substantial increase in births

to minors in the 1980s. By 1989, only 13 states enforced such laws; thus, most minors had access to abortion services in nearby states that did not require parental involvement.<sup>27,44</sup> Also, the authors likely underestimated the standard errors on the coefficient of the parental involvement law and thus overestimated their ability to detect an effect size as small as 1%.<sup>16</sup>

## Infant and Child Health Outcomes

The final set of studies analyzed the association between parental involvement laws and infant and child outcomes. The broad hypothesis unifying these studies is that such laws lead to changes in unintended childbearing, which in turn affect the neglect and maltreatment of infants and young children.

We present these studies because they are part of the literature, but we are skeptical that they have the statistical power to uncover a link between parental involvement laws and adverse outcomes among infants and young children. First, none of the studies provide convincing evidence that these laws have resulted in sizable changes in unintended births. Even if we consider the findings of Joyce et al.<sup>41</sup> for Texas convincing, they showed a marginally significant increase in births among white and Hispanic 17-year-olds of just 2–4%, and only a subset of children whose births are unintended are likely to be abused or maltreated. This implies that tests of parental involvement laws and child well-being need sufficient statistical power to detect changes of 1% in often crudely measured abuse outcomes at the state level. Second, the literature on the effects of unintended childbearing on child well-being is far from clear, although births to minors would likely be the highest-risk births among those resulting from unintended pregnancies.<sup>†</sup>

Bitler and Zavodny<sup>45</sup> used annual state-level data from all states on the number of reports of child abuse and neglect from 1976 to 1996 to assess a correlation between parental involvement laws and these child outcomes. Data were obtained from the American Humane Association and the National Committee to Prevent Child Abuse. The authors theorized that unwanted children may be more subject to abuse or neglect by parents or caretakers; if an abortion restriction leads to more births of unwanted

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\*County fixed effects account for both state- and county-specific factors that are time-invariant. Adding the state-by-year effects to the models absorbs all of the state-specific variation.

or unplanned children, a secondary effect may be child maltreatment. They developed a population-weighted regression model that controlled for state, year and state-specific fixed effects, as well as demographic and political factors. It also controlled for economic factors that may influence child abuse (current and previous year's unemployment rate, the log of real average income per capita and the log of real welfare payments). The authors considered three outcomes: all child abuse and neglect reports, substantiated reports and age-specific reports.

They found that enforced parental involvement laws were positively associated with the rate of age-specific child abuse and neglect reports. However, when parental involvement restrictions were assessed at the time of conception (as opposed to time of abuse assessment), there was no association with lower rates of these reports. The authors theorized that this may have been due to a reduction in the teenage birthrate as a result of the parental involvement laws. In addition, they found when enforcement of these laws was enjoined, the total rate of reported child abuse and the rate of substantiated age-specific child abuse and neglect were reduced. Bitler and Zavodny concluded that the effects of parental involvement laws were unclear.

The study data had some limitations that may have affected the results. As the authors noted, not all instances of child maltreatment are reported, and not all instances of reported abuse are valid. Further, reporting requirements differ across states. Finally, the apparent effect of parental involvement laws is not credible since the analysis found that laws that were not being enforced often had stronger associations with the child abuse outcomes than did laws that were being enforced.

Another study by Bitler and Zavodny<sup>46</sup> also examined the relationship between abortion availability and economic factors at the time of a conception and child maltreatment. The authors assessed the impact of both parental involvement restrictions that were enforced and ones that were not. Child maltreatment was ascertained from state-

level rates of child abuse and neglect reports, the fraction of children receiving social services and the number of child deaths and murders. (These measures were likely somewhat incomplete, as noted above.) The study time frame was 1976–1996 (excluding 1988 and 1989, for which data were not available). A regression model with year fixed effects and state-specific time trends was used. The model controlled for economic conditions and various demographic variables (both at the time of conception and at the time of assessment). Parental involvement laws whose enforcement was enjoined were associated with a decrease of 0.693 in child abuse and neglect reports per 1,000 children. No other results were statistically significant. Bitler and Zavodny concluded that parental involvement laws were associated with a decrease in child maltreatment, but this conclusion is questionable if nonenforced laws have a similar effect as enforced laws.

***The remaining studies that we reviewed assessed somewhat less direct measures of the impact of parental involvement laws—the impact...on the proportion of households headed by single women [and] on measures of infant and child health and well-being.***

Sen<sup>47</sup> tested the hypothesis that state-level restrictions on abortion were linked to increases in children's rates of fatal injuries. Her reasoning, similar to that of Bitler and Zavodny, was that abortion restrictions might disproportionately increase the birth of unwanted children, as well as births to young, single and low-income women, which might, in turn, lead to adverse child outcomes. The author used state-level data for all 50 states to study injury-related deaths among children aged 0–4 in 1981–2002. This age range was chosen because past analyses had suggested that children this age are the most vulnerable to fatal injuries associated with abuse or neglect. Three causes of injury-related deaths were considered: homicide, unintentional causes of any type and unintentional causes other than motor vehicle crashes in which the child was a passenger in the car. A count data model with state and year fixed effects was used for estimation. Results for each cause were presented for white and black children. The parental involvement laws of the state of interest and of neighboring states were coded into the model.

The author found that for white children, parental consent laws were associated with a 20% increase in deaths by homicide. Further, the presence of border states with no parental involvement laws was associated with a 6% decrease in homicides. Findings related to parental notification were not statistically significant, and

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†The Institute of Medicine concluded that unintended childbearing adversely affects child well-being (source: Committee on Unintended Pregnancy, Institute of Medicine; Brown SS and Eisenberg L, eds., *The Best Intentions*, Washington, DC: National Academy Press, 1995). However, that conclusion was soon challenged. Joyce et al., for example, used the NLSY to compare the outcomes of siblings whose mother said that one child's conception was unintended and the other's was intended (source: Joyce T, Kaestner R and Korenman S, The effects of pregnancy intention on child development, *Demography*, 2000, 37(1):83–94). They found no association between unintended pregnancy and measures of child well-being. They concluded that the association between unintended pregnancy and child outcomes in the Institute of Medicine study was due to inadequate controlling for the socioeconomic status of the family.

no associations were found for unintentional fatal injuries. Results for black children were imprecise but suggested that parental involvement laws were associated with a decrease in the rate of unintentional and nonmotor unintentional fatal injuries, which the author suggested may be due to pregnancy avoidance behaviors for black minors. She concluded that abortion restrictions tend to lead to “detrimental” outcomes for children in terms of fatal-injury rates, but acknowledged that measurement errors or missing variables may have confounded her results.

The magnitude of the effect reported by Sen underscores the glaring limitations of these studies. As noted above, the link between parental involvement laws and even abortion rates in the 1980s and early 1990s remains questionable, since many studies failed to account for abortions of minors outside their state of residence. If abortion rates did not change substantively, then changes in birthrates become even more suspect. Without a change in unintended childbearing associated with the law, there are no longer even the necessary conditions for an association with child abuse, let alone an increase of 20%.

# Discussion

As this review shows, researchers have used a wide range of strategies to assess the impact of parental involvement laws on multiple outcomes. The clearest impact documented is the increase in the number of minors who travel outside their home states to obtain services in states that do not have such laws or that have less restrictive ones. Studies documented such travel in Massachusetts,<sup>27</sup> Mississippi<sup>28,39</sup> and Missouri.<sup>36,37</sup> For example, in Massachusetts, 29% of minors who had abortions did so in neighboring states, most in response to a parental consent requirement.<sup>27</sup> In South Carolina, on the other hand, where the law applied only to minors younger than age 17 and a grandparent could satisfy the consent requirement, no out-of-state travel was detected.<sup>29</sup> A study in Minnesota assumed (without confirmatory data) that no minors went out of state.<sup>40</sup> In Texas, however, relatively few minors evidently did so.<sup>41</sup> In general, the impact of these laws on minors' travel appears to vary widely, depending on the specifics of the requirements, the abortion regulations of surrounding states and the state's geography.

***Researchers have used a wide range of strategies to assess the impact of parental involvement laws on multiple outcomes. The clearest impact documented is the increase in the number of minors who travel outside their home states to obtain services in states that do not have such laws or that have less restrictive ones.***

Several studies addressed the core question of the effect of parental involvement laws on minors' rates of abortion, birth and pregnancy. These laws might reduce abortion rates by causing minors either to continue unwanted pregnancies or to take steps to avoid pregnancy. Many of these studies had to make serious compromises in their methodologies, and their results varied widely. The studies that pooled data from all or most states faced obstacles that weakened their results. Most serious was their inability to adequately account for minors who cross state lines to avoid their home state's parental involvement requirement. Such travel would decrease the apparent abortion rate in the restrictive state and increase it in less restrictive and nonrestrictive states even if the total

number of abortions was unaffected by the law. In addition, parental involvement laws may reduce the number of minors from other states who would normally have abortions in the state for reasons of convenience or cost. These effects could explain why several studies found that such laws result in a decrease in minors' abortion rates,<sup>26,32,38</sup> while few found effects on birthrates.<sup>14,30</sup>

A number of studies found that the laws were associated with reductions in the abortion rates of women aged 18–19 and older women,<sup>31,32,38</sup> or that laws that were not enforced affected abortion rates.<sup>22,26</sup> Such implausible findings reduce the credibility of the studies and methods, and suggest that uncontrolled factors could account for some or all of the relationships found. Time-series studies that found decreased abortion rates showed a smaller effect when state-specific trends in the abortion rate were accounted for.<sup>14,21</sup> This finding suggests that in states that enacted parental involvement laws, abortion rates were changing in a different way than they were elsewhere, independently of the effect of the laws.

Several studies found a greater impact on white than on black teenagers,<sup>34,41</sup> a plausible result, since white minors are more likely than their black counterparts to conceal a pregnancy from their parents. On the other hand, white teenagers are also more likely to have the resources to travel out of state for abortion services.

The state case studies yielded mixed results concerning effects of parental involvement laws on minors' abortion rates. In Massachusetts, the number of abortions was about what would have been expected if preexisting trends had continued and out-of-state abortions were counted.<sup>27</sup> Similarly, the Mississippi law appeared to have little effect on the number of abortions or births.<sup>28,39</sup> In South Carolina, however, the abortion ratio among white 16-year-olds fell even in analyses that took into account out-of-state terminations.<sup>29</sup> In Minnesota, the number of abortions also fell, with no corresponding increase in births, although the number of minors who sought services out of state is unknown.<sup>36,40</sup> The clearest result is from Texas, where the abortion rate decreased and the birthrate increased among women slightly younger than age 18 in comparison with women slightly older than this age.<sup>41</sup>

Taken together, these findings suggest that most parental involvement laws have little impact on minors' abortion rate and, by extension, on birthrates and pregnancy rates. However, the Texas study illustrates that in some cases, the laws may compel a small proportion of minors to continue unwanted pregnancies.<sup>41</sup> The similar pattern found in Missouri<sup>37</sup> could be real, or it could reflect missing data on residents' out-of-state abortions.

More controversial are the studies that have looked at the impact of parental involvement laws on minors' pregnancy rates. None of them has definitively shown a reduction in pregnancy rates, and the study of Massachusetts,<sup>27</sup> which had data on abortions performed in other states, found no measurable effect on pregnancies. In Texas, the pregnancy rate of 17-year-olds was unchanged compared with that of 18-year-olds.<sup>42</sup> Since the studies did not find any change in the abortion rates, there is likely no effect on birthrates either. However, one cannot rule out the possibility that over time, minors adjust to parental involvement laws and become more conservative in their sexual behavior. This is difficult to test, however, since the longer a law has been in effect, the greater the confounding is from other factors.

***We found no studies that evaluated increased costs in obtaining abortion due to delays, travel or bypass proceedings; the impact on minors of being forced to consult their parents; or minors' opinions about the parental involvement laws. These are also important areas for future study.***

A number of studies analyzed the association between parental involvement laws and the timing of abortion, using changes in mean gestation and the proportion and the rate of second-trimester abortions as outcomes. The results were mixed. Rogers et al.<sup>40</sup> reported no increase in the rate of second-trimester abortions in Minnesota, but they did find an increase in the ratio of late to early abortions. Similarly, Ellertson,<sup>36</sup> using the same data, reported an increase in the odds of abortions after eight weeks' gestation. These seemingly conflicting findings are not contradictory, since the rate can remain unchanged even if the proportion rises. Some evidence suggested that mean gestational age rose in Mississippi after enforcement of a consent statute, but the probability of a second-trimester abortion did not.<sup>39</sup> Data limitations and lack of statistical power hamper the analyses of timing. One issue is that minors who obtain court bypass waivers almost by definition experience at least a few days of delay, and those who travel out of state usually experience even greater delays; however, these are a minority of all minors obtain-

ing abortions. Another issue is that minors who leave the state are usually not included in analyses. The Texas study overcame issues of both a small sample size and out-of-state travel.<sup>41</sup> In that study, the law was associated with a rise in the proportion of second-trimester abortions, but this was limited to minors who were just old enough to delay the termination until they turned 18.

The health outcomes of children born to women who may have been affected by parental involvement laws are also unclear, as the three studies included in this review found differing results. In the first study by Bitler and Zavodny,<sup>45</sup> there was no clear correlation between these laws and child abuse or maltreatment. However, in a similar study two years later, the authors found that such laws led to a decrease in child abuse and maltreatment.<sup>46</sup> They attributed this to fewer teenagers having children, inferring that the presence of these laws in a state leads to change in the sexual or contraceptive behaviors of these youth. Sen,<sup>47</sup> by contrast, found that parental involvement laws were associated with an increase in child abuse and maltreatment. Her design was arguably more sophisticated, since it included information about border states in the model, differentiated between parental involvement and parental consent, and stratified the results by race. However, any effect on child abuse is implausible because it would operate through the effect on unwanted births, and parental involvement laws have at best a small effect on such births.

In conclusion, the studies we reviewed provide important information on and insight into the impact of parental involvement laws on minors. Their limitations highlight areas where novel research design and methodology will be needed. Perhaps equally important are certain gaps in the evidence uncovered by the review. We found no studies that evaluated increased costs in obtaining abortion due to delays, travel or bypass proceedings; the impact on minors of being forced to consult their parents; or minors' opinions about the parental involvement laws. These are also important areas for future study.

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**TABLE 1. Selected characteristics of studies of the effects of laws mandating parental involvement in minors' abortions, 1983–2008**

Authors/ year	Area/study period	Outcome variables and their sources	Key findings	Summary assessment
Averett et al., 2002 <sup>33</sup>	All states; 1995	Probability that women aged 15–19 were sexually active, used contraceptives; NSFG data	<ul style="list-style-type: none"> <li>Parental involvement laws were not associated with the probability that teenagers were sexually active or used contraceptives at last intercourse.</li> </ul>	<ul style="list-style-type: none"> <li>The study lacked statistical power, so it may have been unable to detect small changes in the outcomes studied.</li> <li>The effect of the laws was ascertained from cross-state variation in policies, which may be vulnerable to confounding.</li> </ul>
Bitler and Zavodny, 2001 <sup>22</sup>	All states with data; 1974–1997	Abortion rate, and proportion and rate of abortions occurring after 12 weeks' gestation among women aged 15–44; CDC data	<ul style="list-style-type: none"> <li>Enforced parental involvement laws were associated with a one–percentage point increase in the proportion of abortions occurring after 12 weeks' gestation and a 5.5% decrease in the abortion rate.</li> <li>Laws whose enforcement was enjoined* were associated with a 1.6–percentage point increase in the share of second-trimester abortions and a 41% increase in the rate of these abortions.</li> </ul>	<ul style="list-style-type: none"> <li>The finding that nonenforced laws had a larger effect than enforced laws points to confounding or misspecification.</li> <li>The effects found are implausibly large.</li> </ul>
Bitler and Zavodny, 2002 <sup>45</sup>	All states; 1976–1996	Rates of child abuse and neglect reports; data from American Humane Association and National Committee to Prevent Child Abuse	<ul style="list-style-type: none"> <li>The impact of parental involvement laws was unclear.</li> <li>Laws whose enforcement was enjoined* had impacts in some models.</li> </ul>	<ul style="list-style-type: none"> <li>The study assumed that parental involvement laws reduce birthrates, but studies show little or no such effect.</li> <li>The finding that nonenforced laws had an impact is implausible.</li> </ul>

\*Laws whose enforcement is enjoined are legally prohibited from taking effect and hence are not enforced. Notes: Superscripted numbers refer to the reference list (see page 29). NSFG=National Survey of Family Growth. CDC=Centers for Disease Control and Prevention.

Authors/ year	Area/study period	Outcome variables and their sources	Key findings	Summary assessment
Bitler and Zavodny, 2004 <sup>46</sup>	All states with data; 1976–1996, excluding 1988 and 1989	Rates of child abuse and neglect reports, fraction of children receiving social services, rates of child deaths and mur- ders; data from federal agencies and national organizations	<ul style="list-style-type: none"> <li>• Laws that were being enforced were not associated with any changes in the outcomes studied.</li> <li>• Laws that were not being enforced were associated with a decrease of 0.693 child abuse and neglect reports per 1,000 children.</li> </ul>	<ul style="list-style-type: none"> <li>• The authors' conclusion that the laws were associated with a decrease in child maltreatment is questionable if nonenforced laws have at least as great an effect as enforced laws.</li> </ul>
Blank et al., 1996 <sup>19</sup>	49 states and District of Columbia; 1974–1988, excluding 1983 and 1986	Abortion rate of women aged 15–44 residing in each state or district; Guttmacher and CDC data	<ul style="list-style-type: none"> <li>• No statistically significant association with parental involvement laws was found.</li> </ul>	<ul style="list-style-type: none"> <li>• The study lacked statistical power because it examined all women of reproductive age rather than focusing on minors; hence, it may have been unable to detect small changes in the outcomes studied.</li> </ul>
Cartoof and Klerman, 1986 <sup>27</sup>	Massa- chusetts; 1977–1982	Numbers of abortions and births to minors; data from state vital statistics agencies	<ul style="list-style-type: none"> <li>• The number of Massachusetts minors obtaining abortions in the state fell by 43% during the 20 months after the parental involvement law went into effect.</li> <li>• However, this decline was offset by a 300% increase in the number obtaining abortions in neighboring states.</li> <li>• Evidence of an effect of the law on birthrates was less clear.</li> </ul>	<ul style="list-style-type: none"> <li>• The study convincingly demonstrated that in response to a parental involvement law, minors would travel to nearby states without a law for an abortion and that the abortion rate was not reduced.</li> <li>• The study is not necessarily generalizable to areas that are distant from states without parental involvement laws.</li> </ul>

Authors/ year	Area/study period	Outcome variables and their sources	Key findings	Summary assessment
Colman et al., 2008 <sup>42</sup>	Texas; 1998–2001	Abortion and pregnancy rates, and birthrates of teenagers; individual-level data from the state's vital statistics agency	<ul style="list-style-type: none"> <li>On the basis of age at outcome, the parental involvement law was associated with a fall in abortion rates, birthrates and pregnancy rates of 26%, 7% and 11%, respectively, of 17-year-olds relative to 18-year-olds.</li> <li>However, on the basis of age at conception, the abortion rate fell 15%, the birthrate rose 2%, and the pregnancy rate was unchanged.</li> </ul>	<ul style="list-style-type: none"> <li>The study's findings are convincing, given the unique data from Texas and the difficulty Texas minors had accessing abortion providers in states without parental involvement laws.</li> <li>A follow-up to Joyce et al.,<sup>41</sup> this study showed more clearly the bias introduced by measuring the age of teenagers at the time of the abortion or birth instead of age at the time of conception.</li> </ul>
Donovan, 1983 <sup>7</sup>	Minnesota, Massachusetts and Rhode Island; 1980–1983	Numbers of abortions and births among minors; data from state vital statistics agencies, and interviews with experts, administrators, officials and judges	<ul style="list-style-type: none"> <li>A court bypass to the parental involvement law was available to many minors in all three states.</li> <li>In Minnesota and Massachusetts, the number of abortions obtained by minors in the state fell 33–34% and, in the latter state, the number of births to minors changed little after the law went into effect.</li> <li>About one-third of Massachusetts minors having abortions obtained them in neighboring states.</li> </ul>	<ul style="list-style-type: none"> <li>This study provides a good description of the way three states implemented the court bypass provisions.</li> <li>Statistical data were preliminary and lacked comparison groups.</li> </ul>
Ellertson, 1997 <sup>36</sup>	Minnesota, Missouri and Indiana; 1977–1990	Abortion rates and birthrates, and timing of abortion among women aged 15–24; individual-level data from state vital statistics agencies	<ul style="list-style-type: none"> <li>In each state, the in-state abortion rate for minors relative to older women fell 16–26% after the parental involvement law went into effect, but the number of minors obtaining abortions out of state was largely unknown.</li> <li>There was no evidence that birthrates increased for minors.</li> <li>Findings concerning delays in obtaining abortions were mixed; minors' abortions appeared to be delayed past eight weeks' gestation in Minnesota.</li> </ul>	<ul style="list-style-type: none"> <li>The analysis of the trend in birthrates would have benefited from a comparison with similar neighboring states.</li> <li>It is difficult to draw conclusions about rates or delays without data on minors who went out of state.</li> <li>The data on abortions in Missouri differed inexplicably from those published by the individual who collected the data.</li> </ul>

Authors/ year	Area/study period	Outcome variables and their sources	Key findings	Summary assessment
Haas- Wilson, 1993 <sup>24</sup>	All states with data; 1985 and 1988	Abortion rates; abortion ratios and proportion of all abortions to minors (women younger than age 18); CDC and Guttmacher data	<ul style="list-style-type: none"> <li>Compared with states having no parental involvement laws in effect, states with such laws had a lower rate of abortion among minors (9.9 vs. 13.2 abortions to minors per 1,000 women aged 15–19) and a lower proportion of abortions accounted for by minors (9% vs. 12%).</li> <li>States with such laws also had comparatively fewer abortion providers (0.21 vs. 0.54 per 10,000 women of childbearing age).</li> </ul>	<ul style="list-style-type: none"> <li>Differences in abortion rates between states with and without parental involvement laws may reflect unmeasured characteristics that are correlated with the law, and the study did not control for potentially confounding factors.</li> <li>The cross-sectional design precludes causal interpretation of the results.</li> <li>Differences between states with and without parental involvement laws may be spurious if minors in states having these laws leave their state for an abortion and nonresident minors stop traveling to these states.</li> </ul>
Haas- Wilson, 1996 <sup>26</sup>	All states with data; 1978–1990	Abortion rate and ratio of abortions to births among minors (women younger than age 18); CDC data	<ul style="list-style-type: none"> <li>Enforced parental involvement laws were associated with a reduction in the abortion rate of 17% and in the abortion-to-birth ratio of 13% among minors.</li> <li>Laws whose enforcement was enjoined* were associated with a 9% increase in minors' abortion rates.</li> </ul>	<ul style="list-style-type: none"> <li>The study appropriately focused on abortions among minors and included state and year fixed effects.</li> <li>However, some or all of the effect could have resulted from minors' traveling to neighboring states for abortions.</li> <li>Findings that nonenforced laws affect abortion rates raise questions as to the credibility of the other findings.</li> </ul>
Henshaw, 1995 <sup>28</sup>	Mississippi; 1993	Ratio of minors to adults obtaining abortions in the state, and gesta- tional age at abortion; tabulations provided by Mississippi Department of Health	<ul style="list-style-type: none"> <li>The number of abortions obtained by minors younger than 18 years of age fell by only 5%, or less if some minors had abortions in Louisiana.</li> <li>The ratio of resident minors to adults having abortions in the state fell by 13%, a decrease offset by a 32% increase in the ratio of minors to adults obtaining abortions out of state.</li> <li>After the law, there was a marginally significant 19% increase in the ratio of minors to adults who obtained abortions after 12 weeks of gestation.</li> </ul>	<ul style="list-style-type: none"> <li>The study was somewhat limited by the relatively small number of abortions in Mississippi and the lack of data on abortions in Louisiana.</li> <li>The comparison group of all women aged 18 or older is not ideal for minors.</li> <li>The study would have benefited from comparison with trends in other states.</li> </ul>

Authors/ year	Area/study period	Outcome variables and their sources	Key findings	Summary assessment
Joyce and Kaestner, 1996 <sup>39</sup>	South Carolina, Tennessee and Virginia; 1986–1991	Probability that a pregnancy would end in abortion by age; individual-level data from state vital statistics agencies	<ul style="list-style-type: none"> <li>Parental involvement laws had no impact on the abortion probability in South Carolina or Tennessee (which had such laws) relative to Virginia (which did not) in most age-groups and racial groups.</li> <li>The exception was nonblack 16-year-olds in South Carolina, whose probability of abortion fell by 10 percentage points.</li> </ul>	<ul style="list-style-type: none"> <li>Some women in the groups analyzed as being exposed to the law would in fact not have been exposed when they conceived, which would tend to bias the estimates toward no effect.</li> <li>The study's main focus was pregnancy resolution, and not birthrates and abortion rates.</li> <li>Older women had much higher abortion rates and birthrates than minors; hence, the results are sensitive to whether changes are measured in absolute or relative terms.</li> </ul>
Joyce and Kaestner, 2001 <sup>39</sup>	Mississippi, 1992–1994; South Carolina, 1989–1991	Abortion rate by age, probability of second- trimester abortion, mean gestational age at abortion among women aged 20 or younger; individual-level records from state vital statistics agencies	<ul style="list-style-type: none"> <li>In Mississippi, after the law went into effect, the number of abortions among minors obtained out of state increased by 17%; the probability of second-trimester abortion increased by a nonsignificant 2.9 percentage points; and the mean gestational age increased by more than half a week.</li> <li>In South Carolina, the law had no effect on these measures.</li> </ul>	<ul style="list-style-type: none"> <li>Older teenagers' abortion rates were twice as great as minors' abortion rates in the prelaw period, which raises questions as to the validity of the comparison group.</li> </ul>
Joyce et al., 2006 <sup>41</sup>	Texas; 1997–2003	Birthrates and abortion rates by age at concep- tion among teenagers; individual-level data from state records	<ul style="list-style-type: none"> <li>When the law went into effect, the abortion rate of 17.5–17.75-year-olds fell 16% and their birthrate rose 4% relative to the rates among 18.0–18.2-year-olds.</li> <li>Changes in these rates were limited to white and Hispanic teenagers; no associations were found among black teenagers.</li> <li>The odds of abortions past 12 weeks increased among the younger group of minors relative to the older group.</li> </ul>	<ul style="list-style-type: none"> <li>This study broke new ground by classifying exposure to the law by age at conception instead of age at the time of the abortion or birth.</li> <li>With the woman's exact date of birth, the authors were able to compare the oldest group of minors exposed to the law with the youngest group of teenagers unexposed to the law. This enhanced internal validity.</li> <li>The conclusion that the parental involvement law caused some minors to continue their pregnancies is persuasive.</li> </ul>

<b>Authors/ year</b>	<b>Area/study period</b>	<b>Outcome variables and their sources</b>	<b>Key findings</b>	<b>Summary assessment</b>
Kane and Staiger, 1996 <sup>30</sup>	All U.S. counties; 1973–1988, excluding 1983 and 1986	Birthrate of women aged 15–17, 18–19 and 20–29; data from national vital statistics	<ul style="list-style-type: none"> <li>• Among white women, parental involvement laws were associated with birthrates that were 3% lower among those aged 15–17, but also 2% lower among those aged 18–19 and 1% lower among those aged 20–29.</li> <li>• The authors concluded that there was no strong evidence that parental consent laws influenced minors' birthrates.</li> </ul>	<ul style="list-style-type: none"> <li>• The authors used a sophisticated approach to assess whether teenagers' reproductive choices respond to abortion restrictions.</li> <li>• Analyses assumed that teenagers were aware of changes in the location of abortion clinics and used this knowledge in making reproductive choices.</li> <li>• The study may not have been able to control for all confounding factors.</li> <li>• The authors should have omitted counties with small populations.</li> </ul>
Klick and Stratmann, 2008 <sup>34</sup>	All states; 1981–1998	Gonorrhea rates by year, state, age and race/ethnicity; CDC data	<ul style="list-style-type: none"> <li>• Parental involvement laws in force were associated with reductions in gonorrhea rates among teenage women (15–19-year-olds) relative to older women.</li> <li>• The rate fell by 9.5 cases per 100,000 white female teenagers and 12 cases per 100,000 Hispanic female teenagers—reductions of 12% and 21%, respectively.</li> <li>• There was no statistically significant association for black female teenagers or for male teenagers.</li> <li>• Parental involvement laws whose enforcement was enjoined* did not have any effect.</li> <li>• The authors concluded that raising the cost of abortion induced a substantial proportion of teenagers to avoid risky sex.</li> </ul>	<ul style="list-style-type: none"> <li>• The study should have compared teenage women with women aged 20–24 rather than with all those aged 20 or older.</li> <li>• The teenage group included 18- and 19-year-olds, who were not affected by the law.</li> <li>• The estimates presented are too large to be credible.</li> <li>• It is not plausible that such laws would affect female but not male teenagers.</li> </ul>

Authors/ year	Area/study period	Outcome variables and their sources	Key findings	Summary assessment
Levine, 2003 <sup>14</sup>	All states; 1985, 1988, 1992 and 1996 (for abortion rates)	Birthrates by age (national vital statistics), abortion rates by age (Guttmacher Institute) and rates of sexual activity and unprotected sexual activity among women aged 15–18 (NSFG)	<ul style="list-style-type: none"> <li>Parental involvement laws were associated with a 22% reduction in the abortion rate of minors (15% when state-specific trends were included), but had little effect on birthrates.</li> <li>There was also a 6% decrease in unprotected sexual activity (not quite statistically significant) among women aged 15–18, but an increase of 12% in the proportion of women aged 19–24 who were sexually active.</li> </ul>	<ul style="list-style-type: none"> <li>This study used the most comprehensive approach to the analysis of parental involvement laws that can be achieved with state aggregates on a national scale.</li> <li>The resident abortion rates took into account interstate travel by women of all ages, but would not have captured teenagers who went out of state to avoid parental involvement requirements.</li> <li>The estimates were relatively sensitive to the inclusion of state-specific trends, and states that enacted laws during this time period differed greatly from states that did not.</li> <li>The analysis included approximately a dozen states for which the abortion rates were estimated by the Guttmacher Institute</li> <li>The analysis of sexual activity associated with parental involvement laws was of questionable value.</li> </ul>
Lichter et al., 1998 <sup>43</sup>	All states ex- cept Alaska and Hawaii; 1980 and 1990	Proportion of women in a county who were sin- gle heads of households with children younger than age 18; decennial census data	<ul style="list-style-type: none"> <li>Parental involvement laws were associated with an increase in the county-level proportion of women who were heads of households by 0.05–0.06 per 1,000 women.</li> <li>When women were stratified by race, the effects of these laws on the proportion were positive for white (0.05), black (0.06) and Hispanic (0.09) women, but were statistically significant only for the white group.</li> </ul>	<ul style="list-style-type: none"> <li>It is questionable as to whether the study has uncovered a meaningful association.</li> <li>There is no evidence that parental involvement laws caused a substantial increase in births to minors in the 1970s and 1980s.</li> <li>The authors likely underestimated the standard errors on the coefficient of the parental involvement law and thus overestimated their ability to detect an effect size as small as 1%.</li> </ul>

Authors/ year	Area/study period	Outcome variables and their sources	Key findings	Summary assessment
Matthews et al., 1997 <sup>21</sup>	All states; 1978–1988	Resident abortion rates (Guttmacher Institute) and birthrates (national vital statistics) of women aged 15–44	<ul style="list-style-type: none"> <li>Parental involvement laws were not associated with state birthrates or with abortion rates among women of childbearing age as a whole.</li> </ul>	<ul style="list-style-type: none"> <li>The study assessed rates among all women of childbearing age, making it difficult to statistically identify an effect of the laws on minors, who account for only a small proportion of abortions and births.</li> </ul>
Medoff, 2007 <sup>31</sup>	50 states; 1982, 1992 and 2000	Abortion ratio of minors and all women of childbearing age; Guttmacher data	<ul style="list-style-type: none"> <li>Parental involvement laws were associated with a 14% reduction of the abortion ratio among minors (ages 15–17) and a 13% reduction among all women of childbearing age.</li> </ul>	<ul style="list-style-type: none"> <li>The study used a cross-sectional analysis, which relied on variation between states to identify effects of parental involvement laws.</li> <li>Regardless of the controls, the potential for confounding in cross-sectional studies is great, as illustrated by the implausibility of the results.</li> </ul>
Meier et al., 1996 <sup>20</sup>	All states; 1982–1992	Abortion rate of women aged 15–44; Guttmacher and CDC data	<ul style="list-style-type: none"> <li>Parental involvement laws were not associated with a change in the abortion rate among women of childbearing age as a whole.</li> </ul>	<ul style="list-style-type: none"> <li>The study examined all women of childbearing age instead of focusing on minors.</li> <li>The study lacked statistical power and an appropriate research design; hence, it may have been unable to detect small changes in abortion rate.</li> <li>The regression analysis included a lagged abortion rate, an inappropriate way to control state fixed effects.</li> </ul>

Authors/ year	Area/study period	Outcome variables and their sources	Key findings	Summary assessment
New, 2007 <sup>32</sup>	Varying numbers of states, depending on the year; 1985–1999	Abortion rate of minors; CDC data	<ul style="list-style-type: none"> <li>Parental involvement laws were associated with average reductions in the abortion rate of 1.67 abortions per 1,000 among women aged 13–17 and of 1.05 abortions per 1,000 among all women of childbearing age.</li> </ul>	<ul style="list-style-type: none"> <li>The study used abortions by state of occurrence instead of by state of residence; that is, it did not correct for minors' interstate travel, which could account for some of the reductions.</li> <li>State parental involvement laws are not randomly assigned, and states that enact such laws tend to be more conservative than states that do not; this could account for the observed reductions.</li> <li>It is implausible that the laws could have an effect on all women of childbearing age almost as great as the effect on minors alone.</li> </ul>
Ohsfeldt and Gohmann, 1994 <sup>25</sup>	35 states; 1984, 1985 and 1988	Abortion and pregnancy rates, ages 15–17, 18–19 and 20–44; Guttmacher data	<ul style="list-style-type: none"> <li>Best estimates suggested that the law was associated with a reduction in the ratio of adolescent to older teenager abortion rates of 18% and pregnancy rates of 8%.</li> </ul>	<ul style="list-style-type: none"> <li>The study used a cross-sectional design, which cannot correct for unmeasured differences between states.</li> <li>Standard errors were underestimated, which increased the probability of falsely concluding that the results were significant.</li> <li>The results could be explained by minors' crossing state lines to circumvent parental involvement requirements.</li> </ul>

Authors/ year	Area/study period	Outcome variables and their sources	Key findings	Summary assessment
Pierson, 1995 <sup>37</sup>	Missouri; 1980–1992	Age-specific rates of abortion, fetal death and birth; individual-level data from the state's vital statistics agency	<ul style="list-style-type: none"> <li>Compared with women aged 18–19, women aged 15–17 had declines in pregnancy and abortion rates, and in the proportion of pregnancies ended by abortion after the parental involvement law took effect.</li> <li>Only 3% of infants born to minors were adopted, a proportion that was unchanged from the prelaw period.</li> <li>The proportion of abortions past 12 weeks' gestation increased by 21% compared with that of older teenagers.</li> </ul>	<ul style="list-style-type: none"> <li>The study's results are completely descriptive, with no statistical tests of changes in outcomes before and after Missouri's consent law took effect.</li> <li>The results could be explained by minors' obtaining abortions in Illinois and Iowa.</li> <li>The finding regarding gestation is suggestive but not definitive because of the lack of data on abortions performed in Illinois and Iowa.</li> </ul>
Rogers and Miller, 1993 <sup>6</sup>	Minneapolis; 1975–1988	Birthrates by age and ethnicity; data from the state's vital statistics	<ul style="list-style-type: none"> <li>An increase in the birthrate of minors in Minneapolis was concentrated among nonwhites, especially Asians, during a period when there was an influx of Hmong immigrants, who are "known to bear children at early ages."<sup>6</sup></li> </ul>	<ul style="list-style-type: none"> <li>The study provides convincing evidence that the parental involvement law in Minnesota did not cause an increase in minors' birthrates, which in any case did not increase in the state as a whole.</li> <li>The study demonstrates the sensitivity of changes in the birthrates of minors in small areas due to population shifts.</li> </ul>
Rogers et al., 1991 <sup>40</sup>	Minnesota; 1975–1987	Age-specific abortion rates and birthrates; tabulations provided by the state's vital statistics agency	<ul style="list-style-type: none"> <li>The parental involvement law was associated with a greater reduction in the abortion rate of minors aged 15–17 (28%) than of women aged 18–19 (9%).</li> <li>The proportion of second-trimester abortions (those past 12 weeks' gestation) increased more for minors (25%) than for 20–44-year-olds (6%), although the second-trimester abortion rate of minors decreased.</li> <li>There was no impact on the birthrate of minors.</li> </ul>	<ul style="list-style-type: none"> <li>The study did not account for abortions obtained out of state. The authors assert that travel outside Minnesota is difficult for minors because of the distances involved, but some parents likely took their daughters to other states to avoid involving the other parent or going to court.</li> <li>The authors present no data supporting increased use of contraception or reductions in unprotected sex.</li> <li>Trends were not compared with those in other states, and 18–19-year-olds are an imperfect comparison group for minors, as their trends might differ.</li> </ul>

Authors/ year	Area/study period	Outcome variables and their sources	Key findings	Summary assessment
Sen, 2007 <sup>47</sup>	All states; 1981–2002	Child abuse rates; data from the Web-based Injury Statistics Query and Reporting Systems	<ul style="list-style-type: none"> <li>Parental consent laws were associated with a 20% increase in deaths by homicide for white children, but no effect was found for black children or for parental notification laws.</li> <li>The presence of border states with no parental involvement laws was associated with a 6% decrease in deaths by homicide.</li> </ul>	<ul style="list-style-type: none"> <li>A link between parental involvement laws and child abuse assumes that the laws cause an increase in births, a questionable assumption for the 1980s and early 1990s, when many minors could access abortion services in neighboring nonrestrictive states.</li> <li>The magnitude of the effect reported is implausible in view of the small or nonexistent effect of the laws on birthrates.</li> <li>Measurement errors or missing variables may have confounded the results.</li> </ul>
Tomal, 1999 <sup>38</sup>	11 states; 1995	County-level abortion rates and birthrates of women aged 15–17 and 18–19; tabulations provided by states' vital statistics agencies	<ul style="list-style-type: none"> <li>Parental consent laws were associated with abortion rates that were 16% lower among women aged 15–17 and 31% lower among those aged 18–19. Parental notification laws were associated with rates that were 34% and 15% lower, respectively.</li> <li>Birthrates were also markedly higher in the presence of these laws: 42% and 26% higher for the two age-groups with consent laws, and 39% and 44% higher with notification laws.</li> </ul>	<ul style="list-style-type: none"> <li>The cross-sectional design was unable to control for important differences between states and led to implausible results.</li> <li>The findings of large effects among older teenagers, who were unaffected by the laws, indicate that uncontrolled variables likely account for the findings among minors.</li> </ul>

\*Laws whose enforcement is enjoined are legally prohibited from taking effect and hence are not enforced. *Notes:* Superscripted numbers refer to the reference list (see page 29). NSFG=National Survey of Family Growth. CDC=Centers for Disease Control and Prevention.







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