Long Term Effects of Abortion Parental Involvement Laws

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We examine whether women growing up in states with abortion parental involvement laws had more children when they were between the ages of 21 and 32 than women growing up in states without such laws. Our results indicate that these laws were associated with an increase in fertility of white and black women, a decrease in educational attainment and, among white women, an increase in the probability of receiving public assistance. The results, which mask much larger effects for women actually affected by the laws, suggest that these laws increase teen births and that this increase is long lasting (JEL: J13, I18)

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I. Introduction

The U.S. Supreme Court's decision in *Roe vs. Wade* (1973) legalized abortion, but left states some discretion to enforce abortion regulations and restrictions. One of the most prevalent forms of state regulation is legal requirements for minors to either notify a parent(s) or obtain their consent before obtaining an abortion. While there were relatively few parental involvement (i.e., consent or notification) laws prior to 1989, the resolution of a series of legal cases in the 1980s and early 1990s greatly expanded states' passage of such laws.[1] Between 1989 and 1999, 20 states passed laws requiring parental involvement (consent or notification) in a minor's abortion. Currently, thirty-five states require some type of parental involvement in a minor's abortion and another seven have laws that are not enforced.[2]

Parental involvement (PI) laws target minors in a way similar to many other laws regulating adolescent health and social behavior (e.g., tobacco and alcohol prohibitions, age of consent and statutory rape, age of majority for marriage, voting, and entering legal contracts). Similar to these laws, there has been substantial controversy surrounding PI laws, but the moral and ethical dimensions of the issue, as well as the relationship of PI laws to the broader debate over abortion, has made PI laws particularly controversial. While there are many legal and ethical aspects of the debate over PI laws, from a positivist point of view, one of the most important issues is the effect of PI laws on teen births. Supporters of PI laws claim that these laws reduce abortions and decrease births because the laws encourage young women to become more cautious in terms of sexual activity and use of contraception including abstinence. Opponents of PI laws argue that these laws deter minors from having an abortion, have little effect

[1] See Section IV for a discussion of the legislative and legal history of parental involvement laws.
[2] MD and CT have PI laws that are technically in effect but minors can *de-facto* access abortion services without involving a parent. Therefore, these states are treated as not having PI laws.

on teen sexual behavior, and increase unwanted births. Thus, an important difference between the opposing views is whether PI laws cause births to decrease or increase, as both sides argue that PI laws will likely decrease abortions. Notably, empirical evidence on the effect of PI laws on births is quite limited.

Another important, but neglected, aspect of the debate over PI laws is whether they have long term effects. PI laws may alter the timing and number of births and, through changes in fertility, alter investments in human capital that are known to affect adult socioeconomic wellbeing. Potential long term consequences of PI laws such as these should be included in any evaluation of the costs and benefits of PI laws. However, there are no studies of the long term consequences of PI laws. All previous research related to PI laws has focused on minors. More generally, the study of PI laws is relevant to the literature on consequences of teen birth. Despite substantial research, there is no consensus as to the causal effect of a teen birth on mother's human capital and well being (Ashcraft and Lang [2006]; Hoffman and Maynard [2008]). If PI laws increase births, then this exogenous change in births can be used to evaluate the effect of teen births on future outcomes at least for women affected by the laws.

In this paper, we examine the long term consequences of PI laws. We focus on births because of the centrality of this outcome and investigate whether PI laws are associated with fertility of women with an average age of 27 and an age range between 21 and 32. We also evaluate whether PI laws are associated with outcomes affected by fertility, specifically, educational attainment, employment and receipt of public assistance. Our empirical analysis exploits the natural experiments afforded by state enactment of PI laws. We compare outcomes of cohorts of women that have been more or less exposed to the restrictions imposed by PI laws to assess whether PI laws have a long term effect on fertility, human capital investment and other socio-economic outcomes.

Results from our analysis indicate that PI laws are associated with between a 1.5 to 3.5 percent increase in fertility of white women and between a 5 to 9 percent increase in fertility of black women. These results, which mask much larger effects for women actually affected by the laws, suggest that PI laws increase teen births and cause a permanent increase in fertility at least through the age range of women in our sample (until age 32). In fact, estimates suggest that every additional teen birth caused by PI laws increases fertility by more than one birth. We also find that PI laws are associated with a significant decrease in educational attainment of white and black women, and an increase in receipt of public assistance among white women.

II. Previous research

Previous research on the effects of PI laws is limited, and focused on the effect of these laws on teen abortions. Only a few studies examined the effect of PI laws on births. This literature is characterized by national analyses and state-specific studies (see Dennis et al. [2009] and Levine [2006] for reviews). National studies use aggregate, state-level abortion data and difference-in-differences research designs (i.e., year and state fixed effects). Results from these studies indicate that PI laws are associated with significantly lower abortion rates (Haas-Wilson [1996]; Levine [2003]). The main limitation of these studies is that abortion data are by place of occurrence. This allows for the possibility that the true effect of the laws may be confounded by the effect of interstate travel, as some minors leave their state of residence to get an abortion and fewer non-resident teens enter the state for an abortion after enforcement of a PI law. However, Levine (2003) reported a negative association between PI laws and teen abortion rates using abortion data by state of residence. [3]

[3] Alan Guttmacher Institute publishes abortion data by state of residence (see Dennis et al. [2009] for a discussion about the limitation of AGI teen abortion data by state of residence).

Other studies focus on PI laws in specific states using individual level data on abortion from state health departments. Results from these studies generally show that PI laws are negatively correlated with abortion rates. These studies too have limitations: the completeness and quality of abortion data varies by state, and many abortions of teens leaving their state are not measured because most states lack a system of reciprocal reporting. Absent information on abortions obtained by residents out-of-state, it is therefore difficult to assess to what extent the decrease of in-state abortions reflects a true decrease or greater out-of-state travel (e.g. Cartoof and Klermann [1986]; Rogers [1991]; Ellerston [1997]; Joyce and Kaestner [2001]). More recent studies are less vulnerable to this problem because PI laws have become more prevalent reducing the options for interstate travel for most minors. In particular, Joyce et al. (2006) and Colman et al. (2008) show that PI laws are associated with lower teen abortions rates in Texas.[4]

Overall, while the evidence from previous research is mixed, our reading of it is that the preponderance of studies, particularly studies with better research designs, indicates that PI laws are associated with a decrease in abortions, although the effect is moderated by whether out-of-state travel is feasible.(Levine [2003]; Joyce et al. [2006]; Colman et al. [2008]). The decline in abortions in response to PI laws motivates the analysis of longer term outcomes, particularly whether PI laws are associated with long term changes in fertility.

Relatively few papers analyzed the effect of PI laws on birth rates and these studies have not produced a consensus finding. Some studies of PI laws find no effect (or a decrease in) on birth rates and attribute this finding to the fact that

[4] The credibility of the findings of Joyce et al. (2006) is bolstered by their use of information on age at the time of conception to define plausible treatment and comparison groups. The use of age at time of conception is particularly important when using 18 year-olds as comparison group because a very large fraction of 17 year-olds conceiving as minors give birth when they are 18. More generally, the choice of appropriate comparison groups is an important issue. Most studies use older teens and/or older women as comparison group without adequately assessing whether the level and trends in abortions and births differ across those groups.

PI laws may cause minors to abstain from sex or to use contraception more effectively (Rogers et al. [1991]; Kane and Staiger [1996]; Ellerston [1997]; Levine [2003]). Other studies show a positive effect of PI laws on births (Joyce et al. [2006]; Colman et al. [2008]).[5] While there is little consistent evidence on the effect of PI laws on births, the fact that PI laws decreased teen abortions raises the possibility that PI laws also affected fertility.

The study of PI laws is similar to other laws that affect the cost of fertility control, and several studies have studied the long term impact of changes in cost of fertility control using similar types of "natural experiments" such as *Roe v. Wade* and the introduction of the Pill. Results from these studies suggest that improved fertility control may delay timing of the first birth, permanently decrease women's fertility, and improve socio-economic outcomes such as educational attainment and labor force participation later in life (Angrist and Evans [1999]; Goldin and Katz [2002]; Bailey [2006]; Ananat et al. [2007]).

Our brief review of the literature has highlighted several points. First, PI laws are associated with a decrease in teen abortions, particularly when out-of-state travel for an abortion is limited. Second, the effect of PI laws on teen births is not well known, as there is relatively little research and results from it are mixed. Third, studies of laws similar to PI laws, in that they affect the cost of fertility control among teens, have found long term effects on fertility and other outcomes. In sum, there are plausible reasons to believe that PI laws may have long term impacts on women. Assessing whether there are long term consequences of PI laws is important because it provides a more complete picture

[5] A significant problem that these studies face is limited statistical power to identify reliably changes in births. Few minor women are actually affected by the laws because few become pregnant each year and decide to have an abortion without involving their parents. This implies that the increase in births due to the laws is likely to be a small fraction of the births that would have been observed had the law not been enforced.

of the consequences of these laws and provides evidence relevant to the debate over the consequences of teen births.

III. Conceptual Framework

Parental involvement laws raise the cost of having an abortion for a minor. Therefore, minors are likely to have fewer abortions, and potentially more births. If births are affected, it may represent a change in the timing of births or a permanent change in fertility (i.e., number of children); there would be an increase in (completed) fertility if the additional birth was not offset by fewer subsequent births. Furthermore, changes in the timing and/or number of births may affect subsequent education choices and other investments that influence fertility later in life. For example, if a woman becomes pregnant as a teenager, she may drop out of school or not go to college, and such decisions are likely to have substantial consequences on current and future human capital investments and subsequent fertility (see Klepinger et al. [1999] for a review of consequences of teen births). PI laws, however, may affect sexual activity and contraceptive behavior. Kane and Staiger [1996] and Levine [2003] have argued that PI laws, by increasing the costs of abortion, induce minors to be more cautious in terms of sexual behavior and/or contraceptive use, which will reduce pregnancy rates. If so, women may have fewer pregnancies and fewer children.

These different hypotheses are illustrated in Figure I. The solid line represents the fertility pattern over a woman's lifecycle in the absence of PI laws. In this case, the number of children grows with age (we have drawn it as a smooth curve for convenience). Now, consider the implementation of a PI law affecting women between ages 15 to 17. The law increases the cost of abortion and, if pregnancy behavior does not change, this will increase their fertility at those ages. This is shown by an increase in fertility prior to age 18 (dashed line). If the law affects only the timing of births, after age 18, fertility would slowly return to the level and path occurring in the absence of a PI law. In this case, there would be no difference in fertility at older ages between women affected and unaffected by PI laws. Alternatively, the PI law could put women on a higher fertility trajectory that would be above the level and path occurring in the absence of a PI law. In this case, there would be a difference in fertility at older ages between women affected and unaffected by a PI law. Furthermore, there may be a multiplier effect. As mentioned above, the timing of a first birth can affect other choices (e.g., education), which in turn may affect fertility choices throughout life. In this case, the gap in the number of children ever born to women with different exposure to PI laws may grow with age and be larger than the one implied by a "mechanical" substitution of one unwanted birth for an abortion.

However, it is possible that the PI laws change sexual and contraceptive behavior of teens, which would result in a decrease in fertility prior to age 18 (dotted line). If this decrease persisted and is not offset by subsequent changes (return to level and path of fertility in the absence of PI laws), then, we would observe a reduction in the number of children ever born to women affected by a PI law as a minor compared to women unaffected.

The upshot of this discussion is that at age 28, fertility of women affected by a PI law may be higher, the same, or lower than the fertility of women unaffected by such laws. A higher level of fertility is consistent with a model that posits that PI laws raise the cost of abortion, decrease abortions, increase teen births, and this increase in births is permanent. A lower level of fertility is consistent with the model of Kane and Staiger (1996) in which teens change their sexual and contraceptive behavior and these changes result in a permanent reduction in fertility. No difference in fertility is consistent with temporary changes in fertility, or no effect of PI laws on births.

IV. Brief History of Abortion Parental Involvement Laws

Throughout the first half of the 20th century, abortion was illegal in the U.S. and allowed only to preserve a woman's life. In 1962, the American Law

Institute promulgated the new Model Penal Code (MPC), which liberalized abortion under limited circumstances including rape, statutory rape, incest and severe physical-mental defects of fetus or mother. In the late 1960s, 13 states began to allow abortions under the MPC provisions starting with Colorado in 1967 (Mertz et al. [1995]; [1996]).[6] Eight of these states included in their statutes some type of parental involvement before a minor could obtain an abortion.[7]

The U.S. Supreme Court decision in *Roe v. Wade* (410 U.S. 113 January, 1973) legalized abortion nationally, but because *Roe* did not involve a minor, the question of parental involvement in a minor's abortion remained unanswered. Shortly after *Roe*, some states that previously adopted the MPC provisions continued to enforce the parental involvement component of their statutes, and a few other states enacted some form of parental involvement law (Mertz et al. 1995; 1996).[8] At the same time, numerous legal attacks were launched upon these parental consent requirements.

In the late 1970s the Supreme Court decided a series of cases that defined more clearly the boundary of admissible parental involvement. The first major case was *Planned Parenthood of Central Missouri v. Danforth* (July 1976). The case was in regard to a Missouri law that required consent of the spouse for married individuals and parental consent for minors to obtain an abortion. The court found the law unconstitutional because it allowed an "absolute, and possibly arbitrary, veto" power on minors, thereby violating their privacy. Shortly after *Danforth*, most of the PI laws in effect before 1976 were either declared unenforceable, unconstitutional, or were permanently enjoined. By the end of the decade, only two states had PI laws in effect.

[6] The others were CA and NC (1967), MD (1968), AR, DE, GA, NM and OR (1969), SC, KS and VA (1970) and FL (1972).
[7] These states were CO, NC, AR, DE (not enforced), NM, OR, SC, VA and FL.
[8] IN, ID, KY, LA, MO, MT, NV, ND, OH, SD (not enforced) and UT. In most cases these laws were in addition to common law rules requiring parental consent for minors' medical care.

In *Bellotti v. Baird II* (July 1979), the Supreme Court evaluated a Massachusetts parental consent law, which unlike *Danforth*, contained a judicial bypass allowing minors to waive parental consent by means of a court order. The possibility for the minor to circumvent parental involvement became a key element in subsequent court decisions. In fact, the Supreme Court has consistently upheld notification laws containing a bypass mechanism. Shortly after *Bellotti*, subsequent rulings further clarified the parameters of acceptable judicial bypass (*H.L. versus Matheson*, 1980; *City of Akron v. Akron center of Reproductive Health*, 1983; *Planned Parenthood v. Ashcroft*, 1983; *Hodgson v. Minnesota*, 1990; *Ohio v. Akron center of Reproductive Health*, 1990). Finally, subsequent to *Planned Parenthood of S.E. Pennsylvania versus Casey* (1992), states are allowed to regulate abortion access as long as regulations do not impose an "undue burden" on women, and PI laws were included among the policies that do not impose an undue burden (to the extent that judicial bypass is included).

This series of court rulings shaped the pattern of introduction of PI laws from the mid-1980s through the mid-1990s shown in Table I and Figure II. In the late 1970s-early 1980s, few states enforced PI laws (they were nine in 1984), but that number increased to 15 by 1990 and to 26 by 1996. Today thirty-five states have PI laws in effect. Given the timing of state enactment of PI laws, our empirical analysis focuses on PI laws enacted in the early to mid 1980s. We describe the empirical approach next.

V. Empirical Strategy

A. Empirical Specification

The purpose of the analysis is to obtain estimates of the association between women's outcome when adult and exposure to PI laws as a minor (between ages 15 to 17) that can be interpreted as causal. To accomplish this task, we use regression methods using the following model specification:

$$Y_{sat} = \alpha_o + \beta EXP_{sc} + \delta_s + \phi_a + \gamma_t + \delta_c + Z_{cs} + u_{cas}, \qquad (1)$$

In equation (1), Y_{ast} represents the outcome of women of age *a* observed in current year *t* (and thus born in year *c* since *c = t – a)* and born in state *s*. We examine several outcomes: fertility (number of children), educational attainment, employment and receipt of public assistance. Equation (1) includes fixed effects for state-of-birth (*s*), age (*a)*, year-of-birth cohort (*c*) and current (survey) year (*t)*. Year-of-birth cohorts are measured in five-year intervals.[9] Also included in equation (1) are state-level measures (*Z*) of unemployment rate, labor force participation rate, per-capita income and the average number of abortion providers in state-of-birth (per 100,000 women of childbearing age) measured at the time a given cohort was between ages 15 to 17. The variable EXP_{cs} measures exposure to a PI law between ages 15 to 17 for women born in year *c* and state *s.* It is calculated as the fraction of time between ages 15 to 17 a woman is exposed to PI laws for abortion. This measure varies only by state-of-birth and year-of-birth. The focus is on minors aged 15 to 17 since very few abortions occur among women younger than 15. In some models, we also include (linear and quadratic) state-of-birth specific trends in year-of-birth to allow for changing state-specific, birth-cohort characteristics.

Ideally we would like to know the state of residence of women when they were age 15 to 17. However, no dataset provides this information. Instead, we use state-of-birth. A majority of adolescents reside in the state where they are born. Estimates from the 1990 and 2000 censuses show that between 77 and 80 percent of women aged 15 to 17 reside in the state where they were born. Therefore, using the state-of-birth should be a reasonable proxy for the state of residence

[9] Some type of restriction is necessary because of the linear dependence between age, period and cohort effects. We experimented using also other cohort groupings (like ten years) and results are similar to those reported in the text.

between ages 15 to 17. The extent of measurement error and potential bias due to it are discussed in more detail later in the paper.

The interpretation of estimates of associations between PI laws and outcomes depends on the assumptions of our research design (i.e., identification strategy). We assume that PI laws are exogenous in that they were not the result of any individual choice. This seems plausible because it is unlikely that minors and their families moved in response to these laws. Thus, the primary threat to validity comes from omission of factors that vary at the state-of-birth and birth year level that are correlated with the variable measuring exposure to a PI law (*EXP*) and outcomes (*Y*). To address this potential threat to validity, we include several characteristics measured at the state-of-birth and birth year level that are likely to be relevant including the unemployment rate, the labor force participation rate, per-capita income and the average number of abortion providers (per 100,000 women of childbearing age). In addition, we include state-of-birth specific trends in year-of-birth. We believe these solutions are a reasonable approach and that estimates obtained from the analysis are plausibly interpreted as causal.

However, we provide additional evidence to bolster this argument. Specifically, we examined outcomes unlikely to be affected by PI laws (e.g., physical and sensory disabilities) and associations between PI laws and men's education and labor supply. While men's education and labor supply may be affected by PI laws through the marriage (mating) market, effects of PI laws on men should arguably be smaller than effects of PI laws on women. Generally, results from these analyses provide substantial evidence to support the validity of our identification assumptions.

We estimate equation (1) using ordinary least squares regression methods. The sample, which we describe below, is stratified by race because there are substantial differences by race in teen abortion and birth rates (Henshaw, 2008).

Standard errors are clustered at the state-of-birth level to allow for non-independence within state-of-birth.

B. Data

The data used to in the study come from the 2000 (one percent sample) Census and the American Community Survey (2001-2009), which is a monthly survey of US households (IPUMS, Ruggles et al. [2010]). The ACS sample design approximates the Census 2000 sample and it is intended to eventually replace it. The advantage of the ACS lies in its annual collection that allows cohorts (defined by year-of-birth) to be tracked over time.

The primary outcome of interest is women's fertility, and ideally, we would use a measure of the number of children ever born to women. However, this information is not available in the data.[10] Instead, we used the number of own in children in the household as an estimate of the number of children ever born (CEB). The use of number of own children in the household in place of number of children ever born is likely to bias estimates downward unless we restrict the sample to younger women. Women with restricted legal access to abortion when young are more likely to have a child early in life. As a result, these women observed at, say age 40, will appear to have had fewer children, compared to women of the same age who were not exposed to the law because older children are more likely to have left the household. Thus, greater exposure to abortion restrictions would be correlated with fewer own children in the household, but this would be just due to measurement error. By focusing on

[10] In addition to fertility at age 27, we would like to know whether part of the apparent increase in fertility occurred when the woman was a minor. Unfortunately the ACS has no direct information available on age at first birth. We attempted to construct an indirect measure of early-birth using information on age of mother and age of eldest own child in the household. However, means of this measure did not match well estimates from national data and revealed that our measure was always a significant underestimate. Given this substantial measurement error, which is likely to be systematically correlated with PI laws, we did not pursue this analysis.

relatively younger women (below age 33), we mitigate this measurement issue because, at those ages, children will still be in the household.

To set the age cutoff, we examined data from the 1990 census (the last year in which the CEB variable is available) to assess how well the number of children in the household tracks the number of children ever born by age. As can be seen in Figure III, the number of own children in the household almost perfectly tracks the number of children ever born (CEB) for women 32 years of age and younger. Few children leave the household before age 18 and few births occur before age 15. Thus, up to age 32, most children ever born reside with the mother. Therefore, we limit the sample to women age 32 or younger.[11] As Figure III indicates, some children do not reside with their mother. If PI laws cause early fertility and teens affected by the laws are more likely to choose adoption, then the probability of not residing with the mother is positively correlated with the PI law.[12] In this case, estimates of the association between PI laws and fertility will be downward biased (less positive). However, the impact of the effect of adoption on estimates of associations between PI laws and other outcomes (for example education and labor supply) may be less severe because there is no extra child in the household that causes changes in these behaviors. This means that associations between PI laws and education (labor supply) may be less (downward) biased than associations between PI laws and fertility.

[11] Ananat et al. (2007) found that abortion legalization observed changes in fertility at age 25 that remained relatively constant over the rest of women's childbearing ages.

[12] The legal abortion environment women face may be associated with the probability of adoption if prospective mothers take into account the *full cost* of abortion in their pregnancy resolution decision and view birth, adoption and abortion as imperfect substitutes. To the extent that the number of children adopted can be view a rough proxy for the number of "unwanted" children, women exposed to a more(less) restrictive abortion environment would be expected to have a higher (lower) probability to relinquish their children for adoption. For example, some research suggests that states that legalized abortion before *Roe* experienced a substantial decline in the rate of adoptions (Bitler and Zavodny, 2002).

The sample used in the analysis consists of women born between 1968 and 1988, who were 15 to 17 between 1983 and 2005, and observed between 2000 and 2009 at ages 21 to 32. These sample characteristics are a result of the fact that we focus on the period when PI laws were rapidly changing (e.g., 1990s), the age restriction we impose because of measurement issues, and the survey years we use. Women born in 1968 were between ages 15 to 17 in the early-mid 80s when most states did not have parental involvement laws in effect.[13] Women born in 1988 were in their middle teen years in the early 2000s when most states had parental involvement laws. Thus, our sample includes cohorts of women from the same state with differing exposure to PI laws. Foreign born women are excluded because of the high likelihood of not being affected by PI laws and the absence of information on state of birth or state of residence at age 15. Hispanic women are excluded because they are mostly foreign born, and those born in US are geographically concentrated limiting the amount of variation in PI laws. Data are aggregated at the race (non-Hispanic black and non-Hispanic white), state-of-birth s, cohort c and age a (and thus current year) level. Cells with a sample size smaller than 25 observations are excluded, which reduces the number of states available for black women to 22.

We measure exposure to PI laws in two ways. The first is as the fraction of time between ages 15 and 17 (women born in cohort c and state s) during which the state had a PI law. Adjustment is made for years when the law was in effect for only part of the year. This measure of exposure varies between zero and

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[13] A potential concern is that early cohorts of the sample are born before abortion legalization during a time at which past research has examined the impact of abortion legalization on the selection of who was born (Ananat et al., 2009; Gruber et al.,1999). If states that legalized abortion early are correlated with those who do not have parental consent laws, this could be a problem. To control for this potentially confounding factor, we tested the robustness of our estimates to the inclusion of an indicator for whether women are born before abortion legalization. This variable varies only by year-of-birth and (group of) state (i.e. *Repeal* vs. *Roe*). Results, not reported, are robust to this specification and are available upon request.

one with greater values indicating greater exposure. Second, to incorporate the idea that PI laws are more restrictive when out-of-state travel is not an alternative, we created an adjusted measure of exposure that takes into account the legal environment in surrounding states. For example, suppose a minor is exposed to PI laws in her own state. Consider two scenarios. In the first, the surrounding states have no laws. In the second, all bordering states enforce PI laws. The adjusted exposure measure would reflect a more restrictive environment in the latter scenario. Specifically, we calculated the fraction of time between ages 15-17 in which a woman could obtain an abortion without parental involvement in each of the bordering states and took the average across these states. Larger values of this variable indicate that it was easier for a woman to avoid the parental requirement in her own state by traveling to neighboring states. We then scaled the simple measure of exposure by the inverse of this variable, so that the exposure variable takes smaller values when a given woman can more easily avoid the restriction by going to a bordering state.

As noted, we used state of birth to construct exposure to PI laws. Estimates from the 1990 and 2000 censuses show that between 77 and 80 percent of women aged 15 to 17 reside in the state where they were born. Other evidence also indicates that a large majority of women will be correctly assigned. Approximately 67 percent of women in our age range currently reside in the state they were born-in. For the remaining 33 percent, calculations using data on migration patterns of state of birth to state of residence, indicate that approximately 51 percent of women not residing in their state-of-birth currently reside in a state that would have had the same legal abortion environment. In other words, these women would have faced the same abortion restrictions between ages 15 to 17 regardless of whether state-of-birth or state of current residence are used as a proxy for their actual (unobserved) residence at those ages. This means that another 17 percent of the sample is correctly assigned. Thus, overall we expect that at least 84 percent of sample is assigned the correct legal abortion environment even when using the state of birth as a proxy for state of residence at age 15.

VI. Results

A. Number of Children

In Table II estimates of the association between exposure to PI laws when young and the number of own children per woman are reported separately for white and black women. The format of the table is as follows. For each racial group, several model specifications are estimated. Estimates in column one are from models that include state-of-birth fixed effects, year fixed effects and age fixed effects. Column two adds control for cohort fixed effects (grouped by 5 years). Column three adds state variables at the time the cohort was between ages 15 to 17. Columns four and five add state-specific linear and quadratic trends by year of birth, respectively. Finally, each model is estimated twice, using the simple measure of exposure and the adjusted measure of exposure that takes into account the legal environment in surrounding states.

Panel A of Table II reports estimates for white women. Estimates obtained using the simple measure of exposure indicate that there is no significant association between exposure to restrictive PI laws between ages 15-17 and later fertility. Estimates are relatively small, for example, the estimate in column (5) indicates an effect size of 1.5 percent, and always insignificant. Also evident in Table II is the fact that estimates are sensitive to the model specification—there are substantial trends in the number of children by state-of-birth and birth year at least for white women. However, adding a quadratic trend term (column 5) has relatively little effect on estimates suggesting that the state-level variables and linear trend are adequately controlling for most of these unmeasured trends. Estimates obtained using the adjusted measure of exposure, which takes into account abortion options in surrounding states, are similar, but larger in absolute value. Estimates in columns four and five suggest that moving from an environment of never being exposed to restrictive parental laws to always being exposed between ages 15 to 17 (both in own state and all surrounding states) is associated with an increase in white women's fertility of 0.02 to 0.027 children per woman (a 2.6 to 3.5 percent effect relative to the mean). While these estimates appear small, they mask much larger effects for those actually affected by the law. Only a small fraction of women in each cohort are affected by the law. We return to this issue shortly.

Estimates in Panel B are for a sample of black women. In this case, there is great uniformity of estimates; all estimates are positive, of approximately the same magnitude, and either statistically significant at the 0.10 level or close to being so. Estimates pertaining to the simple measure of exposure indicate that always being exposed to restrictive abortion laws when young is associated with an increase in fertility later in life of 0.04 to 0.05 children per woman (approximately a 4 to 5 percent effect relative to the mean). Estimates obtained using the adjusted measure are larger and indicate that facing a restrictive abortion environment when young (both in own state and in surrounding states) is associated with a 5.3 to 8.7 percent increase in the number of children.

Overall, estimates in Table II suggest that exposure to a restrictive abortion environment between ages 15 to 17 is associated with higher fertility as young adults. However, estimates are only statistically significant for black women. While estimates for white women are not statistically significant in Table II, we provide alternative estimates below that address concerns over measurement error in determining exposure that are statistically significant and of the same approximate magnitude. Estimates also underscore the importance of taking into account the abortion environment in neighboring states, as associations are larger when obtained using the adjusted exposure measure. This is exactly what would be expected based on previous findings that teens do travel out-of-state to avoid the PI laws.

To further assess whether the associations in Table II are real or spurious, we re-estimated models using a sample that also includes older women—women up to age 39. As we described earlier, the use of number of own children in the household in place of number of children ever born is likely to bias estimates downward (and therefore we restricted our sample to women up until age 32). This problem would be particular severe if we included older women because PI laws are likely to cause teen births and older women will have fewer of their ever born children in the household. Thus, estimates of associations between PI laws and fertility should be less positive as we add older women. Estimates on this sample are of the same sign, but less positive and/or less significant compared to the sample of younger women, as expected (results are not shown but are available upon request). These results suggest that PI laws are associated with early births and are consistent with the evidence presented in Table II.

B. Interpretation of the Results

Estimates of the long term impact of the PI laws in Table II may seem relatively small, but it is important to recognize that the estimated effects are obtained using a sample of women most of whom were unaffected by PI laws between ages 15 to 17. Here, we try to assess the magnitude of the implied effect for those minors actually affected by the law.

Consider, for example, the results for whites. The discussion is focused on estimates in columns 4 and 5, as these are arguably the preferred estimates given evidence that the inclusion of state-specific trends for year-of-birth matter. Estimates in Table II using the adjusted exposure measure suggest that always being exposed to PI laws when young increases the number of children per woman by 0.02 to 0.027 (a 2.6 to 3.5 percent effect).

To identify the group of minors actually at risk we need to consider that, over the relevant time period (e.g., late 1980s and 1990s), about 3 to 5 percent of non-Hispanic white minors become pregnant each year. Thus, on average, approximately 12 percent of white females become pregnant between ages 15 to 17. Among these, about 50 percent have an abortion, but not all of them are at risk of not telling their parents. There is only limited evidence about the extent to which parents know about minors' pregnancy and desire to have an abortion. Henshaw and Kost (1992) reported that approximately 40 percent of minors do not involve parents in their decision to have an abortion (48 percent of whites and 33 percent of blacks).

Based on these figures, it follows that the proportion of whites teens at risk (i.e., pregnant and wanting an abortion without telling parent) is approximately 3 percent (i.e. 0.12*0.50*0.48). Assume the law caused 1 of 2 minors at risk to have a birth (Joyce et al., 2006 present evidence consistent with this assumption). Then, 1.5 percent of teens will have a birth instead of an abortion because of the PI law. It follows that the 0.02 to 0.027 increase in fertility among white women documented in Table II (columns four and five) is driven by only 1.5 percent of minors. If we inflate these estimates, the resulting implied effect for minors actually affected by the law is 1.3-1.8 (i.e. 0.02/0.015- 0.027/0.015). In other words, among women affected, every abortion averted is associated with about 1.3 to 1.8 additional births for women with an average age of 27. Similar calculations for black women suggest that every abortion averted because of the PI law is associated with 1.9 to 2.6 additional births.[14]

[14] About 42 percent of black teens become pregnant between ages 15 to 17. Assume half of them will have an abortion; 33 percent do not tell their parents. Assume the law causes 1 of 2 minors at risk to have an abortion. Then the fraction of minors affected is 0.42*0.5*0.33*0.5=0.035. Note that there are no studies assessing the impact of PI laws on minors by race except for Joyce et al. (2006). However the black sample size is very small to draw definitive conclusions.

It is important to keep in mind that the purpose of this exercise is not to obtain exact estimates, but to evaluate the plausibility of the estimates in Table II. Admittedly, the calculations are approximate, but the estimates produced seem reasonable. An implied effect of PI laws on those affected greater than one would be consistent with the hypothesis that having an early birth may cause education to decrease and worsen labor market outcomes, making more children less expensive to bear and more likely (Keplinger et al. [1999]). In other words, women exposed to the laws when young will have higher fertility as an adult not only because they "mechanically" substitute unwanted births for abortions, but also because the timing of their first birth affects other outcomes which, in turn, influence fertility later in life. Indeed, as shown in the next set of results, exposure to PI laws is also found to be associated with other socio-economic outcomes (in particular educational attainment).

C. Educational Attainment

In this section we analyse the effect of exposure to PI laws on the probability of completing high school and on the probability of having some college education.[15] Estimates for this analysis are presented in Tables III and IV, which have the same format as Table II. Estimates in Tables III and IV are quite uniform. All estimates are negative indicating that PI laws are associated with decreased educational attainment. For white women, estimates in columns 4 and 5 (preferred estimates) indicate that PI laws are associated with approximately a 0.4 to 1 percent decline in the probability of completing high school and a 1 to 2.7 percent decline in the probability of obtaining some college education. Most of these estimates are significant at the 0.10 level. For black women, estimates in columns 4 and 5 indicate that PI laws are associated with approximately a 2 to 4

[15] Both the 2000 census and ACS (until 2007) do not distinguish between regular high school diploma or GED. Thus, high school completion encompasses both measures. Tabulations using the 2008-2009 data show that women with GED only constitute a relatively small fraction of women in our age range (about 3.5 percent for both whites and blacks).

percent decline in the probability of completing high school and a 5 to 10 percent decline in the probability of obtaining some college education. All of these estimates are significant at the 0.10 level.

The estimates of the association between PI laws and education are consistent with estimates of the association between PI laws and fertility. With respect to the effect of PI laws on education, the affected group is women whose fertility was altered by PI laws. For white women, we estimated this to be approximately 1.5 percent of the population. Thus, a decline in the proportion of the sample that completed high school of 0.3 to 0.8 percent (percentage points), as indicated by estimates in Table III, is consistent with the size of the group at risk. Among black women, we estimated that approximately 3.5 percent of teens were affected, which is consistent with the 2 to 3 percent (percentage point) decline in the proportion of black women that completed high school as indicated by estimates in Table III. Overall, estimates in Tables II and IV indicate that between 20 and 50 percent of white teens who had an additional birth because of PI laws also failed to graduate high school; the similar figures for black teens are 60 to 85 percent. For college attendance, estimates suggest that virtually all teens who had an additional birth because of PI laws failed to attend college.

D. Labor Market Outcomes

The results of previous section show that PI laws are associated with higher fertility and lower educational attainment. Women who have an early birth and invest less in their education may also face different opportunity costs and returns in the labor market. Tables V and VI present estimates of associations between PI laws and weeks worked last year. We examined the association at both the extensive margin—whether women worked or not in the past year—and the intensive margin—whether women worked at least 50 weeks in the past year.

Table V reports estimates of the association between PI laws and whether a woman worked last year. Estimates in Panel A pertaining to white women indicate that there is no significant association between PI laws and whether a woman worked last year. Although all estimates are negative, effect sizes are small (0.3 to 0.55 percent) and estimates are not statistically significant. Estimates obtained using the adjusted measure of exposure, which takes into account abortion options in surrounding states, are similar although larger in magnitude and in some case statistically significant at the 0.10 level or close to being so. Our reading is that there is, at most, marginal evidence that PI laws are associated with a lower probability of working among white women. Estimates for black women (Panel B) are considerably smaller and there is no consistent pattern to the estimates to suggest an association between the simple (or adjusted) measure of exposure to PI laws when young and probability of working.

Table VI presents estimates of the association between PI laws and whether women worked at least 50 weeks in the past year. Estimates in the top panel show that, for whites, there is no consistent and practically important association between PI laws and working full year. In contrast, estimates for blacks (Panel B) show a more consistent pattern. All are negative, indicating that PI laws are associated with fewer weeks worked, and effect sizes (columns 4 and 5) are between 3 and 6 percent when the simple measure of exposure is used and 4 to and 9 percent when the adjusted measure of exposure is used. However, estimates are somewhat sensitive to the inclusion of state-specific trends. Finally, all estimates in Table VI are consistent with the at risk sample being approximately 1.5 percent for whites and 3.5 percent for blacks (estimates of percentage point changes are smaller than these percentages).

E. Receipt of Public Assistance

The next outcome we investigate is the receipt of income from public assistance, which includes income from TANF and other general assistance programs. Estimates are presented in Table VII. Results in Panel A indicate that, for whites, there is a consistent and uniform pattern to the estimates, which suggest a significant and positive association between PI laws and the probability of receiving income from public assistance. Effect sizes pertaining to the simple (adjusted) measure of exposure are relatively large, between 13 and 16 percent (19 and 22 percent). While relative effect sizes are large, the absolute values of estimates are in line with the affected group being 1.5 percent of the population. Among blacks (Panel B), estimates show no consistent pattern to suggest an association between PI laws and receipt of income from public assistance.

VII. Low Migration States

A limitation of the data is that there is no information on state-of-residence between ages 15 and 17 that can be used to construct a measure of exposure of women to PI laws at those ages. Instead, we used the state-of-birth to assign the relevant laws. The potential misclassification of the legal abortion environment resulting from this lack of information is a concern, and to address it, we used information about both state-of-birth and current state-of-residence to refine the sample. We are reasonably confident that the legal abortion environment has been assigned correctly for women currently residing in their state of birth. Therefore, to reduce the amount of measurement error, we selected a set of states that had a relatively high proportion of current residents born in that state. In particular, we classify as low migration states those where more than 60 percent of current (non-foreign born) resident women in our age group were born in that state.[16] This group of states represents approximately 74 percent of the female U.S. born population in our age range. In these states, 75 percent of women in our age range currently reside in the state they were born-in. Thus, this group of states will have a very large fraction (more than 84 percent accounting for other factors) of the sample has been assigned the correct legal abortion environment.

[16] AL, AR, CA, CT, IL, IN, IA, KS, KY, LA, ME, MA, MI, MN, MS, MO, NE, NJ, NY, ND, OH, OK, PA, RI, SD, TN, TX, UT, WV, WI.

If measurement error is random, we would expect larger (absolute value) estimates because random measurement error biases estimates downward. However, heterogeneous effects by state can result in larger or smaller estimates. Nevertheless, the estimates from the analysis using this sub-group of states will be less affected by measurement error.

Each column in Table VIII represents a separate regression. Estimates are obtained using the preferred specification (including quadratic state-specific trends) for the various outcomes and for whites only because there are too few states to conduct an analysis using black women. For each outcome, coefficients pertaining to both the simple and adjusted measure of exposure are reported. The results show that, in preferred models, the effect of exposure on the number of children is always statistically significant at 0.10 level with effect sized between 3 and 6 percent, which are slightly larger than the corresponding estimates in Table II. Estimates also indicate that PI laws are associated with approximately a 0 to 1 percent decline in the probability of completing high school and a statistically significant 1.7 to 2.9 percent decline in the probability of obtaining some college education. Finally, we find no statistically significant association between PI laws and labor market outcomes, but a positive and significant association with probability of receipt of welfare income (when the adjusted measure of exposure is used). All estimates are qualitatively and quantitative very similar to the corresponding results obtained using the full sample of states

In sum, the results obtained focusing on low migration states confirm that PI laws are associated with higher fertility, lower educational attainment and, among white women, also increase in receipt of public assistance. Estimates also confirm that associations are larger when the adjusted measure of exposure is used, which underscore the importance of the legal abortion environment in surrounding states in defining abortion options for minors.

VIII. Falsification Tests

Overall, results presented in previous sections suggest a causal association between PI laws and fertility, education, weeks worked for black women and receipt of public assistance income for white women. To further bolster the causal interpretation of these results, we conducted analyses for outcomes that are arguably not affected, or much less likely to be affected, by PI laws. For women, given the available data, it was difficult to identify outcomes that would not be plausibly affected by changes in fertility due to PI laws. We selected a measure of whether a woman had a sensory or physical disability. While men may be affected by PI laws through the marriage (mating) market, PI laws should have a much smaller if not zero effect on men. Thus, we re-estimated models of education and employment using a sample of men. We also examined associations between PI laws and disability for men. Estimates from these analyses are shown in Table IX. We present estimates from the preferred specification (including state-specific, quadratic trends) for the following outcomes: physical/sensory disability for women, physical/sensory disability for men, men's education and weeks worked. For each outcome the coefficients pertaining to both the simple and adjusted measure of exposure are reported.

For whites, all estimates in Panel A are statistically insignificant and very small (relative to mean). These results provide strong support for the research design underlying previous analyses. Results for blacks are mixed. There is no significant association between exposure and disability outcomes (except for women when the adjusted exposure measure is used). However, exposure to PI laws is significantly associated with lower educational attainment for men, and the estimated impacts tend to be somewhat larger compared to women. While the negative of effect of PI laws on education for black men is possible, the relatively large magnitude of the effect (relative to women) seems implausible. No significant associations are found between PI laws and weeks worked.

Overall, estimates in Table IX provide substantial evidence that the research design and identification assumptions underlying our analyses are valid. For only one outcome, education, and only for black women, was there evidence that suggested omitted factors may be confounding the reported associations. In all other cases, the results from the placebo analyses are consistent with a valid research design and support claims that most estimates of the association between PI laws and fertility and socioeconomic outcomes may plausibly be interpreted as causal.

IX. Conclusions

Currently, the majority of states require minors to involve their parents in their choice to obtain an abortion. Given the controversial nature of PI laws, it is important to understand whether and to what extent these laws affect minors. The public policy debate has traditionally focused on the contemporaneous effect of PI laws on teen abortion. Notably there is limited evidence of the effect of PI laws on births. This is an important gap in knowledge because the effect of PI laws on teen births is one of the most important issues that divides opponents and proponents of PI laws. An even larger gap in research relates to whether PI laws have long term consequences. The lack of evidence about long term consequences makes a complete assessment of PI laws impossible. While short-term impacts are important, they may be small relative to the long term effects.

In this paper, we addressed these gaps in research. We obtained estimates of the association between PI laws and fertility and socio-economic outcomes that are affected by fertility for women between the ages of 21 and 32 (average age 27). Our estimates suggest that women who were exposed to PI laws as minors have more children and lower levels of education at age 27. White women are also more likely to receive public assistance because of PI laws, and there is some evidence that black women exposed to laws work fewer weeks. Importantly, estimates also show that effects of PI laws are larger when surrounding states also have PI laws, which is the current state of affairs for most teens. Furthermore, while PI laws affect relatively few teens, these laws have large effects on the teens affected. Rough calculations suggest that for every teen abortion averted by PI laws, fertility increases by more than one birth by age 27 and a large fraction of the teens who have an additional birth fail to graduate high school, attend college, and in the case of white women, fail to become economically self sufficient (i.e., receive public assistance).

Findings from our analysis are relevant to the question of whether teen births adversely affect socioeconomic outcomes, which is a question that remains unresolved despite much study it (Geronimus and Korenman [1993]; Grogger and Bronars [1993]; Hoffman [1998]; Hotz et al. [2005]; Fletcher and Wolf [2009]). Here, we show that PI laws are associated with an increase in fertility, and we have argued and provided evidence that this increase is exogenous. Other evidence presented indicated that PI laws also reduced educational attainment, and adversely affected labor force participation (black women) and receipt of public assistance (white women). Therefore, it is reasonable to conclude that PI laws increased teen births and that these additional births were associated with large effects on socioeconomic outcomes. For example, estimates indicated that between 20 to 50 percent of white women and 60 to 85 percent of black women that had a birth because of PI laws failed to graduate high school.

In sum, our evidence is consistent with the hypothesis that PI laws induce minors to substitute unwanted births for abortions and that these early births affect other types of human capital investments that, in turn, influence fertility and socioeconomic outcomes later in life. Overall, the evidence presented in this paper is not consistent with claims that PI laws decrease births. Finally, we believe it is reasonable to interpret most of the estimates presented as causal associations, particularly for white women.

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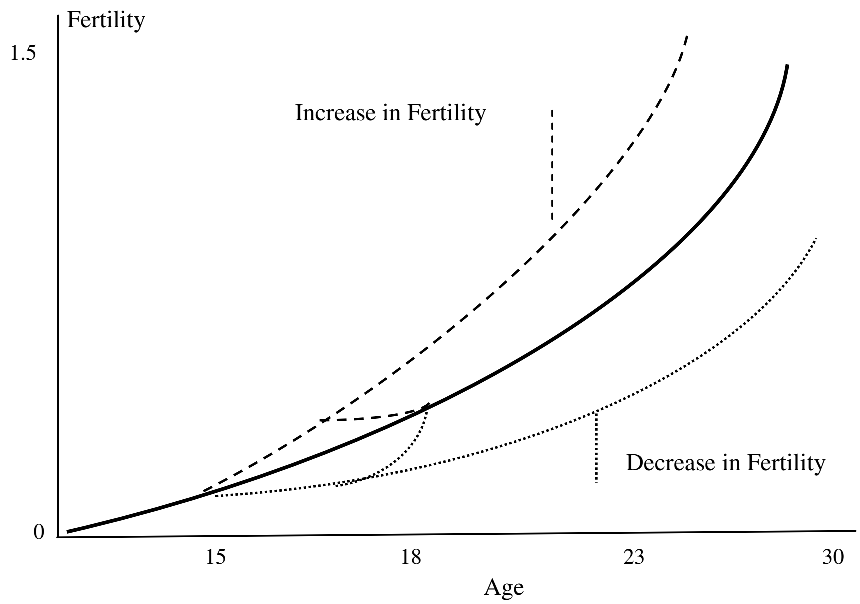

FIGURE I: Impact of Parental Involvement Laws on Fertility

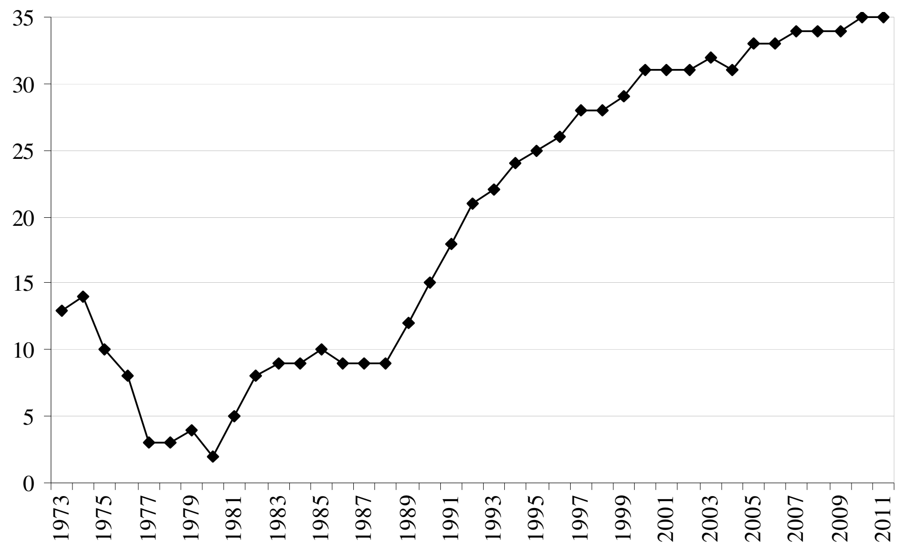

FIGURE II: Number of states with minors' abortion restrictions, 1973-2011.

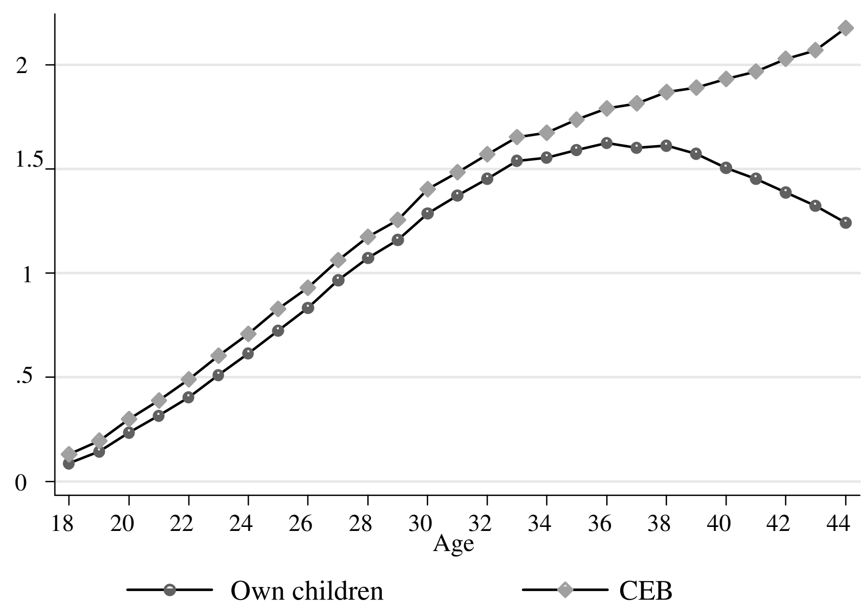

FIGURE III: Own children in the household and children ever born (CEB), per woman-1990 Census. *Source*: authors' calculations using 1990 Census of Population, 1% sample (Ruggles et al., 2010).

Table I
Parental Involvement Laws by Periods Enforced

Alabama	1987-present	Louisiana	1973-1976	North Carolina	*1967-1973*; 1995-present
Alaska	2010-present		1978-1980; 1981-present	North Dakota	1975-1979; 1981-present
Arizona	1982-1985; 2000-present	Maine	1989-1994	Ohio	1974-1976; 1990-present
Arkansas	*1969-Roe*; 1989-present	Maryland		Oklahoma	2005-present
California		Massachusetts	1981-present	Oregon	*1969-Roe*
Colorado	*1967-1975*; 2003-present	Michigan	1991-present	Pennsylvania	1994-present
Connecticut		Minnesota	1981-1986; 1990-present	Rhode Island	1982-present
Delaware	1995-present	Mississippi	1993-present	South Carolina	*1970-1973; 1974-1976;*
DC		Missouri	1974 -1975; 1983;		1990-present
Florida	*1972-1973*; 2005-present		1985-present	South Dakota	1997-present
Georgia	1991-present	Montana	1974 -1976	Tennessee	1979; 1992-present
Hawaii		Nebraska	1973-1975; 1977-1978;	Texas	2000-present
Idaho	1999-2003; 2007-present		1991-present	Utah	1973-present
Illinois		Nevada	1973-1976	Vermont	
Indiana	1973-1975; 1982-1983;	New Hampshire		Virginia	*1970-1976*; 1997-present
	1984-present	New Jersey		Washington	1970-1975
Iowa	1996-present	New Mexico	*1969-1976*	West Virginia	1984-present
Kansas	1992-present	New York		Wisconsin	1992-present
Kentucky	1974; 1994-present			Wyoming	1989-present

Notes: Minors restrictions include explicit PI (either parental consent or notification laws) as well as early restrictions enacted under the MPC provisions (in italics). *Roe* indicates that the provision was declared unenforceable very shortly after *Roe*. Present indicates February 2011. Source: thorough 1994 Mertz et al (1995; 1996). After 1994, AGI reports (various years) and NARAL pro choice America.

Table II
Effect of Exposure to Parental Involvement Law on Number of Children [a]

	(1)	(2)	(3)	(4)	(5)
Panel A: WHITES					
EXPOSURE	-0.0040	-0.0080	0.0006	0.0096	0.0118
	(0.0177) [b]	(0.0173)	(0.0174)	(0.0126)	(0.0135)
	[-0.0052] [c]	[-0.0104]	[0.0008]	[0.0125]	[0.0154]
ADJUSTED-EXPOSURE	-0.0122	-0.0162	-0.0019	0.0200	0.0268
	(0.0229)	(0.0231)	(0.0233)	(0.0189)	(0.0212)
	[-0.0159]	[-0.0212]	[-0.0025]	[0.0261]	[0.0350]
mean of dependent variable	*0.7657*	*0.7657*	*0.7657*	*0.7657*	*0.7657*
observations	*5605*	*5605*	*5605*	*5605*	*5605*
Panel B: BLACKS					
EXPOSURE	0.0480*	0.0434	0.0451*	0.0463*	0.0530
	(0.0266)	(0.0260)	(0.0252)	(0.0239)	(0.0372)
	[0.0447]	[0.0404]	[0.0420]	[0.0432]	[0.0494]
ADJUSTED-EXPOSURE	0.0595*	0.0564*	0.0596*	0.0682**	0.0934**
	(0.0310)	(0.0315)	(0.0315)	(0.0252)	(0.0349)
	[0.0555]	[0.0526]	[0.0555]	[0.0636]	[0.0870]
mean of dependent variable	*1.073*	*1.073*	*1.073*	*1.073*	*1.073*
observations	*1942*	*1942*	*1942*	*1942*	*1942*
state of birth fixed effects	yes	yes	yes	yes	yes
age fixed effects	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes
cohort group fixed effects [d]	no	yes	yes	yes	yes
state-cohort variables [e]	no	no	yes	yes	yes
linear trend	no	no	no	yes	yes
quadratic trend	no	no	no	no	yes

Notes: [a] The dependent variable is the number of own children per woman in the cell (defined by state-year-of-birth and age). [b] Standard errors are clustered at the state-of-birth level, regressions are weighted by the relevant cell population. [c] Coefficient divided by the mean in brackets.
[d] Indicate dummies for the following year-of-birth cohort-groups: 1968-1972; 1973-1977; 1978-1982; 1983-1988. [e] Unemployment, labor force participation rate, per-capita income, abortion providers when cohort was ages 15-17. *** 1%, ** 5%, * 10% significance level.

Table III
Effect of Exposure to Parental Involvement Law on Completed High School [a]

	(1)	(2)	(3)	(4)	(5)
Panel A: WHITES					
EXPOSURE	-0.0037*	-0.0020	-0.0017	-0.0043*	-0.0034
	(0.0020) [b]	(0.0019)	(0.0019)	(0.0026)	(0.0040)
	[-0.0040] [c]	[-0.0021]	[-0.0018]	[-0.0046]	[-0.0037]
ADJUSTED-EXPOSURE	-0.0057**	-0.0031	-0.0025	-0.0076*	-0.0085*
	(0.0025)	(0.0025)	(0.0025)	(0.0039)	(0.0049)
	[-0.0061]	[-0.0033]	[-0.0027]	[-0.0082]	[-0.0091]
mean of dependent variable	*0.9308*	*0.9308*	*0.9308*	*0.9308*	*0.9308*
observations	*5605*	*5605*	*5605*	*5605*	*5605*
Panel B: BLACKS					
EXPOSURE	-0.0268**	-0.0242**	-0.0238**	-0.0224*	-0.0195*
	(0.0112)	(0.0101)	(0.0098)	(0.0122)	(0.0099)
	[-0.0311]	[-0.0281]	[-0.0276]	[-0.0260]	[-0.0226]
ADJUSTED-EXPOSURE	-0.0392***	-0.0364***	-0.0345***	-0.0337**	-0.0317**
	(0.0115)	(0.0110)	(0.0105)	(0.0155)	(0.0141)
	[-0.0455]	[-0.0423]	[-0.0401]	[-0.0391]	[-0.0368]
mean of dependent variable	*0.8613*	*0.8613*	*0.8613*	*0.8613*	*0.8613*
observations	*1942*	*1942*	*1942*	*1942*	*1942*
state of birth fixed effects	yes	yes	yes	yes	yes
age fixed effects	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes
cohort group fixed effects [d]	no	yes	yes	yes	yes
state-cohort variables [e]	no	no	yes	yes	yes
linear trend	no	no	no	yes	yes
quadratic trend	no	no	no	no	yes

Notes: [a] Dependent variable is proportion of women in cell (defined by state-year-of-birth-age) with completed high school. [b] Standard errors are clustered at the state-of-birth level; regressions are weighted by the relevant cell population. [c] Coefficient divided by the mean in brackets. [d] Indicate dummies for the following year-of-birth cohort-groups: 1968-1972; 1973-1977; 1978-1982; 1983-1987. [e] Unemployment, labor force participation rate, per-capita income, abortion providers when cohort was ages 15-17. *** 1%, ** 5%, * 10% significance level.

Table IV
Effect of Exposure to Parental Involvement Law on Some College [a]

	(1)	(2)	(3)	(4)	(5)
Panel A: WHITES					
EXPOSURE	-0.0038	-0.0013	-0.0020	-0.0058	-0.0110*
	(0.0043) [b]	(0.0043)	(0.0044)	(0.0046)	(0.0061)
	[-0.0054] [c]	[-0.0019]	[-0.0029]	[-0.0083]	[-0.0157]
ADJUSTED-EXPOSURE	-0.0037	0.0007	-0.0005	-0.0097	-0.0186**
	(0.0059)	(0.0058)	(0.0060)	(0.0067)	(0.0072)
	[-0.0053]	[0.0010]	[-0.0007]	[-0.0139]	[-0.0266]
mean of dependent variable	*0.70*	*0.70*	*0.70*	*0.70*	*0.70*
observations	*5605*	*5605*	*5605*	*5605*	*5605*
Panel B: BLACKS					
EXPOSURE	-0.0192	-0.0163	-0.0169*	-0.0295**	-0.0358*
	(0.0123)	(0.0106)	(0.0096)	(0.0106)	(0.0173)
	[-0.0346]	[-0.0293]	[-0.0304]	[-0.0531]	[-0.0644]
ADJUSTED-EXPOSURE	-0.0282**	-0.0245*	-0.0256**	-0.0431***	-0.0550**
	(0.0134)	(0.0123)	(0.0110)	(0.0129)	(0.0231)
	[-0.0507]	[-0.0441]	[-0.0461]	[-0.0776]	[-0.0990]
mean of dependent variable	*0.5556*	*0.5556*	*0.5556*	*0.5556*	*0.5556*
observations	*1942*	*1942*	*1942*	*1942*	*1942*
state of birth fixed effects	yes	yes	yes	yes	yes
age fixed effects	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes
cohort group fixed effects [d]	no	yes	yes	yes	yes
state-cohort variables [e]	no	no	yes	yes	yes
linear trend	no	no	no	yes	yes
quadratic trend	no	no	no	no	yes

Notes: [a] The dependent variable is proportion of women in cell (defined by state-year-of-birth and age) with some college. [b] Standard errors are clustered at the state-of-birth level; regressions are weighted by the relevant cell population. [c] Coefficient divided by the mean in brackets. [d] Indicate dummies for the following year-of-birth cohort-groups: 1968-1972; 1973-1977; 1978-1982; 1983-1987. [e] Unemployment, labor force participation rate, per-capita income, abortion providers when cohort was ages 15-17. *** 1%, ** 5%, * 10% significance level.

Table V

Effect of Exposure to Parental Involvement Law on Whether Worked Last Year [a]

	(1)	(2)	(3)	(4)	(5)
Panel A: WHITES					
EXPOSURE	-0.0032	-0.0037	-0.0047	-0.0024	-0.0045
	(0.0040) [b]	(0.0040)	(0.0037)	(0.0030)	(0.0039)
	[-0.0038] [c]	[-0.0044]	[-0.0055]	[-0.0028]	[-0.0053]
ADJUSTED-EXPOSURE	-0.0055	-0.0063	-0.0081*	-0.0048	-0.0090
	(0.0048)	(0.0049)	(0.0047)	(0.0042)	(0.0056)
	[-0.0065]	[-0.0074]	[-0.0095]	[-0.0056]	[-0.0106]
mean of dependent variable	*0.8505*	*0.8505*	*0.8505*	*0.8505*	*0.8505*
Observations	*5605*	*5605*	*5605*	*5605*	*5605*
Panel B: BLACKS					
EXPOSURE	-0.0035	-0.0022	-0.0035	0.0018	-0.0018
	(0.0102)	(0.0100)	(0.0109)	(0.0121)	(0.0132)
	[-0.0043]	[-0.0027]	[-0.0043]	[0.0022]	[-0.0022]
ADJUSTED-EXPOSURE	-0.0006	0.0010	-0.0007	0.0006	-0.0046
	(0.0114)	(0.0114)	(0.0120)	(0.0151)	(0.0172)
	[-0.0007]	[0.0012]	[-0.0009]	[0.0007]	[-0.0056]
mean of dependent variable	*0.8145*	*0.8145*	*0.8145*	*0.8145*	*0.8145*
Observations	*1942*	*1942*	*1942*	*1942*	*1942*
state of birth fixed effects	yes	yes	yes	yes	yes
age fixed effects	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes
cohort group fixed effects [d]	no	yes	yes	yes	yes
state-cohort variables [e]	no	no	yes	yes	yes
linear trend	no	no	no	yes	yes
quadratic trend	no	no	no	no	yes

Notes: [a] The dependent variable is proportion of women in cell (defined by state-year-of-birth-age) who worked last year. [b] Standard errors are clustered at the state-of-birth level; regressions are weighted by the relevant cell population. [c] Coefficient divided by the mean in brackets. [d] Indicate dummies for the following year-of-birth cohort-groups: 1968-1972; 1973-1977; 1978-1982; 1983-1988. [e] Unemployment, labor force participation rate, per-capita income, abortion providers when cohort was ages 15-17. *** 1%, ** 5%, * 10% significance level.

Table VI-Effect of Exposure to Parental Involvement Law on Whether Worked at Least 50 Weeks Last Year [a]

	(1)	(2)	(3)	(4)	(5)
Panel A: WHITES					
EXPOSURE	-0.0001	-0.0019	-0.0043	0.0012	0.0023
	(0.0050) [b]	(0.0051)	(0.0047)	(0.0038)	(0.0059)
	[-0.0002] [c]	[-0.0036]	[-0.0082]	[0.0023]	[0.0044]
ADJUSTED-EXPOSURE	0.0015	-0.0011	-0.0051	0.0017	0.0022
	(0.0070)	(0.0071)	(0.0069)	(0.0049)	(0.0077)
	[0.0029]	[-0.0021]	[-0.0098]	[0.0033]	[0.0042]
mean of dependent variable	*0.5227*	*0.5227*	*0.5227*	*0.5227*	*0.5227*
observations	*5605*	*5605*	*5605*	*5605*	*5605*
Panel B: BLACKS					
EXPOSURE	-0.0216**	-0.0196*	-0.0202**	-0.0127	-0.0280**
	(0.0102)	(0.0094)	(0.0090)	(0.0135)	(0.0132)
	[-0.0441]	[-0.0400]	[-0.0412]	[-0.0259]	[-0.0571]
ADJUSTED-EXPOSURE	-0.0197	-0.0175	-0.0197*	-0.0222	-0.0443***
	(0.0118)	(0.0114)	(0.0116)	(0.0160)	(0.0147)
	[-0.0402]	[-0.0357]	[-0.0402]	[-0.0453]	[-0.0904]
mean of dependent variable	*0.49*	*0.49*	*0.49*	*0.49*	*0.49*
observations	*1942*	*1942*	*1942*	*1942*	*1942*
state of birth fixed effects	yes	yes	yes	yes	yes
age fixed effects	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes
cohort group fixed effects [d]	no	yes	yes	yes	yes
state-cohort variables [e]	no	no	yes	yes	yes
linear trend	no	no	no	yes	yes
quadratic trend	no	no	no	no	yes

Notes: [a] The dependent variable is proportion of women in cell (defined by state-year-of-birth-age) who worked at least 50 weeks last year. [b] Standard errors are clustered at the state-of-birth level; regressions are weighted by the relevant cell population. [c] Coefficient divided by the mean in brackets. [d] Indicate dummies for the following year-of-birth cohort-groups: 1968-1972; 1973-1977; 1978-1982; 1983-1987. [e] Unemployment, labor force participation rate, per-capita income, abortion providers when cohort was ages 15-17. *** 1%, ** 5%, * 10% significance level.

Table VII- Effect of Exposure to Parental Involvement Law on Receipt of Public Assistance Income [a]

	(1)	(2)	(3)	(4)	(5)
Panel A: WHITES					
EXPOSURE	0.0040***	0.0035***	0.0034***	0.0030***	0.0031*
	(0.0012) [b]	(0.0011)	(0.0011)	(0.0010)	(0.0017)
	[0.1632] [c]	[0.1429]	[0.1388]	[0.1224]	[0.1265]
ADJUSTED-EXPOSURE	0.0055***	0.0049***	0.0048***	0.0046***	0.0050**
	(0.0015)	(0.0015)	(0.0014)	(0.0014)	(0.0022)
	[0.2245]	[0.2000]	[0.1959]	[0.1878]	[0.2041]
mean of dependent variable	*0.0245*	*0.0245*	*0.0245*	*0.0245*	*0.0245*
observations	*5605*	*5605*	*5605*	*5605*	*5605*
Panel B: BLACKS					
EXPOSURE	0.0048	0.0031	0.0042	-0.0103	-0.0163*
	(0.0064)	(0.0061)	(0.0065)	(0.0074)	(0.0091)
	[0.0611]	[0.0394]	[0.0534]	[-0.1310]	[-0.2073]
ADJUSTED-EXPOSURE	0.0082	0.0064	0.0085	-0.0068	-0.0099
	(0.0072)	(0.0072)	(0.0078)	(0.0108)	(0.0118)
	[0.1043]	[0.0814]	[0.1081]	[-0.0865]	[-0.1260]
mean of dependent variable	*.0786*	*.0786*	*.0786*	*.0786*	*.0786*
observations	*1942*	*1942*	*1942*	*1942*	*1942*
state of birth fixed effects	yes	yes	yes	yes	yes
age fixed effects	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes
cohort group fixed effects [d]	no	yes	yes	yes	yes
state-cohort variables [e]	no	no	yes	yes	yes
linear trend	no	no	no	yes	yes
quadratic trend	no	no	no	no	yes

Notes: [a] The dependent variable is proportion of women in cell (defined by state-year-of-birth and age) receiving income from public assistance. [b] Standard errors are clustered at the state-of-birth level; regressions are weighted by the relevant cell population. [c] Coefficient divided by the mean in brackets. [d] Indicate dummies for the following year-of-birth cohort-groups: 1968-1972; 1973-1977; 1978-1982; 1983-1987. [e] Unemployment, labor force participation rate, per-capita income, abortion providers when cohort was ages 15-17. *** 1%, ** 5%, * 10% significance level.

Table VIII
Effect of Exposure to Parental Involvement Law -Low Migration States

	Number of children [a]	High school completion [b]	Some college [c]	Whether worked last year [d]	Whether worked at least 50 weeks [e]	Income from public assistance [f]
Panel A: WHITES						
EXPOSURE	0.0233*	-0.0023	-0.0125*	-0.0035	0.0082	0.0023
	(0.0131) [g]	(0.0041)	(0.0068)	(0.0039)	(0.0064)	(0.0020)
	[0.0306] [h]	[-0.0025]	[-0.0176]	[-0.0041]	[0.0156]	[0.0934]
ADJUSTED-EXPOSURE	0.0480**	-0.0080	-0.0204**	-0.0084	0.0099	0.0049*
	(0.0196)	(0.0052)	(0.0085)	(0.0059)	(0.0087)	(0.0026)
	[0.0631]	[-0.0086]	[-0.0288]	[-0.0098]	[0.0187]	[0.1856]
state of birth fixed effects	yes	yes	yes	yes	yes	yes
age fixed effects	yes	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes	yes
cohort group fixed effects [i]	yes	yes	yes	yes	yes	yes
state-cohort variables [l]	yes	yes	yes	yes	yes	yes
linear trend	yes	yes	yes	yes	yes	yes
quadratic trend	yes	yes	yes	yes	yes	yes
mean of dependent variable	*0.7603*	*0.9343*	*0.7068*	*0.8532*	*0.5268*	*0.0246*
observations	*3596*	*3596*	*3596*	*3596*	*3596*	*3596*

Notes: The dependent variable is: [a] the number of children per woman in the cell (defined by state-year-of-birth and age); [b] proportion of women in cell (defined by state-year-of-birth and age) with completed high school; [c] with some college; [d] who worked last year; [e] who worked at least 50 weeks last year; [f] who receive income from public assistance. [g] Standard errors are clustered at the state-of-birth level, regressions are weighted by the relevant cell population. [h] Coefficient divided by the mean in brackets. [i] Indicate dummies for the following year-of-birth cohort-groups: 1968-1972; 1973-1977; 1978-1982; 1983-1988. [l] Unemployment, labor force participation rate, per-capita income, abortion providers when cohort was ages 15-17. *** 1%, ** 5%, * 10% significance level.

Table IX
Effect of Exposure to Parental Involvement Law on
Outcomes Unlikely to be Affected

	Physical /sensory disability-women [a]	Physical /sensory disability-men [b]	Men high school completition [c]	Men some-college [d]	Men worked last year [e]
Panel A: WHITES					
EXPOSURE	-0.0018	-0.0014	-0.0008	-0.0044	0.0006
	(0.0020) [f]	(0.0020)	(0.0030)	(0.0061)	(0.0022)
ADJUSTED-EXPOSURE	-0.0011	-0.0013	-0.0042	-0.0107	0.0022
	(0.0030)	(0.0024)	(0.0044)	(0.0079)	(0.0032)
mean of dependent variable	*0.0342*	*0.0376*	*0.9092*	*0.6103*	*0.9361*
observations	*5605*	*5557*	*5557*	*5557*	*5557*
Panel B: BLACKS					
EXPOSURE	0.0100	0.0032	-0.0349*	-0.0531***	0.0034
	(0.0077)	(0.0074)	(0.0184)	(0.0167)	(0.0104)
ADJUSTED-EXPOSURE	0.0188**	0.0052	-0.0435**	-0.0609***	0.0021
	(0.0086)	(0.0100)	(0.0204)	(0.0205)	(0.0131)
mean of dependent variable	*0.0436*	*0.0512*	*0.8103*	*0.4314*	*0.7765*
observations	*1942*	*1620*	*1620*	*1620*	*1620*
state of birth fixed effects	yes	yes	yes	yes	yes
age fixed effects	yes	yes	yes	yes	yes
year fixed effects	yes	yes	yes	yes	yes
cohort group fixed effects [g]	yes	yes	yes	yes	yes
state-cohort variables [h]	yes	yes	yes	yes	yes
linear trend	yes	yes	yes	yes	yes
quadratic trend	yes	yes	yes	yes	yes

Notes: The dependent variable is: [a] proportion of women in cell (defined by state-year-of-birth and age) with a physical/sensory disability; [b] proportion of men in cell (defined by state-year-of-birth and age) with a physical/sensory disability; [c] proportion of men in cell (defined by state-year-of-birth and age) with completed high school; [d] proportion of men in cell (defined by state-year-of-birth and age) with some college; [e] proportion of men in cell (defined by state-year-of-birth and age) who worked last year. [f] Standard errors are clustered at the state-of-birth level, regressions are weighted by the relevant cell population. [g] Indicate dummies for the following year-of-birth cohort-groups: 1968-1972; 1973-1977; 1978-1982; 1983-1988. [h] Unemployment, labor force participation rate, per-capita income, abortion providers when cohort was ages 15-17. *** 1%, ** 5%, * 10% significance level.