NBER WORKING PAPER SERIES

DO PARENTAL INVOLVEMENT LAWS DETER RISKY TEEN SEX?

Silvie Colman
Thomas S. Dee
Theodore J. Joyce

Working Paper 18810
http://www.nber.org/papers/w18810

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2013

The study was supported by a grant from the Robert Wood Johnson Foundation Public Health Law Initiative to Mathematica Policy Research, Inc. from December 2009 through May 2011. We thank Ruoding Tan for outstanding research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

At least one co-author has disclosed a financial relationship of potential relevance for this research. Further information is available online at http://www.nber.org/papers/w18810.ack

Do Parental Involvement Laws Deter Risky Teen Sex?
Silvie Colman, Thomas S. Dee, and Theodore J. Joyce
NBER Working Paper No. 18810
February 2013
JEL No. I18

ABSTRACT

Parental involvement (PI) laws require that physicians notify or obtain consent from a parent(s) of a minor seeking an abortion before performing the procedure. Several studies suggest that PI laws curb risky sexual behavior because teens realize that they would be compelled to discuss a subsequent pregnancy with a parent. We show that prior evidence based on gonorrhea rates overlooked the frequent under-reporting of gonorrhea by race and ethnicity, and present new evidence on the effects of PI laws using more current data on the prevalence of gonorrhea and data that are novel to this literature (i.e., chlamydia rates and data disaggregated by year of age). We improve the credibility of our estimates over those in the existing literature using an event-study design in addition to standard difference-in-difference-in-differences (DDD) models. Our findings consistently suggest no association between PI laws and rates of sexually transmitted infections or measures of sexual behavior.

Silvie Colman
Health Researcher
Mathematica Policy Research, Inc.
600 Alexander Park
Princeton, NJ 08540
SColman@mathematica-mpr.com
Phone (609) 750-4094
SColman@mathematica-mpr.com

Thomas S. Dee
Stanford University
520 Galvez Mall, CERAS Building, 5th Floor
Stanford, CA 94305-3084
and NBER
tdee@stanford.edu

Theodore J. Joyce
Baruch College & Graduate Center, CUNY
365 Fifth Ave, 5th Fl
New York, NY 10016-4309
and NBER
theodore.joyce@baruch.cuny.edu

I. INTRODUCTION

The ongoing and heated debates over federal and state policies that influence access to abortion turn in large part on strongly held normative beliefs. However, the positive evidence on how abortion policies influence risky sexual behavior—particularly among minors—also figures prominently in these discussions. Specifically, over the past three decades, most states have introduced controversial laws that mandate parental involvement (PI) in minors' access to abortion services.[1] Supporters of PI laws contend that these regulations reduce risky sexual behavior among teens because teens realize that they would be compelled to discuss a subsequent pregnancy with a parent.[2] Such predictions implicitly view teens as forward-looking decision makers who are aware of PI laws and take the implied costs of discussing a possible pregnancy with a parent into account when making decisions about risky sex. In contrast, one would expect PI laws to have no meaningful effects on risky sexual behavior if teens are generally unaware of these regulations until they become pregnant or if they can circumvent these restrictions by obtaining an abortion in a neighboring state without a PI law.

Several previous studies have engaged this question empirically by evaluating the effects of PI laws on two proximate measures of sexual risk-taking among teens: self-reports of sexual activity and contraceptive use (Levine 2001; Argys et al. 2002; Levine 2003) and the prevalence of the sexually transmitted infection (STI) *Neisseria gonorrhoeae* (Dee and Sen 2006; Klick and Stratmann 2008). However, mainly due to methodological differences, these studies provide contradictory evidence on whether PI laws have influenced risky sexual behavior among teens. Drawing on multiple (and updated) sources of data, this study seeks to reconcile the disparate findings in the existing literature and to provide new and comprehensive evidence on the association between PI laws and rates of STIs among teens. More specifically, the evidence presented in this study makes five distinct contributions to the existing literature.

[1] These laws require either notifying a parent or securing the consent of a parent. States began introducing PI laws in 1973, soon after abortion was legalized nationwide. Thirty-seven states currently require PI, up from nine states in 1988 (Merz et al. 1995; Guttmacher Institute 2011).

[2] As Texas State Representative Phil King, who sponsored the change from a parental notification to a parental consent statute, stated, "I think it will do what [parental notification] intended to do by bringing parents into the decision-making process, and when that happens we'll see a reduction in abortion and in teenage pregnancy" (Associated Press 2005).

First, we explore the robustness of prior findings and emphasize that previous analyses of gonorrhea rates have not engaged the substantive measurement problems with the data that are available from STI surveillance systems. Specifically, in many states and years, the data on STI prevalence by race and ethnicity are subject to high levels of non-reporting. Furthermore, the STI counts in specific states are frequently reported as zeros when they clearly represent missing values. Our analysis suggests that these pervasive measurement issues are empirically relevant with regard to the estimated effects of PI laws.

Second, we present updated evidence on whether PI laws have influenced the prevalence of gonorrhea and we analyze an entirely different STI, *Chlamydia trachomatis,* among teens and minors. The prevalence of both chlamydia and gonorrhea among teens is a major public health concern (Hampton 2008). However, chlamydia has a particular appeal in this context because it is roughly 10 times more common among young adults and teens than gonorrhea. In a population-based screening of young adults ages 18 to 26, 4.2 percent tested positive for chlamydia compared with 0.43 percent for gonorrhea (Miller et al. 2004). Our analysis of gonorrhea rates includes 10 more years of data than was available for analysis in prior studies. This longer panel enables us to include the eight states that introduced PI laws most recently, two of which are highly populous states (Texas and Florida).

We improve the credibility of our findings over those of the existing literature by paying special attention to the robustness of our estimates across different specifications of the conventional difference-in-difference-in-differences (DDD) model. Specifically, we implement a DDD analysis to evaluate the association between PI laws and rates of STIs among teens ages 15 to 19, using young adults ages 20 to 24 as a comparison group. We assess the validity of the identifying assumptions of the DDD approach using an *event-study* design and evaluate whether rates of STIs among young adults in the pre-law period are a good proxy for the rates among teens. As an additional test of the reliability of our DDD estimates, we perform placebo tests by evaluating the association between PI laws and rates of STIs among young women ages 20 to 24, for whom PI laws should be irrelevant.

Third, we also explore the robustness of our results using new and uniquely disaggregated data that we were able to collect from multiple states. Specifically, we compare changes in the rates of STIs among a treatment group of minors ages *15 to 17* with those of a comparison group ages *18 and*

19 in the 21 states that have supplied us with data by single year of age. The conventional data from the CDC are available only in five-year age groups (i.e., 15-19, 20-24). However, approximately 55 percent of all STIs among teens ages 15 to 19 occur among older teens ages 18 and 19. Including teens ages 18 and 19 in the treatment group might obscure the effects of PI laws on minors, because such laws do not apply to the older group. The comparison between groups ages 15 to 17 and 18 and 19 might increase our power to detect a potential link between PI laws and STIs.

Finally, we test whether PI laws are associated with changes in sexual activity, unprotected sex, the number of sexual partners and use of the contraceptive pill with data on a nationally representative sample of high school students from the Youth Risk Behavior Surveillance (YRBS) system. Although these data are limited by the number of participating states and years, they provide useful information on a key mediator. A decrease in sexual activity or an increase in condom use associated with a PI law would be a key link in the causal chain to fewer STIs.

To preview the findings, our estimates provide no evidence that the introduction of PI laws generate meaningful changes in the rates of chlamydia and gonorrhea among teens or minors. The lack of any association between PI laws and measures of teen sexual activity in the YRBS provide further support for our conclusions. Prior evidence linking PI laws to reductions in gonorrhea (Klick and Stratmann 2008) is, in general, sensitive to reasonable specification choices and to approaches informed by the frequent non-reporting of state-year STI data by race and ethnicity. Overall, the results of this study are consistent with models in which teens are largely unaware of state laws regulating abortion access, in which many teens discuss pregnancies with a parent regardless of the legal environment, and in which teens are not fully forward-looking when making decisions about risky sexual behaviors.

The study is organized as follows. Section II discusses the features and variation in PI laws, theoretical frameworks for understanding their effects, and the prior empirical literature examining their impact. Section III and IV introduce the data and research designs used in this study. Section V presents our results and related robustness checks. Section VI concludes with a brief summary of these results and thoughts on their implications for both policy and our understanding of risky behaviors, more generally.

II. ABORTION ACCESS AND RISKY SEXUAL ACTIVITY AMONG TEENS

Parental Involvement Laws

After the U.S. Supreme Court in 1973 established the constitutional right to terminate a pregnancy by abortion, several states introduced policies regulating abortion access, such as limitations on public funding, mandatory waiting periods, and parental involvement (PI) laws. Twelve states established enforceable PI laws at some point during the 1980s. Over the past two decades, these state-level abortion restrictions expanded dramatically and, currently, 36 states have an enforceable PI law in effect (Guttmacher Institute 2011). As the data in Table 1 indicates, there were periods in several states during which states were legally enjoined from enforcing a PI law. Furthermore, some state PI laws require parental notification only of a teen's intent to have an abortion, whereas other states mandate parental consent. We discuss our coding of these laws in Section III.

Theoretical Considerations

There are at least two competing theories about the behavioral response of adolescents to changes in access to reproductive health services. Standard economic models of sexual behavior generally conceptualize abortion as a form of insurance against an unwanted birth (for example, Levine 2003; Levine and Staiger 2004), and policy levers that restrict access to abortion, such as PI laws, are viewed as increases in the effective cost of acquiring an abortion. In this framework, forward-looking minors would react to a PI law by either reducing sexual activity or increasing the use of contraceptives (Levine 2003). Because condoms and birth control pills are the most widely used contraceptive methods among minors, PI laws could increase the use of either or both methods of birth control among minors. A reduction in sexual activity and/or an increase in condom use could yield a reduction in STIs.

A competing theory of teen reproductive behavior argues that teens give little consideration to the costs of an unwanted pregnancy when deciding to have sex (Paton 2006). Under this model, changes in laws that alter the price of accessing abortion services have little effect on sexual activity. It is only after an act of unprotected intercourse or contraceptive failure, when faced with an unwanted pregnancy or with the possibility of one, that teens consider the cost of their behavior. Under this assumption, the introduction of a PI law would not cause a change in teens' sexual behavior. Evidence consistent with this model would be a lack of change in rates of STIs. However, no change in the rate of STIs associated with a PI law does not prove that the behavior of minors was unaffected. The law could induce greater use of the pill or other hormonal contraception that would reduce the risk of pregnancy without altering exposure to STIs.

PI laws might also have limited or no effects when information about the abortion restrictions in a state is not readily available to teens. For example, some studies suggest that teens are largely unaware of their state's PI laws (Stone and Waszak 1992; Blum et al. 1987). PI laws could also have limited relevance because a substantial proportion of teens appear to discuss their pregnancies with a parent in the absence of these regulations. Specifically, Henshaw and Kost (1992) found that 61 percent of minors seeking an abortion in states without PI laws had already told their parents about the procedure. The ability of teens to obtain abortions in nearby states or use the judicial bypass procedure can also be expected to limit the behavioral consequences of PI laws (Cartoff and Klerman 1986; Blum et al. 1990; Henshaw 1995; Joyce 2010).

Prior Empirical Evidence

These theoretical and practical considerations suggest that the question of whether PI laws have behavioral consequences is, ultimately, an empirical one. A number of studies have examined whether PI laws have lead to a decline in abortions among minors. However, the impact of PI laws on abortion rates remains unresolved (Dennis et al. 2009). One reason is the limited data on abortions among minors by state of residence. Some evidence suggests that minors from states with PI laws seek abortions in nearby states without such restrictions. Abortions obtained outside the state of residence are often unaccounted for in evaluations of PI laws, which leads to a spurious decline in minors' abortion rates, overstating the true impact of the law (Cartoff and Klerman 1986; Henshaw 1995; Joyce and Kaestner 1996; Ellertson 1997).

One evaluation based on resident teen abortion rates from the Guttmacher Institute reported a decline of 15 percent in the teen abortion rate associated with PI laws (Levine 2003). However, the data were available for only 4 years (1985, 1988, 1992 and 1996) which left sizeable gaps between the year a PI law went into effect and when the pre- and post-law abortion rates were measured. But a more serious problem with the data used in Levine (2003) is that the estimates of the abortion rate by state of residence collected by the Guttmacher Institute do not take into account cross-state travel by teens in response to a PI law. For this reason, the analyst who estimated the resident abortion rates, explicitly noted that they should not be used to evaluate PI laws (Dennis et al. 2009). A more recent study of a PI law in Texas found that abortion rates of 17-year olds fell 15 percent and that birth rates rose 2 percent (Colman, Joyce and Kaestner 2008). The authors were able to show that relatively few minors left the state for an abortion because all but one of the surrounding states also enforced a PI law. Overall the literature suggests that PI laws from the 1980s and early 1990s had relatively little impact of the abortion rate of minors since abortion services in states without PI laws were relatively accessible. But as more states enacted PI laws, travel to avoid compliance with a local PI statute became more difficult.

If one were to assume that PI laws did generate meaningful reductions in teen abortions, the corresponding evidence that there were no increases (and possible decreases) in teen birth rates would provide indirect evidence that PI laws reduced risky sexual behavior (Kane and Staiger 1996; Levine 2003; Joyce et al. 2006; Colman et al. 2008). However, a link between PI laws and changes in the rates of abortions, births, or pregnancies does not correspond exactly to how PI laws may influence STI prevalence (Ohsfeldt and Gohmann 1994; Levine 2003; Colman et al. 2008). If PI laws induce minors to reduce their risk of an unwanted pregnancy, this can be achieved by increasing the use of hormonal contraceptives or switching to a more effective hormonal method. A reduction in STIs, on the other hand, requires that minors either reduce sexual activity or the number (or riskiness) of partners or increase the use of condoms. Thus, a negative association between PI laws and pregnancies or births or abortions would not imply that PI laws reduce the risk of STIs. The relationship between PI laws and the risk of STIs can be determined only by evaluating the effect on the prevalence of STIs or STI-related risky behavior, such as rate of sexual activity and consistency of condom use.

More direct evidence on this question has come from studies that focus on the variation in self-reports of risky sexual behavior and in rates of gonorrhea among youth. For example, using pooled cross-sections from the Youth Risk Behavioral Surveillance (YRBS) system, Levine (2001) found that PI laws had no detectable effect on the frequency of sexual activity or the likelihood of condom use. However, as Levine (2001) pointed out, this evaluation might have been underpowered because only five states had within-state variation in PI laws in the 1991–1997 YRBS. In a second study based on data from the 1988 and 1995 National Survey of Family Growth (NSFG), Levine (2003) found that PI laws had statistically insignificant effects on unprotected sex or the sexual activity of teens ages 15 to 18 (n = 2,027). However, in the smaller subsample of teens that had sex within the past three months (n = 711), Levine (2003) found a 16.5 percentage point decrease in the reported failure to use contraception at most recent intercourse attributable to PI laws. This suggestive result implies that PI laws did reduce sexual risk-taking. However, the size of this estimate is suspiciously large (a 76 percent decrease relative to the sample mean of 25 percent). Moreover, the sample size is small: only 711 teens distributed over two years and 50 states and the District of Columbia (an average of 7 teens per state and year), which suggests that the results should be interpreted with caution.

Given limitations of survey-based data for the evaluation of the effect of PI laws, a compelling direction for this literature has been to examine the effects of PI laws on a reasonably well-documented (and independently policy-relevant) proxy for risky sexual behavior: the prevalence of STIs. Dee and Sen (2006) examined the effects of PI laws and other abortion restrictions on female gonorrhea rates using a 1981 to 1995 state-year panel on the prevalence of this STI defined separately for white and black females and three age groups: minors ages 15 to 19, young people ages 20 to 24, and adults ages 25 to 29. The CDC collected these STI data, using the quarterly and annual reports made by state health departments and programs. Dee and Sen (2006) recognized that STI counts submitted to CDC frequently did not indicate the race of the patient. They relied on CDC's prorating of the "race unknown" STI data under the assumption that the distribution of the data of unknown race resembled that of the known cases within each state-age-gender cell.

DD specifications suggested that PI laws had no effects on teen gonorrhea rates among white females. Falsification results (and corresponding DDD specifications) using the contemporaneous state-year data among older women confirmed this finding. Dee and Sen (2006) also found no effects of PI laws on gonorrhea rates among female black teens with the sole exception of population-weighted DD results, which suggested that PI laws reduced the prevalence of gonorrhea rates among black teens. However, these weighted least squares (WLS) results were not robust in DDD specifications; WLS estimates also suggested that PI laws had similarly large effects on the gonorrhea rates of older black females who were not actually constrained by these laws. The implausibility of these estimates led Dee and Sen to question the validity of population weighting in this context and to conclude that PI laws did not appear to influence gonorrhea or, by implication, risky sexual behavior among teens.

A related study by Klick and Stratmann (2008), hereafter KS, examined 1981 to 1998 state-year gonorrhea rates among *all* women younger than 20 and *all* those ages 20 and older. The study concluded that PI laws led to substantive reductions in gonorrhea rates: 12 percent among white females younger than 20 and 21 percent among Hispanic females younger than 20 but no change among black non-Hispanics. These inferences are based on population-weighted DD specifications that also condition on linear state trends and the prevalence rate among women older than 20 (KS, 2008, Table 2). DDD specifications that condition on state-year, state-age, and year-age fixed effects (KS 2008, Table 3) provide mixed evidence in support of these DD results. Another substantive (and previously unrecognized) source of concern with these inferences is that they are based on the race- and ethnicity-specific gonorrhea rates reported by CDC. As discussed in more detail below, these data have a surprisingly large rate of under-reporting. A third concern is that the estimated effects reported by KS are extremely large. Sixty-one percent of minors involve parents or guardians in their decision to have an abortion (Henshaw and Kost 1992). In a separate survey, 60 percent of minors also report their parents or guardians were aware that they were accessing sexual health services (Jones et al. 2005). Thus, less than half of all minors would have an incentive to change their behavior in response to a PI law. This implies that the 20 percent decline in rates of gonorrhea among Hispanics, as reported by KS, resulted from a 50 percent decline among the sub-population of Hispanic minors who would not have involved their parents. If effect sizes this large are unrealistic, then it raises questions of statistical power to detect more realistic responses, an issue we return to below.[3]

[3] Changing the behavior of 50 percent of minors exposed to the law seems extremely large based on the recent literature on emergency contraception (EC). Free provision of EC would lower the cost of unprotected sex and thus be expected to increase the incidence of STIs. In a review of 11 randomized studies of EC, researchers reported no change in STIs or unintended pregnancy among women who were provided free courses of emergency contraception (Polis et al. 2007). Although different from PI laws, the lack of an association between the provision of EC and risky sex underscores the difficulty of changing sexual behavior.

III. DATA AND SAMPLES

Two of the strengths of our analysis over the existing literature are the multiple sources of data and a study period spanning approximately the past three decades. In this chapter, we outline the multiple sources of data on the rates of gonorrhea and chlamydia, the advantages and limitations of each, and provide a discussion of the coding of PI laws for the analysis.

A. Surveillance Data on Gonorrhea and Chlamydia

We use reported female cases of gonorrhea from 1981 to 2008 in all 50 states and the District of Columbia (hereafter 51 states) as maintained by CDC. For chlamydia we use data from 1996 to 1999 in 48 states and all 51 states from 2000 to 2008. We stratify the data by age. Two percent of gonorrhea cases and 1.2 percent of chlamydia cases lacked information on age over the study period. There were virtually no cases with unknown gender. Gonorrhea and chlamydia are part of the national public health notifiable-disease reporting system. Reports of cases are collected by local health departments, aggregated by state health departments, and sent to CDC, which helps support and coordinate surveillance. The great value of the surveillance system is the availability of data by gender, age, state, and year.

Reports of STIs vary substantially by state and region (Meyerson and Gilbert 2010). Part of the differences reflect true variation in the prevalence of STIs across the states, but there are substantial disparities in the resources states expend on surveillance and prevention, which also affect the number of reported cases.[4] Moreover, the surveillance system is passive in that reported cases are based on positive tests filed by laboratories and/or health facilities to the local health department (Institute of Medicine 1997). Because a large proportion of gonorrhea and chlamydia infections are asymptomatic, prevalence depends on access to screening and adherence to recommended screening protocols across facilities. As a result, CDC case reports underestimate true prevalence. Universal

---

[4] In the Appendix to CDC's annual STI surveillance report the authors write, "Trends in Chlamydia morbidity reporting from many areas are more reflective of changes in diagnosis, screening, and reporting practices than of actual trends in disease incidence. In particular, morbidity trends are likely highly influenced by changes in test technology, as areas increase their usage of more sensitive nucleic acid amplification tests. As areas develop Chlamydia prevention and control programs, including improved surveillance systems to monitor trends, the data should improve and become more representative of true trends in disease." Available at http://www.cdc.gov/std/stats07/app-interpret.htm#a6.

screens for gonorrhea and chlamydia based on a population-based survey of youth suggest that the CDC counts of reported cases underestimate true prevalence by as much as 50 percent in the case of chlamydia (Miller et al. 2004). The use of panel-based specifications controls for the potential biases related to state and time-specific reporting propensities. However, we do discuss the internal-validity threats related to the possibility of policy-endogenous reporting propensities as well as the inconsistent reporting of data for race-ethnic subgroups within states over time.

2. Surveillance Data by Single Year of Age

CDC will release cases of STIs only in five-year age groups. The CDC data enable us to evaluate the association between PI laws and the rates of STIs among teens ages 15 to 19 using the rate of STIs among young adults ages 20 to 24 as the comparison group. However, 18- and 19-year-olds are unaffected by PI statutes. Including them in the treatment group might obscure effects of the law on minors. Thus, we have also obtained an unbalanced panel of cases of gonorrhea and chlamydia for teens by single year of age from 21 states beginning in 1990.[5] With this subsample, we compare changes in STIs among minors ages 15 to 17 relative to older teens ages 18 and 19. Isolating minors should increase power and comparing their change in STI rates with older teens rather than with young adults is likely to improve the credibility of the counterfactual.[6]

B. Youth Risk Behavioral Survey (YRBS)

The YRBS is a probability multi-stage survey of youth administered in classrooms to students enrolled in grades 9 through 12 in public and private schools. There are two versions. One is designed to be nationally representative (YRBS-N). The other is conducted at the state and local level and is designed to be representative of high school youth within the state or local jurisdiction (YRBS-S). Each is administered biennially with the first survey beginning in 1991. We use the

[5] The CDC maintains a list of each state's STI office and director. With the CDC's permission, we used the list to contract directors. These are the states that agreed to release data. The states include Arizona, California, Colorado, Florida, Illinois, Kansas, Massachusetts, Michigan, Missouri, North Carolina, New Jersey, New Mexico, New York, Ohio, Oregon, Pennsylvania, Tennessee, Texas, Virginia, Washington, and Wisconsin.

[6] A possible limitation of using rates of STIs among 18- and 19-year-olds is that minors who delay sex in response to a PI law might take more risks as 18-year-olds. Put differently, the law might simply delay risky behavior, which would contaminate the comparison group. The use of young people ages 20 to 24 as a comparison group provides a check against that source of contamination.

national YRBS because it provides the longest panel in the most populous states. However, the YRBS-N is not designed to be representative of the state population, and youth in less populous states are surveyed sporadically. We therefore perform analyses for the full but unbalanced panel from the national survey in addition to analyses from a sub-sample of states that reported in 8 of the possible 10 years of the national survey from 1991 to 2009. [7]

We analyze five dichotomous outcomes: 1) whether the teen ever had sex; 2) whether the teen had sex in the last three months; 3) whether the teen had sex in the last three months and did not use contraception; 4) whether the teen was using the contraceptive pill at last intercourse and 5) whether the teen had sex in the last three months and did not use a condom. The latter is a measure of whether the teen was at risk for an STI. An affirmative answer to each question is coded as 1. In none of the questions do we condition on having sex because, PI laws could, in theory, deter minors from engaging in sexual activity.

C. Population Data

Population by state, year, age, gender, and race is from the Surveillance Epidemiological and End Results (SEER) of the National Cancer Institute (NCI). The SEER population data "represent a modification of the Vintage 2008 annual time series of July 1 county population estimates by age, sex, race, and Hispanic origin produced by the U.S. Census Bureau's Population Estimates Program, in collaboration with the National Center for Health Statistics, and with support from the NCI through an interagency agreement" (see http://seer.cancer.gov/popdata/methods.html). The modification affects only the state of Hawaii. The SEER population data are available from 1969 to 2008.

D. Data on Timing of PI Laws

Accurate coding of the enforcement of PI laws is essential for the validity of our estimates. We use three sources to ensure the most comprehensive and up-to-date information about the status of the laws in each state over time: Merz et al. (1995); "Who Decides? The Status of Women's

---

[7] These are AL, AZ, CA, FL, GA, IL, MI, MO, NJ, NY, NC, OH, PA and TX.

Reproductive Rights in the United States" by NARAL Pro-Choice America Foundation (NARAL) (2011), and "State Policies in Brief: Parental Involvement in Minors' Abortions" by the Guttmacher Institute (2011). The dates enforcement of PI laws began in each state based on these sources are presented in Table 1. For each state, we consider the date the law went into effect as the date of enforcement. Thirty-five states began enforcement of either a parental notification or consent law during our study period (1981 to 2008); another 5 states enacted a law before 2008, but the laws in these states have either been deemed unconstitutional or have been temporarily enjoined by the courts, and therefore have not been enforced. Finally, 10 states had not enacted a PI law before 2008 and one state has had a law in effect since 1974.[8]

IV. METHODS

As noted earlier, we use DD and DDD designs for evaluating the effect of PI laws on rates of STIs among teens and minors. In addition, we use an event-study design to evaluate the dynamic pattern of these impact estimates and assess the validity of the identifying assumptions for these designs. To help reconcile any differences between the findings of this study and those in the existing literature, we replicate the models used in KS (2008) with our data on STIs and PI laws and assess whether our results are sensitive to different model specifications. Finally, we evaluate whether our findings for rates of STIs are supported by changes in sexual risk behavior using survey responses from the YRBS.

A. Replicating the Models Employed in KS (2008)

We begin our analysis by replicating the model implemented in KS (2008), both to demonstrate that race-specific estimates of the impact of PI laws on rates of gonorrhea are most likely unreliable, and to facilitate comparison between the findings in this study and those in previous literature. Specifically, we evaluate the sensitivity of KS's findings for White non-Hispanic, Black non-Hispanic, and Hispanic women to the reporting error in CDC's race-specific data in several ways. In KS's preferred model, the dependent variable is the race-specific rate of gonorrhea for women

[8] We focus only on the date of enforcement and not the date of enactment because in some states laws are enacted but never enforced due to court rulings and in other cases there can be years between enactment and enforcement also because of protracted legal struggles.

younger than 20. The independent variables include an indicator for states that enforce a PI law, the race-specific rate of gonorrhea for women ages 20 and older, and state and year fixed effects. The inclusion of the gonorrhea rate for women ages 20 or older is meant to control for general trends in the rate among women younger than 20 in the state. We replicate these results including estimates from models with and without controls for state-specific trends. We also explore the robustness of these findings by reporting estimates for blacks and Hispanics that omit clearly misreported observations (i.e., state-year cells with zero counts) and for whites based only on states with large concentrations of white teens in the population. As an additional check, we also report results based on all cases to circumvent the misreporting of state-year outcomes by racial and ethnic groups.

B. Methods Employed in Current Study

Our preferred approach for evaluating the effects of PI laws on STI rates is based on a difference-in-difference-in-differences (DDD) framework. First, we implement a traditional DDD analysis. For this, we use the state-year CDC data available for 15-19 year-olds (most of whom would be constrained by PI laws) and for 20-24 year olds (none of whom are constrained by such laws). However, we complement this with the unique state-year STI data we were able to collect for some states by single year of age; treating 15, 16, and 17-year olds as the treatment groups within state-year cells and 18 and 19 year olds as the controls. Second, we complement our DDD analysis with corresponding evidence from an "event study" methodology, which allows us to consider dynamic treatment effects as well as the validity of the identifying assumptions in the DDD specification. We also explore our DDD results by presenting the results from DD specifications applied separately to the data from those directly bound by PI laws (15-19 year olds) and those who were not (20-24 year olds). Given the intractable limitations of the race-specific state-year data, we continue our analysis using a pooled sample that includes all reported cases of gonorrhea, regardless of race.

1. Difference-in-Difference-in-Differences Analysis

In Figures 1 and 2 we show the rates of gonorrhea and chlamydia by five-year age groups based on the data obtained from the CDC. As shown in the figures, rates of gonorrhea and chlamydia among teens ages 15 to 19 and young adults ages 20 to 24 far exceed rates among the other age groups and follow similar trajectories over the study period. PI laws apply only to minors younger than 18 and should have little impact on the behavior of young women ages 20 to 24. The latter, therefore, become a natural comparison group.

The DDD model for evaluating the association between PI laws and rates of STIs among teens is presented in Equation (1):

$$(1) \qquad R_{sta} = \beta \boldsymbol{PI_{sta}} + (\lambda_s * Teen_a) + (\tau_t * \lambda_s) + (\tau_t * Teen_a) + e_{sta}$$

where, R_{sta} is the rate of STIs by state, year, and age group (15 to 19 versus 20 to 24); the rate for both chlamydia and gonorrhea is defined as the number of female cases per 100,000 female population of the same age. PI_{sta} is a dichotomous indicator that equals one for the years and states in which young adults ages 20-24 are exposed to a PI law and zero otherwise (this variable is always zero for teens ages 15-19). $Teen_a$ is a dichotomous indicator that equals one if the outcome refers to teens ages 15 to 19 and zero for young adults ages 20 to 24. State and year fixed effects are represented by λ_s and τ_t, respectively.[9] Thus, the coefficient, β, identifies effect of PI laws on teens relative to the young adults in the same state-year cell (i.e., conditional on $\tau_t*\lambda_s$) and conditional on the unobserved determinants unique to each state-age interaction (λ_s*Teen_a) and each year-age interaction (τ_t*Teen). We estimate this specification using data from all state, year, age cells. However, given the differences in reported cases of STIs across states, we also estimate the DDD separately for the sample of states that passed a PI law during our study period and for which we have data on at least one pre-law and one post-law year (30 states in the case of gonorrhea and 9 states in the case of chlamydia).

Limiting the analysis to teens ages 15-19 and young adults ages 20-24 is an important difference between our study and that of KS (2008), who compare rates of gonorrhea of all women under age 20 to rates among all women ages 20 and older. The use of much older and much younger women who are clearly irrelevant to PI laws potentially introduces spurious trends not only in gonorrhea but in population estimates as well. However, more proximate age groups could be subject to treatment contamination because of sexual relations across these age groups. For example, if PI laws lower

[9] All age, state and year main effects are redundant when the full set of interactions are included.

STIs among female minors, STIs among their older male partners could also fall. However, since we analyze STIs among females only, the more likely source of contamination would come from lower rates of STIs among males young than 18 affecting rates of STIs among their older female partners. This is improbable since male minors are much less likely to have sex with older female partners than are female minors to have sex with older men. Data from the National Longitudinal Study of Adolescent Health (AddHEALTH) indicate that only 14 percent of male teens had sex with female partners two or more years older as compared to 32 percent of female teens (Ford, Sohn and Lepkowski 2002). Data from the National Survey of Family Growth (NSFG) reveal the same age pattern of sexual partners by gender. Among 18 to 24 year olds who had sex before age 20, only 5 percent of females had sex with a younger partner as compared to 42 percent of males (Abma et al., 2004, Table 8).

We also estimate Equation (1) using the unique single year-of-age data we acquired from 21 states. These DDD models effectively compare rates of STIs among minors ages 15 to 17 to rates among older teens ages 18 and 19. As noted earlier, rates of gonorrhea and chlamydia among all teens are dominated by the rate among those ages 18 and 19. Because PI laws should not influence the behavior of this older group, analyses that instead included them in the treatment group could have limited power to detect significant policy effects. In other words, contrasting rates of STI among minors ages 15 to 17 to those of teens 18 and 19 within state-year cells may improve our ability to detect a link between PI laws and rates of STIs.[10]

[10] Although we do not expect PI laws to affect the behavior of 18- and 19-year-olds in the year the law is enacted, such laws could become relevant for this age group in later years if, for instance, minors ages 15 to 17 forgo engaging in sexual activity in response to a PI law only to engage in risky behavior at age 18 when they are no longer required to involve their parents should they want to have an abortion. Alternatively, PI laws might affect the behavior of 18- and 19-year-olds if they have a lasting impact on those exposed to the law as minors. Both scenarios would contaminate the comparison group and could lead to biased estimates. Recent evidence on the association between PI laws and rates of abortions in Texas suggest a decline among minors ages 17 and younger at the time of conception, but no change among 18-year-olds at conception. Given this evidence, we believe it is reasonable to assume that PI laws do not influence the risky sexual behavior of older teens in either direction. If the assumption that PI laws do not affect the behavior of 18- and 19-year-olds holds, then we should have more power to detect an association between PI laws and rates of STIs among minors.

2. Event-Study Design

We complement our canonical DDD methodology with an event-study approach that allows for an unrestricted examination of the differences in the rates of STIs between the treatment and comparison groups in the years before and after the enactment of a law. This approach allows us to assess the possibly dynamic treatment effects of PI laws. Critically, it also provides ad-hoc evidence on the identifying assumptions in this specification. Specifically, if there were substantial differences in STI trends between teens and young adults *before* a PI law, it would suggest that we have not adequately controlled for the unobserved determinants varying within states over time that are unique to the younger age group bound by PI laws.

The event-study approach is represented by the following equation:

$$(2)\quad R_{sta} = \sum_{k=-6}^{k=6}(\beta_k * PI_{sa} * 1(t - T_t^* = k+1) + (\lambda_s * Teen_a) + (\tau_t * \lambda_s) + (\tau_t * Teen_a) + e_{sta}$$

The notation in Equation (2) is similar to that of Equation (1) with the following modifications. The variable PI_{sa} is one for teens in states that ever passed a PI law. Let T^* be the year the law went into effect and let k vary from 1 to 6 or more and from -1 to less than or equal to -6. The year before the law goes into effect in a given state is normalized to zero. The indicator function creates the leads and lags for that state and age group. For example, if a PI law goes into effect in 1990, k=0 for 1989 and thus the indicator function is zero; k=1 for 1990 and the indicator function is one. For each state with a law there is a maximum of 12 parameters since any period 6 or more years before or after the law is captured by a single coefficient. In this specification, coefficients, β_k, estimate the DDD for the periods before and after implementation of each state's PI law. If the state, year and age interactions effectively control for trends, then plots of β_k should fluctuate around zero in years before the law.

C. Analysis of Sexual Risk Behavior

Finally, we supplement our analysis of STI rates with an analysis of teen sexual risk behavior using individual-level data on youth ages 15-17 from the YRBS. Because the survey does not collect equivalent data on older youth, our analysis relies on a simple difference-in-differences (DD) method exploring within-state variation over time in behavior to identify effects of the law. The DD model is expressed in equation (3). Specifically, R_{iast} is a dichotomous indicator of sexual risk behavior of individual i of age a, in state s, and year t; PI_{st} is a dichotomous indicator for states and years a PI law is in effect; X includes controls for age and race; and the last two terms capture state and year fixed effects. We estimate equation (3) by ordinary least squares (OLS); estimates based on logistic regression are similar to OLS estimates. As in the previous models we are interested in the coefficient on the PI law, β in this instance. Our measures of risky sexual activity should be negatively correlated with enforcement of a PI statute, if teens take the law into account when considering sexual activity, but positively correlated with use of the contraceptive pill.

$$(3) \qquad R_{iast} = \beta \boldsymbol{PI_{st}} + \mathbf{X}\boldsymbol{\alpha} + \tau_t + \lambda_s + e_{sta}$$

We use two samples: the first contains data from 14 states that surveyed teens in at least 8 of the 10 survey years biennially from 1991 to 2009; the second includes teens from every state that participated in the national YRBS. The 14-states sample includes over 70 percent of the available observations from the national YRBS.

V. ESTIMATED IMPACTS OF PI LAWS ON RATES OF STIs

A. Replication of Methods Applied by Klick and Stratmann (2008)

In Table 2 we present estimates based on models reported in KS (2008). We replicate their results in columns (1), (3) and (5) of Panels A and B. For each racial group we show coefficients on the PI law from models with and without state-specific trends as well as estimates weighted by race-specific female population younger than 20 and unweighted estimates. Results from KS's preferred specification indicate a statistically significant negative association between PI laws and rates of gonorrhea among white non-Hispanic and Hispanic women younger than 20. Specifically, they found that PI laws were associated with a decline of 9.5 cases of gonorrhea per 100,000 among white non-Hispanics and a decline of 12.0 cases per 100,000 among Hispanics (KS 2008, Table 2). However, the findings are sensitive to the inclusion of state-specific trends as controls and to weighting. For instance, none of the estimates from models that do not control for state-specific trends, or control for state-specific trends but are not weighted by the size of the population are statistically significant (Panel B-D, columns (1), (3) and (5)).

The Limitations of STI Reporting by Race and Ethnicity

As suggested earlier, a particularly salient limitation of the CDC system is the large number of cases for which race and ethnicity are unknown or misreported. This inconsistent reporting is unfortunate because racial and ethnic differences in the prevalence of gonorrhea and chlamydia are profound. Data from the National Longitudinal Study of Adolescent Health indicate that 12.5 percent of blacks, 1.9 percent of whites, and 5.9 percent of Hispanics ages 18 to 25 were infected with chlamydia in 2001–2002. Differences in the prevalence of gonorrhea by race/ethnicity are even greater (Miller et al. 2004).

If the percentage of unknown cases reported to CDC by race and ethnicity were relatively modest and stable over time, then race-specific analyses by KS would be more justifiable. However, the proportion of unknown cases by race and ethnicity varies dramatically by state and year. For instance, Figure 3 shows the percentage of gonorrhea cases with unknown race in the 10 most and 10 least populous states along with the percentage unknown in all 51 states from 1981 to 2008. The most populous states are generally those with the largest proportion of racial and ethnic subgroups. The percentage of cases with unknown race is relatively large (i.e., typically 15 to 20 percent in all states and twice as large in the most populous states). The year-to-year fluctuations are also substantial, particularly in the most populous states. Figure 4 shows the percentage of cases with unknown race for chlamydia from 1996 to 2008. The pattern is similar if not worse than with gonorrhea: the percentage of chlamydia cases of unknown race is roughly 25 percent across all states though there is less variation in the percentage of these STI cases of unknown race/ethnicity.

As an additional illustration of the limitation of race-specific data in the context of a difference-in-differences analysis, in Figure 5, we show the number of reported gonorrhea cases among Hispanic females between 1981 and 1998 in the four states with the largest Hispanic population and a PI law (AZ, GA, MI, PA) and then similar states without a PI law in effect during this period (CA, FL, NY, TX). In both sets we contrast the trend in reported cases among Hispanics to the trend in the number of cases with unknown race. In Arizona, the state with the largest Hispanic population that implemented a PI law between 1981 and 1998, there is a downward trend in the number of reported cases among Hispanics, and an upward trend in the number of reported cases with unknown race right around the period the law went into effect (1985). In Michigan, Pennsylvania and Georgia—states with a PI law with the second, third, and fourth largest Hispanic population, respectively—the reported cases among Hispanics is zero or close to zero throughout the period, while the cases with unknown race fluctuate wildly. For example, in Michigan, the number of cases among Hispanics is zero between 1981 and 1992, and only between 40 and 70 in the later years. At the same time, the number with unknown race is zero in 10 out of 18 years, and is between 4,000 and 8,000 during the other 8 years. The reported cases in the four largest states without a PI law between 1981 and 1998 are presented in the bottom half of Figure 5. They reveal the same reporting problems in the race-specific data. Notably, in New York, the number of reported cases among Hispanic women is close to 0 throughout the study period which makes no sense given the size of the Hispanic population, while the number with unknown race is as high as 25,000 in 1986 then gradually declines to about 5,000 in 1998. While California seems to have somewhat better reporting among Hispanics compared to New York, the number of reported cases is between 1,100 and 5,500 during 1981-1998, which seems very low given the size of the Hispanic population in the state.

Another problem with the data on gonorrhea by race and ethnicity pertains to cells with zero reported cases that clearly represent non-reporting rather than true zeros. Figure 6 shows cases of gonorrhea among black non-Hispanic females in four populous states. Klick and Stratmann (2008) treated the years with no reported cases as true zeros when they should have been categorized as missing.

We assess the sensitivity of KS's findings to the reporting error in CDC's race-specific data in three steps. First, we estimate KS's model separately for White non-Hispanics, Black non-Hispanics, and Hispanics after removing from the data all observations with a zero rate of gonorrhea that are clearly not true zeros.[11] Second, we estimate the KS model for White non-Hispanics in states in which at least 85 percent of female population under 20 was white according to the 1990 census. These included Idaho, Indiana, Iowa, Kentucky, Maine, Minnesota, Nebraska, New Hampshire,

[11] Cells with values greater than zero are not necessarily credible, either. There is extensive underreporting by race, especially for Hispanics as is evident in New York (Figure 5), but there is no objective way to eliminate cells with too few cases to be credible.

North Dakota, Oregon, South Dakota, Montana, Utah, Vermont, West Virginia, Wisconsin and Wyoming. Finally, we evaluate whether the race-specific findings by KS hold up for the sample of all women, and compare the estimates among all women for the period covered by KS (1981-1998) to the estimate based on 10 years of additional data (1999 to 2008). Results of these analyses are presented in Table 2.

As noted above, results in columns (1), (3), and (5) of panels A and B replicate KS's estimates (i.e., their Table 2). The remaining results in Table 2 explore the robustness of these findings. For example, we drop cells with zero cases of gonorrhea for Hispanics and Blacks in columns (2) and (4) respectively. In column (6) we include only whites from predominantly white states (the 17 states where at least 85 percent of the women under 20 were white). Interestingly, the estimate for Hispanics falls almost in half and is statistically insignificant when these "zero" cells are omitted from the specification preferred by KS (i.e., Panel A). Similarly, the coefficient for whites (i.e., column (6)) becomes statistically insignificant (and has the opposite sign) when we limit the sample to states where misreporting is less pronounced (i.e., states with high concentrations of whites). None of the other estimates in columns (2), (4) and (6) of panels B-D provide evidence of an association between PI laws and rates of gonorrhea. In fact, the evidence from these unweighted regressions implies that population weighting exacerbates the measurement error as states with the largest minority population have the greatest proportion of unknowns. For instance, the coefficient on PI laws in the weighted regression of Hispanics changes from negative to positive when unweighted (column 2, Panels A and C). In columns (8) we analyze rates of gonorrhea from 1981-1998 among all women for which there are relatively few unknowns and in column (9) we extend the sample to include data through 2008. There is no association with PI laws in any specification.[12]

Statistical Power

The comparative results in Table 2 demonstrate the sensitivity of the results reported by KS both to the previously undiagnosed misreporting of race-specific STI data and to specification

[12]Our coding of PI laws differs in some states from that of Klick and Stratmann (2008). We re-estimated the model in column (8) using our coding through 1998 and the results did not differ significantly that that of KS. The coefficient was (-21.31) with a standard error of (12.87) in KS preferred specification. Additional results are available upon request.

choices (i.e., conditioning on linear state trends and the use of weighting). However, it is important to note that the alternative results presented in Table 2 are in general not estimated with sufficient statistical precision to clearly reject the large KS estimates. For example, KS report a point estimate of -12.0 in the rate of gonorrhea among Hispanics, a decline of 18.1 percent evaluated at the mean of 66.49 (Table 2, Panel A, column 1). When state-year cells with zero reported Hispanic cases are omitted (Table 2, Panel A, column 2), the confidence interval for the resulting impact estimate (-14.75) includes that reported by KS. However, it should be noted that the results that correct for misreporting of race-specific STI data do have sufficient statistical power to reject the KS estimate for the impact of PI laws on white teens. Specifically, the 95-percent confidence interval for the point estimate based only on data where a clear majority of teens are white (Table 2, Panel A, column 7) does not include the large point estimate reported by KS (column 5).

We also question whether a decline in gonorrhea rates of 10 percent among whites and 20 percent among Hispanics, as suggested by KS, is plausible. As noted above, only 40 percent of minors would not involve parents in their decision n to abort based on a survey of minors in states without a PI law (Henshaw and Kost 1992). Thus a 10 percent decline in the rate of gonorrhea suggests a 25 percent decline among minors sensitive to the law, a sizeable change. In addition, KS contrast gonorrhea rates of all women less than 20 years of age to all women 20 years and older which further diminishes power since relatively small proportion of women in the younger age group are affected. In results that follow, we likely increase statistical power by: (1) narrowing the analysis to the age group most likely affected by the law (15-19 year-olds), (2) analyzing all minors and women, which eliminates the measurement error associated with race and ethnicity, (3) extending the series to 2008, and (4) analyzing the association between PI laws and chlamydia, a much more prevalent STI.

B. Results for Gonorrhea (1981-2008) and Chlamydia (1996-2008): DDD and Event Study Design

1. Gonorrhea

The point estimates of interest from Equation (1) for teens are presented in Table 3. The DDD estimates based on the data in 5-year age groups and on the data by single year of age consistently indicate that PI laws had statistically insignificant effects on gonorrhea rates. In the models based on the 5-year age groups the point estimates are particularly imprecise. In contrast, the results from models based on the single year of age data imply fairly tight bounds. For example, model (8) in Table 3 implies a 95-percent lower confidence limit of approximately 41 (i.e., under 6 percent relative to the sample mean).

The event-study results based on the gonorrhea data strengthen the case for these null findings. In Figure 7 we plot the unweighted estimates of β based on Equation (2), which allows the DDD estimates to vary for each pre- and post-implementation year. Estimates of β range from -41 to 8.8 during the pre-implementation years and from -71 to -17 during the post-law years. The estimates are small relative to the standard errors, but even in the first two years after implementation of a law, in which the confidence intervals are much tighter, we observe no association between rates of gonorrhea and PI laws.

2. Chlamydia

The DDD estimates based on equation (1) for chlamydia are presented in Table 4. Across both types of data—data by 5-year age groups and by single year of age—, all of these estimates are statistically indistinguishable from zero. The estimates for teens ages 15 to 19 are positive in models that only include states that enforced a PI law during our study period (columns 1, 2 and 5, 6). They become negative when all available states are included, but are relatively small when contrasted with the mean rate for teens. The findings are similar for models based on the data by single year of age. The DDD estimates based on the 7 states with a change in the laws between 1997 and 2007 are positive and rather large, whereas the ones based on 21 states are negative and much smaller in magnitude. None of the estimates is statistically significant.

In Figure 8 we show separate DDD estimates for each pre- and post-implementation year based on equation (2) for teens ages 15 to 19 in 50 states and the District of Columbia. The lack of statistically significant findings for any of the seven post-implementation years, and the pattern of the coefficients confirm the lack of association between PI laws and rates of chlamydia among teens suggested by our earlier results. While the estimate for the year of implementation is negative, it is relatively small in magnitude (-24). Estimates for all subsequent years are positive and nonsignificant.

Furthermore, while none of the coefficients for the five pre-law years are statistically significant, their magnitude and pattern is close to the coefficients for the post-law years.

3. Placebo Tests for Gonorrhea and Chlamydia

To this point we have emphasized results that use older women within each state who were unexposed to a PI law as a counterfactual. We prefer relying on a DDD procedure and "controls" specific to each state-year cell because it provides more credible causal inferences. For example, DDD estimates would be robust to the hypothetical bias that would be created if the state adoption of PI laws were correlated for some reason with state-specific changes in STI reporting propensities. Nonetheless, conventional DD estimates (which are not necessarily robust to this criticism) provide a useful complement to our main results. An advantage of the DD specifications is that they allow for a transparent placebo test, because there should be no association between PI laws and the rate of STIs among women ages 20 to 24. Results from this exercise are displayed in Table 5. Panel A pertains to gonorrhea and Panel B to chlamydia. The results for teens ages 15 to 19 show that PI laws are negatively correlated with gonorrhea but the standard errors are relatively large and we could not reject the null of a 20 percent decline. The reported prevalence of chlamydia is roughly three times greater than that of gonorrhea and more precisely estimated. We find that PI laws are negatively associated with rates of chlamydia among teens but we obtain almost the same results for young adults ages 20 to 24 (Table 5, lower panel), Indeed, if we scale the coefficients by their respective means, the relative declines among the two age groups are even more similar. Finally, we note that these results from the placebo tests are consistent with the DDD estimates in Tables 3 and 4. To see this, note that the difference in the coefficients in models 1 and 3 and 2 and 4 for both gonorrhea and chlamydia in Table 5 are very close to the DDD estimates in Tables 3 and 4 (columns 1 and 2), respectively. The DDD estimates in Tables 3 and 4 also provide noticeably more statistical precision for both STI outcomes. In sum the placebo tests underscore the importance of controlling for within-state trends in STI in testing their association with PI laws; they also confirm the lack of a credible association between PI laws and rates of STIs among teens.

C. Analysis of the YRBS

Estimates of β from equation (3) for each outcome and samples of states are shown in Table 6. The estimated coefficients for age and race conform to the prior literature. For example, risky sexual behavior increases with age and is more prevalent among black and Hispanic minors relative to whites (Santelli et al. 2004). The last two columns show estimates of the association between PI laws and the contraceptive pill. Although not a risky behavior per se, an increase in pill use in response to a PI law would be associated with a decrease in abortions and pregnancies, but no change in STI's all else constant. Estimates of the effect of PI laws are all positive and relatively small in magnitude, suggesting that there is little evidence that PI laws are associated with a reduction in risky sexual behavior. Nor are PI laws associated with pill use. Moreover, the coefficients from the sample of 14 states vary little from those based on all available states. Although we cluster the standard errors by states, the small number of states and highly unbalanced panel may render the adjustment less effective. In results not shown, we aggregate the data to the state level and re-estimate the models. Aggregation eliminates the intra-class correlation among individuals in the same states. We then estimate regressions with the aggregate data adjusted for first-order autocorrelation. The coefficients on the PI laws are always positive and thus counter to the hypothesis that PI laws encourage risky sex, but none are statistically significant (results available upon request).

VI. CONCLUSION AND IMPLICATIONS

Currently, laws in 37 states require that physicians notify or obtain consent from a parent or parents of a minor before performing an abortion. Advocates of these laws argue that they reduce risky sexual behavior among teens. However, prior evidence on this question has been mixed. This study examined this issue using multiple and updated sources of data. We found little evidence that PI laws were associated with changes in rates of gonorrhea or chlamydia among teens ages 15 to 19 and minors ages 15 to 17. Similarly, we uncovered no association between PI laws and direct measures of sexual activity in a subset of states that participated in the YRBS. Our findings are at odds with a previous study that reported a decrease in rates of gonorrhea among white non-Hispanic and Hispanic women younger than 20 in the wake of a PI statute (KS 2008). However, race is poorly reported in the CDC data as the most populous and diverse states have the greatest percentage of cases with unknown race and ethnicity. In an effort to reconcile the conflicting findings, we estimated the same model as KS but for all women instead of by race. We found little evidence to support their conclusion that PI laws are negatively related to rates of gonorrhea.

Our findings have important public health implications. Given the alarmingly high rate of STIs among teens, decreasing risky sexual behavior and thereby reducing the spread of STIs among young individuals remains a vital policy objective. Our results suggest that PI laws do not induce minors to change sexual behaviors that might lower the incidence of STIs. Results from the YRBS, although limited by the sample, suggest more proximate measures of sexual behavior are also unaffected. One interpretation is that the constraints implied by PI laws influence minors when making decision regarding sexual activity. Another explanation for the lack of association is that the law does not affect a sufficiently large portion of minors so as to be detectable at the population level. Upwards of 60 percent of minors discuss their decision to obtain an abortion with their parent(s) in states without a PI law, and thus are unlikely to be affected by a PI requirement. In addition, a substantial proportion of minors obtain an abortion via a court bypass procedure (Blum et al. 1990; Joyce 2010). This suggests that a relatively small proportion of minors even potentially view the law as increasing the cost of an abortion. In sum, the findings suggest that states look beyond PI laws in seeking to curb risky sexual activity and the rate of STIs among teens.

References

Abma, J.C., G.M. Martinez, W.D. Mosher, and B.S. Dawson. "Teenagers in the United States: Sexual activity, contraceptive use, and childbearing, 2002." National Center for Health Statistics. *Vital Health Statistics* 23(24). 2004.

Argys, L.M., S.L. Averett, and D.I. Rees. "The Impact of Government Policies and Neighborhood Characteristics on Teenage Sexual Activity and Contraceptive Use." *American Journal of Public Health,* vol. 92, no. 11, 2002, pp. 1173–1178.

Associated Press. "Parental consent urged for abortions: Proposal would require girls under 18 to get approval." As printed in the Houston Chronicle, February 18, 2005. Available at http://www.chron.com/disp/story.mpl/metropolitan/3045585.html. Accessed September 5, 2010.

Blum, R., M. Resnick, and T. Stark. "The Impact of a Parental Notification Law on Adolescent Abortion Decision-Making." *American Journal of Public Health,* vol. 77, 1987, pp. 619–620.

Blum, R., M. Resnick, and T. Stark. "Factors Associated with the Use of Court Bypass by Minors to Obtain Abortions." *Family Planning Perspectives,* vol. 22(4), 1990, pp. 158–160.

Cartoff, V.G., and L.V. Klerman. "Parental Consent for Abortion: Impact of the Massachusetts Law." *American Journal of Public Health,* vol. 76, no. 4, 1986, pp. 397–400.

Centers for Disease Control and Prevention. *Sexually Transmitted Disease Surveillance, 2007.* Atlanta, GA: U.S. Department of Health and Human Services, December 2008.

Colman, S., T. Joyce, and R. Kaestner. "Misclassification Bias and the Estimated Effect of Parental Involvement Laws on Adolescents' Reproductive Outcomes." *American Journal of Public Health,* vol. 98, no. 10, 2008, pp. 1881–1885.

Dee, T.S., and B. Sen. "Do Parental Involvement Laws Promote Safe Sex Among Female Teens?" Working paper, 2006.

Dennis, A., S.K. Henshaw, T.J. Joyce, L.B. Finer, and K. Blanchard. "The Impact of Laws Requiring Parental Involvement for Abortion: A Literature Review." New York: Guttmacher Institute; 2009. Available at http://www.guttmacher.org/pubs/ParentalInvolvementLaws.pdf. Accessed October 27, 2010.

Ellertson, C. "Mandatory Parental Involvement in Minors' Abortions: Effects of the Laws in Minnesota, Missouri, and Indiana." *American Journal of Public Health,* vol. 87, no. 8, 1997, pp. 1367–1374.

Ford, K., W. Sohn, and J. Lepkowski. "American Adolescents: Sexual Mixing Patterns, Bridge Partners, and Concurrency." *Sexually Transmitted Diseases*, vol. 29., no. 1, 2002, pp. 13-19.

Guttmacher Institute. "State Policies in Brief: Parental Involvement in Minors' Abortions." New York; Guttmacher Institute, May 2011.

Hampton, T. "Researchers Seek Ways to Stem STDs." *Journal of the American Medical Association,* vol. 299, no. 16, 2008, pp. 1888–1889.

Henshaw, S.K. "The Impact of Requirements for Parental Consent on Minors' Abortions in Mississippi." *Family Planning Perspectives,* vol. 27, no. 3, 1995, pp. 120–122.

Henshaw, S.K., and K. Kost. "Parental Involvement in Minors' Abortion Decision." *Family Planning Perspectives,* vol. 24, no. 5, 1992, pp. 196–207 and 213.

Institute of Medicine. The Hidden Epidemic: Confronting Sexually Transmitted Diseases. Edited by T.R. Eng and W.T. Butler. Washington, DC: National Academy Press, 1997.

Jones, R., A. Purcell, S. Singh, and L.B. Finer. "Adolescents' Reports of Parental Knowledge of Adolescents' Use of Sexual Health Services and Their Reactions to Mandated Parental Notification for Prescription Contraception." *Journal of the American Medical Association*, vol. 293, no. 3, 2005, pp. 340-348.

Joyce, T., R. Tan, and Y. Zhang. "Changes in Teen Fertility Following Access to the Pill and Abortion in the Early 1970s." Manuscript, 2010.

Joyce, T., and R. Kaestner. "State Reproductive Policies and Adolescent Pregnancy Resolution: The Case of Parental Involvement Laws." *Journal of Health Economics,* vol. 15, no. 5, 1996, pp.579–607.

Joyce, T., R. Kaestner, and S. Colman. "Changes in Abortions and Births and the Texas Parental Notification Law." *New England Journal of Medicine,* vol. 354, no. 10, 2006, pp. 1031–1038.

Kane, T., and D. Staiger. "Teen Motherhood and Abortion Access." *The Quarterly Journal of Economics,* vol. 111, no. 2, 1996, pp. 467–506.

Klick, J., and T. Stratmann. "Abortion Access and Risky Sex Among Teens: Parental Involvement Laws and Sexually Transmitted Diseases." *Journal of Law, Economics, and Organization,* vol. 24, no. 1, 2008, pp. 2–21.

Levine, P.B. "The Sexual Activity and Birth-Control Use of American Teenagers." In *Risky Behavior Among Youth: An Economic Analysis,* edited by J. Gruber. Chicago and London: University of Chicago Press, 2001.

Levine, P.B. "Parental Involvement Laws and Fertility Behavior." *Journal of Health Economics,* vol. 22, no. 5, 2003, pp. 861–878.

Levine, P.B. and D. Staiger. "Abortion Policy and Fertility Outcomes: The Eastern European Experience." *Journal of Law and Economics*, vol. 47, 2004, pp. 223-243.

Merz, J.F., C.A. Jackson, and J.A. Klerman. "A Review of Abortion Policy: Legality, Medicaid Funding, and Parental Involvement 1967–1994." *Women's Rights Law Reporter,* vol. 17, 1995, pp. 1–61.

Meyerson, B.E. and L.K. Gilbert. "Show Me the Money: State Contributions Toward STD Prevention, 2007." *Journal of Public Health Management Practice,* vol. 16, no. 3, 2010, pp. 232–239.

Miller, W.C., C.A. Ford, M. Morris, M.S. Handcock, J.L. Schmitz, M.M. Hobbs, M.S. Cohen, K.M. Harris, and J.R. Udry. "Prevalence of Chlamydial and Gonococcal Infections Among Young Adults in the United States." *Journal of the American Medical Association,* vol. 29, no. 7, 2004, pp. 2229–2236.

NARAL Pro-Choice America Foundation. "Who Decides? The Status of Women's Reproductive Rights in the United States." 20th edition. Washington D.C.: NARAL, January 2011.

Oshfeldt, R. and S. Gohmann. "Do Parental Involvement Laws Reduce Adolescent Abortion Rates?" *Contemporary Economic Policy,* vol. 12, 1994, pp. 65–76.

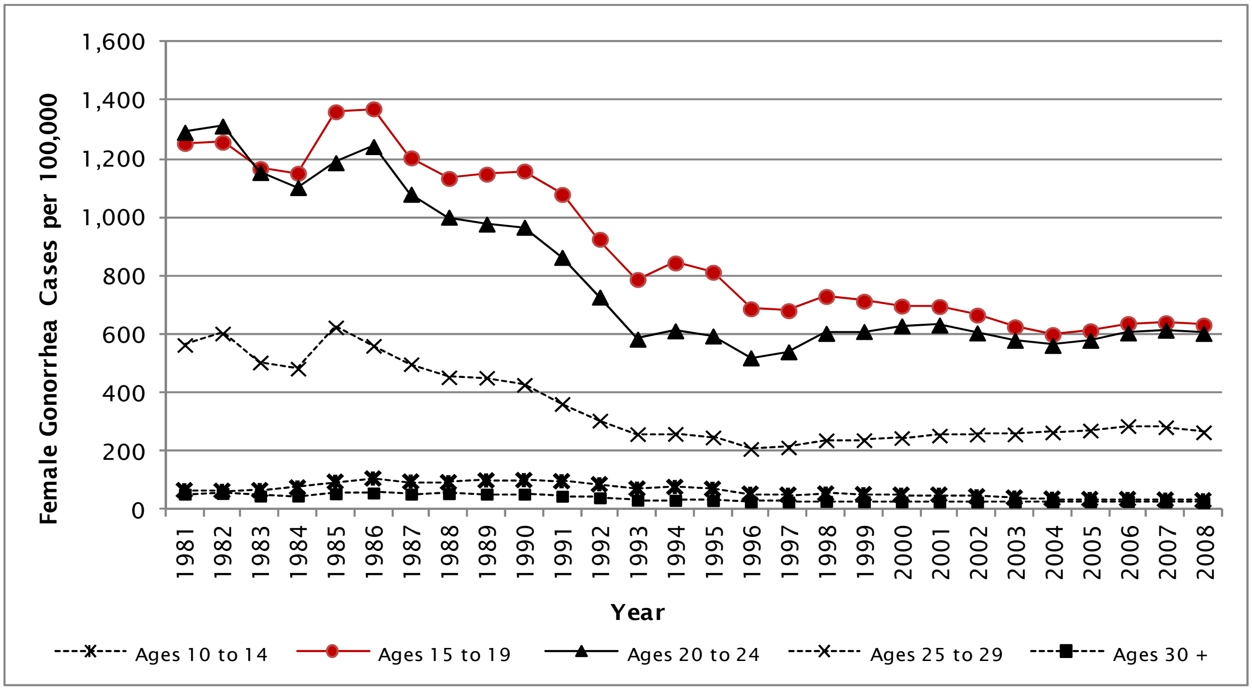
Paton, D. "Random Behavior or Rational Choice? Family Planning, Teenage Pregnancy and Sexually Transmitted Infections." *Sex Education,* vol. 6, no. 3, 2006, pp. 281–308.

Polis, C.B., D.A. Grimes, K. Schaffer, K. Blanchard, A. Glasier,and C. Harper. "Advance provision of emergency contraception for pregnancy prevention." Cochrane Database of Systematic Reviews 2007, Issue 2. Art. No.: CD005497. DOI:10.1002/14651858.CD005497.pub2.

Santelli, J.S., J. Abma, S. Ventura, L. Lindberg, B. Morrow, J.E. Anderson, S. Lyss, and B.E. Hamilton. "Can Changes in Sexual Behaviors among High School Students Explain the Decline in Teen Pregnancy Rates in the 1990s?" *Journal of Adolescent Health*, vol. 25, no. 2, 2004, pp. 80-90.

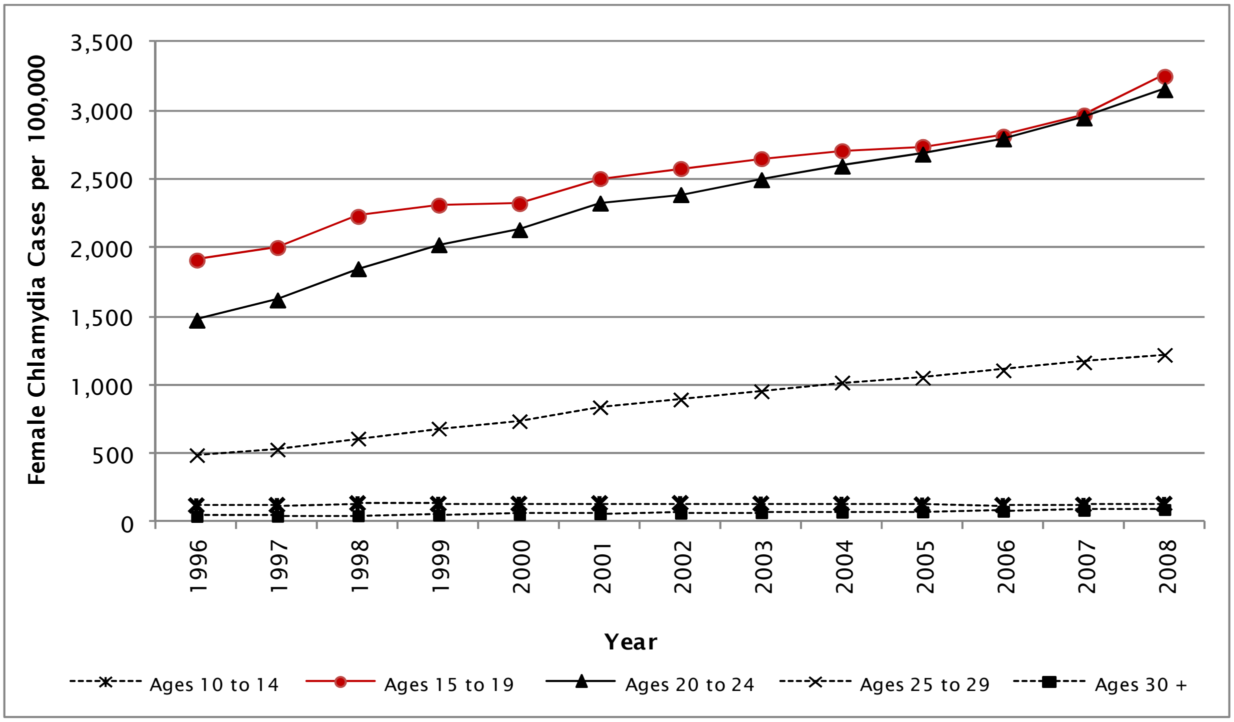
Stone, R. and C. Waszak. "Adolescent Knowledge and Attitudes About Abortion." *Family Planning Perspectives,* vol. 24, 1992, pp. 52–57.

Figure 1. Female Gonorrhea Rates, by Age Group and Year; 1981-2008



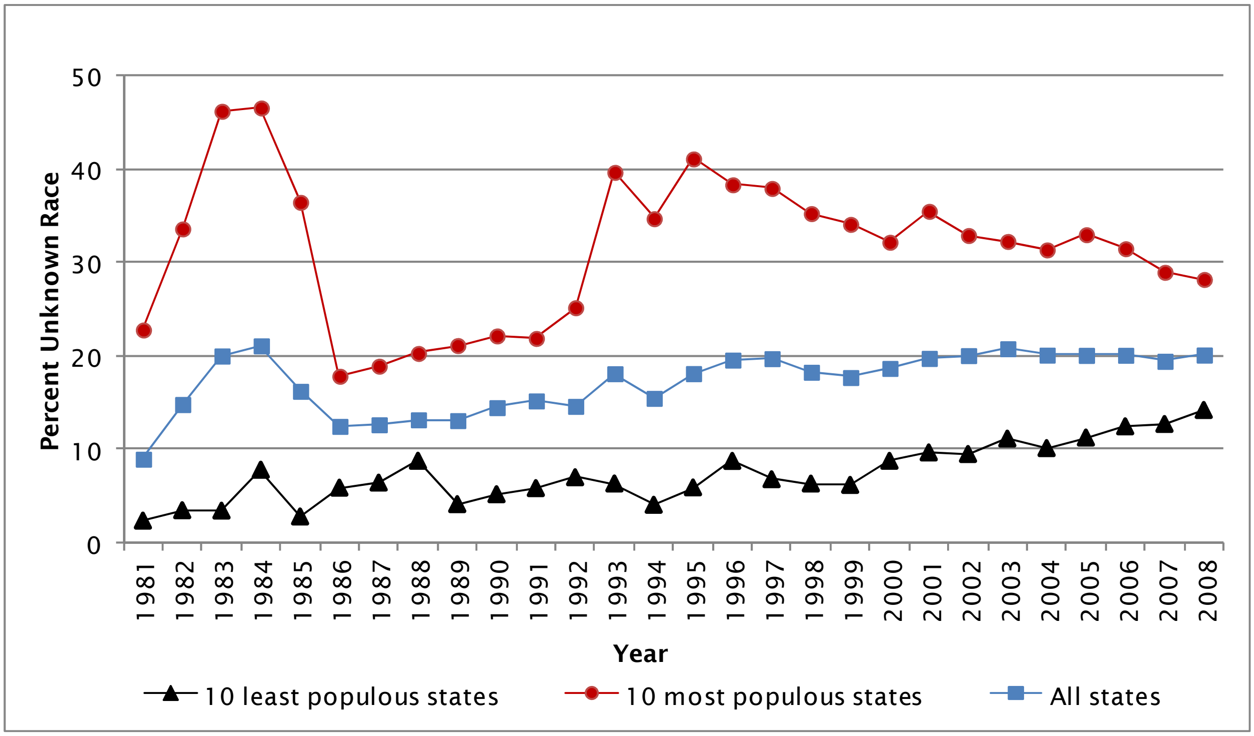
Source: Authors' calculation based on data reported to the National Center for HIV/AIDS, Viral Hepatitis, STD, and TB Prevention (NCHHSTP), Centers for Disease Control and Prevention (CDC).

Figure 2. Female Chlamydia Rates, by Age Group and Year, 1996-2008



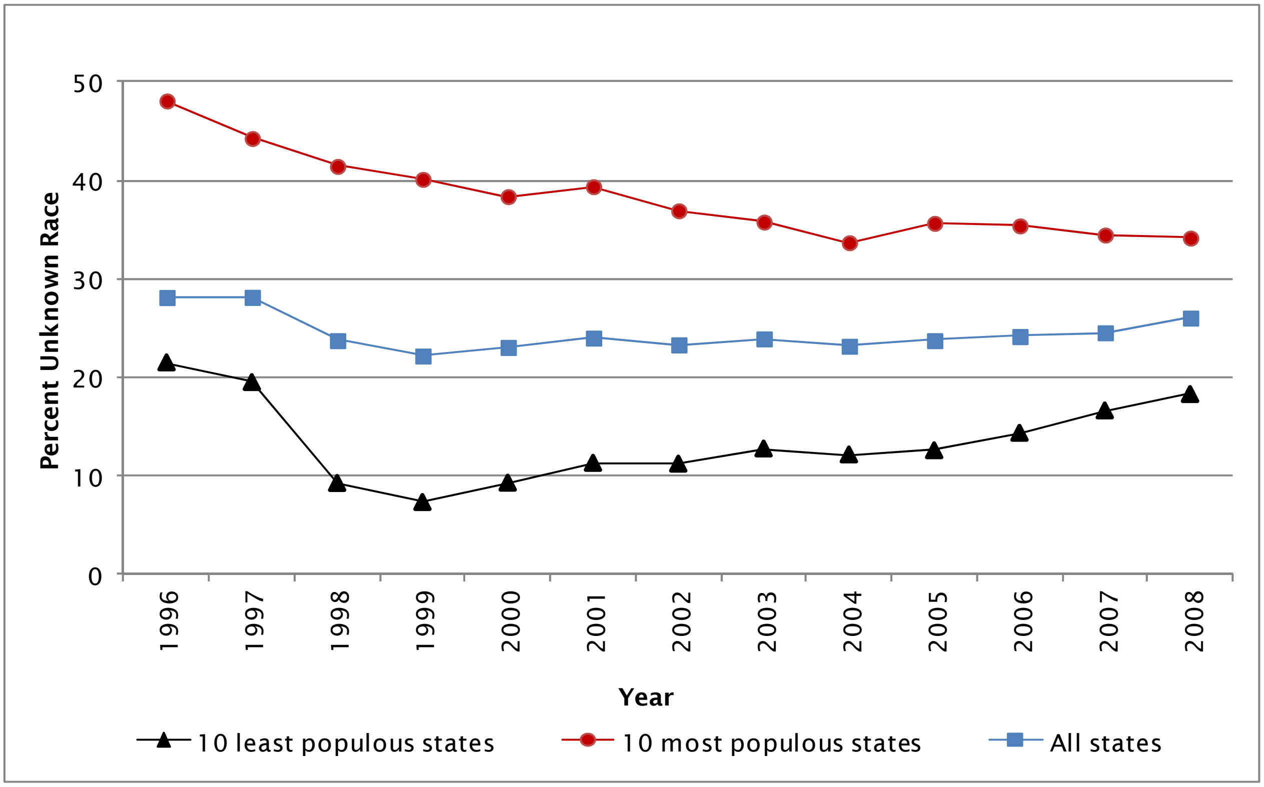
Source: Authors' calculation based on data reported to the National Center for HIV/AIDS, Viral Hepatitis, STD, and TB Prevention (NCHHSTP), Centers for Disease Control and Prevention (CDC).

Figure 3. Percentage of Female Gonorrhea Cases with Unknown Race/Ethnicity, by State Population Size and Year



Source: Authors' calculation based on data reported to the National Center for HIV/AIDS, Viral Hepatitis, STD, and TB Prevention (NCHHSTP), Centers for Disease Control and Prevention (CDC).

Figure 4. Percentage of Female Chlamydia Cases With Unknown Race/Ethnicity, by State Population Size and Year



Source: Authors' calculation based on data reported to the National Center for HIV/AIDS, Viral Hepatitis, STD, and TB Prevention (NCHHSTP), Centers for Disease Control and Prevention (CDC).

Figure 5: Cases of Gonorrhea to Hispanics and Cases of Unknown Race/Ethnicity by Selected States with Parental Involvement Laws (AZ, GA, MI, PA) and States Without (CA, FL, NY, TX), 1981-98

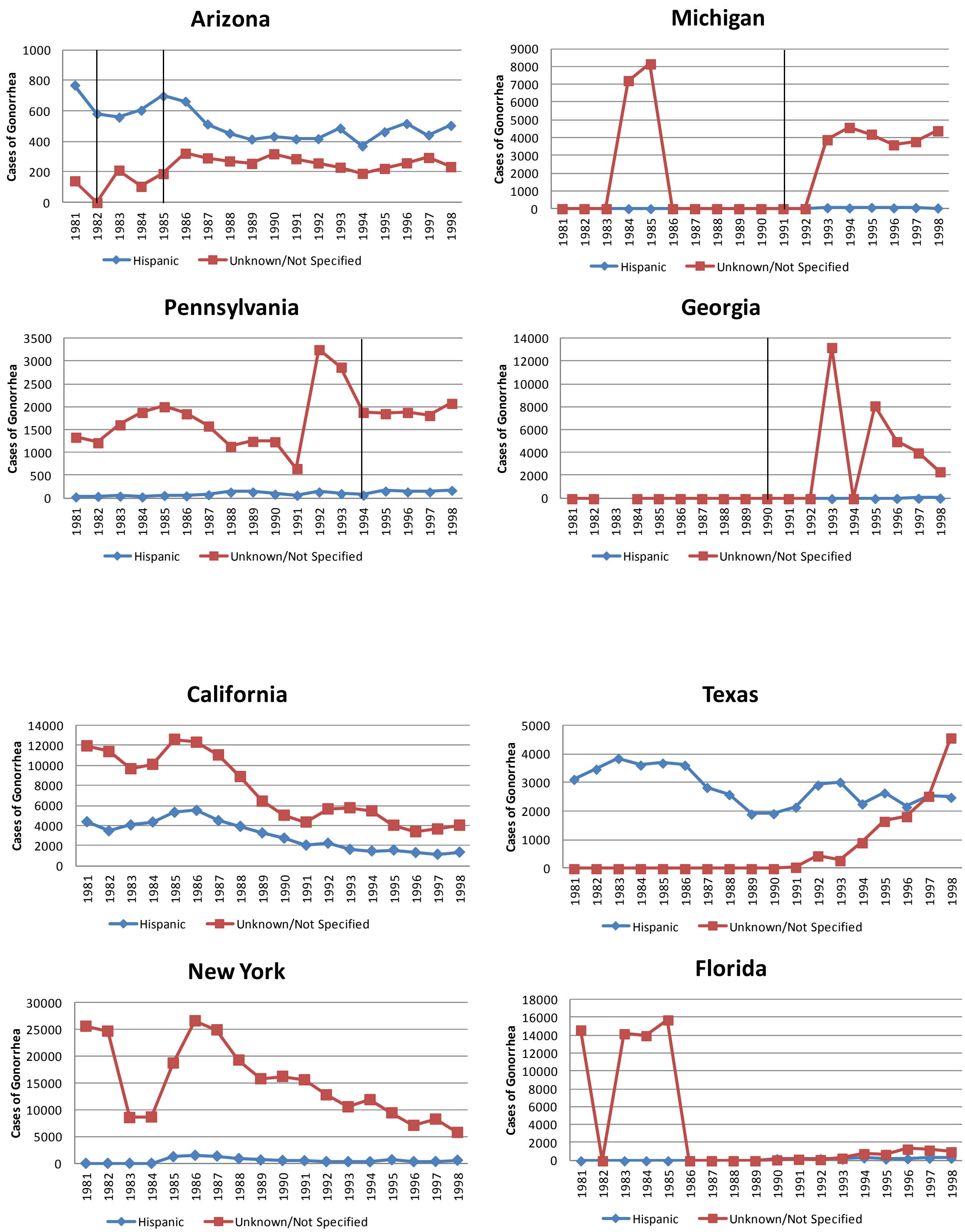


Figure 6. Reported Cases of Gonorrhea Among Black Non-Hispanic Females in States with a Black Non-Hispanic Population of More than 100,000: The Four States with the Least Complete Reporting Between 1981 and 1990

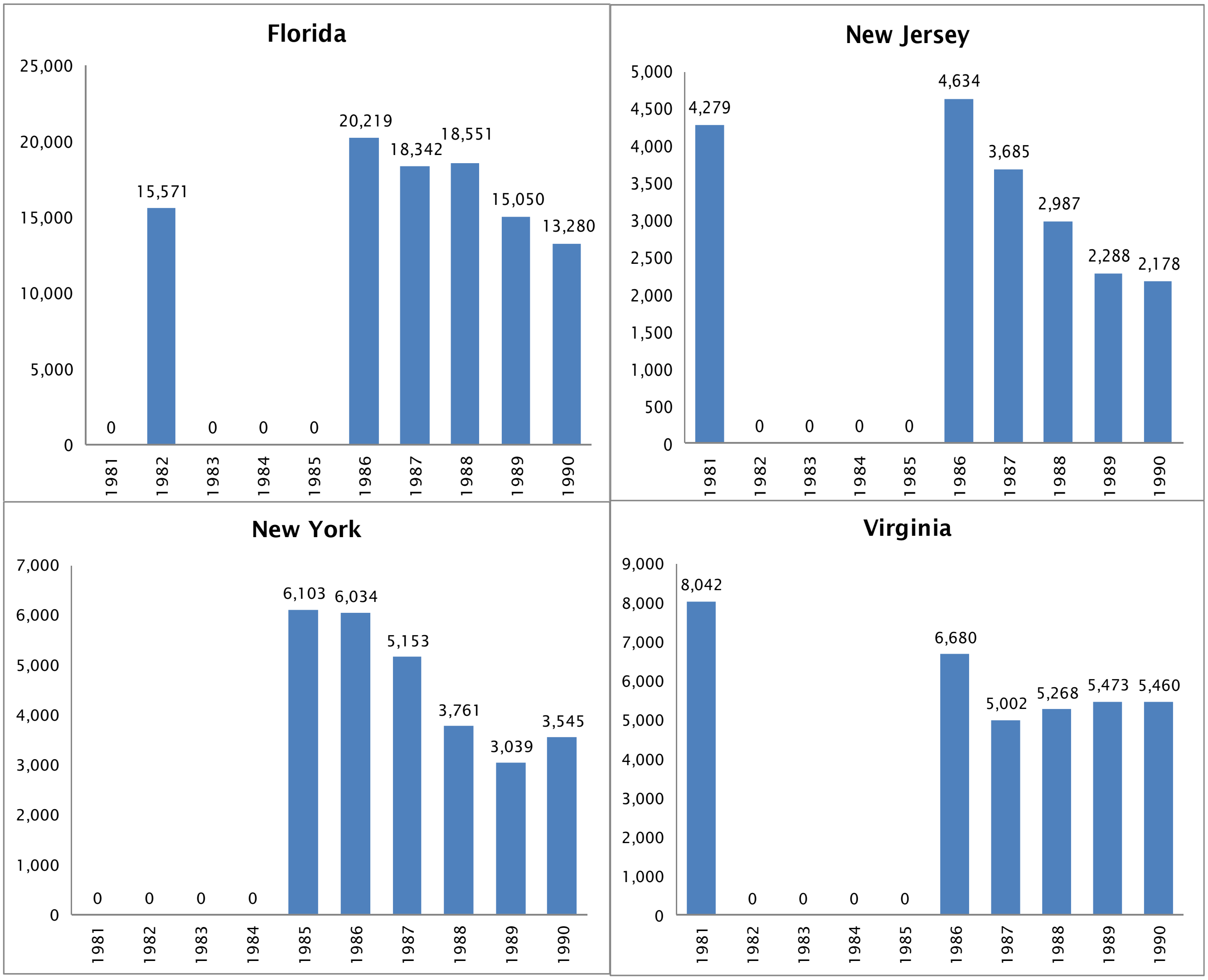
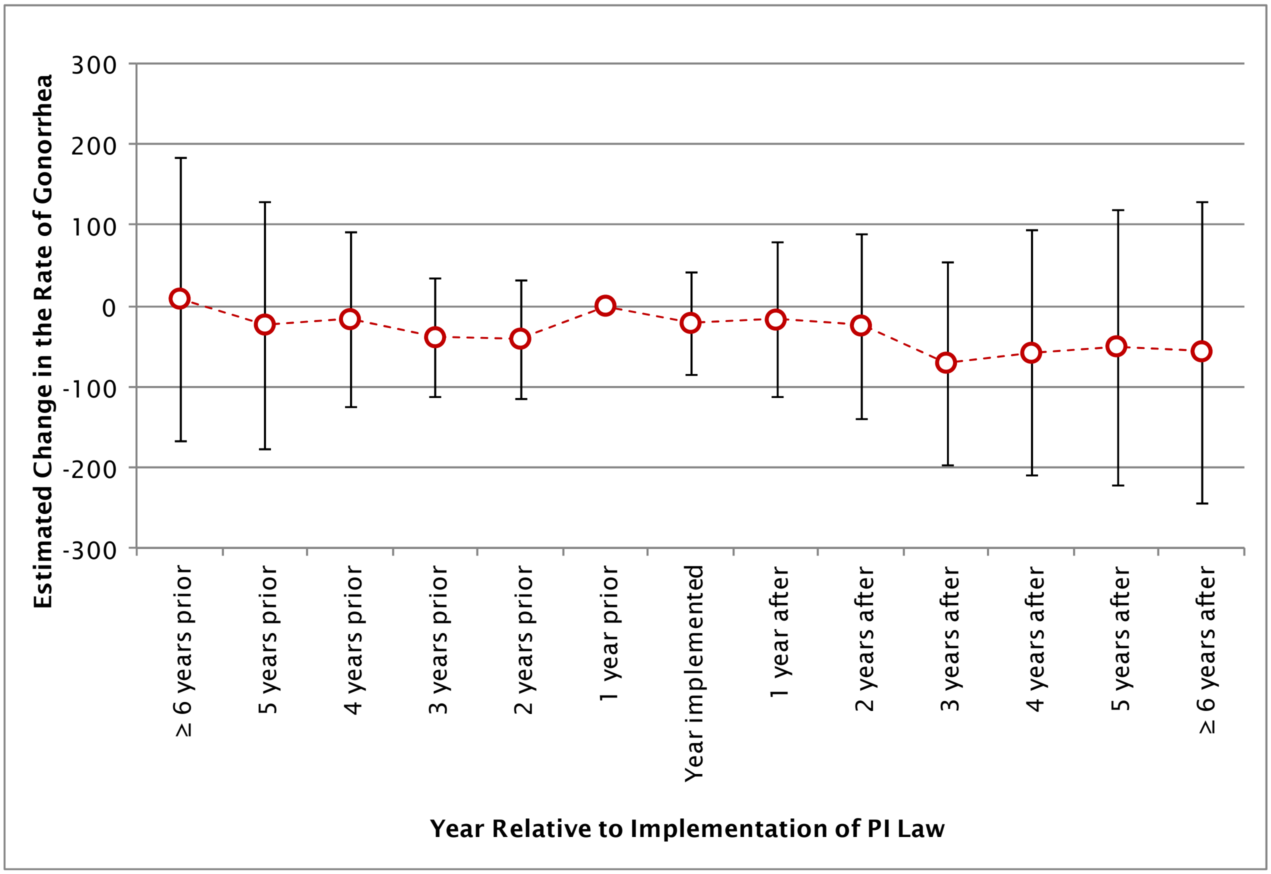
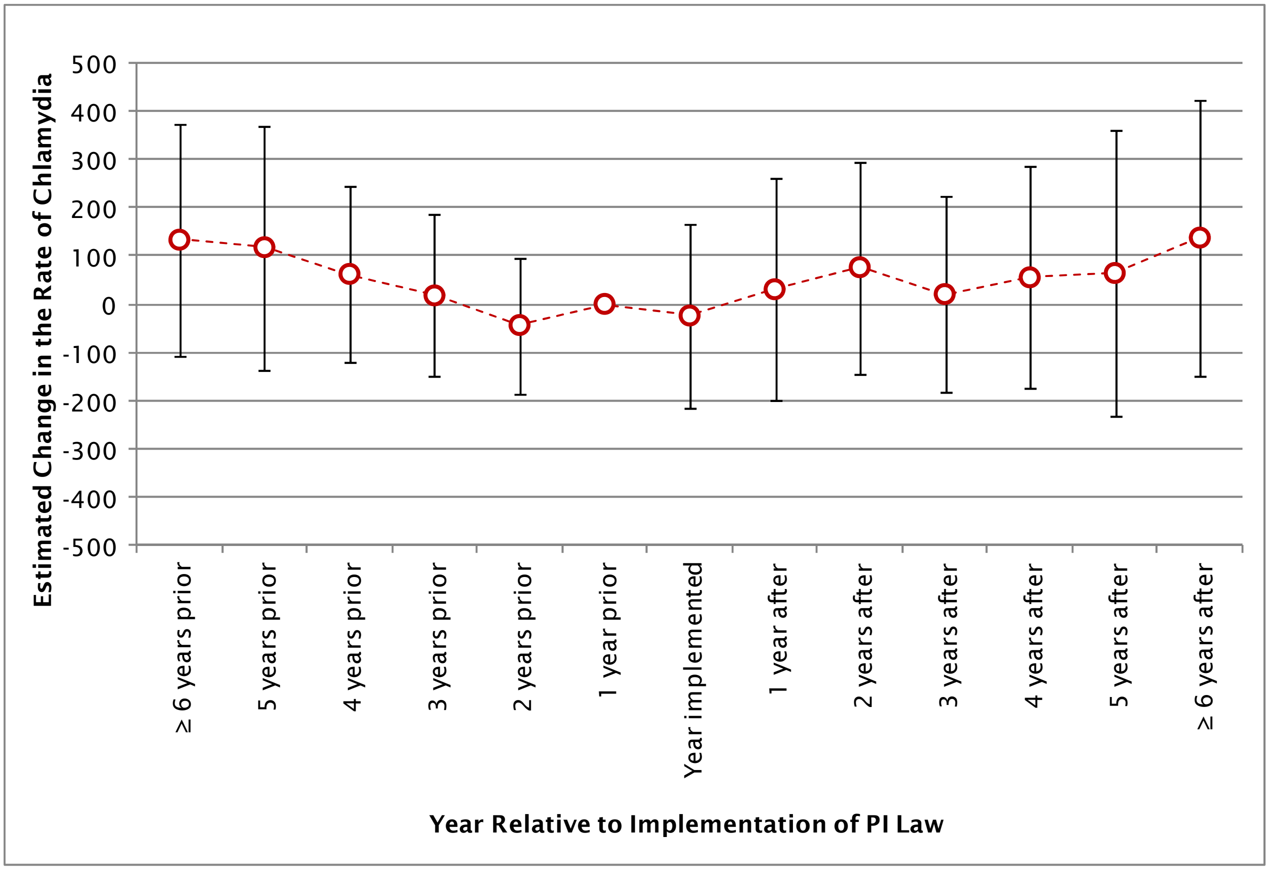


Figure 7. DDD Estimates and 95% Confidence Intervals of the Association Between PI Laws and Rates of Gonorrhea Among Female Teens (15-19) Relative to Young Adults (20-24) each Year pre and post Implementation of a PI Law.



Note: The coefficients shown in the figure are estimates of β_{k_k} from Equation (2). The dependent variable is the age-specific female cases of gonorrhea per 100,000 age-specific female population. Standard errors are adjusted for a general form of heteroskedasticity. All coefficients are from a single regression.

Figure 8. DDD Estimates and 95% Confidence Intervals of the Association Between PI Laws and Rates of Chlamydia Among Female Teens (15-19) Relative to Young Adults (20-24) each Year pre and post Implementation of a PI Law.



Note: The coefficients shown in the figure are estimates of β_{k_k} from Equation (2). The dependent variable is the age-specific female cases of chlamydia per 100,000 age-specific female population. Standard errors are adjusted for a general form of heteroskedasticity. All coefficients are from a single regression.

Table 1. Dates of Implementation of PI Laws, by State

State	Most Recent Law Enforcement Date	State	Most Recent Law Enforcement Date
Alabama	9/23/1987	Montana	enacted but unenforced
Alaska	enacted but unenforced	Nebraska	9/6/1991
Arizona	8/9/2001	Nevada	enacted but unenforced
Arkansas	3/3/2005	New Hampshire	
California	enacted but unenforced	New Jersey	
Colorado	11/2/2003	New Mexico	enacted but unenforced
Connecticut		New York	
Delaware	10/15/1995	North Carolina	10/30/1995
Dist. of Columbia		North Dakota	3/31/1981
Florida	7/1/2005	Ohio	10/1/1990
Georgia	9/16/1991	Oklahoma	5/21/2005
Hawaii		Oregon	
Idaho	3/27/2007	Pennsylvania	4/13/1994
Illinois		Rhode Island	9/1/1982
Indiana	9/16/1984	South Carolina	5/26/1990
Iowa	7/1/1997	South Dakota	7/2/1998
Kansas	7/1/1992	Tennessee	1/15/2000
Kentucky	7/15/1994	Texas	1/1/2000
Louisiana	11/18/1981	Utah	4/4/1974
Maine		Vermont	
Maryland	12/3/1992	Virginia	7/1/1997
Massachusetts	4/15/1981	Washington	
Michigan	3/31/1993	West Virginia	5/22/1984
Minnesota	10/16/1990	Wisconsin	7/1/1992
Mississippi	5/26/1993	Wyoming	6/9/1989
Missouri	8/7/1985		

Sources: Merz et al. (1995), NARAL (2011); Guttmacher Institute (2011).

Table 2. Replication of Model by Klick and Stratmann (2008): Sensitivity to Missing Data, Weighting & State Trends

	1	2	3	4	5	6	7	8	9
	Hispanics		Blacks		Whites		All cases		
	all	**w/o 0's**	**all**	**w/o 0's**	**all**	**17 states**	**17 states**	**all states**	**all states, 1981-1998, Author's coding**
Panel A: Estimates from Models with State-Specific Trends, Weighted									
PI law	-12.048**	-7.116	-40.723	-52.503	-9.541*	5.74	11.601	-14.377	-4.79
	(5.648)	(8.086)	(69.777)	(67.884)	(5.309)	(6.295)	(14.179)	(13.554)	[10.87]
R^2	0.938	0.938	0.947	0.944	0.962	0.957	0.97	0.962	0.96
Panel B: Estimates from Models without State-Specific Trends, Weighted									
PI law	-4.905	-13.46	-36.61	-51.928	-0.816	9.911**	24.336**	0.398	-13.07
	(6.350)	(8.569)	(57.081)	(58.382)	(4.814)	(4.410)	(10.383)	(11.930)	[11.24]
R^2	0.92	0.912	0.938	0.929	0.945	0.942	0.959	0.948	0.94
Sample Size	911	601	912	802	912	286	286	912	1423
Mean Dep. Var.	66.49	73.75	1080	1167	84.12	71.77	158.5	288.6	245.4
Panel C: Estimates from Models with State-Specific Trends, Unweighted									
PI law	-8.499	6.358	-56.475	-88.754	-5.322	-1.079	-5.143	0.472	0.11
	(6.690)	(11.952)	(66.487)	(58.132)	(4.472)	(5.765)	(13.592)	(12.582)	[9.87]
R^2	0.809	0.82	0.927	0.928	0.906	0.945	0.958	0.961	0.95
Panel D: Estimates from Models without State-Specific Trends, Unweighted									
PI law	-0.574	-2.59	-23.678	-64.509	-1.619	9.255	13.812	-3.635	-8.14
	(6.438)	(8.614)	(38.088)	(44.563)	(5.206)	(6.283)	(10.627)	(11.442)	[18.53]
R^2	0.784	0.782	0.917	0.913	0.839	0.92	0.944	0.947	0.93
Sample Size	911	601	912	802	912	286	286	912	1423
Mean Dep. Var.	56.82	86.13	1006.00	1144.00	89.46	64.01	114.60	287.20	242.832

Notes: Estimates in columns (1), (3) and (5) of Panels A & B replicate the results in Klick and Stratmann (2008, Table 2). The dependent variable is the gonorrhea rate among women less than 20, by state and year. The independent variables include an indicator of PI laws, the gonorrhea rate among women ages greater than or equal to 20, and state and year fixed effects. Models 2 and 4 exclude all cells with zero reported cases of gonorrhea. Model 6 includes the 17 states in which at least 85 percent of women less than 20 years of age were white in 1990. Models 7 and 8 include all women and model 9 includes all women and covers the years 1981-2008. The coefficients presented in the table are the coefficients on the indicator of PI laws. The four panels, A-D, show estimates from models weighted (by population of women less than 20 years of age) and unweighted, with and without state-specific trends. The standard errors have been adjusted for clustering at the state level.

*** $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.**

Table 3. DDD Estimates of the Association between PI Laws and Rates of Gonorrhea

	15 to 19 vs. 20 to 24				15 to 17 vs. 18 and 19			
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
PI law	-28.51	-24.46	-33.78	-30.69	23.38	19.35	10.54	-0.45
	(40.21)	(41.40)	(44.88)	(41.43)	(19.75)	(27.56)	(21.01)	(20.52)
R^2	0.96	0.95	0.95	0.96	0.84	0.82	0.87	0.85
Number of states	30	30	51	51	8	8	21	21
Mean dependent variable	920.65	1006.37	794.54	848.82	883.74	872.76	695.07	707.45
Years	1981-2008	1981-2008	1981-2008	1981-2008	1986-2009	1986-2009	1986-2009	1986-2009
Weighted	No	Yes	No	Yes	No	Yes	No	Yes
States with change in law during study period	Yes	Yes	No	No	Yes	Yes	No	No
Sample Size	**1672**	**1672**	**2846**	**2846**	**640**	**640**	**1470**	**1470**

Notes: All estimates are based on a model presented in Equation (1). The dependent variable is age-specific female cases of gonorrhea per 100,000 age-specific female population. The coefficients presented in the table are the coefficients on the PI law indicator (β). Standard errors adjusted clustering at the state level are in parentheses. Models 1, 2, 5, and 6 include only states that enacted a PI law during our study period (between 1982 and 2007 for models 1 and 2, and between 1987 and 2008 for models 5 and 6). Models 3 and 4 include all 50 states and Washington, D.C.; models 7 and 8 include all 21 states for which we have data by single year of age. Estimates from models 1 to 4 are based on a balanced panel of states and years.

Data on rates of gonorrhea are missing for 1983 in Indiana, Georgia, and Idaho, and for 1984 in Tennessee.

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 4. DDD Estimates of the Association between PI Laws and Rates of Chlamydia

	15 to 19 vs. 20 to 24				15 to 17 vs. 18 and 19			
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
PI law	51.92	79.18	-35.81	-46.96	160.51	163.24	-26.1	-18.94
	(99.52)	(72.17)	(82.10)	(70.13)	(226.99)	(177.41)	(106.63)	(101.98)
R^2	0.97	0.98	0.98	0.98	0.69	0.67	0.72	0.69
Number of states	9	9	51	51	7	7	21	21
Mean dependent variable	2230.20	2456.88	2349.35	2454.12	2603.01	2621.51	2444.14	2480.07
Years	1996-2008	1996-2008	1996-2008	1996-2008	1990-2009	1990-2009	1990-2009	1990-2009
Weighted	No	Yes	No	Yes	No	Yes	No	Yes
States with change in law during study period	Yes	Yes	No	No	Yes	Yes	No	No
Sample Size	**234**	**234**	**1326**	**1326**	**530**	**530**	**1430**	**1430**

Notes: All estimates are based on a model presented in Equation (1). The dependent variable is age-specific female cases of chlamydia per 100,000 age-specific female population. The coefficients presented in the table are the coefficients on the PI law indicator (β). Standard errors adjusted clustering at the state level are in parentheses. Models 1, 2, 5, and 6 include only states that enacted a PI law during our study period (between 1997 and 2007 for models 1 and 2, and between 1991 and 2008 for models 5 and 6). Models 3 and 4 include all 50 states and Washington, D.C.; models 7 and 8 include all 21 states for which we have data by single year of age. Estimates from models 1 to 4 are based on a balanced panel of states and years.

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 5. Cross-State DD Estimates of the Association between PI Laws and Rates of STIs Among Teens and Young Adults

	Teens Ages 15 to 19		Young Adults Ages 20 to 24	
	Model 1	Model 2	Model 3	Model 4
Panel A: Rates of Gonorrhea				
PI law	-127.87	-133.64	-94.09	-104.74
	(95.08)	(87.53)	(83.73)	(80.01)
R^2	0.82	0.8	0.78	0.75
Number of states	51	51	51	51
Mean dependent variable	854.92	892.46	734.16	806.66
Years	1981-2008	1981-2008	1981-2008	1981-2008
Weighted	No	Yes	No	Yes
Sample size	**1423**	**1423**	**1423**	**1423**
Panel B: Rates of Chlamydia				
PI law	-157.14**	-157.49*	-121.34	-113.04*
	(73.52)	(80.98)	(73.22)	(58.59)
R^2	0.86	0.84	0.86	0.88
Number of states	51	51	51	51
Mean dependent variable	2453.33	2545.38	2245.37	2360.23
Years	1996-2008	1996-2008	1996-2008	1996-2008
Weighted	No	Yes	No	Yes
Sample Size	**663**	**663**	**663**	**663**

Notes: The dependent variable in Panel A is age-specific female cases of gonorrhea per 100,000, and the dependent variable in Panel B age-specific female cases of chlamydia per 100,000 age-specific female population. In both panels, the dependent variables include an indicator of PI laws that equals one for the post-law years in states that enacted a law during our study period, and state and year fixed effects. The coefficients presented in the table are the coefficients on the PI law indicator. Standard errors adjusted for clustering at the state level are in parentheses.

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 6. Association between PI Laws and Sexual Activity among High School Students ages 15-17; YRBS 1991-2009

	Ever have sex		Unprotected sex		Sex last 3 months		At risk for STDs		Pill Used	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PI Law	0.045*	0.017	0.016	0.008	0.032	0.009	0.007	-0.003	0.005	0.001
	(0.021)	(0.018)	(0.012)	(0.009)	(0.02)	(0.016)	(0.017)	(0.013)	(0.007)	(0.007)
R^2	0.101	0.098	0.024	0.025	0.071	0.072	0.035	0.04	0.020	0.027
Number of states	14	47	14	47	14	47	14	47	14	47
Mean dependent variable	0.445	0.445	0.088	0.086	0.316	0.317	0.124	0.128	0.050	0.056
Sample size	75,693	110,937	73,451	107,611	75,537	110,606	74,692	109,418	73451	107611

Notes: Coefficients are from a linear probably model. Standard errors are adjusted for the sampling design with Stata's survey regression procedure and corrected for clustering at the state level. There are two samples for each outcome. The first includes the 14 states with at least 8 years of data and the second includes all states with data. Each model includes indicators for age, race/ethnicity, state and year.

* $p < 0.05$; ** $p < 0.01$.