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THE EFFECTS OF STATE-MANDATED ABSTINENCE-BASED SEX EDUCATION ON TEEN HEALTH OUTCOMES

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SUMMARY

In 2011, the USA had the second highest teen birth rate of any developed nation, according to the World Bank. In an effort to lower teen pregnancy rates, several states have enacted policies requiring abstinence-based sex education. In this study, we utilize a difference-in-differences research design to analyze the causal effects of state-level sex education policies from 2000–2011 on various teen sexual health outcomes. We find that state-level abstinence education mandates have no effect on teen birth rates or abortion rates, although we find that state-level policies may affect teen sexually transmitted disease rates in some states.

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1. INTRODUCTION

Despite the resources spent on lowering teen birth rates, nearly 330,000 US teenagers became mothers in 2011, placing the USA near the top of developed countries worldwide. Teen childbearing can be costly to individuals and society, and many state governments invest in preventative measures to curb future costs.[1,2] One such investment, sex education, has become more prevalent in an effort to combat the high rates of teen pregnancy and sexually transmitted diseases (STDs). However, there has been a substantial debate over the content of sex education classes.

In the past two decades, sex education has moved away from more comprehensive programs in favor of abstinence-based curricula that stress the importance of monogamous sexual relationships with a spouse (Lindberg *et al.*, 2006; Perrin and DeJoy, 2003). Comprehensive programs, in contrast, cover a more broad range of prevention tactics and include education about contraception. The potential consequences of abstinence education are unclear. Advocates of abstinence education argue that these programs discourage teenage sexual frequency and onset by increasing the perceived cost of having sex, leading to a lower incidence of teen pregnancy and STDs. Critics of abstinence-based sex education argue that teens' decisions to engage in sexual intercourse are independent of school curriculum, and the lack of information about contraception encourages risky sexual behavior, which could lead to higher rates of pregnancy and STDs.

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[1]According to the National Campaign to Prevent Teen and Unplanned Pregnancy, the national cost of teen childbearing, including costs for welfare, public sector health care costs, and lost tax revenues, topped $10.9bn in 2008.

[2]There is some evidence that teenage mothers tend to be worse off in terms of educational attainment, lifetime wages, and health (Geronimus and Korenman, 1992; Bronars and Grogger, 1994; Hoffman *et al.*, 1993; Holmlund, 2005; Lee, 2010). Furthermore, delaying childbearing can have positive effects on a woman's firstborn child's educational achievement (Miller, 2009) and increases in teen parenting cause later increases in crime rates (Hunt, 2006).

While there is a large literature examining the effectiveness of sex education in general, there is less evidence on the effects of content requirements. Most studies focus on school or district-level interventions and analyze survey data about teen sexual behavior. Many of these studies find that neither abstinence-based programs nor comprehensive programs are significantly correlated with teen intercourse, although comprehensive programs are associated with decreased risky sexual behavior (Kohler *et al.*, 2008; Lindberg and Maddow-Zimet, 2012; Sabia, 2006; Trenholm *et al.*, 2008).[3]

Even though most sex education content requirements are mandated on the state level, few studies have analyzed the relationship between state-level policies and teen health outcomes. Because the states with such mandates are inherently different from those without them, causal inference is challenging in this setting. Previous studies have shown that state-level abstinence mandates are correlated with higher STD rates (Hogben *et al.*, 2010) and higher teen birth rates (Stanger-Hall and Hall, 2011). However, no studies to our knowledge have estimated a causal link between state-mandated sex education curriculum and teen health outcomes.

Although the causal effects of abstinence mandates have remained unstudied, recently a number of studies have looked at the effects of similar state-level policies. Kearney and Levine (2012) control for state-level sex education content requirements and sex education funding within a model aimed at determining demographic trends in childbearing and find no effects on teen birth rates. Cannonier (2012) uses a difference-in-differences methodology and finds that Title V abstinence-based funding only significantly decreases birth rates for White 15–17-year olds and does not effect other race or age groups. While Title V funding aims to create incentives for states to emphasize abstinence in sex education, changes to funding may be less likely to affect teen health outcomes than required curriculum changes.

This paper fills a void in the literature by examining the causal effect of state-mandated abstinence education on teen pregnancy and STD rates. To do so, we use a difference-in-differences research design to determine whether states that adopt abstinence-based sex education mandates experience changes in teen birth rates, STD rates, or abortion rates relative to other states over the same time period. The identifying assumption is that absent the sex education mandate, adopting and non-adopting states would have experienced similar changes in teen health outcomes.

Several exercises lend support to this identifying assumption. First, we provide graphical and statistical evidence that the trends for the two groups were not diverging prior to the enactment of the sex education policy. Moreover, we show that the inclusion of important time-varying covariates does not affect our estimates. This suggests that the within-state variation we are exploiting for identification is orthogonal to observable determinants of health outcomes, which gives us some comfort that our estimates might also be unaffected by unobserved variables (Altonji *et al.*, 2005). Finally, we perform placebo tests showing that changes in abstinence mandates do not affect birth rates for women who graduated from high school prior to the policy change. This provides further support for the assumption that teen health outcomes would have changed similarly across adopting and non-adopting states, absent the change in policy.

Our results generally indicate that state-level abstinence mandates have no effect on teen birth rates, STD rates, or teen abortion rates. Importantly, this result holds even for the youngest group, 15–17-year olds, who were less likely than 18–19-year olds to be either sexually active or exposed to sex education before the adoption of the policy. We present some evidence, though, that abstinence mandates may increase teen sexually transmitted disease rates in states that had no policies in effect prior to mandating abstinence curricula.

The primary contributions of our paper are twofold. First, we are the first to our knowledge to use a quasi-experimental research design to estimate the impact of abstinence-based sex education on teen birth rates, STD rates, and abortion rates using state-level data representing broad populations of interest. Second, our

[3]For an extensive review of randomized controlled experiments on this topic, see Bennett and Assefi (2005). They report that some programs that emphasized abstinence but also taught contraception decreased sexual frequency, although several studies found no effects on teen sexual behavior.

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study speaks directly to the effectiveness of an important policy parameter—state mandates—and, in doing so, informs the policy debate as to the consequences of these laws. Indeed, our study suggests that while the political and financial costs of abstinence policies are quite high, teens do not appear to be reaping any benefits with respect to sexual health.[4]

2. STATE-LEVEL SEX EDUCATION POLICIES

About 90% of schools taught some form of sex education from 2006–2008, and 96% of teens reported having some sort of formal sex education before they turned 18 (Martinez *et al.*, 2010). The existence of formal sex education in schools is overwhelmingly supported by parents (Ito *et al.*, 2006; Santelli *et al.*, 2006) and is linked to less risky teen sexual behavior, decreases in teen births (Cavazos-Rehg *et al.*, 2012), and increases in the use of contraception (Mueller *et al.*, 2008; Kirby, 2007).[5]

Over the past two decades, state policies have propelled the shift to more abstinence-based sex education. In 1996, as part of the Welfare Reform Act, the federal government increased abstinence funding for states by $50m per year with the creation of the Title V abstinence-only-until-marriage program.[6] The inception of abstinence-based funding created incentives for the enactment of state-level abstinence-based sex education mandates. Since the onset of Title V, such mandates have been upheld in over 20 states and have been adopted in six states (Alan Guttmacher Institute).

Importantly, there is a significant body of research demonstrating that superintendents and teachers follow state-level sex education mandates, which suggests that these policies are affecting the sex education content that students receive in the classroom (e.g., Landry *et al.*, 1999; Darroch *et al.*, 2000; Gold and Nash, 2001; Forrest and Silverman, 1989; Muraskin, 1986; Moore and Rienzo, 2000; Sonfield and Gold, 2001).[7] Thus, there is every reason to believe that state-level mandates regarding changes in sex education content could have effects on teen health outcomes.

3. DATA

We use state-level policy data on mandated sexual education curriculum from monthly reports from the Alan Guttmacher Institute (AGI). Since 2000, the Alan Guttmacher Institute has assessed the language of every state-level sexual education mandate to determine whether the state requires educators to “stress” or “cover” abstinence and/or contraception. We adopt this terminology throughout this paper for consistency.

We consider a state to be treated if a stress-abstinence policy was enacted during 2000–2011. Five states meet these criteria and serve as the treatment group. These states include Maine, Michigan, Washington, Wisconsin,

[4]For example, in 2011 alone, nearly $200m was given to states for sex education programs (SIECUS, 2013).

[5]See Kirby (2002) and Kirby (2008) for an excellent and thorough review of this literature. Generally, researchers find that sex education does not hasten the onset of teen sexual intercourse nor does it increase the number of sexual partners or frequency.

[6]Title V funds are tied to an eight-point policy, which strictly defines “abstinence education.” Section 510 (b) of Title V of the Social Security Act, P.L. 104-193 defines abstinence education as follows: Abstinence education has as its exclusive purpose teaching the social, psychological, and health gains to be realized by abstaining from sexual activity, teaches abstinence from sexual activity outside marriage as the expected standard for all school-age children, teaches that abstinence from sexual activity is the only certain way to avoid out-of wedlock pregnancy, sexually transmitted diseases, and other associated health problems, teaches that a mutually faithful monogamous relationship in the context of marriage is the expected standard of sexual activity, teaches that sexual activity outside of the context of marriage is likely to have harmful psychological and physical effects, teaches that bearing children out-of-wedlock is likely to have harmful consequences for the child, the child’s parents, and society, teaches young people how to reject sexual advances and how alcohol and drug use increase vulnerability to sexual advances, and teaches the importance of attaining self-sufficiency before engaging in sexual activity. All programs that receive these funds are obliged to teach what is specified in these points (SIECUS).

[7]For example, in 1997, a school board in North Carolina ordered that chapters containing information on contraception and sexually transmitted diseases be deleted from its ninth grade textbooks in order to comply with a new state law that required educators to stress abstinence (Donovan, 1998).

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Table I. Policy summary of states that enacted a stress-abstinence mandate from 2000–2011

State	Effective year	Stress-abstinence	Cover contraception	Sex/STD Ed. mandated	Previous policy
Maine	2002	Yes	Yes	Yes	No content requirements
Michigan	2005	Yes	No	Yes	Comprehensive
Wisconsin	2006	Yes	No	Yes	No content requirements
Washington	2006	Yes	Yes	Yes	Comprehensive
Colorado	2007	Yes	Yes	No	No content requirements

Policy language of states that enacted a stress-abstinence mandate from 2000–2011

State	Effective year	Stress-abstinence policy language
Maine	2002	Sex education “promotes responsible sexual behavior with an **emphasis on abstinence**; addresses the use of contraception; promotes individual responsibility and involvement regarding sexuality; and teaches skills for responsible decision making.”
Michigan	2005	Teachers must “**stress** that **abstinence** from sex is a responsible and effective method for restriction and prevention of these diseases and is a positive lifestyle for unmarried young people... Instruction must stress the benefits of abstinence but districts are not prohibited from teaching about behavioral risk reduction strategies, including the use of condoms, within a sex education program.”
Wisconsin	2006	“Presents abstinence from sexual activity as the preferred choice of behavior for unmarried pupils. **Emphasizes** that **abstinence** from sexual activity before marriage is the only reliable way to prevent pregnancy and sexually transmitted diseases, including human immunodeficiency virus and acquired immunodeficiency syndrome.”
Washington	2006	“All sexual health education programs must include an **emphasis on abstinence** as the only one hundred percent effective means of preventing unintended pregnancy, HIV and other sexually transmitted diseases.”
Colorado	2007	“The curriculum must fulfill the following requirements: ...(2) **Emphasize abstinence** (as defined in the statute) and teach that it is the only certain way to avoid pregnancy...”

Notes: Alan Guttmacher Institute provided data on state-level sex education policies from 2000–2011. Comprehensive policies require that schools teach both abstinence and contraception as methods to prevent pregnancy. Wisconsin eventually added a cover contraception policy in 2010, 4 years after the stress-abstinence policy went into effect. Data on state policy language were provided by the National Association of States Boards of Education Center for Safe and Healthy Schools. STD Ed., sexually transmitted disease education.

and Colorado.[8] The five adopting states mandate that school districts emphasize monogamous sexual relations with a spouse as the most effective way to prevent unintended pregnancy and STDs, although some variation in the requirements of these policies exists across states.[9] See Table I for more details on the content requirements and policy language of each treatment state's stress-abstinence policy.

We use the 21 states that maintained comprehensive sex education policies throughout 2000–2011 as the control group.[10] States are considered to have a comprehensive policy if abstinence is covered but not stressed or if contraception education is mandated either in STD or sex education. We use this subset of states as a

[8]New Jersey added a stress-abstinence policy in 2002 but switched back to a comprehensive program in 2006. Therefore, it is dropped from all further analysis. When included as a treatment state, we find a statistically insignificant effect of −0.3 percent, which corresponds to the estimate of 1.2% reported in Panel A Column 5 of Table III. These estimates are not statistically different at the 99% level.

[9]Colorado does not mandate schools to teach sex education, but does require educators to stress abstinence when it is taught. Nearly 80% of Colorado high schools in 2008 taught pregnancy prevention and the benefits of being sexually abstinent in a required health class (Brener *et al.*, 2009), which suggests that many schools choose to teach sex education even when it is not mandated.

[10]These states are Alabama, California, Connecticut, Delaware, Florida, Georgia, Hawaii, Illinois, Kentucky, Maryland, Missouri, New Mexico, New York, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, Vermont, Virginia, and West Virginia.

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control group because it improves the match on trends prior to the enactment of the sex education policy for our treatment states.[11,12]

The data source for birth data for various age groups during the sample period is the National Center for Health Statistics, Division of Vital Statistics Natality Files.[13] One advantage of using the natality files is that the administrative nature of these data allows for more reliable estimates than self-reported behavioral data. Additionally, we use state-level teen STD data, composed of the total yearly number of gonorrhea, chlamydia, and syphilis cases for men and women, from the online, publicly-available Centers for Disease Control and Prevention (CDC) Atlas. Rates for each health measure were calculated using the number of cases per 1000 relevant individuals.[14] It is important to note that these STD data account for only reported STDs. If STD testing rates are affected by the change in sex education requirements, then the results may not speak to the underlying change in STD rates directly. If abstinence-based sex education reduces testing by increasing the stigma surrounding such diseases, this would attenuate our results towards zero.

We use data on abortion rates by age from yearly state-level estimates reported by the CDC. The CDC is the only source to publish annual estimates on abortion rates by state and age group. Unfortunately, these data are currently available only up to 2009. Because centers are not required by law to annually submit abortion data, there are some inconsistencies within these estimates. Therefore, we omit 11 states that either had missing observations during the sample period or experienced unusual spikes or declines of over 30% in teen abortion rates.[15,16] We attribute these large fluctuations to errors in reporting and deliberate omissions by multiple states, which is a well-documented occurrence in these data (Blank *et al.*, 1996).

To control for the effects of economic factors and race, we use annual data from the Census Bureau Current Population Survey and the Bureau of Labor Statistics on median family income and unemployment rates by state, respectively. Population data from the Census Bureau provides gender-specific estimates for the number of White, Black, and Hispanic teens in each state.

In order to directly control for changes in abortion legislation or policies that might be correlated with abstinence-based education, we use annual report card data published by NARAL Pro-Choice America. These data contain state-level rankings and standard letter grades based on multiple variables that serve as proxies for a woman's legal ability to seek out an abortion. "A+" states are those with the most relaxed abortion laws, while "F" states have the most restrictive abortion laws. We use these data to construct yearly "abortion grade" dummy variables for each state to serve as a measure of abortion access.[17]

Altogether, we construct a state level, 12-year panel spanning from 2000–2011. Summary statistics are presented in Table II. Birth rates across treatment and control states average 39 per 1000 female teenagers over the sample period, with the older cohort, teens aged 18–19, responsible for over half. STD rates

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[11]This approach is similar in spirit to synthetic control methods (Abadie and Gardeazabal, 2003) in which a more similar group of non-treated states (based on pre-treatment data) is selected as the control group for a single treated state in order to reduce the potential for bias.

[12]See Tables A1-A3 in the appendix for a complete replication of our main findings using all non-treated states as controls. Note that some of the coefficients for the leading indicator variables in Column 9 in Table A1 and Columns 6 and 9 in Table A2 are statistically different from zero, suggesting that trends for control and treatment groups are diverging prior to treatment. We can therefore assume that the model using all states is misspecified and is inappropriate for any analysis which utilizes a difference-in-differences methodology.

[13]Birth data aggregated by state is publicly available via the online CDC Wonder database.

[14]In particular, for the teen birth rate, we calculate the number of births to female teenagers aged 15–19, multiply by 1000 and divide by the state population of female teenagers aged 15–19. To calculate STD rates, we used the entire teen population aged 15–19.

[15]The states that were dropped for abortion models include California, Colorado, Delaware, Florida, Illinois, Kentucky, Maryland, New York, Rhode Island, Vermont, and West Virginia.

[16]Our results are not sensitive to this omission. When including these states in our model, we estimate a statistically insignificant effect of −0.9%, which corresponds to the our estimate of −2.7% in Column 5 of Table VI. These estimates are statistically similar at the 99% level.

[17]Because of the wide variation of possible grades, we eliminate the plus or minus signs for simplicity. For example, we consider a state that received a grade of "A-", "A", or "A+"to be an "A" state.

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Table II. Summary Statistics

	Mean	St. Dev.
Outcome Variables		
Births per 1000 female teenagers aged 15–19	38.8	11.4
Births per 1000 female teenagers aged 15–17	20.2	6.8
Births per 1000 female teenagers aged 18–19	66.6	18.9
Births per 1000 female teenagers aged 30–34	92.5	11.2
STDs per 1000 teenagers aged 15–19	20.6	7.3
Abortions Rate per 1000 female teenagers aged 15–19	14.9	7.1
Births per 1000 Black female teenagers aged 15–19	52.7	17.6
Births per 1000 White female teenagers aged 15–19	23.7	11.1
Births per 1000 Hispanic female teenagers aged 15–19	92.8	45.0
Control Variables		
Median family income	47,227	7575
Teen unemployment rate	18.3	5.5
Percent Black teens	0.15	0.11
Percent White teens	0.72	0.15
Percent Hispanic teens	0.13	0.14
NARAL grade A	0.35	0.48
NARAL grade B	0.10	0.30
NARAL grade C	0.08	0.27
NARAL grade D	0.19	0.39
NARAL grade F	0.30	0.46

Notes: We use birth data for various age groups during the sample period from the National Center for Health Statistics, Division of Vital Statistics natality files via the online Centers for Disease Control and Prevention (CDC) Wonder database. State-level teen STD data, composed of the total number of gonorrhea, chlamydia, and syphilis cases per year, are from the online, publicly available CDC Atlas. We use abortion data by age from yearly state-level estimates reported by the CDC Abortion Surveillance System. NARAL Pro-Choice America report card data contain proxies for legal abortion access. "A" states are those with the most relaxed abortion laws, while "Fs" are given to the states with the most restrictive abortion laws. STD, sexually transmitted disease; St. Dev., standard deviation.

across states average approximately 21 per 1000 teens, with a standard deviation of 7.3. Teen abortions average 15 per 1000 female teenagers, although, as previously mentioned, this is expected to be underestimated (Blank *et al.*, 1996).

4. METHODS AND IDENTIFICATION

4.1. Difference-in-differences model

In order to identify the causal effect of state-mandated sex education policies, we exploit the within-state variation in the adoption of stress-abstinence laws. The main identifying assumption of our difference-in-differences approach is that outcomes in adopting states would have changed in a way similar to control states if they had not changed their law. We compare states that added a stress-abstinence policy from 2000–2011 to states whose sex education policies are most similar to those of the treatment states before the change in policy, as explained in Section 3, and we test this identification assumption multiple ways (which we describe in Section 4.2). Formally, we estimate the following equation:

$$y_{it} = \beta \; abstinence\ policy_{it} + \gamma X_{it} + \alpha_i + \lambda_t + u_{it} \tag{1}$$

where y_{it} measures teen health outcomes such as logged birth rates, logged abortion rates, and logged STD rates, $abstinence\ policy_{it}$ is a dummy variable equal to one when a treated state i has an abstinence-based sex education policy in year t, and X_{it} is a vector of control variables including teen racial demographics, a measure of abortion access, and state-level economic variables including median family income and

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unemployment rates.[18] State and year fixed effects, α_i and λ_t, are added to control for time-invariant, state-level confounders, and time-varying shocks to teen health outcomes that are constant across states, respectively. Robust standard errors are clustered at the state level to allow for shocks to be correlated within states over time.[19]

We estimate effects of stress-abstinence policies on teen health outcomes using unweighted ordinary least squares (OLS), weighted least squares (WLS), and Poisson models.[20] There is large variation in female teen population across states, and using weighted least squares increases precision and allows us to observe heterogeneous treatment effects. Moreover, to check that our results are not sensitive to one particular specification, we report results from a fixed effects Poisson model to account for the count nature of birth data.

Additionally, we estimate the following model, which contains leading and lagged indicator variables:

$$\begin{aligned} y_{it} = {} & \beta_0\, policy\ enacted_{it} + \beta_1\, policy\ enacted_{it-1} + \ldots + \beta_4\, policy\ enacted_{it-4+} \\ & + \delta_1\, policy\ enacted_{it+1} + \delta_2\, policy\ enacted_{it+2} + \gamma\, X_{it} + \alpha_i + \lambda_t + u_{it} \end{aligned} \tag{2}$$

where y_{it} measures teen health outcomes such as logged birth rates, logged abortion rates, and logged STD rates; $policy\ enacted_{it-k}$ is a dummy variable equal to one when a treated state i enacted an abstinence-based sex education policy in year $t-k$. For positive values of k, the regressor is a lagged treatment effect, and for negative values of k, it is a leading indicator. In the year of enactment, k is zero, and β_0 is the immediate effect of treatment.

This policy may have a delayed effect if, for example, an effect was only present for teens who had never received sex education instruction before the policy. We include lagged indicator variables for each of the first 4 years after enactment. Some states treated later do not have more than 4 years after enactment in the panel because of their enactment year. For this reason, we create a lag denoting that enactment occurred four or more years before (the 4+ years lag).

4.2. Identification

We estimate the leading indicators in Eq. (2) to formally test for divergence of the treatment and control groups before the treatment actually occurred. If the coefficients on the leads were not zero, it would suggest that the control and treatment groups were not on the same trajectory before treatment, which would lead us to question our identification assumption. Additionally, we plot the coefficients for the leading indicators to graphically demonstrate that adopting states closely tracked control states prior to the policy change. Furthermore, we check whether the difference-in-differences estimates change significantly with the addition of control variables. Intuitively, we ask whether observable time-varying factors appear to be correlated with the within-state policy adoption. To the extent that estimates are unaffected by the inclusion of observable factors such as access to abortion, unemployment rates, and median family income, it gives us some comfort that estimates will not be subject to omitted variable bias.

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[18] Our identifying assumption for a model with logged outcomes is that in the absence of the stress-abstinence policy, treatment states would have had a similar proportionate change in birth rates, STD rates, and abortion rates as compared with control states. Because we are using state-level data, we are more comfortable with the assumption of an increase in relative rates rather than absolute rates. This makes a practical difference because, for example, a 10% decline in the teen birth rate in Maine represents an absolute decrease of about 2.5 births per 1000 female teenagers, while a 10% decline in Arizona represents a decrease of about 7 births per 1,000 female teenagers. Our results are not sensitive to this assumption. When estimating effects on birth rates, we obtain a statistically insignificant effect of 0.74, or 1.9%, which corresponds to the 1.2% effect of logged birth rates reported in Table III Panel A Column 5. These estimates are not statistically different at the 99% level. See Tables A4–A6 for a complete replication of the main estimates without logging the dependent variables.

[19] Our conclusions remain unchanged when estimating models using a wild bootstrap-t method to account for our relatively small number of clusters. For example, the p-value for Table III Panel A Column 5 when clustering is 0.62, compared with a p-value of 0.60 when using the wild bootstrap method.

[20] Specifically, we use analytic weights where the weight for teen birth rates and teen abortion rates is the average state teen female population from 2000–2011, and the weight for teen STD rates is the average state teen population from 2000–2011.

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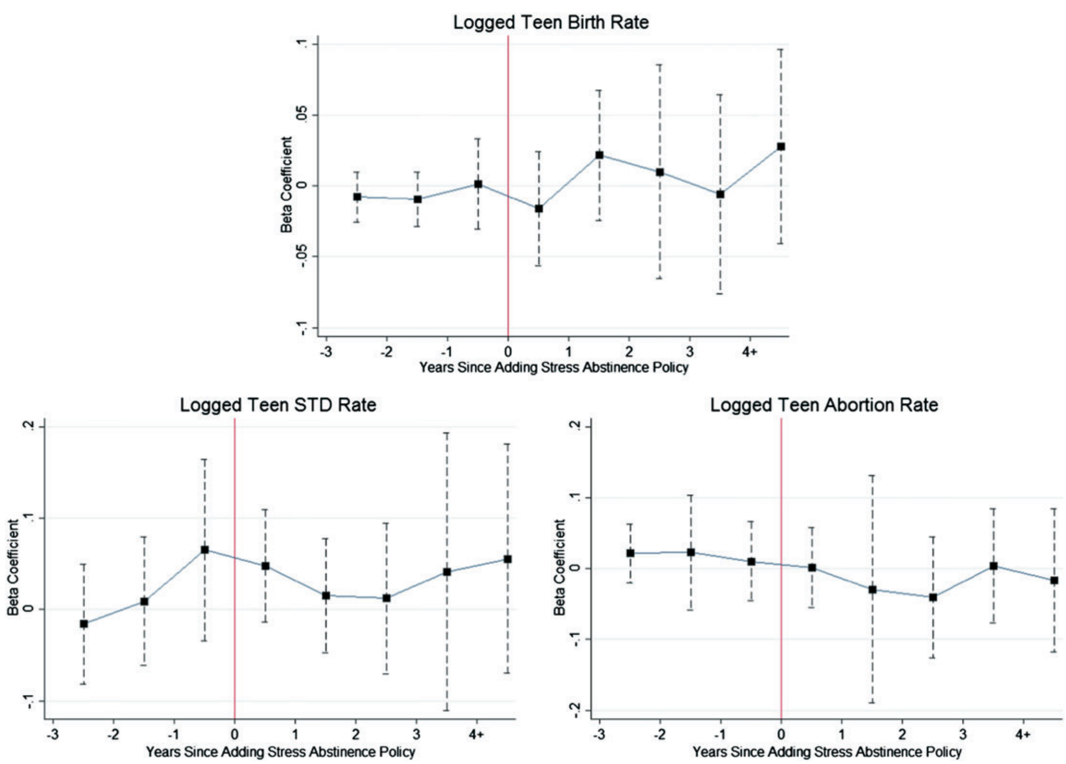
5. RESULTS

Before presenting model-based estimates, we first show a graphical analysis that corresponds to our difference-in-differences identification strategy. Figure 1 graphs the estimated lags and leads from Eq. (2) to test for the divergence in trends between treatment and control groups prior to the abstinence mandate. Figure 1a corresponds to the weighted least squares model for logged teen birth rates which includes state and year fixed effects as well as economic and demographic controls. The coefficients for the leads (the points to the left of the vertical line) are all close to zero, which indicates that the treatment and control groups were not diverging prior to treatment. Figure 1b and 1c similarly graph the estimates over time for logged teen sexually transmitted disease rates and abortion rates, respectively, and shows that the trends in health outcomes for the treatment states similarly track trends in health outcomes in the comparison states prior to the policy change, lending some support to our identification assumption. Finally, all graphs show an estimated zero effect on teen health outcomes after the adoption of stress-abstinence sex education mandates, which we further investigate in the discussion of results below.



Notes: The figure displays the coefficients and their 95% confidence intervals for the leading indicators and lagged treatment effects from weighted least squares regressions, accounting for state and year fixed effects and covariates. Full results from these regressions are shown in Panel B Column 6 in Tables 3, 4, and 6. The control variables include percent of female teenagers ages 15–19 who are Black, percent of female teenagers ages 15–19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates, and median family income. STD, sexually transmitted disease.

Figure 1. Divergence in teen health outcomes before and after adoption of a stress-abstinence policy, relative to the difference three or more years before adoption. [Colour figure can be viewed at wileyonlinelibrary.com]

Table III. Effect of stress-abstinence policy on logged teen birth rates

	(1) OLS	(2) OLS	(3) OLS	(4) WLS	(5) WLS	(6) WLS	(7) Poisson	(8) Poisson	(9) Poisson
Panel A. average effects									
Abstinence mandate in effect	−0.006	−0.002	−0.005	0.016	0.012	0.012	0.006	0.002	−0.000
	(0.026)	(0.026)	(0.031)	(0.021)	(0.025)	(0.026)	(0.020)	(0.023)	(0.024)
1 year prior to enactment			−0.007			0.004			−0.000
			(0.018)			(0.016)			(0.015)
2 years prior to enactment			−0.008			−0.007			−0.010
			(0.015)			(0.008)			(0.008)
Panel B. dynamic effects									
Effect of policy in year of enactment	−0.028*	−0.023	−0.027	−0.016	−0.013	−0.014	−0.022	−0.023	−0.025
	(0.016)	(0.018)	(0.024)	(0.016)	(0.017)	(0.018)	(0.014)	(0.016)	(0.017)
1 year after enactment	0.002	0.019	0.015	0.017	0.024	0.024	0.009	0.014	0.012
	(0.021)	(0.017)	(0.020)	(0.022)	(0.021)	(0.021)	(0.020)	(0.019)	(0.019)
2 years after enactment	−0.016	−0.011	−0.015	0.011	0.013	0.012	−0.000	0.001	−0.001
	(0.031)	(0.032)	(0.036)	(0.032)	(0.035)	(0.036)	(0.031)	(0.033)	(0.033)
3 years after enactment	−0.020	−0.017	−0.020	0.002	−0.003	−0.004	−0.008	−0.015	−0.018
	(0.031)	(0.034)	(0.037)	(0.027)	(0.032)	(0.033)	(0.026)	(0.030)	(0.031)
4+ years after enactment	0.016	0.012	0.009	0.041	0.030	0.030	0.034	0.023	0.021
	(0.033)	(0.037)	(0.041)	(0.027)	(0.030)	(0.033)	(0.028)	(0.030)	(0.032)
1 year prior to enactment			−0.009			0.004			−0.001
			(0.019)			(0.015)			(0.015)
2 years prior to enactment			−0.008			−0.007			−0.011
			(0.016)			(0.008)			(0.009)
State-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Notes: *p<0.1, **p<0.05, ***p<0.01. Estimates are based on annual data for 26 states from 2000–2011. Each column by panel represents coefficients from a separate regression. Robust standard errors are clustered at the state level and are shown in parentheses. The control variables include percent of female teenagers ages 15–19 who are Black, percent of female teenagers ages 15–19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates, and median family income. OLS, ordinary least squares, WLS, weighted least squares.

5.1. Effects on teen birth rates

Table III presents estimates of the effect of a stress-abstinence mandate on logged teen birth rates based on ordinary least squares, weighted least squares, and Poisson models. Panel A presents the average treatment effect from a difference-in-differences model as described by Eq. (1), while Panel B presents estimates from models that include lagged and leading indicator variables as described by Eq. (2). Column 1 displays estimated effects from a baseline OLS model, while Column 2 shows the estimated effects from an OLS model that adds controls for state-level race and economic variables, as well as a measure of abortion access. We note that estimates change little when we include time-varying controls, which suggests there may be little scope for omitted, unobserved factors to bias our estimates. Estimates from these two columns indicate that the policy change had no effect on teen birth rates. Column 3 includes a specification that additionally controls for 1- and 2-year leading indicator variables, which serves as an additional check on our identifying assumption. These coefficients are all statistically insignificant and close to zero, and their addition does not cause the coefficient of interest to change significantly, suggesting that the identification assumption is likely valid.

Columns 4–6 repeat this exercise for a weighted least squares model. These estimates address the possibility that estimates from the baseline OLS model may be imprecise if the variance of the error terms are proportional to the number of female teenagers in a given state. Therefore, we weight the estimates by the average state-level teen female population and display both average and dynamic treatment effects in Columns 4, 5, and 6. Across all columns, none of the estimates are statistically different from zero.

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Finally, Columns 7–9 utilize a Poisson fixed effects model to account for the discrete nature of natality data. Although Poisson models are typically used to estimate counts and not rates, we note that this model can be alternatively expressed as one that estimates the natural log of the expected count of teen births while controlling for the population of female teenagers and constraining its coefficient to be equal to one. This allows us to create estimates that are analogous to the weighted least squares estimates shown in Columns 4-6. Across columns, all estimates are statistically insignificant.

There is little evidence that state-level abstinence policies affect teen birth rates. Importantly, estimates for our preferred specification in Panel A Column 5 are sufficiently precise to rule out large effects in teen birth rates. For example, the 95% confidence interval lower bound and upper bound are −3.7% and 6.1%, respectively. The 90% confidence interval falls between −2.9% and 5.3%.

5.2. Effects on teen sexually transmitted disease rates

Behavioral changes may also cause changes to STD rates, especially if students are less knowledgeable about other forms of contraception as a result. In order to examine these effects, we estimate the same difference-in-differences model for logged teen STD rates and report the results in Tables IV and V.

Weighted least squares estimates in Table IV indicate that abstinence mandates have no effect on STDs. All leading indicators and lags are statistically indistinguishable from zero. However, unweighted OLS estimates in Columns 1–3 reveal an increase in STD rates for the year of enactment and the following few years. These

Table IV. Effect of stress-abstinence policy on logged teen sexually transmitted disease rates

	(1) OLS	(2) OLS	(3) OLS	(4) WLS	(5) WLS	(6) WLS	(7) Poisson	(8) Poisson	(9) Poisson
Panel A. average effects									
Abstinence mandate in effect	0.042	0.057	0.074*	0.022	0.025	0.041	0.027	0.034	0.055
	(0.047)	(0.038)	(0.041)	(0.055)	(0.038)	(0.042)	(0.052)	(0.033)	(0.040)
1 year prior to enactment			0.043			0.070			0.092**
			(0.054)			(0.047)			(0.046)
2 years prior to enactment			0.035			0.014			0.009
			(0.024)			(0.027)			(0.022)
Panel B. dynamic effects									
Effect of policy in year of enactment	0.053	0.066**	0.084**	0.044	0.035	0.053	0.047*	0.037	0.059*
	(0.033)	(0.027)	(0.031)	(0.031)	(0.027)	(0.032)	(0.029)	(0.025)	(0.033)
1 year after enactment	0.044	0.066	0.083**	0.006	0.004	0.020	−0.001	−0.006	0.014
	(0.041)	(0.039)	(0.039)	(0.036)	(0.035)	(0.033)	(0.032)	(0.033)	(0.030)
2 years after enactment	0.040	0.057	0.076*	0.004	0.000	0.017	−0.005	−0.006	0.015
	(0.045)	(0.036)	(0.039)	(0.044)	(0.035)	(0.039)	(0.040)	(0.031)	(0.035)
3 years after enactment	0.032	0.047	0.064	0.020	0.031	0.046	0.034	0.051	0.070
	(0.065)	(0.062)	(0.064)	(0.082)	(0.075)	(0.079)	(0.080)	(0.070)	(0.076)
4+ years after enactment	0.039	0.050	0.066	0.027	0.045	0.060	0.039	0.072	0.092
	(0.064)	(0.058)	(0.060)	(0.087)	(0.064)	(0.067)	(0.075)	(0.051)	(0.058)
1 year prior to enactment			0.044			0.070			0.091**
			(0.053)			(0.047)			(0.045)
2 years prior to enactment			0.036			0.014			0.009
			(0.025)			(0.027)			(0.022)
N	312	312	312	312	312	312	312	312	312
State-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Notes: *p<0.1, **p<0.05, ***p<0.01. Estimates are based on annual data for 26 states from 2000–2011. Each column by panel represents coefficients from a separate regression. Robust standard errors are clustered at the state level and are shown in parentheses. The control variables include percent of female teenagers ages 15–19 who are Black, percent of female teenagers ages 15–19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates, and median family income. OLS, ordinary least squares, WLS, weighted least squares.

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Table V. Effect of stress-abstinence policy on logged teen sexually transmitted disease rates in Maine and Colorado

	(1) OLS	(2) OLS	(3) OLS	(4) WLS	(5) WLS	(6) WLS	(7) Poisson	(8) Poisson	(9) Poisson
Panel A. average treatment effects									
Abstinence mandate in effect	0.124***	0.098***	0.098***	0.095**	0.054	0.045	0.084**	0.048	0.039
	(0.031)	(0.022)	(0.025)	(0.040)	(0.032)	(0.030)	(0.035)	(0.031)	(0.030)
1 year prior to enactment			−0.070			−0.069			−0.061**
			(0.060)			(0.047)			(0.027)
2 years prior to enactment			0.068**			0.018			0.012
			(0.031)			(0.031)			(0.031)
N	276	276	276	276	276	276	276	276	276
Panel B. dynamic treatment effects									
Effect of policy in year of enactment	0.106***	0.096***	0.096***	0.089***	0.042	0.031	0.081***	0.023	0.013
	(0.019)	(0.024)	(0.030)	(0.021)	(0.033)	(0.031)	(0.020)	(0.027)	(0.026)
1 year after enactment	0.146***	0.148***	0.148***	0.132***	0.111***	0.101**	0.131***	0.111***	0.102***
	(0.022)	(0.020)	(0.029)	(0.034)	(0.038)	(0.037)	(0.032)	(0.039)	(0.036)
2 years after enactment	0.137***	0.119***	0.118***	0.116**	0.054	0.043	0.108***	0.040	0.030
	(0.030)	(0.035)	(0.039)	(0.042)	(0.052)	(0.051)	(0.040)	(0.049)	(0.049)
3 years after enactment	0.109*	0.081*	0.083**	0.060	0.029	0.019	0.040	0.006	−0.004
	(0.054)	(0.040)	(0.039)	(0.054)	(0.052)	(0.047)	(0.045)	(0.043)	(0.040)
4+ years after enactment	0.122**	0.052	0.051	0.081	0.037	0.026	0.061	0.056	0.046
	(0.046)	(0.033)	(0.040)	(0.058)	(0.040)	(0.047)	(0.046)	(0.035)	(0.036)
1 year prior to enactment			−0.069			−0.070			−0.064***
			(0.057)			(0.046)			(0.025)
2 years prior to enactment			0.069*			0.016			0.009
			(0.035)			(0.032)			(0.031)
N	276	276	276	276	276	276	276	276	276
State-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Notes: *p<0.1, **p<0.05, ***p<0.01. Estimates are based on annual data for 23 states from 2000–2011. Each column by panel represents coefficients from a separate regression. Robust standard errors are clustered at the state level and are shown in parentheses. The control variables include percent of female teenagers ages 15–19 who are black, percent of female teenagers ages 15–19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates, and median family income. OLS, ordinary least squares, WLS, weighted least squares.

Table VI. Effect of stress-abstinence policy on logged teen abortion rates

	(1) OLS	(2) OLS	(3) OLS	(4) WLS	(5) WLS	(6) WLS	(7) Poisson	(8) Poisson	(9) Poisson
Panel A. average effects									
Abstinence mandate in effect	−0.038	−0.039	−0.034	−0.032	−0.027	−0.023	−0.013	−0.008	0.000
	(0.038)	(0.034)	(0.041)	(0.043)	(0.035)	(0.042)	(0.029)	(0.024)	(0.028)
1 year prior to enactment			0.013			0.004			0.018
			(0.022)			(0.022)			(0.021)
2 years prior to enactment			0.009			0.015			0.024
			(0.032)			(0.034)			(0.028)
Panel B. dynamic effects									
Effect of policy in year of enactment	−0.041*	−0.034	−0.029	−0.017	−0.009	−0.005	−0.006	0.002	0.011
	(0.022)	(0.023)	(0.031)	(0.022)	(0.019)	(0.025)	(0.020)	(0.015)	(0.019)
1 years after enactment	−0.072	−0.069	−0.064	−0.047	−0.040	−0.036	−0.012	−0.003	0.006
	(0.058)	(0.062)	(0.071)	(0.067)	(0.072)	(0.079)	(0.045)	(0.046)	(0.050)
2 years after enactment	−0.052	−0.033	−0.028	−0.072	−0.051	−0.047	−0.050	−0.036	−0.028
	(0.049)	(0.040)	(0.047)	(0.051)	(0.034)	(0.040)	(0.036)	(0.028)	(0.031)
3 years after enactment	−0.005	−0.013	−0.009	−0.012	−0.006	−0.002	0.003	0.005	0.013
	(0.047)	(0.028)	(0.036)	(0.050)	(0.030)	(0.038)	(0.033)	(0.022)	(0.027)
4+ years after enactment	0.009	−0.042	−0.037	0.022	−0.027	−0.023	0.039	−0.002	0.006
	(0.031)	(0.049)	(0.050)	(0.031)	(0.051)	(0.051)	(0.024)	(0.038)	(0.034)
1 year prior to enactment			0.010			0.003			0.018
			(0.025)			(0.023)			(0.022)
2 years prior to enactment			0.009			0.015			0.024
			(0.033)			(0.034)			(0.029)
State-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Notes: *p<0.1, **p<0.05, ***p<0.01. Estimates are based on annual data for 15 states from 2000-2011. Each column by panel represents coefficients from a separate regression. Robust standard errors are clustered at the state level and are shown in parentheses. The control variables include percent of female teenagers ages 15–19 who are Black, percent of female teenagers ages 15–19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates and median family income. Eleven states were removed from the analysis because of missing or inconsistent data. The states that were dropped for these regressions include California, Colorado, Delaware, Florida, Illinois, Kentucky, Maryland, New York, Rhode Island, Vermont, and West Virginia. OLS, ordinary least squares, WLS, weighted least squares.

contradicting results can signal heterogeneous effects across states of different sizes (Solon *et al.*, 2015), and smaller states are more likely to be driving the perceived increase.

To explore this heterogeneity, in Table V, we replicate Table IV using only our smallest treatment states (Maine and Colorado) as the treatment group. The effects for these two states are quite stark as we find a significant positive effect on STDs of 10% overall and between 10% and 14% for the year of enactment and the following 2 years. Neither of these states had any sex education requirements before their abstinence mandates were enacted. Along with social and idiosyncratic differences between states, this policy difference may have contributed to the effects reported in Table IV.[21] The heterogeneous effects could suggest that students who have never been exposed to comprehensive sex education are more likely to exhibit higher STD rates because of the enactment of abstinence-based sex education mandates. Thus, this finding could be indicative of increased risky sexual behavior and/or higher transmission rates because of increased stigma of STD screening and treatment.

[21] When estimating our model without these two treatment states, we find a statistically insignificant effect of 1.2%, which corresponds to the 2.5% estimate reported in Panel A Column 5 of Table IV. These estimates are statistically indistinguishable at the 99% level.

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5.3. Effects on teen abortion rates

Abstinence-based sex education may also affect teen abortion rates, either through changing the number of unintended pregnancies or increasing stigmatization of abortions. We empirically assess this hypothesis by estimating Eqs. (1) and (2) for logged teen abortion rates.

Results for logged teen abortion rates are shown in Table VI.[22] Average effects, shown in Panel A, range from -3.9% to 0%, and all are statistically insignificant. Estimates are similar in Panel B, and all are statistically insignificant at the 95% confidence level. Overall, results suggest that teen abortion rates also seem to be unaffected by sex education policy changes.

6. SUBGROUP ANALYSIS

Teen birth rates are commonly measured for the 15–19-year old age group, and we follow this convention in the above analysis. However, if sex education policies do change teen behavior, younger teens who may have not been sexually active or exposed to sex education before the adoption of policy are more likely to be affected.[23] Most 18– and 19-year olds are not in high school any longer and would not have been exposed to the curriculum. Additionally, 18– and 19-year olds who attend college are arguably exposed to a different sexual culture that could counteract their high school sex education training.

In Columns 1–4 of Table VII, we consider logged birth rates for 15–17-year olds and 18–19-year olds separately to determine if younger and older teens respond differently to the stress-abstinence policies. Panel A reports unweighted results, Panel B reports weighted results, and Panel C reports results from a Poisson model. For both age groups, estimates from all model specifications are statistically insignificant and close to zero. Even for the younger age group, who are most likely to be affected by the policy, we are able to rule out large effects. The 95% confidence interval for the estimate in Panel B Column 2 is bound by -5.1% and 5.5%, and the 90% confidence interval is −4.2% to 4.7%. This echoes the main findings and rules out the possibility that insignificant effects for the older group are washing out a measurable effect on the younger group.

While we address potential bias earlier by directly controlling for time-varying factors such as access to abortion and economic conditions, here we offer an additional test to ensure that other state-level conditions affecting fertility did not also change at the time of treatment. Specifically, we estimate policy effects on the logged birth rates of women between the ages of 30–34. These women were just old enough during the sample period to not have been in the teen population in 2000 and, therefore, could not have been affected by the policy changes. Thus, if the fertility of these women appears to have been affected by the policy change, then it would suggest that our identifying assumption was violated.

The results for women aged 30–34 are shown in Columns 5 and 6. Estimates range from −0.5% to 0.0% and are not statistically different from zero. This finding is consistent with our identifying assumption and suggests that there was no other state-level determinant that changed at the time of the policy that affected fertility more generally. Additionally, Table A8 provides estimates from a triple differences model in which we also use the 30-34-year old females as a within-state control group to net out the state's secular trend in birth rates. These estimates further emphasize that general fertility levels were not changing systematically at the time of the policy changes.

Because of the differences in average teen birth rates by race and ethnicity, we may expect to observe heterogenous effects of abstinence intervention programs by these attributes. Panels D, E, and F present the ordinary least squares, weighted least squares, and Poisson estimates for the effect of stress-abstinence policies

[22]The regressions for Table VI account for a smaller subset of states because many states do not consistently or accurately report teen abortion data. The states that are dropped for these regressions include California, Colorado, Delaware, Florida, Illinois, Kentucky, Maryland, New York, Rhode Island, Vermont, and West Virginia because of data fluctuations and inconsistencies. See section 3 for a more detailed explanation. Our results are not overly sensitive to this selection. See Table A7 for a replication of Table VI when these 11 states are included in the analysis.

[23]For example, Cannonier (2012) finds that state-level abstinence funding affects only white teens aged 15–17.

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Table VII. Effect of stress-abstinence policy on logged teen birth rates for age and race subgroups

	15–17-year olds		18–19-year olds		30–34-year olds	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Ordinary Least Squares						
Abstinence mandate in effect	−0.006	−0.005	0.008	0.013	−0.000	−0.000
	(0.027)	(0.027)	(0.031)	(0.034)	(0.016)	(0.014)
Panel B. Weighted Least Squares						
Abstinence mandate in effect	0.015	0.002	0.034	0.032	−0.005	−0.004
	(0.023)	(0.027)	(0.026)	(0.031)	(0.016)	(0.013)
Panel C. Poisson						
Abstinence mandate in effect	−0.003	−0.012	0.011	0.001	−0.004	−0.004
	(0.021)	(0.024)	(0.018)	(0.015)	(0.015)	(0.012)
	White teens		**Black teens**		**Hispanic teens**	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel D. Ordinary Least Squares						
Abstinence mandate in effect	−0.034	−0.021	−0.033	−0.030	−0.075	−0.064
	(0.036)	(0.027)	(0.046)	(0.052)	(0.095)	(0.110)
Panel E. Weighted Least Squares						
Abstinence mandate in effect	0.026	0.007	0.039	0.014	−0.008	−0.014
	(0.044)	(0.026)	(0.041)	(0.018)	(0.042)	(0.066)
Panel F. Poisson						
Abstinence mandate in effect	−0.014	−0.017	0.033	0.005	−0.016	−0.025
	(0.028)	(0.022)	(0.036)	(0.013)	(0.037)	(0.054)
State-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes

Notes: *p<0.1, **p<0.05, ***p<0.01. Estimates are based on annual data for 26 states from 2000–2011. In Panels A, B and C, the dependent variable for Columns 1–2 is the logged birth rate for female teenagers aged 15–17, for Columns 3–4 is the logged birth rate for female teenagers aged 18–19, and for Columns 5-6 is the logged birth rates for female adults aged 30–34. In Panels D, E, and F, the dependent variable for Columns 1–2, 3–4, and 5–6 is the White teen birth rate, Black teen birth rate, and Hispanic teen birth rate, respectively. Robust standard errors are clustered at the state level and are shown in parentheses. All columns with control variables account for a policy-based measure of abortion access, state unemployment rates, and median family income. The control variables for Columns 1–2 in the first three panels additionally include percent of female teenagers ages 15–17 who are Black and percent of female teenagers ages 15–17 who are Hispanic, the control variables for Columns 3–4 in the first three panels control for the percent of female teenagers ages 18–19 who are Black and the percent of female teenagers ages 18–19 who are Hispanic, and the control variables for Columns 5–6 in the first three panels control for the percent of female adults ages 30–34 who are Black and the percent of female adults ages 30–34 who are Hispanic.

on logged birth rates for White, Black, and Hispanic teens, by column. Mirroring previous results, we find no policy effects for any subgroup.

7. DISCUSSION AND CONCLUSION

Many politicians, activists, and child development scientists have argued that the content of sex education in public schools is an important factor in teen sexual health outcomes such as birth rates, STD rates, and abortion rates. This paper adds to this discussion by considering how state-mandated abstinence-based sex education affects teen health outcomes. We show empirically that adopting or switching to a stress-abstinence policy

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does not have an effect on teen birth rates or abortion rates. However, state-level policies may increase STD rates in states with relatively small populations. Our findings rule out any effects greater than two births per 1000 teens, or a 6% change in teen birth rates. This suggests state policies are relatively ineffective at reducing unintended pregnancy as compared with pregnancy prevention programs, increases in contraception access, or media interventions, which have been reported to decrease teen birth rates by 6% to 25% in the short run (Thomas, 2012; Lindo and Packham, 2015; Guldi, 2008; Kearney and Levine, 2014). While we can rule out large effects, we cannot dismiss modest effects of abstinence sex education on teen pregnancy. Our findings fit into a greater literature on the general ineffectiveness of state policies as a tool for reducing teen pregnancy. Recent research shows that policies such as oral contraceptive access, welfare reform, and family planning services similarly result in little to no reduction in teen pregnancy (Myers, 2012; Kearney, 2004; Kearney and Levine, 2009).

One might be concerned that the reason that stress-abstinence mandates have no effect is because they do not actually affect the material being taught in the classroom. However, survey evidence suggests that superintendents and teachers take action to follow mandates (Landry *et al.*, 1999; Darroch *et al.*, 2000; Gold and Nash, 2001). A more probable explanation is that the change in classroom instruction did not change teens' knowledge about sex. Most abstinence programs censor information about contraception, and all aim to increase the perceived cost of sexual activity by expounding upon abstinence as the only perfect method of birth control and exploring the emotional and health risks associated with sexual activity. Conversely, all comprehensive programs provide lessons on contraceptive use and teach that sex is a normal, healthy part of life. However, both curricula are unlikely to affect teens' actual knowledge about sex and contraceptives because all teens likely experience some additional learning outside of the classroom from peers and media influences. Furthermore, evidence from surveys suggests that parents are strongly opposed to politicians choosing sex education content and prefer that the choice be delegated to health care professionals and teachers (Ito *et al.*, 2006). Therefore, it is reasonable to believe that parents, churches, and community groups may fill in the gaps when sex education curriculum changes. Alternatively, it could be that teens' knowledge of sex does change after the policy, but that teen behavior is simply unresponsive to that knowledge.

Regardless of the mechanism underlying our main findings, our results indicate that teen pregnancy is unresponsive to mandated changes to sex education curriculum. Moreover, as teen birth rates continue to decline over time, it will become increasingly more difficult for policy levers to reduce unintended pregnancy rates. Millions of dollars are at stake each year based on what a state decides to mandate, and this topic is often at the center of political battles. Thus, our study provides suggestive policy implications for the future allocation of state-level political resources and abstinence-based sex education funding.

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