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The Effect of State-Level Sex Education Policies on Youth Sexual Behaviors

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Abstract

Two types of sex education are generally offered in the U.S. abstinence-only and comprehensive sex education. There is no clear scientific consensus over which approach minimizes the risk of unintended pregnancy and sexually transmitted diseases for youth. While there have been many studies of specific programs in clinical or quasi-experimental settings, there are very few evaluations of how state-level sex education policies affect the youth population. We estimate the impact of various state-level sex education policies on youth sexual activity and contraceptive use using data from four waves of the Youth Risk Behavior Surveillance System from 39 states. We found that states that require sexuality (sex and/or HIV/STD) education and contraceptive content or states that mandate education but leave the actual content up to local districts have lower rates of sexually active youth and higher rates of contraception use when youth are sexually active. States that require sexuality education and require abstinence content increase the rate at which youth are sexually active, and youth in those states are less likely to use hormonal birth control if they are sexually active. In conclusion, we found that state policies regarding sex and HIV/STD education had statistically significant effects that are meaningful in magnitude from a public health perspective.

Keywords Sex education · Youth sexual behavior · Youth sexual activity · Youth contraceptive use

Introduction

In the U.S., two types of sex education are generally offered to students in public schools: abstinence-only and comprehensive sex education (Kirby, 2008). Abstinence-only programs aim to decrease negative outcomes associated with youth sex by advocating abstinence from all sexual activity as the only acceptable behavioral option for adolescents until marriage, while comprehensive (or abstinence-plus) programs also educate students about the pregnancy and disease prevention benefits of contraception (Perrin & DeJoy, 2003).

Both approaches have critics and advocates, and there is no clear scientific consensus regarding the superiority of either philosophy. This lack of scientific consensus has not served to moderate the policy debate. Although there is an important and large body of research evaluating the effects of sex education broadly, one difficulty is that there is very little evidence about the effect that state-level sex education policies have at the youth population level. Much of the extant literature evaluates school-level or district-level interventions. We are aware of only one paper (Carr & Packham, 2017) that has examined the causal effect of specific state sex education policies on youth health outcomes (abortion rates, STI rates, and teen birth rates).[1]

The Policy Debate

Policy debate surrounding school-based sex education in the U.S. has historically been cyclical in nature with different approaches dominating the discussion in different decades but has generally always been contentious. The earliest policy debates were prompted by a coalition of clergy, temperance activists, and physicians in the early decades of the twentieth century who advocated increased access to (largely) pregnancy prevention education in public schools (Irvine, 2004). These issues became increasingly salient with the introduction of birth control pills and other effective means

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[1] Sabia (2006) examined the causal effect of the receipt of sex education on youth outcomes.

of contraception. During the 1960s, the Sex Information and Education Council of the United States (SIECUS) was formed to promote comprehensive sex education in the nation's schools (Irvine, 2004; SIECUS, 2012a).

Over time, the debate moved to whether or not it was appropriate for schools to educate students about sex (Boonstra, 2009). Due to growing concerns regarding teenage pregnancy and AIDS prevention in the 1970s and 1980s, sex education became more popular, and states began developing sex education policies (Boonstra, 2009). The focus of the debate then shifted to the content of sex education. Congress passed the Adolescent Family Life Act (AFLA) in 1981, which provides federal grant money to programs focused on abstinence-only sex education (Boonstra, 2009). As of 2009, AFLA had spent $13 million in support of abstinence-only sex education programs (Boonstra, 2009).

As part of the 1996 welfare reform, Congress instituted a $50 million a year matching grant for states spanning 1998–2002 that funded abstinence-only programs (Daley, 1997; Perrin & DeJoy, 2003; Trenholm et al., 2007). This policy set specific requirements for abstinence-only education (Perrin & DeJoy, 2003; Santelli et al., 2006; Trenholm et al., 2007). Initially, the policy indicated that not all of the guidelines needed to be met (although they could not be contradicted) in order to receive funding. However, in 2000, $20 million was dedicated to programs that satisfied all eight of the federal requirements for abstinence-only education, and this funding level eventually doubled (Perrin & DeJoy, 2003; SIECUS/Advocates for Youth, 1999). Opponents of abstinence-only sex education efforts argue that comprehensive sex education can be more effective by promoting abstinence for youth who have yet to initiate sex but also teaching contraception for youth who are sexually active (Kirby, 2008).

Despite the focus on abstinence-only sex education by policy makers at the Federal and (most) state levels, public attitudes have been very consistently ambivalent about it. In the mid-1990s, around 93% of respondents to an opinion poll expressed the view that sex education should be part of school curriculum and 92% believed that pregnancy prevention should be part of that education (Mayer, 1997). Currently, national estimates find that over 80% of U.S. adults favor providing comprehensive education in schools (Bleakley, Hennessy, & Fishbein, 2006). Surveys in individual states find similar super-majority support for comprehensive sex education rather than abstinence-only education (Eisenberg, Bernat, Bearinger, & Resnick, 2008; Raymond et al., 2008). As a result, a number of states began rejecting federal abstinence-only education funds.

Specific Programs

Although there are few studies focused on the evaluation of state sex education policies, the literature contains multiple analyses of specific sex education programs and curricula. In a review of abstinence and comprehensive sex education programs in the U.S., Kirby (2008) summarized findings of studies examining the impacts of nine abstinence sex education programs and 48 comprehensive sex education programs. Kirby found that three of the nine abstinence programs were shown to result in beneficial outcomes, including delayed sexual initiation, reduced sexual frequency, and reduced number of partners. The other abstinence programs reviewed did not have any positive effect on reproductive health behaviors of participants.

Of the comprehensive sex education programs Kirby reviewed, it was found that approximately two-thirds of these programs had positive benefits including delayed sexual initiation and increased condom use. Kirby (2008) concluded that the review supported the expansion of comprehensive sex education but called the continuation of abstinence-only programs into question. Advocates for Youth, an organization that works to improve adolescent responsibility and decision-making with regard to reproductive health-related issues, produced a review of the evaluation literature identifying programs that had been shown to be successful (Alford, Bridges, & Gonzalez, 2011). Of the 26 qualifying programs, 23 included some comprehensive education aspect.

In 1997, Congress sanctioned an evaluation of the Title V, Section 510 Abstinence Education Program. This evaluation was completed by *Mathematica* and focused on four Title V, Section 510 abstinence education programs over a multi-year period using an experimental research design. Overall, the findings indicated a lack of beneficial impact of abstinence-only sex education programs. More specifically, researchers found no significant difference in the treatment and control groups with regard to abstention from sexual activity, number of partners, or differences in initiation of sexual activity. The researchers did not find evidence of negative effects of these programs either (Trenholm et al., 2007).

A major hurdle in much of the literature evaluating the effect of sex education on youth is the potential for endogenous selection effects: youth who engage in risky behaviors may be more likely to receive sex education (or, remember receiving it) than youth who do not engage in risky behaviors. Such selection bias may lead researchers to mis-measure the direction of the treatment effect from sex education. One of the few studies in the literature to systematically assess whether selection effects are important and address them was by Sabia (2006). Sabia used 2 years of data from the National Longitudinal Study of Adolescent Health (AddHealth) to evaluate whether youth exposed to sex education were more likely to have worse outcomes; these included: early initiation, more frequent sex, more unprotected sex, becoming pregnant, and contracting a STD.

Sabia (2006) found that failure to control for non-random selection likely accounted for many of the significant effects

observed in the literature. Sabia's instrumental variables-based findings suggest that sex education did not significantly change sexual frequency or contraceptive use; however, Sabia did find that sex education was associated with earlier sexual initiation. Our work presented here expands upon that of Sabia in that we will: (1) use newer data (repeated cross-section) on a larger population; (2) examine behaviors over a much longer time frame; (3) avoid the endogeneity concerns raised by Sabia using a method that directly controls for the unobservables associated with selection, and (4) measure the effect of state policies, rather than measure the effect of program receipt as he did.

State Policy Effects

Given the energy with which state and federal policy makers debate the relative merits of mandating abstinence-only or comprehensive sex education, one would expect an equally vibrant literature evaluating the variety of state policies' effects on youth sexual behaviors and outcomes. However, the policy evaluation literature on the topic is remarkably sparse. To our knowledge, only two studies have attempted to link state-level policies to actual outcomes. The first by Hogben, Chesson, and Aral (2010) linked the impact of abstinence-only requirements at the state level to STD rates in the U.S. Using a panel of state-level gonorrhea and chlamydia rates from 2001 to 2005, they found that states that mandate abstinence education had higher rates of STD than states with no mandate. They were, however, unable to control for unobservable characteristics of the states that might have been correlated with both adopting the abstinence-only mandate and with the STD rates.

The second study examined the relationship between state-level youth birth rates and 13 measures of average state classroom coverage of sex education topics (for example, HIV infection prevention, pregnancy prevention, STD prevention, and so forth) (Cavazos-Rehg et al., 2012). They found little consistent evidence of average educational effects on youth fertility. However, there were a number of factors that may have contributed to their null findings. First, they did not attempt to characterize the actual state policies but rather relied on a biannual survey of school health educators to determine whether students were taught about specific issues; thus, they could not evaluate state policy directly. They were also limited to 24 states. Finally, they attempted to control for unobservable characteristics of states using (essentially) fixed effects, which absorbed much of the variation in their data.

To our knowledge, our study is the first to estimate the impact of state sex education content policies on youth behaviors and addresses the potential endogeneity of these state policies. Thus, while much attention, political capital, and emotion are regularly expended in attempts to change sex education laws by state legislatures, it is unclear whether state policies matter. Our paper will address this lack of evidence directly by estimating the impact of various state-level sex education policies on youth sexual activity and contraceptive use. We use data on individual high school student sexual behaviors from four waves (2004, 2006, 2008, and 2010) of the Youth Risk Behavior Surveillance System (YRBS) from 39 states. We merge a set of comprehensive measures of state sex and HIV/AIDS education regulations onto the YRBS. Although considerable variation in state regulations exists regarding the type of education (sex vs. HIV/AIDS) and the content (abstinence-only vs. contraception), little research on the impact of such policies on reproductive health outcomes for youth exists. One likely reason for this gap stems from the fact that state-level sex education policies are difficult to categorize (Brown, 1997; Constantine, 2008). Instead of developing our own classifications of state sex education policy, we relied on characterizations maintained for more than a decade by the Alan Guttmacher Institute, a group that focuses on providing information regarding sexual and reproductive health. Our analysis also controlled for the potential endogeneity of state sex education policies in our models of student decision-making.

Method

Sample and Measures

The primary data for our analysis were taken from the YRBS System. This biannual survey was developed by the Centers for Disease Control and Prevention (CDC) beginning in 1990 and voluntarily conducted by states that opt into the system in the Spring of each odd-numbered year (February through May). Within each participating state, schools are chosen in a two-stage sampling design, and students in each school are administered a nationally standardized survey instrument (though states have the option of dropping items that they do not wish to ask and also adding additional questions). When states have achieved adequate sample sizes and response rates, each observation is given a sampling weight based upon the Primary Sampling Unit (PSU) and within-PSU stratum. Only weighted data are released for analysis.

State participation varied each year, and the number of states that provided weighted (i.e., accessible) data also varied. In 1991, only nine states provided weighted data. By 2009, the number of available states had risen to 42. We obtained data on 39 states that participated for at least 2 years

Table 1 Summary statistics or descriptive statistics

	Mean
Student age (in years)	15.9
Student has had sex within past 3 months	33.20%
Student used birth control at last sexual encounter	74.20%
State requires sex/HIV education but not content	14.50%
State requires sex/HIV education and mandates abstinence education	70.50%
State requires sex/HIV education and mandates contraceptive education	43.90%
Student is female	51.00%
Student is African-American	11.80%
Student is Hispanic	16.20%
Student is other race	10.20%
Student self-assessed somewhat or very overweight	26.70%
Observations	369798

from the 2003 through 2009 cycles.[2] This time frame coincided with available data on state sex education policies. We extracted variables for each student describing basic sociodemographic characteristics including: age, race/ethnicity (separate indicators for African-American, Hispanic, and other race status, with Caucasian as the omitted categorical variable to serve as the comparison group), gender (female = 1), current grade, and self-assessed overweight status (somewhat or very overweight = 1).

In addition, students were asked a number of questions regarding recent sexual activity. These included whether the student had had sex in the past 3 months and the primary method of birth control at last sex. We used these questions to construct our two dependent variables: a binary indicator for whether the student had sex in the last 3 months and a multinomial variable measuring the birth control choice at last sex (0 = no birth control, 1 = condom, 2 = hormonal birth control), conditional on having sex in the last 3 months. Recall that the birth control measure is multinomial, not ordered since there is nothing inherently "greater" about choosing condoms, for example, over hormonal birth control.[3] Descriptive statistics are shown in Table 1. In our sample, approximately 33% of the students reported being sexually active in the 3 months prior to the survey, and of those sexually active youth, about 74% reported using contraception (either condom or hormonal birth control) at their last sexual encounter.

We found three sources of information on state sex education policies that bring the disparate laws and regulations into a unified framework and potentially allow policies to be tracked over time. The first is a series of articles in the *Georgetown Journal of Gender and Law* that cover 2001 to 2010 (e.g., Natbony, 2010). Unfortunately, while the law review authors summarize complex law, they did not do so consistently across the entire time period. The second source of data originates from the SIECUS (2012b), which published an annual report that, among other things, abstracts state laws and regulations related to sex education in a brief, simple, and consistent format. These abstracts are available historically and could permit identification of key aspects of sex education laws for all states from 2002 to 2010; however, there is little consistency in the laws' language, and so the abstracts nonetheless would require significant interpretation.

A final source of information on state polices can be found in a series of monthly State Policy Briefs on sex and HIV education policies published by the Alan Guttmacher Institute (Guttmacher Institute, 2012). These reports are concise and consistent and are maintained historically back to the end of 2001, which allows a complete characterization of state laws over the decade of the 2000s. In addition, the staff at the Guttmacher Institute specialize in interpreting and synthesizing the various state policies, and the reports undergo continuous quality review (including retrospective corrections, when needed). The Guttmacher State Policies in Brief are often used in empirical analysis of the effects of a variety state policy (e.g., abortion restrictions, contraceptive parity mandates, and sex education policies) on a variety of outcomes (e.g., abortion rates, STI rates, contraceptive use, and unintended births) (Atkins & Bradford, 2014; Carr & Packham, 2017; Johnston & Adams, 2017; Trudeau & Conway, 2018). Given the long time series available and specialized expertise in interpreting state laws and regulations at Guttmacher, we

[2] These states were: Alabama, Alaska, Arizona, Arkansas, Connecticut, Delaware, Florida, Idaho, Illinois, Indiana, Iowa, Kentucky, Louisiana, Massachusetts, Maine, Maryland, Michigan, Mississippi, Missouri, Montana, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, North Dakota, Oklahoma, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, and Wyoming.

[3] The YRBS actually asks about several types of birth control: birth control pills, condoms, Depo-Provera, withdrawal, some other method, not sure, and no method. We combined "birth control pills" and "Depo-Provera" into a single "hormonal" category due to the low frequency of Depo-Provera use. Finally, we grouped "withdrawal" and "not sure" into the "no birth control" category.

used this source of data to identify which states had laws that mandated or permitted sex education, HIV/STD education, contraception education, and abstinence-only education.

We captured the state sexuality education environment using the base Alan Guttmacher Institute data. We defined sexuality education as encompassing sex education, HIV/STD education, or both. We measured the policy environment using three variables, where

$$P^{A}_{s,t} = \begin{cases} = 1 \text{ if state required sexuality education but not content} \\ = 0 \text{ otherwise} \end{cases}$$

$$P^{B}_{s,t} = \begin{cases} = 1 \text{ if state required sexuality education and contraception education} \\ = 0 \text{ otherwise} \end{cases}$$

$$P^{C}_{s,t} = \begin{cases} = 1 \text{ if state required sexuality education and abstinence content} \\ = 0 \text{ otherwise} \end{cases}$$

However, it is unclear how students get sex education in schools. Are the discussions primarily in health classes devoted to pregnancy prevention or in health classes devoted specifically to disease prevention? States have different regulations for each. So, we chose to be as general as possible with regard to the policy measures. Instead of differentiating between sex and HIV/STD education, we included both in our measure of state policies. For example, our measure of local control (the state requiring sexuality education, but not mandating content) captured states that require sex education, HIV/STD education, or both and do not dictate content. A relatively small portion of our sample (14.5%, which are actually drawn from only eight states in 2003 and four states beginning in 2006) utilized the local control option, so we were cautious about the conclusions drawn from this set of states. About 70% of the states in our sample required sexuality education and mandated abstinence education, while almost 44% mandated sexuality education and contraceptive coverage. It is important to note that the states mandating abstinence or contraceptive education content in sexuality education are not mutually exclusive categories.

Statistical Analysis

Consider a general model of individual behavior that is intrinsically nonlinear (recalling that there are two versions, one for any sexual activity and one for contraceptive choice):

$$y_{ist} = M\left(x^{o}_{ist}\beta + P_{st}\beta_P + x^{u}_{st}\beta_u\right) + \varepsilon_{ist} \quad (1)$$

where

- is the nonlinear sexual behavior choice (discussed above) for the *i*th youth in state *s* and year *t*;
- are the observable characteristics of the individual youth;
- are (potentially) endogenous policies of state *s* in year *t* that affect the individual's sexual choices;
- are unobservable characteristics of state s in year *t* that are correlated with the outcome.

For our purposes, are the sex education policies set by the states? These are determined in part by such things as the culture of sexuality of the students in the state, which are part of x^{u}_{st}. This culture of sexuality may affect choices of the individual students, so it may also be important predictor of sexual behaviors. However, since is unobservable by assumption, it must be omitted in any actual regression of (1) above. Thus, in actual empirical application, we can only estimate:

$$y_{ist} = M\left(x^{o}_{ist}\beta + P_{st}\beta_P\right) + \eta_{ist} \quad (2)$$

and since by assumption is correlated with the estimated parameters on the policies will be biased.

One approach to addressing omitted state-level variables that are unobservable is to include state-fixed effects, but this solution requires that there are no time-varying unobservables. If this assumption is not met, then there is still omitted variable bias present. For example, consider that some states may have a general trend toward less religiosity, while others may not. If this is the case, and if the general religious environment is predictive of youth sexual behavior, then there would be a trend in some states toward more sexual activity due to this unobservable, and this trend would not be present in other states. Of course, as long as the change in culture was always moving in the same direction and at the same pace in each state (religiosity may increase in some and decrease in others, as long as it moves in the same direction and amount within state), then state-specific time trends would address this omitted variables problem.

But, what if the unobservable factors do not change in a monotonic (and constant) way within state over time? For example, one might assume that underlying labor market conditions could affect the degree to which parents are able to monitor their high school child's sexual behavior. If so, the opportunity cost of sexual activity to the youth will vary with the state-specific business cycle sometimes rising and sometimes falling over time. In addition, it is plausible that such changes in the opportunity cost of sexual activity may change in ways not captured by observable factors such as

state-level unemployment rates. If this example were accurate, then state-specific time trends will still yield estimates on the policy variables that suffer from endogeneity (omitted variables) bias.

For these reasons, researchers often opt for instrumental variables as the solution to the problem of omitted unobservable state variables that would (in our model) influence both the policy variables and the outcomes (youth sexual behavior). If it were possible to obtain some estimate of the state-level unobservable variables to control for in the regression model, then bias could be avoided. One approach to this is to use Two-Stage Residual Inclusion (2SRI), which is the version of 2SLS that is consistent for nonlinear models. 2SRI corrects endogeneity from omitted variables by estimating the state-level unobservable variables and then including these estimates in the model of interest, thus controlling for state-level unobservables. For more details on this method, see Terza, Basu, and Rathouz (2008) and Terza, Bradford, and Dismuke (2008). In 2SRI, the first-stage regression is estimated by the following specification:

$$P_{st} = z_{st}\alpha + x^{u}_{st}. \tag{3}$$

where P_{st} is the policy variable of interest, z_{st} are the instrumental variables and are the state-level unobservables. In practice, since we do not have measures of, we actually estimate:

$$\hat{P}_{st} = z_{st}\hat{\alpha} \tag{4}$$

Note that the difference between Eqs. 3 and 4 is a measure of the state-level unobservables:

$$P_{st} - \hat{P}_{st} = z_{st}\alpha + x^{u}_{st} - z_{st}\hat{\alpha} \equiv \hat{\upsilon}_{st}, \tag{5}$$

where $\hat{\upsilon}_{st} = x^{u}_{st}$.

In other words, the residual from this first-stage regression is an estimate of the state-level unobservables that are the root of the unobservable variables problem.

With estimates of $\hat{\upsilon}_{st} = x^{u}_{st}$ at the state level in hand, these can be merged onto the individual-level data. Then, the second stage of the model thus proceeds by estimating the augmented maximum likelihood estimators for the outcomes of interest and controlling for the unobservables recovered from the first-stage regression, thus eliminating endogeneity bias and omitted variables bias associated with missing state-level variables and missing regional variables.

In order to estimate the first-stage regression, instruments for each policy variable need to be identified. The instruments must satisfy the requirements that they are:

(i) uncorrelated with the state-level unobservables,
(ii) pass the usual tests for weak instruments, and
(iii) are not predictive of the outcomes of interest (i.e., youth sexual behaviors)

One set of candidate instruments for policy p in state s would be the sex education policies of the states bordering state s. The policy diffusion literature has shown that border states' policies can be predictive of own-state policy (Berry & Berry, 1990). Further, border state policies should not be predictive of youth sexual choices in the state of interest. Thus, one might believe that border state policies would be viable instruments.

However, recall that one of the conditions of being a candidate for an instrumental variable is that the instruments in z_{st} are not predictive of the outcomes of interest and therefore excludable from the second-stage regression. It is possible that using border state policies for instruments may fail this criterion. There may be regional cultural variables that influence both youth sexual behaviors in the state of interest and border state policies. If so, even the usual instrumental variable approach will not actually eliminate endogeneity bias. For example, consider the Southeast. We may want to predict Alabama's sexuality education policies using its border states' sexuality education policies (including Tennessee, Mississippi, Georgia, and Florida). Although youth behaviors in Alabama should not be affected by any border state policy, it is possible that some regional characteristics, such as religiosity or political conservatism, common to all these states influence both youth sexual behaviors in Alabama and policy adoption in the border states. If this were the case, the border state policy decision would include the regional influences and would therefore not be exogenous to Alabama youths' behavior.

However, if we could purge the regional influences from the border state policy decisions, this problem could be averted. One way to accomplish this would be to recognize that the average of a set of border state policies will include the regional influence, which is constant across this set of border states in each time period. Thus, each border state policy minus the average of the border state policies will difference out the regional influences, as:

$$\Delta P_{jt} = P_{jt} - \bar{P}_{st} \tag{6}$$

here j indexes one of the border states for state s, and $\bar{P}_{st}$ represents the average policy in the states that border state s. In principle, we could include all of the ΔP_{jt} as instruments. However, since states have different numbers of border states, this would be inconvenient. So, we will use the modal ΔP_{jt} from all of state s border states as our instrument.[4]

[4] If there were two modes, we used the maximum of the two. Also, one state in our data, Maine, technically only has one border state, New Hampshire. For the purposes of this research, we classified New Hampshire, Vermont, and Massachusetts as "bordering" Maine.

Table 2 *t*-tests for changes in rates of sexual activity, birth control use and sexual initiation, by state requiring sex/HIV education

	(1) Requires sex/HIV education but has no content requirement	(2) Requires sex/HIV education and has contraceptive requirement	(3) Requires sex/HIV education and has abstinence requirement
	$\mu_{Law=1}-\mu_{Law=0}$ (*t*-statistic)	$\mu_{Law=1}-\mu_{Law=0}$ (*t*-statistic)	$\mu_{Law=1}-\mu_{Law=0}$ (*t*-statistic)
Student has had sex within past 3 months	.007*** (3.03)	.007*** (4.18)	−.003* (1.67)
Student used birth control at last sexual encounter	−.011*** (3.40)	−.013*** (5.63)	−.008*** (3.08)
Observations	314986	314986	314986

Pooled data from 2003, 2005, 2007, and 2009 Youth Risk Behavioral Survey state data

*$p<.10$, **$p<.05$, ***$p<.01$

The specific policies we used as instruments to calculate modal ΔP_{jt} include border state measures of: (1) sex education; (2) HIV/STD education; (3) covering/stressing abstinence if sex education is taught; (4) covering/stressing abstinence if HIV/STD education is taught; (5) covering contraception if sex education is taught; and (6) covering contraception (condoms) if HIV/STD education is taught. These instruments are supported by two arguments. First, the literature on policy diffusion finds that states are more likely to adopt policies when their neighbors have previously adopted them (Berry & Berry, 1990). Second, while the border states may have adopted policies because of the behaviors of their own residents, they will not have done so because of the behaviors of the residents of state *s* (except for the common regional influences, which our "difference in border state averages" method eliminates). Thus, the ΔP_{jt}'s pass the conceptual test for valid instruments. As an empirical matter, they also pass the usual tests for weak instruments, with partial-*F* statistics well over 200 in each of our models. Finally, all models were estimated using Stata's svy: menu of commands to account for the two-stage sample design and clustering. Since we pooled data from many states, we also accounted for clustering at the state level.[5]

Results

Before delving into the regression modeling results, it is helpful to examine gross differences in average youth behaviors across states that do and do not have the various sexuality education policies. Results of simple *t*-tests on the hypotheses that the average rates of any sexual contact in the past 3 months and the rates of using any birth control (hormonal or condom) at last sex are the same in states that do (Law = 1) and do not (Law = 0) have each policy are shown in Table 2. The top number in each cell is the difference in state mean outcomes.

States that had a mandate but leave content up to local districts (column 1) and states that had a mandate and require contraception education (column 2) had statistically significantly higher rates of youth sex and lower rates of any birth control conditional on youth choosing to have sex. Additionally, we saw a reduction in the frequency of youth being sexually active but still saw a decrease in any birth control use when youth were sexually active for states that mandate districts offer sexuality education and cover/stress abstinence (column 3).

Thus, on an aggregate level, the impact of sex education policy appeared to be mixed. However, the question is whether these mixed results will hold in more detailed multivariate regression models particularly those that control for the potential endogeneity of the policies themselves. This is the question we explored in Tables 3, 4, 5, 6, and 7.

Table 3 shows the first-stage results from the 2SRI models. Tables 4 and 6 present the coefficients from the probit and multinomial logit models. Although we are not interested in interpreting the coefficients from the nonlinear probit and multinomial logit models, we included these tables to present the results of the test for policy endogeneity. Recall that if the predicted residuals are significant in the second-stage

[5] Merely combining the data into a single data set and estimating via svy: commands would not accomplish the required clustering since each state uses the same PSU identifier (i.e., each state will have PSU units labeled "1", "2", etc.). Therefore, we created pseudo-PSUs, by generating unique state-PSU identifiers using the state Federal Information Processing Standards (FIPS) code (a number that uniquely identifies a state) and YRBS PSU identifiers. With PSUs defined for each state, we proceeded with the svy: commands in the standard way. Thus, our models control for clustering at the state level and for clustering based on the within-state multi-stage sampling design.

Table 3 First-stage linear probability models for endogenous policy 2SRI regressions

	Sex/HIV Ed-No content	Must cover contraception	Must cover abstinence
Mode of ΔP_{jt} for requiring sex education	−0.026***	−0.25***	−0.17***
	(−12.15)	(−88.96)	(−62.48)
Mode of ΔP_{jt} for requiring HIV education	−0.23***	−0.035***	−0.32***
	(−90.09)	(−10.65)	(−101.29)
Mode of ΔP_{jt} for requiring abstinence education in HIV education	0.19***	−0.11***	−0.18***
	(79.14)	(−36.44)	(−59.99)
Mode of ΔP_{jt} for requiring contraceptive education in HIV education	0.30***	−0.96***	−0.81***
	(129.22)	(−324.58)	(−285.10)
Mode of ΔP_{jt} for requiring abstinence education in sex education	−0.20***	0.12***	0.11***
	(−91.50)	(42.00)	(42.73)
Mode of ΔP_{jt} for requiring contraceptive education in sex education	0.18***	0.39***	0.98***
	(57.31)	(94.83)	(250.64)
Student age	0.0070***	−0.021***	−0.013***
	(16.39)	(−38.80)	(−25.55)
Student is female	0.0068***	0.00081	−0.00072
	(6.27)	(0.58)	(−0.54)
Student is African-American	−0.033***	0.15***	0.12***
	(−19.26)	(66.29)	(58.83)
Student is Hispanic	−0.0062***	0.15***	0.032***
	(−3.99)	(76.22)	(16.64)
Student is other race	−0.014***	0.060***	−0.011***
	(−7.43)	(25.51)	(−4.99)
Student self-assessed somewhat or very overweight	−0.0033***	−0.016***	−0.0031**
	(−2.72)	(−10.42)	(−2.05)
Time	−0.024***	0.0083***	0.019***
	(−95.34)	(26.21)	(62.58)
Constant	0.24***	0.66***	0.89***
	(33.63)	(73.15)	(102.64)
N	360836	360836	360836
F	5709.1	12005.4	9246.5

*$p < .10$, **$p < .05$, ***$p < .01$

regressions, this indicates policy endogeneity. Tables 5 and 7 show the marginal effects of the key policy variables from the various models, rather than their coefficients. Marginal effects are interpreted as the change in the probability of a positive outcome [for Eqs. (4) and (4a)] or in the probability of each outcome [for Eqs. (6) and (6a)] given a one-unit increase in the variable of interest. While each model included all of the variables discussed above (and described in each table's footnote), we only present the marginal effects of the key policy variables in Tables 5 and 7 for ease of exposition.[6]

The first stage of the 2SRI models are shown in Table 3. These results indicated that the instruments are "strong" since they were both correlated with the potentially endogenous policy variables (they all have large *t*-statistics individually and were jointly significant in each first-stage model) and they also passed the usual tests for weak instruments, with partial-*F* statistics well over 200 in each of our models.

Table 4 shows the coefficients for the impact of state policies on the probability of any sex in the last 3 months, assuming exogenous policies in column 1 and endogenous policies in column 2. The purpose of this set of results was to check for the presence of policy endogeneity. Since all of the first-stage residuals were significant (column 2), we did find evidence of policy endogeneity.

We show the marginal effects of the impact of each policy on the probability of any sexual activity in the previous 3

[6] The full set of marginal effects are available from the authors upon request.

Table 4 Probit coefficients for probability of any sex in last 3 months

	(1) Exogenous policies	(2) Endogenous policies
State requires sex/HIV education but not content	−0.060***	−0.50***
	(−4.83)	(−5.30)
State requires sex/HIV education and mandates abstinence education	0.0083	0.17***
	(0.64)	(4.10)
State requires sex/HIV education and mandates contraceptive education	−0.020	−0.29***
	(−1.52)	(−4.83)
Student age	0.28***	0.27***
	(64.65)	(63.51)
Student is female	0.078***	0.081***
	(8.83)	(9.21)
Student is African-American	0.35***	0.36***
	(24.74)	(24.30)
Student is Hispanic	0.16***	0.19***
	(10.82)	(11.26)
Student is other race	0.00066	0.016
	(0.04)	(1.03)
Student self-assessed somewhat or very overweight	−0.13***	−0.14***
	(−14.63)	(−15.11)
Time	0.0041	−0.0083**
	(1.62)	(−2.42)
First-stage residual for mandatory contraceptive education in sex education		0.27***
		(4.45)
First-stage residual for mandatory abstinence education in sex education		−0.15***
		(−3.23)
First-stage residual for sex education but no content requirement		0.45***
		(4.68)
Constant	−4.92***	−4.78***
	(−68.47)	(−59.39)
Observations	302296	302296

Survey weights and sampling units used in estimation

*$p < .10$, **$p < .05$, ***$p < .01$

Table 5 Probit state policy marginal effects for probability of any sex in last 3 months. (Calculated at the mean of the data using coefficients from Table 3)

	(1) Exogenous policies	(2) Endogenous policies
State requires sex/HIV education but not content	−0.021***	−0.17***
	(−4.82)	(−5.30)
State requires sex/HIV education and mandates abstinence education	0.0029	0.060***
	(0.64)	(4.11)
State requires sex/HIV education and mandates contraceptive education	−0.0070	−0.10***
	(−1.52)	(−4.83)
Observations	302296	302296

Survey weights and sampling units used in estimation

*$p < .10$, **$p < .05$, ***$p < .01$

months in Table 5. Since the results in Table 4 confirmed that the policies are endogenous, we focused our interpretation on the marginal effect in column 2, which assumes endogenous policies. Local control, or requiring sexuality education but not directing the content, decreased the probability of sexual activity in the previous 3 months by − 17 percentage points.

Table 6 Multinomial logit coefficients for birth control choice

	Exogenous policy		Endogenous policy	
	Pr[Condom]	Pr[Hormonal BC]	Pr[Condom]	Pr[Hormonal BC]
State requires sex/HIV education but not content	0.21***	0.12***	0.90***	1.07***
	(6.64)	(2.74)	(4.36)	(3.50)
State requires sex/HIV education and mandates abstinence education	0.022	−0.054	−0.24**	−0.47***
	(0.80)	(−1.34)	(−2.43)	(−3.25)
State requires sex/HIV education and mandates contraceptive education	0.20***	0.25***	0.70***	1.13***
	(5.94)	(5.33)	(5.17)	(5.68)
Student age	−0.051***	0.32***	−0.047***	0.33***
	(−4.53)	(18.77)	(−4.09)	(19.61)
Student is female	−0.35***	0.35***	−0.35***	0.35***
	(−15.23)	(10.42)	(−15.32)	(10.16)
Student is African-American	−0.060*	−0.87***	−0.084**	−0.92***
	(−1.78)	(−14.14)	(−2.42)	(−14.72)
Student is Hispanic	−0.39***	−0.79***	−0.44***	−0.86***
	(−11.09)	(−12.97)	(−11.06)	(−13.53)
Student is other race	−0.33***	−0.59***	−0.36***	−0.64***
	(−7.86)	(−9.29)	(−8.40)	(−10.05)
Student self-assessed somewhat or very overweight	−0.20***	−0.20***	−0.19***	−0.19***
	(−7.82)	(−4.81)	(−7.49)	(−4.56)
Time	−0.025***	−0.030***	−0.0070	−0.0050
	(−4.38)	(−3.47)	(−0.86)	(−0.43)
First-stage residual for mandatory contraceptive education			−0.55***	−1.01***
			(−3.99)	(−5.04)
First-stage residual for mandatory abstinence education			0.22**	0.37**
			(2.06)	(2.43)
First-stage residual for sexuality education but no content requirement			−0.71***	−0.93***
			(−3.33)	(−2.94)
Constant	2.04***	−5.82***	1.76***	−6.32***
	(10.61)	(−20.22)	(8.58)	(−21.06)
Observations	100346	100346		

Survey weights and sampling units used in estimation

*$p < .10$, **$p < .05$, ***$p < .01$

Table 7 Multinomial logit state policy marginal effects for birth control choice (calculated at the mean of the data using coefficients from Table 5)

	Exogenous policies		Endogenous policies	
	Pr[Condom]	Pr[Hormonal BC]	Pr[Condom]	Pr[Hormonal BC]
State requires sex/HIV education but not content	0.040***	−0.0021	0.13***	0.046
	(6.02)	(−0.49)	(2.95)	(1.55)
State requires sex/HIV education and mandates abstinence education	0.0089	−0.0071*	−0.022	−0.031**
	(1.57)	(−1.91)	(−1.04)	(−2.23)
State requires sex/HIV education and mandates contraceptive education	0.028***	0.012***	0.082***	0.066***
	(4.08)	(2.70)	(2.82)	(3.48)
Observations	100346	100346		

Survey weights and sampling units used in estimation

* $p < .10$, ** $p < .05$, *** $p < .01$

Recall that by the end of our time period only a few of the states in our sample adopted this policy strategy; therefore, we were cautious in interpreting this result. Requiring sexuality education and mandating abstinence content raised the probability of sexual activity in the past 3 months by +6 percentage points, while mandating sexuality education and requiring contraceptive content decreased the likelihood of sex in the past 3 months by −10 percentage points.

The coefficients from the multinomial logit models for contraceptive choice are shown in Table 6. Again, we found evidence that the policies were endogenous since the first-stage residuals were significant in the behavioral regressions (column 2). For this reason, we again focused on the marginal effects for the 2SRI models that assumed policy endogeneity.

We present the marginal effects for the policy variables from the multinomial logit regressions predicting contraceptive choice in Table 7. Since we found that the policies are endogenous based on the results in Table 6, we only discuss the marginal effects assuming policy endogeneity in the second column of Table 7. Requiring sexuality education but leaving the content up to local districts raised the probability of condom use at last sex for sexually active youth by +13 percentage points but had no effect on the probability of hormonal birth control use. Mandating sexuality education and abstinence content had no effect on the probability of condom use at last sex but decreased the likelihood that a sexually active youth used hormonal birth control at last sex by −3.1 percentage points. Finally, requiring sexuality education and contraceptive content raised the probability of condom use by +8.2 percentage points and hormonal birth control use by +6.6 percentage points at last sex, conditional on youth being sexually active.

We conducted sensitivity analyses to check the robustness of our results. First, we estimated separate models for males and females. Based on the coefficients, we found similar results for males and females, and the coefficients from models estimated on those subsamples were qualitatively identical to the full-sample results. The one exception to this was that the male subsample only responds (positively) to sexuality education with contraceptive content with regard to condom use. Sexuality education with local control and sexuality education with abstinence content did not affect male condom use. We also ran a separate sensitivity analysis that broke the sample up into Caucasian and non-Caucasian groups. Like the first sensitivity analysis, the results were generally similar across the subsamples by race and (with one exception) the same as those estimated on the full-sample coefficients. We did find that the Caucasian subsample only increased hormonal birth control use in states that require sexuality education with contraceptive content, which differed from our full-sample and non-Caucasian sample results.

Discussion

Our findings suggest that laws requiring comprehensive sex education decrease sexual activity and raise contraceptive use for youth who are sexually active, while state policies that mandate abstinence only serve to increase sexual activity and decrease hormonal contraceptive use among youth who are sexually active.

Limitations

State sex education content policies aim to dictate what is taught in the classroom setting; however, there may be differences in implementation at the classroom level. It is possible that our models classified some youth as "treated" with a state policy when their specific teacher did not cover required material. If some students were not taught material required by the state policy, then they would not be able to respond. Any such zero responses were averaged in with the actual responses from students who were taught according to state policy; thus, any bias was toward zero and our estimated policy effects were conservative. Another limitation is that students receive information about sex, contraceptives, etc. from a multitude of sources, and, due to data limitations, we could not control for these in our analysis. There is some evidence that the most common source of information on sexual health for youth is from their friends, and the second most common source is sex education/teachers (Bleakley, Hennessy, Fishbein, & Jordan, 2009). Finally, our data were pooled cross sections and so we cannot track students who have moved between states and had their sex education under a different regime, nor can we observe how long in the past our respondents received sex education in their schools. These limitations would add noise to our policy indicators and again bias in the more conservative direction (toward zero).

Conclusion

Requirements that schools offer sexuality education either for pregnancy prevention or for disease prevention have been the subject of long-standing and contentious policy debates in the U.S. In recent decades, one of the primary areas of disagreement and policy diversity is the requirement that schools cover or stress abstinence as the only acceptable approach to youth sexuality. This stance has been ensconced in federal and many states' education laws. Despite the ongoing controversy, however, there is very little in the way of rigorous policy impact evaluation on the subject. To address this problem, we examined data on over 300,000 high school youth in 39 states from the 2003 through 2009 waves of the

YRBS, collected in cooperation between the CDC and state departments of education.

Overall, we found that details of state policies matter. In our preferred models, we characterized policies as being: (1) mandated sexuality education with local control over content; (2) mandated sexuality education with a requirement to teach abstinence; or (3) mandated sexuality education with a requirement to teach about contraception. In conducting the analyses, we also explored whether state policies can be treated as exogenous or whether omitted unobservable characteristics of the state introduce endogeneity bias. Our results suggested that, with the detailed policy variables, endogeneity bias is a concern, and so we corrected for it in our preferred models using an instrumental variables estimator.

We found evidence that mandating sexuality education with local control of the content decreased the probability that high school youth had sex within the 3 months prior to each survey and that requiring sexuality education and contraceptive content also decreased this probability. However, since only a few of the states in our sample opted for local control, we are more confident in the result for the policy of mandating sexuality education with contraceptive content. Although we found that these policies were protective against sexual activity in the prior 3 months, requiring sexuality education with abstinence content increased the likelihood of sex in the past 3 months. Obviously, most policy makers want to design educational requirements that reduce, rather than increase, the rate of teenage sexual activity in order to lower the chances of unintended pregnancies and sexually transmitted diseases. Our findings suggest that this goal is best met by requiring school districts to offer sexuality education that includes contraceptive content.

However, reducing the level of youth sexual activity is not the only policy goal states and districts have. Conditional on being sexually active, public health officials should want to improve the chances that a student chooses some method of pregnancy prevention (condom or hormonal) and disease prevention (condom). Requiring sexuality education but leaving the content up to local control helps accomplish both goals of pregnancy and disease prevention by increasing condom use. However, this conclusion is based on only a few states with this policy. Again, we found that requiring sexuality education with abstinence content can be harmful since it decreased the probability of hormonal birth control use at last sex for sexually active youth, although we found that this policy has no effect on the likelihood of condom use at last sex. As with the sexual activity outcome, we found that mandating sexuality education and requiring contraceptive content were beneficial with regard to pregnancy and disease prevention. The probability of both condom and hormonal birth control use generally increased with this policy stance.

In terms of policy implications, our results suggest that mandating sexuality education and leaving the content up to local control can be beneficial with regard to decreasing sexual activity and increasing condom use; however, we are concerned about suggesting this policy option due to the fact that these conclusions are based on observations from only four states by the end of our sample period. We did find more consistent policy implications when comparing requiring sexuality education and abstinence content versus requiring sexuality education and contraceptive content. We found that the former was associated with higher levels of sexual activity and lower levels of hormonal birth control use. The latter both decreased sexual activity and increased condom and hormonal birth control use. For policy makers interested in decreasing rates of sexual activity while simultaneously increasing the likelihood of contraceptive use of youth who are sexually active, our results suggest that requiring sexuality education (either sex of HIV/STD) and including contraceptive content should help achieve this goal. This suggestion is in line with work by Lerner and Hawkins (2016), arguing that sexuality education decisions should not only evidence-based, but also theory-informed to optimize adolescent welfare.

Ultimately, we found that state policies regarding sex and HIV/STD education do have statistically significant effects that are meaningful in magnitude from a public health perspective. In some sense, this suggests that the debates that policy makers, advocates, and community leaders have had with one another over the past several decades have been worth having. Real behavioral changes and public health consequences are at stake. When we examined data over a moderately long-time frame taken from the majority of states, we found that requiring sexuality education and contraceptive content was protective with regard to sexual activity and contraceptive use and that requiring sexuality education and abstinence content actually increased sexual activity and decreased some types of contraceptive use.

Compliance with Ethical Standards

Conflict of interest Danielle N. Atkins declares that she has no conflict of interest. W. David Bradford declares that he has no conflict of interest.

Ethical Approval This article does not contain any studies with human participants performed by any of the authors.

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