THREE ESSAYS ON THE EFFECTS OF HEALTH AND EDUCATION POLICIES ON TEEN CHILDBEARING

A Dissertation

by

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ABSTRACT

Despite a near-continuous decline over the past 20 years, the United States has maintained one of the highest teen birth rates in the developed world. Two main arguments support the notion that teenage pregnancy can be seen as a public health concern. First, both unintended pregnancy and teen motherhood are associated with adverse maternal and child health outcomes such as delayed prenatal care, premature birth and negative physical and mental health effects for children. Second, because 4 out of 5 teen pregnancies are unintended, teen mothers may be ill-equipped to raise children, and they may impose external costs on friends, family, and community members.

Historically, legislation aimed at decreasing teen pregnancy rates attempts to reduce or delay the initiation and frequency of sex and/or prevent risky sexual behavior. These types of policies include sex education mandates, legal access to contraception, and public funding for women's health clinics. In this dissertation, I use quasi-experimental methods to determine the effects of such health and education policies on teen pregnancy. My combined findings from my three working papers indicate that although abstinence-based sex education requirements do not affect teen birth rates or teen abortion rates, there may be some scope for Title X clinics to effectively lower teen pregnancy rates through increased access to contraception. Providing free long-acting reversible contraceptives to low-income women via publicly funded clinics reduces the teen birth rate by 5%-7%, while dramatically reducing family planning funding increases teen birth rates by approximately 5%.

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1. INTRODUCTION TO RESEARCH

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Historically, legislation aimed at decreasing teen pregnancy rates attempts to reduce or delay the imitation and frequency of sex and/or prevent risky sexual behavior. These types of policies include sex education mandates, legal access to contraception, and public funding for women's health clinics. Advocates for such policies argue that allowing teens to receive low-cost contraceptives as well as counseling on how to properly use condoms and other contraception devices will decrease risky sexual behavior and lower teen pregnancy rates. Critics of government-assisted family planning programs argue that giving teens and low-income women free contraception is a moral hazard problem and will increase sexual frequency, and, as a result, unintended pregnancy rates.

In this dissertation, I use quasi-experimental methods to determine the effects of various health and education policies on teen pregnancy. My combined findings from my three working papers indicate that although abstinence-based sex education requirements do not affect teen birth rates or teen abortion rates, there may be some scope for Title X clinics to effectively lower teen pregnancy rates through increased access to contraception. Providing free long-acting reversible contraceptives to low-income women via publicly funded clinics reduces the teen birth rate by 5%-7%, while dramatically reducing family planning funding increases teen birth rates by about 5%.

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

2.1 Introduction

Despite the resources spent on lowering teen birth rates, nearly 330,000 U.S. teenagers became mothers in 2011, placing the U.S. 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

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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 $10.9 billion in 2008 (National Campaign, 2013).

[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).

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.

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 has 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

[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.

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 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]

2.2 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]

[4]For example, in 2011 alone nearly $200 million 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.

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 $50 million 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 6 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.

[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, 2010).

[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 9th grade textbooks in order to comply with a new state law that required educators to stress abstinence (Donovan, 1998).

2.3 Data

We use state-level policy data on mandated sexual education curriculum from monthly reports from the Alan Guttmacher Institute (AGI). Since 2000, AGI 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 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 A.1 for more details on the content requirements and policy language of each treatment state's stress abstinence policy.

We use the twenty-one 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 control group since it improves the match on trends prior to the enactment of

[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 percent reported in Panel A Column 5 of Table A.3. 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.

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 (NCHS), Division of Vital Statistics natality files (NCHS Natality Files, 2000-2014).[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, comprised 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 (CDC NCHHSTP Atlas, 2015). Rates for each health measure were calculated using the number of cases per 1,000 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 Abortion Surveillance System (CDC Abortion Surveillance, 2005-2012). The CDC is the only source to publish annual estimates on abortion rates by state and age group. Unfortunately, as of 2012 these data are currently available only up

[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 B.1 and Columns 6 and 9 in Table B.2 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 females aged 15-19, multiply by 1,000, and divide by the state population of females aged 15-19. To calculate STD rates, we used the entire teen population aged 15-19.

to 2009. Because centers are not required by law to annually submit abortion data, there are some inconsistencies within these estimates. Therefore, we omit eleven 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 this 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 (CPS) and the Bureau of Labor Statistics (BLS) on median family income and unemployment rates by state, respectively (BLS, 2000-2014). Population data from the Census Bureau provided 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 (NARAL, 2000-2004; NARAL 2005-2011). 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 A.17. Birth rates across treatment and

[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 A.6. These estimates are statistically similar at the 99% level.

[17] Due to 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.

control states average 39 per 1,000 teenage females over the sample period, with the younger cohort, teens aged 15–17, responsible for nearly half. STD rates across states average approximately 21 per 1,000 teens, with a standard deviation of 7.3. Teen abortions average 15 per 1,000 females, although, as previously mentioned, this is expected to be underestimated (Blank et al., 1996).

2.4 Methods and Identification

2.4.1 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:

$$y_{it} = \beta \, abstinence \ policy_{it} + \gamma X_{it} + \alpha_i + \lambda_t + u_{it} \tag{2.1}$$

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 unemployment rates. 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 to control states. Since 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 since, for example, a 10% decline in the teen birth rate in Maine represents an absolute decrease of about 2.5 births per 1,000 teen females, while a 10% decline in Arizona represents a decrease of about 7 births per 1,000 teen females.[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, weighted least squares 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

[18]Our results are not sensitive to this assumption. When estimating effects on birth rates, we get a statistically insignificant effect of 0.74, or 1.9%, which corresponds to the 1.2% effect of logged birth rates reported in Table A.3 Panel A Column 5. These estimates are not statistically different at the 99% level. See Tables B.4-B.6 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 A.3 Panel A Column 5 when clustering is 0.62, compared to 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 if the average state teen population from 2000–2011.

indicator variables:

$$y_{it} = \beta_0 \; policy\ enacted_{it} + \beta_1 \; policy\ enacted_{it-1} + ... + \beta_4 \; policy\ enacted_{it-4+}$$
$$+ \delta_1 \; policy\ enacted_{it+1} + \delta_2 \; policy\ enacted_{it+2} + \gamma \; X_{it} + \alpha_i + \lambda_t + u_{it} \quad (2.2)$$

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 due to their enactment year. For this reason, we create a lag denoting that enactment occurred four or more years before (the 4+ years lag).

2.4.2 Identification

We estimate the leading indicators in Equation (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.

2.5 Results

Before presenting model-based estimates, we first show a graphical analysis that corresponds to our difference-in-differences identification strategy. Figure A.1 graphs the estimated lags and leads from Equation (2) to test for the divergence in trends between treatment and control groups prior to the abstinence mandate. Figure A.1 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. Figures A.1 Panels B and C similarly graph the estimates over time for logged teen sexually transmitted disease rates and abortion rates, respectively, and show 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.

2.5.1 Effects on Teen Birth Rates

Table A.3 presents estimates of the effect of a stress-abstinence mandate on logged teen birth rates based on OLS, WLS and Poisson models, as described by Equation (1). Panel A presents the average treatment effect from a difference-in-differences model, while Panel B presents estimates from models that include lagged and leading indicator variables. 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 one- and two-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 teen females 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.

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 teen females 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%.

2.5.2 Effects on Teen Sexually Transmitted Disease Rates

Behavioral changes may also cause changes to sexually transmitted disease (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 A.4 and A.5.

Weighted least squares estimates in Table A.4 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 contradicting results can signal heterogeneous effects across states of different sizes (Solon et al., 2013), and smaller states are more likely to be driving the perceived increase.

To explore this heterogeneity, in Table A.5 we replicate Table A.4 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 two 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 A.4.[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 due to the enactment of abstinence-based sex education mandates. Thus, this finding could be indicative of increased risky sexual behavior and/or higher transmission rates due to increased stigma of STD screening and treatment.

2.5.3 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 asses this hypothesis by estimating Equations (1) and (2) for logged teen abortion rates.

Results for logged teen abortion rates are shown in Table A.6.[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

[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 A.4. These estimates are statistically indistinguishable at the 99% level.

[22] The regressions for Table A.6 account for a smaller subset of states since 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 due to data fluctuations and inconsistencies. See section 3 for a more detailed explanation. Our results are not overly sensitive to this selection. See Table B.7 for a replication of Table A.6 when these 11 states are included in the analysis.

unaffected by sex education policy changes.

2.6 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 A.7, 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 above 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

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

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 B.8 provides estimates from a triple differences model in which we use 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.

Due to 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 weighted and unweighted least squares estimates for the effect of stress-abstinence policies on logged birth rates for white, black, and Hispanic teens, by column. Mirroring previous results, we find no policy effects for any subgroup.

2.7 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 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 2 births per 1,000 teens, or a 6% change in teen birth rates. This suggests state policies are relatively ineffective at reducing unintended pregnancy as compared to 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.

3. HOW MUCH CAN EXPANDING ACCESS TO LONG-ACTING REVERSIBLE CONTRACEPTIVES REDUCE TEEN BIRTH RATES?

3.1 Introduction

Despite a near-continuous decline over the past 20 years, the teen birth rate in the United States continues to be 6 to 12 times that of other developed countries (Kearney and Levine 2012). Two types of economic arguments support the view that the high rate of teenage childbearing in the United States should be a focus of public policy. The first is based on the idea that teenagers are often not well-positioned to take care of children; as a result, teen childbearing disproportionately imposes costs on family, friends, communities, and public assistance programs. Unless teenagers fully internalize such costs when they make decisions, we would expect them to have children "too often" from a social welfare perspective. The second type of argument focuses on the costs that teenagers' choices impose on teens themselves. Although such arguments carry little weight where standard economic models of behavior can be applied, the extremely high rates of unintended pregnancies among sexually active teens—more than twice the rate of older women (Finer 2010)—suggest that *homo economicus* does *not* apply to teens making choices about sexual activities. It also suggests that policies aimed at reducing unintended pregnancies have the potential to improve teenagers' welfare while reducing the negative externalities associated with teenage childbearing.

There is a long history of policies and initiatives in the United States geared towards reducing unintended pregnancies, particularly among teens.[1] These approaches typically involve attempts: (1) to delay or reduce the frequency of sexual

[1] See Bailey, Guldi, and Hershbein (2013) for an overview of reproductive health policies and various approaches to estimating their causal effects.

intercourse; and/or (2) to increase the use of contraceptives or promote the use of more-effective contraceptives. That said, the results of such policies have often been disappointing. Less than half of published studies that use experimental or quasi-experimental approaches to evaluate comprehensive sex education programs report significant effects on the initiation of sex, frequency of sex, or contraceptive use (Kirby 2008).[2] A randomized control trial of the Parent's Speak Up National Campaign, which promotes parent-child communication about waiting to have sex, finds no effect on adolescent's beliefs that "waiting to have sex is the best way to prevent health risks like pregnancy or HIV/STDs" (Palen et al. 2011). Moreover, it is not clear whether the wave of state policies expanding access to birth control pills during the 1960s and 1970s reduced teen pregnancies (Guldi 2008; Bailey 2009; Ananat and Hungerman 2012; Myers 2012). That said, family planning programs appear to offer significant promise where these other policies do not. Bailey (2012) shows that the establishment of federal family planning programs in the 1960s and 1970s reduced teen birth rates 2.3 percent after 6–10 years. Kearney and Levine (2009) provide more recent evidence on the effects of family planning services in their study of state Medicaid policy changes that expanded access to higher-income women during the 1990s and 2000s: they find that these policy changes reduced teen childbearing by over 4 percent and argue that this effect was accomplished by increased use of contraceptives.

The research described above indicates that family planning services do play a critical role in averting unintended pregnancies and births among teenagers. Yet,

[2] Kirby's (2008) review considers 48 studies of comprehensive programs. It also considers nine abstinence programs, four of which have experimental designs. While some of the non-experimental studies reviewed found significant effects on the initiation of sex and frequency of sex, the experimental studies did not. Moreover, *none* of the studies found significant effects on contraceptive use. More recently, Carr and Packham (forthcoming, 2016) show that state-level abstinence-based sex education mandates have no effect on birth rates or abortion rates.

with over three-quarters of teen births unintended at conception (Mosher et al. 2012), it would seem that there may be some scope for such services to play an even larger role.[3] And because half of those births are to teens using contraception (CDC 2012), many have argued that leveraging recent technological advances could be key. In particular, long-acting reversible contraceptives (LARCs), which include both sub-dermal implants and intrauterine devices (IUDs), are extremely effective at preventing pregnancy. Whereas incorrect and/or inconsistent use of birth control pills, injectables, patches, and rings leads to failure rates between 6 and 9 percent and failure rates of 18 percent for condoms, LARC methods have failure rates of less than 1 percent because they do not require the user to do anything for at least 3 years after the initial procedure.[4] The American College of Obstetricians and Gynecologists' (ACOG) Committee on Adolescent Health Care and the American Academy of Pediatrics both have stated that LARC methods should be "first-line recommendations" for all adolescents (in 2012 and 2014, respectively). LARCs also were the focus of the Centers for Disease Control and Prevention's April 2015 report, "Preventing Teen Pregnancy" (CDC 2015). That said, only 5 percent of American teens who use contraceptives use a LARC method.[5] This low rate of use appears to be due in large part to a lack of awareness, misperceptions about safety, and costs. When these barriers were removed 70 percent of participants aged 14-20 in the St. Louis Contraceptive CHOICE Project chose a LARC method (Mestad et al. 2011). Nonetheless, a fundamental policy question remains unanswered: how much can expanding access to LARCs reduce teen birth rates?

[3] "Unintended" in this context typically refers to situations in which a child was born to a mother who did not want a child (or another child) or who instead wanted to have a child at a later date.

[4] Failure rates are calculated as the number out of every 100 women who experienced an unintended pregnancy within the first year of typical use. See `http://www.cdc.gov/reproductivehealth/UnintendedPregnancy/Contraception.htm`.

[5] Authors' calculation using the 2011–2013 Survey of National Survey of Family Growth.

To answer this question, we consider the first large-scale policy intervention to promote and improve access to LARCs in the United States. Specifically, we examine the Colorado Family Planning Initiative (CFPI), a $23 million program funded by an anonymous donor that began in 2009 with the primary goal of helping low-income women gain access to LARCs through Title X clinics. The state of Colorado has pointed to the subsequent 40 percent reduction in its teen birth rate as evidence of the program's success.[6] However, the fact that teen birth rates fell significantly throughout the United States during the same period suggests that other factors probably contributed to the decline observed in Colorado. The goal of this paper is to separate out the effects of the policy initiative from the effects of these other factors in order to better understand the way in which a major investment in LARCs can affect teen outcomes.

After showing large increases in LARC use among teens visiting clinics participating in the CFPI, we estimate the effects of the CFPI on teen birth rates using an array of quasi-experimental identification strategies with different strengths and weaknesses: (1) a difference-in-differences design that compares changes in Colorado counties with Title X clinics to changes observed in other U.S. counties with Title X clinics; (2) a difference-in-differences design that compares changes in Colorado counties with Title X clinics to changes in Colorado counties without such clinics; (3) a triple-differences design that compares changes in Colorado counties with Title X clinics to changes in Colorado counties without such clinics relative to what is observed in other states; and (4) a state-level synthetic-control design.[7]

The results of these analyses indicate that the success of the CFPI may have been

[6]They also attribute reductions in the teen abortion rate and WIC caseloads to the initiatives. The press release with these statements can be accessed at: `http://www.colorado.gov/cs/Satellite/GovHickenlooper/CBON/1251655017027`.

[7]Scott Cunningham and Christine Piette Durrance also have work in progress estimating the effects of the CFPI using a state-level synthetic control design.

overstated by time-series comparisons; however, it *has* led to significant reductions in teen birth rates. Across the different research designs we consider, the estimates indicate that the CFPI reduced teen birth rates in affected counties by 4.6–7.9 percent over four years, driven by larger effects in its second through fourth years and in counties with relatively high poverty rates. Our preferred estimates are based on the first approach because it yields comparatively precise estimates and because it allows for a separate consideration of high- and low-poverty counties. Based on this approach, the estimates indicate an effect of 5.6 percent over four years, driven by an effect of 7.3 percent in the CFPI's second through fourth years. For counties with poverty rates above Colorado's median, the estimates indicate an effect of 7.3 percent over four years, driven by an effect of 9.4 percent in the CFPI's second through fourth years. As these estimates are based on births to all teenagers, they can be thought of as intent-to-treat estimates that thus understate the effects on teenagers who use Title X clinics and teenagers receiving LARCs through the initiative. The estimated effects on STDs and abortion are inconclusive.

The remainder of this paper is organized as follows. In the next section we discuss LARCs in the context of the contraceptive options that are presently available to teenagers in the United States. We then provide further details on the CFPI. Next we describe our empirical approaches and the results of our analysis, and finally provide some concluding thoughts.

3.2 Background

3.2.1 Long-Acting Reversible Contraceptives (LARCs)

LARCs include intrauterine devices (IUDs) and sub-dermal implants. IUDs are flexible, T-shaped devices that must be inserted and removed by a doctor. The most popular IUDs include the copper IUD, Paragard, and the plastic IUD, Mirena, which can protect against pregnancy for 12 and 5 years, respectively. For both types of IUD the primary mechanism of action is the prevention of fertilization by inhibiting sperm motility. Sub-dermal implants, such as Implanon and Nexplanon, consist of a matchstick-sized rod that contains etonogestrel. The rod is inserted into the inside of the non-dominant upper arm and can remain in place for up to 3 years.

Table A.8 provides information on the various contraceptive options that are currently available and shows that implants and IUDs are as effective at preventing pregnancy as sterilization. During the first year of typical use, fewer than 1 in 1,000 women using an IUD or implant become pregnant. This is true with respect to "perfect use" and "typical use" of these methods because they require nothing of the user after an initial doctor's visit for insertion, thus eliminating the potential for user-compliance error. In contrast, oral contraceptives and condoms are not foolproof and have typical-use effectiveness rates of only 91 percent and 82 percent among all women, respectively, and 80 percent and 82 percent among teenagers under the age of 18 (Dinerman et al., 1995; Grady et al., 1986).

Moreover, because LARCs are invisible, they may be an especially attractive option for teens who do not want their parents to find out they are sexually active.[8] And while LARCs have high upfront costs, they can remain in place for up to 12 years. Therefore, they may be cheaper than other contraceptives in the long run.

Despite the ease of use and the benefits of LARCs, merely 5 percent of the 3.2 million teenage women using contraceptives in the United States chose an implant or IUD in 2013, and only 8.5 percent of all U.S. women using contraceptives choose a LARC (Guttmacher 2014; National Center for Health Statistics NSFG, 2014). This figure stands in stark contrast to other countries where, for example, 41 percent of

[8] In fact, 68 percent of teens report that the primary reason they do not use birth control is because they are afraid their parents will find out (The National Campaign to Prevent Teen and Unplanned Pregnancy 2015).

women in China use a LARC and in Europe rates vary between 6 percent and 27 percent.[9]

There are several potential explanations for the low rate of LARC use among U.S. teens. First, teens may be unaware that LARCs are a viable option. Second, there may be misconceptions about safety and protecting against sexually transmitted diseases (Bharadwaj et al. 2012). Third, insertion is uncomfortable and sometimes painful, and LARCs may cause side effects, such as menstrual pain and bleeding, spotting, headaches, nausea, and mood changes, although these side effects are similar to those associated with other hormonal birth control methods.[10] Fourth, teens may be discouraged by the high upfront costs of the devices. Out-of-pocket costs for implants and IUDs are upwards of $400, and even insured teens may pay up to a $160 copayment to receive a LARC (Trussell et al. 2009; Planned Parenthood, 2014). In support of the importance of this consideration, Mestad et al. (2011) find that 70 percent of adolescents who are aware of the benefits of LARCs choose a LARC when it is offered at no cost.

Interacting with these demand-side factors, there are two main supply-side barriers to LARC access that contribute to the low rate of LARC use among U.S. teens. First, doctors and nurses may themselves be unaware or misinformed about LARC technology, and they must be trained on proper LARC insertion/removal in order to provide them to patients.[11] Second, health clinics that provide free and low-cost

[9] See Finer et al. (2012) for more details. Rates available for European countries are as follows: Austria, 15 percent; Baltics: 14 percent; Czech Republic, 10 percent; Denmark, 18 percent; France, 17 percent; Germany, 10 percent; Spain, 6 percent; Sweden, 21 percent; and UK, 11 percent.

[10] More serious and rare side effects can occur for patients with IUDs and include pelvic inflammatory disease, uterus perforation, and ectopic pregnancies. Risk of pelvic inflammatory disease occurs in 1 in 100 cases, and is no greater with an IUD than the risk to the general population. Uterus perforation occurs in less than 1 in 1000 cases. Ectopic pregnancy is the most serious and rare possible side effect of an IUD. In rare events in which a women becomes pregnant while using an IUD, the risk of having an ectopic pregnancy ranges from 6–50 percent (Grimes, 2007).

[11] The importance of this barrier is documented in Harper et al. (2015), which studies LARC take-up among 18–25 year-old women in a randomized control trial that provided clinics with

contraceptives often cannot afford to offer LARCs to many clients—many Title X clinics do not offer LARCs at all, and those that do usually have to offer them to clients selectively.[12] As discussed in greater detail below, the CFPI sought to improve access to LARCs on a major scale by providing training and assistance to clinics *and* by providing clinics the funding they needed to purchase LARCs to make them available to their clients.

3.2.2 The Colorado Family Planning Initiative and Contraceptive Use

In January 2009 the Colorado Department of Public Health and Environment (DPHE) implemented the CFPI in an attempt to reduce unintended pregnancy via increased access to long-acting reversible contraception.[13] The Colorado DPHE received $23 million in provisional funding from an anonymous donor to provide free LARC methods to low-income women in Title X clinics. All of Colorado's 28 agencies accepted funding, which was to be distributed to Title X clinics in 37 counties through June 2015. Money was allocated proportionally to agencies based on their number of clients and the predicted number of LARC insertions in the following year.

The CFPI provided support for three main objectives: supplying free IUDs and

evidence-based training on on how to counsel patients and how to insert IUDs and implants. Harper et al. (2015) also considers pregnancy, though their estimates are based on only 80 additional women choosing LARCs at treatment clinics. Moreover, they do not discuss the degree to which participating clinics and participating individuals from these clinics compare to the broader populations from which they were drawn, which raises additional concerns about generalizability. This concern is highlighted by the fact that participating individuals from clinics in the control arm increased LARC usage by 12 percentage points, which is highly unusual relative to national trends (discussed in Section 3.2.2). In any case, they do not find a significant effect on pregnancy overall in their one-year followup. Stratifying the estimates by visit type, they find that the intervention reduced pregnancy rates by 51 percent among women attending family planning visits and increased pregnancy rates by 19 percent among women attending abortion care visits. The former estimate is statistically significant while the latter is not, and because of the small sample size, the confidence intervals are large.

[12] Just 39 percent of all Title X clinics in 2010 offered implants, and only 63 percent provided IUDs (FPAR 2013).

[13] Our description of the implementation of the Colorado Family Planning Initiative draws heavily from conversations with the Colorado Department of Public Health and Environment and the detailed discussion provided in Ricketts et al. (2014).

contraceptive implants to low-income women; equipping staff and providers with more knowledge about LARC insertion, promotion, and counseling; and providing technical assistance for billing, coding, and clinic management. Additionally, the CFPI offered general assistance to Title X agencies to increase the utilization of LARCs and supported the provision of NuvaRing, tubal ligations, and vasectomies. However, the use of the NuvaRing remained fairly constant at roughly 5 percent among teen clients after the CFPI was implemented, and tubal ligations and vasectomies are extremely rare among teens.[14]

Title X clinics receive federal and state funds to provide free or low-cost counseling, sexually transmitted disease screening, and contraceptives. At Colorado Title X clinics anyone at or below 100 percent of the poverty level pays nothing, and no client is denied services because of an inability to pay. Patients who earn between 101 and 250 percent of the poverty level pay a discounted rate; clients earning more than 250 percent of the poverty level pay the full cost of the visit. Agencies must accept verbal communication of income and no verification is required.

In Colorado, 90 percent of Title X clients fall into the “very low income" bracket, meaning that nearly all clients pay nothing for contraceptives and doctor visits. The high upfront costs of LARC devices paired with the sliding fee schedule meant that in the past many clinics could not afford to provide implants and IUDs. At clinics that did supply LARCs prior to the CFPI, the devices were inserted only for women who subjectively were considered the most “at risk" for an unintended pregnancy. The CFPI funding was critical for all Title X clinics to be able to stock and provide these highly effective contraceptives to clients. In 2009, 20 out of 28 agencies offered IUDs for the first time, and 16 agencies offered the implant for the first time. At

[14]Nuvaring is a vaginal ring inserted once a month and left in place for three weeks. Like birth control pills, it prevents pregnancy by releasing estrogen and progestin.

the end of the first year of the initiative, all agencies offered IUDs and all but one agency offered implants.

Figure A.2 shows how the primary method of contraception used by female teenagers (ages 15-19) visiting Colorado Title X clinics has evolved over time. In 2008, the year before the initiative began, LARCs had a usage rate of less than 3 percent, which was lower than usage rates for condoms, injections, rings, and birth control pills. By 2014, LARC take-up among teens had risen to nearly 25 percent, surpassing all methods except oral contraceptives. This increase in LARC use is mirrored by a decline in the use of oral contraceptives, indicating that the initiative led to a substitution of LARCs for oral contraceptives. That the substitution appears to be along this margin has important implications for the effects on pregnancy. Most obviously, we would expect this sort of substitution to reduce pregnancy, because LARCs are more effective than oral contraceptives. It also suggests that we would likely expect the effects to be smaller than if the program instead caused substitution away from condoms (as the primary form of contraceptive), because condoms are less effective than oral contraceptives.

Notably, these statistics will almost certainly understate the degree to which LARC use has increased among teenagers served by Title X clinics, because they are based on annual clinic *visitors*, and the long-acting quality of LARCs is expected reduce the likelihood of a return visit to a clinic. This issue is reflected in the fact that a total of 20,377 women of all ages had a LARC inserted between 2009 and 2012, yet only 9,644 of clients visiting a clinic in 2012 had a LARC. Insertion data is not available by age group; however, assuming that the same ratio of visitors using LARCs to cumulative LARC insertions holds for teenagers implies 4,505 insertions between 2009 and 2012.

Further demonstrating this large increase in LARC use, Figure A.3 shows that the increase in LARC use among teens visiting Colorado clinics stands apart from what has happened across the United States as a whole.[15] In particular, despite starting at the same low rate in 2008, LARC usage among teens visiting Title X clinics across the U.S. only grew to approximately 6 percent by 2013 versus 21 percent for Colorado. Further demonstrating what an outlier Colorado has become in promoting the use of LARCs among teens visiting Title X clinics, Figure A.4 presents a state-by-state comparison of teen LARC usage by Title X clients in 2013. It shows that only six states have LARC usage above 11 percent, and Colorado has the highest usage rate at over 21 percent. As a whole, these statistics support the notion that Colorado clinics were successful at introducing teens to highly effective contraceptive methods after the implementation of the CFPI.

Figure A.5 shows the number of teen females visiting a Title X clinic in Colorado over time along with the number of teens visiting a clinic whose primary method of contraception was a LARC. These are of interest because a spike in the number of clients after the program was implemented could suggest that CFPI was effective in attracting new clients to Title X clinics. In fact, Figure A.5 does *not* show a spike in the number of clients; rather, it shows an increase and a subsequent decrease in the number of clients, which is consistent with the fact that the teen population increased and then decreased over the same period.

3.3 Empirical Approach

This section details the data used in our analysis and our strategies for estimating the causal effects of the CFPI.

[15]Numbers for Colorado are authorsâĂŹ calculation based on annual data on Colorado Title X contraception usage by age and method provided by the Colorado Department of Public Health and Environment. Numbers for the United States overall are taken from the Title X Family Planning Annual Report (U.S. Department of Health and Human Services 2013).

3.3.1 Data

Because all Title X agencies accepted CFPI funding, we define all Colorado counties with Title X clinics in 2008 (the year before the CFPI was implemented) as treated.[16] We can identify such counties based on clinic addresses in the Colorado Department of Public Health and Environment's Directory of Family Planning Services. For comparison, we identify counties with Title X clinics in 2008 outside of Colorado by geocoding the addresses of such clinics listed in the U.S. Department of Health and Human Service's 340B Database (US HHS OPA, 2014). According to the National Family Planning and Reproductive Health Association, over 90 percent of Title X clinics participate in the 340B Drug Pricing Program and thus would be reflected in the database (NFPRHA 2013).[17] Figure A.6 depicts counties identified as having, versus not having, Title X clinics using this approach. In total, 72 percent of counties are identified as having a Title X clinic in 2008, accounting for 93 percent of the population of female teenagers in the United States.

In order to estimate the effect of the initiative on teen births, we use restricted-use natality files provided by the National Center for Health Statistics from 2002–2013 (NCHS Natality Files, 2000-2014).[18] These data consist of a record of every birth taking place in the United States over this time period. They include information on the mother's age and the county of the birth, both of which are critical to our analysis, in addition to other details on the mother, the father, and the child. We assign births to the year of conception based on the mother's last menstrual period where

[16]Measures of treatment intensity at the county level are unavailable and cannot be constructed because funding data are available at the agency level and most agencies have clinics across several counties.

[17]For Colorado, one of 37 counties would have been excluded from the analysis if we solely used data from the 340B Database.

[18]The choice of the initial year used for the analysis is motivated by the fact that Broomfield County, Colorado, split off from Adams, Boulder, Jefferson, and Weld counties in November of 2001.

available; otherwise we assume a gestation period of nine months. This approach results in incomplete data on births conceived in 2013; thus, we restrict our analysis to 2002–2012 after using the 2013 natality file to construct our measure of teen births conceived in 2012. We use these data in conjunction with population counts from the National Cancer Institute's Surveillance, Epidemiology, and End Results Program (SEER) in order to consider teen birth rates in our analysis.[19]

To control for time-varying county characteristics, we use SEER data to construct measures of teen demographics (fraction of 15–19 year old females, fraction black, and fraction Hispanic at each age). We measure county-level economic conditions using unemployment rates from the Bureau of Labor Statistics (BLS 2000-2014). Lastly, we include two indicator variables to help capture the broader policy environment around access to contraceptives in a state and year: whether over-the-counter access to emergency contraceptives is permitted, and whether private insurance plans covering prescription drugs are required to cover any FDA-approved contraceptive. These variables are constructed using data collected from the National Conference of State Legislatures (2012), the National Women's Law Center (2010), and Zuppann (2011).

While nearly all of our analyses focus on teen birth rates, which are reliably measured at the county level based on birth records, we also consider the impacts of the CFPI on sexually transmitted diseases (STDs) and abortions. This analysis relies on state-level data collected by the Centers for Disease Control and Prevention (CDC NCHHSTP Atlas 2015). Our analysis of STDs restricts attention to chlamydia and gonorrhea, because labs and doctors are required by law to report cases of these

[19]SEER population estimates are based on an algorithm that incorporates information from the Census, Vital statistics, IRS migration files, and the Social Security database (SEER Program, 1969-2013). Note that we omit from the analysis one county that has a Title X clinic and zero teen females in a year.

diseases. There are no such reporting requirements for abortions, and these data are known to contain omissions and inconsistencies (Blank et al., 1996). Moreover, as of the writing of this paper they are only available through 2011. Thus, our analysis of these abortion data spans 2003-2011 and omits the 18 states that have no annual data for any year during this period (CDC Abortion Surveillance, 2005-2012).

The summary statistics for the variables used in our county-level analysis of birth rates are shown in Table A.9. In particular, this table separately shows the means for Colorado counties with clinics receiving CFPI funding and counties outside of Colorado with Title X clinics, both before and after the CFPI was implemented. These statistics show that teen birth rates were similar in the treated counties and the comparison counties prior to the CFPI, approximately 41 per 1,000 annually. Additionally, they indicate a divergence following the implementation of the CFPI: the teen birth rate declined 32 percent (to 28 per 1,000) in the treated counties and only 24 percent (to 31 per 1,000) in the comparison counties. While this simple comparison provides some useful evidence on the effect of the CFPI on teen birth rates, the empirical analyses described below consider how these effects vary over time and address a wide set of potential confounders, including differences in trends and differential changes in demographics, economic conditions, and state-wide policies.

3.3.2 Identification Strategies

Our preferred approach for estimating the effects of the Colorado Family Planning Initiative is a difference-in-differences design that uses counties with Title X clinics outside of Colorado as the comparison group for Colorado counties with clinics receiving funding (i.e., those with Title X clinics). The identifying assumption underlying this approach is that the proportional changes in birth rates in the comparison counties provide a good counterfactual for the proportional changes that would have been observed in the Colorado counties in the absence of the initiative. We discuss the validity of this identifying assumption in greater detail below.

Given the discrete nature of the births, and because we sometimes have county-year cells with zero teen births, our preferred approach is to use a Poisson model.[20] In particular, our main results are based on Poisson models of the following form:

$$E[TBR_{ct}|CFPI_{c,t-k}, \alpha_c, \alpha_t, X_{ct}] = exp(\sum_{k=1}^{4} \theta_k CFPI_{c,t-k} + \alpha_c + \alpha_t + \beta X_{ct}) \quad (3.1)$$

where TBR_{ct} is the teen birth rate for county c in year t, $CFPI_{c,t-k}$ is an indicator variable that takes a value of one for Colorado counties k years after the CFPI began and zero otherwise, α_c are county fixed effects to control for any systematic differences across counties, α_t are year fixed effects to control for shocks to teen birth rates that are common to all counties in a year, and X_{ct} can include time-varying county or state control variables. Because Poisson models are more typically thought of as considering counts, not rates, we note that this model can be expressed alternatively estimating the natural log of the expected count of births while controlling for the population of female teens and constraining its coefficient to be equal to one. We also present the results of weighted least-squares analogues to Equation 1 (adding one to the count of births for all county-year cells). All analyses allow errors to be correlated within counties over time when constructing standard-error estimates.[21]

Two reasons make it important for the model to allow the estimated effects to vary across years with a set of indicator variables rather than considering the coefficient on a single "post-treatment" indicator. First, the nature of contraceptive choice,

[20] Like linear models, the Poisson model is not subject to the incidental parameters problem associated with fixed effects because they can be eliminated from the model. We relax the assumption of equality between the conditional mean and variance by calculating sandwiched standard errors.

[21] We note that this approach leads to more conservative estimates than those that instead allow for clustering at the state level. Also, we discuss the results of permutation-based inference in the results section.

sexual activity, and childbearing all would suggest that any effect would appear some time after the program's implementation, even when we assign births to their year of conception. In particular, the share of sexually active teens using LARCs is expected to increase over time as they visit clinics and, more generally, become increasingly aware of this option, as is evident in Figure A.2 and A.3. Moreover, teen sexual encounters are often irregular, and sexual encounters only lead to pregnancy with some probability.

Second, we estimate models that include county-specific linear trends in order to address concerns that differences in the pre-existing trends between counties with Title X clinics in Colorado and counties with Title X clinics in other states might bias the estimates derived from Equation 3.1.[22] As explained in Wolfers (2006), estimates of such trends will be biased—as will the estimates of other parameters—when a model does not fully account for time-varying treatment effects. In plain terms, a time-varying treatment effect implies an effect on trends, which in turn implies that including trends that are identified in part by the post-treatment data would be "overcontrolling" (i.e., controlling for an endogenous variable), which can lead to significant bias. This source of bias is not an issue if the post-treatment observations do not contribute to the estimates of the trends; this can be accomplished by allowing the estimated effects to vary over time in a fully non-parametric fashion. In our case, it entails allowing the effect to vary across years. Nonetheless, we note that the estimated effects for each year are sometimes imprecise. As a result, we may prefer to focus on their average across years and on the statistical significance of their average across years.

[22]We have also examined our main results based on models that allow for county-specific quadratic trends. This alternative approach yields estimated effects that are slightly larger but much less precise, as it leads to standard error estimates 50-75 percent larger than those based on the model with county-specific linear trends.

The empirical strategy described thus far is our preferred approach. It has superior power to the alternative quasi-experimental strategies we consider, because it uses a large number of counties for both the treatment group and the comparison group and it allows for a rich set of control variables to reduce the residual variation of the estimated models. That said, these alternative quasi-experimental strategies have advantages of their own and rely on different identifying assumptions. Therefore, they contribute to this study's overall rigor.

We show the results from three alternative strategies. The first alternative strategy is identical to our preferred approach, but uses Colorado counties without Title X clinics as the comparison group (instead of counties outside of Colorado with Title X clinics). The advantage of this approach is that it addresses concerns about states changing their health care policies over the period of time studied in ways that are otherwise difficult to control for. However, a potential disadvantage of this approach is that individuals in Colorado counties without Title X clinics may still have been affected by the policy, because they might travel to and obtain services from the clinics in counties with Title X clinics. We have empirically examined this possibility with an analysis of counties without Title X clinics using our preferred empirical strategy, and found no evidence of such effects. Also, we note that we would expect any such effects to bias our estimated effects towards zero. A further disadvantage of this approach is that there are only 27 counties in Colorado without Title X clinics, and they are relatively small.

Our second alternative strategy is a triple-difference design, comparing the change in outcomes in Colorado counties with Title X clinics to the change in Colorado counties without such clinics relative to what is observed in other states. We operationalize this design using a Poisson model of the following form:

$$E[TBR_{cst}] = exp(\sum_{k=1}^{4} \theta_k CFPI_{cs,t-k} + \alpha_{cs} + \alpha_t + \alpha_{st} + \omega_t TitleX + \beta X_{cst}) \quad (3.2)$$

where s indexes states and $TitleX$ indicates whether the county had a Title X clinic prior to the CFPI; otherwise, the notation is the same as before. Like the difference-in-differences models discussed above, this model controls for fixed county characteristics with fixed effects (α_{cs}). However, we expand on the controls for year fixed effects in a manner that relaxes the assumption that counties with Title X clinics and counties without such clinics would experience the same trends in outcomes in the absence of the CFPI. In particular, this model allows for state-specific year effects that are common across counties (α_{st}) and for time-varying effects specific to counties with Title X clinics (ω_t). As with our first alternative strategy, treatment effects on Colorado counties without Title X clinics could bias towards zero the estimates based on this strategy. Also, the precision of the estimates may be reduced by using relatively small counties (those without Title X clinics) for comparison. Moreover, counties in the 17 states in which every county has a Title X clinic will not contribute to the estimated effects.[23]

Our third alternative strategy is a state-level synthetic control design (Abadie et al. 2010), comparing the outcomes of Colorado to the outcomes of a "Synthetic Colorado." The intuition behind our implementation of this strategy is to use data from 2003–2008 to identify the weighted average of comparison states that provides the best match for the outcomes observed in Colorado over this period of time, i.e., the synthetic control. Under the assumption that the synthetic control also provides

[23] Those states are Alabama, Arkansas, District of Columbia, Delaware, Florida, Hawaii, Kentucky, Massachusetts, Maryland, Maine, North Carolina, New Hampshire, New Jersey, New Mexico, South Carolina, Tennessee, and West Virginia.

a good match for the outcomes that would have been expected in Colorado in the absence of the CFPI, the difference between the outcomes observed for Colorado and the outcomes observed for the synthetic control provides an unbiased estimate of the causal effect of the CFPI. We execute this strategy by selecting the non-negative weights for each potential "donor state" to minimize the function:

$$(X_{CO} - X_{SC}W)'V(X_{CO} - X_{SC}W) \tag{3.3}$$

where X_{CO} is a $(K \times 1)$ vector of variables measuring outcomes from 2003–2008, X_{SC} is a $(K \times J)$ matrix containing the same variables for other states, W is a $(J \times 1)$ vector of weights summing to one, and the diagonal matrix V are the "importance weights" assigned to each variable in X. We include the outcomes observed in 2003, 2005, and 2007 in X. While any number of variables could be included in X, we use these in particular out of our desire to construct a synthetic control that provides a good match for Colorado outcomes in levels and trends without overfitting. We report results that use the data-driven regression based method described in Abadie et al. (2010) to assign the variable weights contained in the V-matrix; however, the results are nearly identical when we instead assign equal weights to each variable.

The major advantage of this third approach for our purposes is that it can be applied to state-level data on STDs and abortions although we also use it to analyze birth rates, whereas the approaches described above cannot. Moreover, while we show a number of results to support the comparison group used in our preferred estimates, using a data-driven approach for selecting a comparison group is appealing for obvious reasons that are elaborated upon in Abadie et al. (2010). A disadvantage of this approach is that it does not make use of the rich set of control variables that otherwise would improve predictive power, and that researchers have yet to devise a method for calculating standard errors or confidence intervals. To conduct statistical inference, we follow Abadie et al. (2010) and estimate the distribution of estimated treatment effects under the null hypothesis of no effect by reassigning treatment to each state in the donor pool and applying the same method to estimate a placebo effect for each state. To construct a p-value for the effect estimated for Colorado, we consider its rank in this distribution. For example, if all U.S. states are used in the donor pool, and Colorado yields the third largest estimate in absolute value, then the p-value would be 3/50=0.06. Abadie et al. (2010) suggest instead using the ratio of the post-intervention mean square predicted error to the pre-intervention mean square predicted error, respecting the notion that we should place greater weight on estimated treatment effects when there is a better pre-period match between the treated unit and the synthetic control.

3.4 Estimated Effects on Childbearing

Before presenting model-based estimates, we present a graphical analysis that corresponds to our preferred difference-in-differences identification strategy. Figure A.7 plots the average of teen birth rates across Colorado counties with Title X clinics, which received funding from the CFPI, against the average across other U.S. counties with Title X clinics. Of particular note for the validity of our empirical approach is the fact that the average birth rate for the Colorado counties appears to track that of other US counties fairly well prior to the CFPI. This supports the notion that changes in the latter can provide a good counterfactual for the former. That said, the teen birth rate trend for the Colorado counties is somewhat more negative than that of the non-Colorado counties. This suggests that we may need to control for such trends in our econometric analysis. Figure A.7 also suggests that the teen birth rate across Colorado counties diverges from that of other U.S. counties following the CFPI, providing some initial evidence that the CFPI had its intended effect of reducing teen birth rates. In our following discussion of the results, we consider the robustness and statistical significance of this apparent effect.

Table A.10 presents model-based estimates from the same comparison shown in Figure A.7, based on the Poisson model described in Equation 1. Column 1 shows the estimated effects from the baseline model (only controlling for county and year fixed effects). These estimates indicate that the CFPI reduced teen birth rates by 4–6 percent in its first year and that the effect grew to 16–17 percent by its third and fourth years. In Column 2, we show estimates after also controlling for county-specific linear trends in order to address potential concerns that Colorado counties with Title X clinics and other U.S. counties with Title X clinics differ in their pre-existing teen birth rate trends. Those estimates are smaller than the ones in Column 1, reflecting the fact that the birth rate trend for the Colorado counties was somewhat more negative than the trend for the non-Colorado counties. Nonetheless, the estimates continue to indicate that the CFPI reduced teen birth rates after its first year: by 4 percent in its second year, 11 percent in its third year, and 8 percent in its fourth year. These estimates imply that the CFPI reduced teen birth rates by 5.6 percent across four years (p-value = 0.012), or by 7.6 percent across its second through fourth years (p-value=0.003).

Columns 4 and 5 address the fact that other state-level policies affecting access to contraceptives changed during the sample period, which could bias the estimated effects of the CFPI. Specifically, Column 4 presents estimates that additionally control for whether over-the-counter access to emergency contraceptives is permitted and whether private insurance plans covering prescription drugs are required to cover any FDA-approved contraceptive. Column 5 presents estimates based on a restricted sample of counties, omitting those counties in states that have significantly cut funding for family planning or implemented policies to deny clinics affiliated with abortion providers access to Title X or Medicaid funds during the period of our analysis.[24] These modifications to the analysis do not meaningfully change any of the estimates.

Although it is not the focus of this study, the coefficient on the indicator for a state permitting over-the-counter access to emergency contraceptives (not shown in the table) suggests that expanding access to such contraceptives also reduces teen birth rates.[25] Based on the full sample, that estimate indicates a reduction in teen birth rates of 1.6 percent (p-value = 0.016); based on the restricted sample, the estimate is 1.3 percent (p-value = 0.068). These estimates are particularly interesting, because prior work investigating the rollout of pharmacy access to emergency contraceptives in Washington finds no evidence of an effect on teen birth rates (Durrance 2013).[26]

Because the CFPI was intended to help those with low income gain access to LARCs, one would reasonably expect its effects to be largest in counties with a relatively large share of low-income individuals. We investigate this hypothesis by separately considering the effects for counties with poverty rates above the median of Colorado counties with Title X clinics and those with poverty rates below this median.[27] Although this approach balances the number of Colorado counties contributing to each estimate, it is noteworthy that Colorado is a relatively low-poverty state. Thus, the median used here (12.2 percent) is higher than the median across

[24] States with major funding cuts prior to 2013 include Texas, New Jersey, Montana, New Hampshire, and Maine. States blocking access to Title X funds include Kansas, New Hampshire, North Carolina, Tennessee, Wisconsin, Indiana, and Texas. States blocking Medicaid reimbursement include Indiana and Arizona.

[25] The coefficient on the state policy variable indicating private insurance plans covering prescription drugs must cover any FDA-approved contraceptive is close to zero and not statistically significant.

[26] There are many reasons why our findings may differ, including the way in which access to emergency contraceptives is measured and the population under consideration. Understanding how best to measure access to emergency contraceptives and why the effects might vary across different populations is an important avenue for future research.

[27] We use each county's poverty rate averaged across 2002–2012 so that this approach maintains a balanced panel.

non-Colorado counties with Title X clinics (15.6 percent).

Table A.11 presents the results of this analysis, restricting attention to estimates based on models with county-specific linear trends, which were earlier demonstrated to be important. These estimates indicate that the CFPI reduced teen birth rates by approximately 7 percent over four years in Colorado's counties with poverty rates above its median. As before, these effects are concentrated in the second through fourth years of the program. The estimated effects for Colorado's counties with lower poverty rates point in the same direction but they are less than half as large as the estimates for higher poverty counties and are not statistically significant at conventional levels.

Tables A.12 and A.13 present results from analyses that assess the validity of the estimates presented thus far. In Table A.12 we show the results from Poisson models that also include indicator variables for Colorado counties prior to the beginning of the CFPI. We do this in order to verify that prior to the initiative the teen birth rate in the Colorado counties receiving funding did not deviate from expected levels relative to the teen birth rate in other U.S. counties with Title X clinics, which otherwise would cast doubt on the notion that the latter provide a good comparison group for our purposes. Indeed, the coefficient estimates on the lead terms are routinely close to zero and are never statistically significant, whether we focus on all counties (columns 1–4), counties with poverty rates above the Colorado median (columns 5–8), or counties with poverty rates below the Colorado median (columns 9–12). Moreover, these results show that the estimated effects of the initiative are robust to the inclusion of these lead terms (though less precise), providing additional support for the validity of the research design.

Table A.13 presents estimates of the effect of the initiative using weighted least squares (WLS) where each cell is weighted by the teen female population it represents. This approach requires an ad hoc solution to address the fact that the natural log of the teen birth rate is undefined for county-year cells with zero teen births; we address this issue by adding one to the birth count in all cells.[28] Nonetheless, it is reassuring that the estimates based on this alternative approach are similar to those discussed above, if somewhat less precise.

As an additional check on our main results, we also conducted statistical inference based on permutation tests. This analysis is motivated by earlier work demonstrating that standard methods of statistical inference may not be appropriate when there are a small number of treated units relative to the number of control units (Conley and Taber 2011). Kaestner (forthcoming) shows this to be relevant in a panel-data analysis of 14 treated counties and 513 control counties. In our case, we have 37 treated counties and 1,717 control counties in our restricted sample, with fewer observations when we consider high and low poverty counties separately. We address this potential concern by repeatedly reassigning treatment to counties at random and obtaining the estimated effects using our preferred specification (i.e., the Poisson model with the full set of controls applied to the restricted set of counties). We then compare the distribution of estimates obtained by randomization to our true estimate: the fraction of randomization-generated estimates that are larger than the true estimate in absolute value provides a p-value. Although this approach provides larger p-values than the conventional approach, it continues to suggest statistically significant effects. For the analysis of all Colorado counties, it yields p-values of 0.09 and 0.04 for the average effect across the first four years of the program and for the average effect across the second through fourth years of the program, respec-

[28]While it is usually useful to also present OLS estimates for comparison with WLS estimates, as described in detail in Solon, Haider, and Wooldridge (2015), we believe that OLS is unreliable in our context because of the weight it gives to small counties for which the outcome variable is disproportionally affected by any ad hoc solution to addressing cells with zero births.

tively. For the analysis of high-poverty counties, these p-values are 0.11 and 0.04, respectively.[29]

Next we consider estimates based on the alternative identification strategies described in detail in Section 3.3.2. Table A.14 presents estimates based on a within-Colorado difference-in-differences design, using Colorado counties without Title X clinics as the comparison group, again focusing on estimates with county-specific linear trends which were demonstrated to be important in the preceding difference-in-differences analysis. We might anticipate that this approach would yield smaller estimates because residents in the comparison counties may also have been affected by the CFPI but it actually yields somewhat larger estimates where there were significant effects previously (for all counties, and for counties with relatively high poverty rates). That said, the standard errors are roughly three times larger than those based on the previous approach, not surprising given that this approach uses fewer comparison counties and those comparison counties are more sparsely populated.

Table A.15 presents the results of a triple-difference strategy that compares changes in Colorado counties with Title X clinics to changes in Colorado counties without such clinics relative to what observed in other states. Intuitively, this approach can be thought of as a modification of the within-Colorado difference-in-differences design; it allows for the possibility that Colorado counties with Title X clinics and Colorado counties without such clinics may be expected to diverge from one another after 2009 for reasons other than the CFPI. Data from other states are used to identify the divergence that should not be attributed to the CFPI. Table A.15 shows estimates from the baseline triple-difference model (Equation 3.2) in Panel A and a modified version that additionally controls for county-specific linear trends in Panel B. The estimates from this research design are consistent with those

[29]Due to computational constraints, these p-values are based on 100 replications.

reported earlier. They are smaller when controlling for county-specific linear trends, and fairly imprecise, but the point estimates continue to suggest that the initiative reduced teen birth rates 2–4 years after its implementation and that its effects are concentrated among relatively high poverty counties.

Finally, we present results from a state-level synthetic control design, which we use to consider teen STDs and abortions as well as birth rates. Because LARCs decrease the probability of having an unintended pregnancy and thus can be thought of as lowering the cost of having sex without a condom, we might expect an increase in LARC usage to be met with increases in sexual activity and reductions in condom use. As such, increasing LARC usage could have the unintended effect of increasing STDs.[30] Recalling from Figure A.2 that the CFPI appears to have caused teenagers to substitute from birth control pills—and not from condoms—to LARCs as their primary form of contraception, we would expect any observed effects on STDs to be driven by effects on secondary use and/or sexual activity. Regarding the analysis of abortions, we hypothesize that the CFPI would cause abortions to decline through reductions in unintended pregnancy.

First, we show state-level, synthetic-control-design estimates for teen birth rates to compare with the estimates discussed above. Here we note here that we only use the restricted sample of states for this analysis, so that the estimates are not confounded by major changes in family planning policies occurring outside of Colorado. Panel A of Figure A.8 shows how the teen birth rate in Colorado evolved over time relative to the weighted average of other states that form the "synthetic control."[31]

[30] On the opposite side of the same coin, Buckles and Hungerman (2015) show that increasing access to condoms causes significant increases in unintended pregnancy, which could result from increases in sexual activity or reductions in the use of more effective contraceptives.

[31] While the synthetic control method does not require non-zero weights, in this instance all states contribute to the synthetic control. The largest weights are given to Delaware, Nevada, and New Hampshire.

Two main features of this figure stand out. First, the synthetic control provides a good match for Colorado prior to the CFPI. Second, the two series diverge following the CFPI, indicating that the initiative reduced teen birth rates relative to what we would have expected based on the synthetic control. We report the corresponding set of estimated effects and permutation-based p-values in Panel A of Table A.16. The estimates are larger than those based on our other research designs, indicating an effect of 7.9 percent across the first four years of the initiative; however, they are not statistically significant at conventional levels. Like our other alternative identification strategies, this strategy finds economically significant but not statistically significant effects, thus highlighting its lack of power. Given the distribution of estimates generated by the permutation tests, as shown in Appendix Figure C.1, it would take an average effect of 15 percent over the four years to produce a p-value of 0.05.

Panels B and C of Figure A.8 and Table A.16 present similar estimates for STD rates and abortion rates. A visual inspection of the time series highlights a less than ideal match between Colorado and the synthetic control for these two outcomes prior to 2009. Recalling that our implementation of the synthetic control design matches on outcomes in 2003, 2005, and 2007, the large differences in outcomes between Colorado and the synthetic control in other years prior to the CFPI suggests that the synthetic control may not provide a very good counterfactual for these outcomes. Still, while we note that our synthetic control estimates yield no evidence that the CFPI either increased or reduced abortion rates (as the estimates point in the opposite direction), we suggest that these estimates be interpreted with a great deal of caution. Efforts to obtain more convincing estimates of the effects on these outcomes could be an important avenue for future research.[32]

[32]One approach to obtaining more convincing estimates in the context of the synthetic control

3.5 Conclusion

By analyzing the first large-scale policy intervention to promote and improve access to LARCs in the United States, this paper provides some groundwork for understanding how improving access to LARCs can affect birth rates of one of the highest at-risk groups for unintended pregnancy, teenagers. Our estimates indicate that this intervention significantly reduced teen birth rates. A back-of-the-envelope calculation based on our estimates suggests that the program prevented approximately 1,050 teen births that would have been conceived between 2009 and 2012.[33] Given that $18 million of CFPI funding was allocated to these years, with the remainder allocated to 2013–2015, this amounts to approximately $22,000 per teen birth avoided. However, it is important to keep in mind that LARCs inserted over this period of time also would be expected to prevent unintended pregnancies in subsequent years. Moreover, the initiative was intended to promote access to LARCs among low-income women in general, not just teenagers. Thus, in order to provide a more complete understanding of the effects of the program, it will be important for future work to revisit its effects once more data becomes available and to consider its effects on older women. It also will be important to further consider how expanding access to LARCs affects sexual activity and reproductive health more generally. Finally, we note that our results suggest that future work on the effects of

approach is to use alternative variables to improve the match between the treated unit and the synthetic control in the pre-treatment period. Our attempts along these lines proved have not proved fruitful.

[33] This number is based on the estimated effect of 5.6 percent across 2009–2012, an average of 156,000 teen females living in Colorado counties with Title X clinics over these years, and a baseline birth rate of 30 per 1,000 teen females. It is quite similar to the effect that we would expect based on: the estimated number of LARCs provided to teenagers under the initiative (4,505); the increase in LARC use representing substitution away oral contraceptives; a typical-use failure rate of 9 percent a year for oral contraceptives (that may or may not apply to the subpopulation receiving LARCs through the initiative); and a typical-use failure rate of zero for LARCs. These numbers would predict an additional 1,013 pregnancies in the absence of the initiative.

expanded access to LARCs may provide useful insights into the effects of unintended pregnancies (or the prevention thereof) on long-run outcomes, such as educational attainment, earnings, and the use of social assistance programs.

4. FAMILY PLANNING FUNDING CUTS AND TEEN CHILDBEARING

4.1 Introduction

For over four decades, publicly funded family planning clinics have provided free or nearly-free contraception, sexually transmitted disease (STD) screenings, and counseling services to low-income women. Many women rely on these clinics as a primary source of health care, and 85% of clients adopt or receive contraceptives at these facilities (Frost et al., 2013; US HHS, 2013).

Women's health centers rely on substantial public funding at both the federal and state levels.[1] While family planning programs have historically held bipartisan favor, support for family planning services has become an increasingly controversial policy issue. In the last five years, over 630 bills related to women's health, family planning and contraception have been introduced in Congress, and the number of newly enacted sexual and reproductive health provisions nearly tripled (GovTrack, 2015; Nash et al., 2015).

Much of the current debate on the provision of family planning services focuses on government funding for clinics. Critics of publicly funded family planning often cite clinic affiliations with abortion providers as political motivation to defund clinics.[2] And while no federally funded family planning clinic may legally provide abortion services, Texas, as well as four other states-New Jersey, Montana, New Hampshire, and Maine-have recently enacted measures to limit spending for family planning services, with many states considering similar legislation (Cadei, 2015).

[1] Public expenditures for family planning services totaled $2.37 billion in 2010 (Sonfield and Gold, 2012).

[2] For example, Governor Rick Perry declared in 2012 that outlawing all abortion is the ultimate policy "goal", and that the Texas legislature would continue to "pass laws to ensure abortions are as rare as possible under existing law" (Bassett, 2012).

The extent of the recent cuts to family planning services in these five states varies substantially. For example, New Jersey eliminated over $7 million in funding, Montana cut funding by $4 million, and Maine reduced funding by only $400,000. New Hampshire reduced family planning funding in 2011, but then restored funding two years later (Nash et al., 2015; Simon, 2012; Szabo and Ungar, 2015). But by far, Texas policymakers approved the most drastic cuts to family planning services to date, with budget cuts totaling $73 million, or $50 million more than the other four states combined. Moreover, the sizable reductions in funding induced 25% of Texas family planning facilities to close.

Although many states have approved or considered legislation to limit funding for family planning services, little is known about how these policies affect women's access to low-cost contraception and, in turn, childbearing. For example, nearly all unintended pregnancies are attributable to women who do not use contraception or use it inconsistently, implying that funding cuts to family planning clinics may indirectly increase unintended pregnancy rates through its effect on contraception use (Guttmacher, 2015b). Given that teenagers are twice as likely as older women to have an unintended pregnancy (Finer, 2010), and are less likely to seek contraception when low-cost options are unavailable (Frost et al., 2013), we may expect teens to be disproportionately affected by defunding policies.

This paper is the first to address to what extent reductions in funding for family planning services affect teen childbearing. In the following discussion and analysis I focus specifically on the effects of the Texas funding cuts, given the scale of the policy change and its considerable impact.[3] Using restricted county-level Natality data, I

[3]Since four other states (New Jersey, Montana, Maine, and New Hampshire) passed similar legislation between 2010-2012, any estimates based on specifications that include counties in these states may understate the true effects of the funding cuts on teen birth rates. Therefore for the main estimates, I include additional specifications that exclude these states.

empirically analyze the effects of the defunding policy in Texas and find that teen birth rates increased significantly as a result of the family planning funding cuts. I further investigate how the policy change differentially affected younger teens and low-income teens. In doing so, this paper informs a fervent policy debate over the efficacy of family planning clinics and fits into a broader literature on the effects of government intervention on teenage pregnancy.

Teen pregnancy is often cited as a policy target by the US Department of Health and Human services and is widely thought of as a public health concern for multiple reasons. First, teen motherhood is associated with poor life outcomes including low graduate rates, poverty, low wages and dependence on government services (Hoffman and Maynard, 2008; Geronimus and Korenman, 1992; Bronars and Grogger, 1994). Second, teens may be ill-equipped to take care of children. More than 75% of teen pregnancies are unintended, implying that, from an economic standpoint, sexually-active teens likely do not fully internalize the expected cost of their decision and behave irrationally. Therefore, teen mothers may be unprepared to take on the responsibility of raising a child and impose external costs on family, friends, and taxpayers (Mosher, 2012).[4] Such externalities can reduce social welfare, suggesting that there is scope for government policies that reduce unintended pregnancy to improve overall well-being.

There is a long history of U.S. policies aimed at reducing unintended pregnancy, especially among teens. Such approaches have typically aimed to delay the onset of sexual activity and/or reduce risky sexual behavior through three main avenues: sex education, legal access to contraception, and the provision of family planning services. Since the 1980s, the federal government has granted over $1.5 billion in

[4]The National Campaign to Prevent Teen and Unplanned Pregnancy estimates that the taxpayer costs for teen childbearing amounted to $9.4 million in 2010.

funding to promote sexual health in schools (SIECUS, 2010). As of 2015, 36 states mandate some form of sex education, and 96% of teens report having received some form of sex education training before they turned 18 (Guttmacher, 2015a; Martinez, 2010). Although millions of dollars are spent each year on sex education, there is little evidence that these programs alter teen sexual behavior (Kirby, 2008; Carr and Packham, forthcoming 2016).[5] One potential reason for the lack of effectiveness of sex education programs is that some teens may be myopic in their unwillingness to abstain from risky sexual behavior. Moreover, while receiving information on how to practice safe sex is relatively costless, implementing these tactics may not be.

Other policies to prevent unintended pregnancy address the legal and financial barriers of obtaining effective contraceptive methods. For example, several states expanded confidential access to birth control pills in the 1960s and 1970s. However, these policies did little to reduce teen fertility (Guildi, 2008; Bailey, 2009; Myers, 2012). To date, the most effective government programs for reducing teen pregnancy rates appear to be those that provide low-income women with free long-acting reversible contraceptives (LARCs), which include intrauterine devices (IUDs) and implants. Although LARCs have very high rates of effectiveness compared to traditional contraceptive methods (99.9% versus 82% for condoms), and eliminate user-compliance error, they are the most expensive contraceptive devices to date, ranging upwards of $400 (Planned Parenthood).

The price of LARCs may explains why only 5% of teens in the US choose these methods (US HHS, 2013). Indeed, when clinics provide these devices to young women for free, uptake is relatively high, ranging from 19% to 70% (Ricketts et al., 2014; Mestad et al., 2012). Moreover, policies that reduce the cost of LARCs are effective

[5]See Kirby (2008) for a comprehensive review of this literature. Out of 56 studies a majority indicate no effect on initiation or frequency of sex, number of partners or contraception use.

at reducing teen pregnancy. Lindo and Packham (2015) use a difference-in-differences design to analyze an initiative in Colorado that provided free LARCs to low-income women at Title X clinics, and find that increasing access to LARCs decreases teen childbearing by 5%. These findings suggest teens face substantial financial barriers to obtaining highly effective contraceptive methods.

Publicly funded family planning clinics address these barriers by providing free or low-cost contraceptives to low-income women. Supporters of family planning funding claim that such clinics provide women with needed contraception and health check-ups and often refer to defunding legislation as a “war on women’s health" (Weigel, 2012). There is a large body of work on the association between expanding contraception access and women’s health. Overall, findings indicate that the rollout of Title X services from 1964-1973 resulted in fewer “unwanted" babies, higher family income, and higher educational attainment for children (Bailey, 2012; Bailey, 2013). Moreover, increased family planning clinic access has also reduced teen fertility. For example, Bailey (2012) utilizes county-level variation in timing of access to Title X clinics and estimates that family planning services are responsible for reducing teen childbearing by up to 3% over time. Kearney and Levine (2009) use a difference-in-differences design to determine that expanding family planning services to women in the 1990s and 2000s reduced teen childbearing by over 4% as a result of increased contraception use.

While the studies described above indicate that expanding family planning services has historically been a useful policy tool for preventing unintended pregnancy, the effects of recent policies that *restrict* access to family planning services are unclear. Lu and Slusky (2014) use zip-code-level survey data matched to a national network of women’s health centers to analyze the effects of recent clinic closures in Texas and Wisconsin on preventative care, and report that increasing distance to a clinic is associated with women receiving fewer annual mammograms, pap smears, and breast exams. Stevenson et al. (2016) analyze a more recent 2013 policy change in Texas that excluded Planned Parenthood affiliates from the Texas Women's Health Program. Using claims data from 2011-2014, they find that restricting public funding leads to reduced contraceptive use and increased Medicaid-covered childbirth in counties with Planned Parenthood clinics. But despite the growing focus on family planning policies in Congress and emergence of new and pending legislation in several states, there exists little to no research on the consequences of family planning funding cuts on overall rates of unintended pregnancy.

My paper is the first to estimate a causal effect of family planning funding cuts on childbearing. Specifically, this study focuses on effects on teen childbearing and, consequently, speaks to an important public health policy target. Therefore, this paper fills an important gap in the literature, by addressing the question: How much can reducing funding for family planning services affect teen birth rates?

To answer this question, I analyze a 2011 policy change in Texas that reduced funding for family planning services by two-thirds. The goal of this paper is to measure the degree to which funding cuts to family planning services can alter teenage sexual behavior. While Texas politicians have pointed to a reduction in teen birth rates and abortion rates in recent years as affirmation for defunding family planning clinics, the fact that teen birth rates fell significantly across the US over the same time period suggests that other factors likely contributed to the decline.[6] To separate the effects of the defunding policy from other factors that affect teen birth rates, I utilize a difference-in-differences method that compares changes in teen birth rates in Texas counties with publicly funded family planning clinics to counties with clinics

[6]For example, in 2015 Texas Governor Greg Abbott wrote, "After Texas defunded Planned Parenthood, both the Unintended Pregnancy & Abortion Rates Dropped" (Selby, 2015).

outside of Texas. The results of this analysis indicate that defunding Texas family planning clinics led to a 6-9% increase in teen birth rates over four years. These effects were concentrated in the third and fourth years following the initial funding cuts and had the strongest effect for teens aged 15-17 and women living in counties with higher poverty rates.

The remainder of this paper is organized as follows. In the next section I provide background information on family planning services in Texas and describe the state's 2011 funding cuts in greater detail. I then discuss the data and methods used for analyzing the causal effects of the funding cuts on teen health outcomes and present the results of the analysis. Lastly, I conclude and provide a discussion on the implications of current and pending family planning policies.

4.2 Background

The Texas Department of State Health Services Family Planning Program funds clinics across the state that provide low-cost reproductive health services to women and men. Funding includes federal and state grants from Title V, Title X, and Title XX. Services available at clinic sites include pregnancy tests and health screenings, sexually transmitted disease testing, preventative care, such as pelvic exams and pap tests, and contraception services.

Since its inception, the Texas Family Planning Program has been targeted towards low-income women. Clients may qualify for free or low-cost family planning services if they live in Texas, are not sterilized or pregnant, and have income below 250% of the federal poverty level. A large majority of clients at Texas family planning clinics are considered "very poor"; over 75% of Texas clients have income levels below 101% of the federal poverty line, and 79% have no health insurance. Nearly all of the clients are women (94%), and almost half are under the age of 25 (US HHS, 2013).

In 2011, the Texas State Legislature restructured government funding for family planning services in two main ways. The first measure reduced the family planning budget by 67%, from $111 million in 2009-2010 to $37.9 million in 2012-2013. The second measure formed a three-tiered system that allocates more of the remaining funding to clinics with comprehensive services over those that provide only family planning services. Tier 1 clinics include public agencies that provide family planning services, such as public health departments and federally qualified health centers. Specialty clinics, such as Planned Parenthood facilities, are classified as third-tier clinics, and faced the brunt of the funding cuts.[7] All remaining non-public entities that provided comprehensive preventive and primary care in addition to family planning were classified as Tier 2 centers.

The first funding cuts took place on September 1, 2011. Fourteen family planning clinics lost funds immediately. By the end of 2012, 25% of clinics shut down, 18% reduced service hours, and nearly 50% fired staff (White et al., 2015). Many providers began implementing a fee-for-service system for services that had previously been free or low-cost, such as well-woman exams and oral contraceptives (White et al., 2015).

Figure A.9 displays the total amount of federal and state funding over time for Texas family planning clinics. Funding totaled nearly $43 mil in 2010. However, by 2012 and 2013, funding levels dropped to merely $21 mil and $12 mil, respectively. Although the legislation was enacted in 2011, a large majority of the funding cuts occurred in 2012 and 2013. Because of this delayed rollout of budget cuts and clinics' reactions to the reducing in funding, we may expect more women to be affected by this policy in the latter two years.

By the end of 2013, over 160 clinics had lost all funding, including 82 Texas

[7]By 2013, no Texas Planned Parenthood facilities received public funding.

clinics that closed as a result of the funding cuts. Figure A.10 displays the number of publicly funded family planning clinics over time. Notably, the number of clinics experienced a lagged response to the initial funding cuts. In the two years following the cuts, the number of publicly funded clinics dropped from 287 to 126.[8] The reduction in family planning facilities is mirrored in Figure A.11, which maps the locations of publicly funded clinics from 2010-2013. Few changes are observed from 2010 to 2011. However, over 56% of clinics lost all funding for family planning services by 2013. Geographically, the Panhandle and South Texas regions, which have large low-income and Hispanic populations, experienced the greatest changes in clinic funding and access, indicating that the budget cuts were not randomly distributed and may have disproportionately large effects on low-income women and Hispanic women.

It is possible that although many family planning clinics closed as a result of the funding cuts, remaining clinics were able to absorb the excess demand for services. I explore this possibility in Figure A.12, which shows the total clients visiting a family planning clinic over time. After the funding cuts, client caseload for publicly funded family planning services dropped dramatically. From 2011-2013, the client caseload dropped by nearly 164,000 clients, or 77%, suggesting that there was little to no substitution effects within the public sector.[9]

Importantly, because many clinics after 2011 began charging for contraceptives that were previously offered at no cost, it may be the case that funding cuts to family planning clinics affected contraception usage. Figure A.13 shows how the primary

[8] It is important to note that although many clinics lost public funding, not all had to shut down. This implies that many entities were able to stay open by supplementing funding through private donors or other outside means.

[9] Below I discuss potential substitution effects into the private sector, and note that while I cannot directly measure the extent to which family planning clients switch doctors, I provide some data on Texas Planned Parenthood donations in Table D.2 that suggests some switching is likely to have occurred as a result of the funding cuts.

method of contraception used by Texas family planning clinic clients has evolved over time. I note that these statistics may overstate the degree to which contraception use has decreased in Texas, because these data are based only on publicly funded clinic visitors. Figure A.13 Panel A displays the total number of family planning clients receiving moderately effective or highly effective contraception at exit.[10] As expected, the total number of clients using contraceptives declines sharply after 2011, closely mirroring the reduction in clients shown in Figure A.12. Notably, the reduction in clients does not account for women that obtained contraceptives at privately funded facilities after a public clinic closure, meaning it is possible that the fraction of women using contraception was unchanged after the funding cuts. That said, Panel B presents to what extent the percent of clients at publicly funded clinics obtained moderately-effective or highly-effective contraceptives. In 2010, before the funding cuts, uptake is 62%, although it drops to 34% and 52% in 2012 and 2013, respectively. These statistics support the notion that the 2011 funding cuts reduced contraceptive usage among Texas women.[11]

4.3 Empirical Approach

This section describes the data and approach I use to estimate the causal effects of Texas's family planning funding cuts on teen pregnancy rates.

[10] Moderately effective or highly effective contraceptive devices include intrauterine devices, implants, injections, oral contraceptives, patches, rings, and cervical caps.

[11] Figure D.1 presents the percent of women receiving contraceptives at publicly funded clinics by type of method over time. In 2012, the number of clients choosing injectible contraceptives sharply declined. This may be due to the fact that these methods require the user to receive subsequent injections every 3 months, and clinic closures prevented or discouraged women from receiving subsequent injections. See Stevenson et al. (2016) for an in-depth analysis of the decline in contraceptive use in publicly funded Texas clinics from 2011-2014.

4.3.1 Data

In Texas, the Department of State Health Services (DSHS) facilitates the funding and organization of the family planning program. For this study, the Texas DSHS provided yearly data on Texas health clinic funding, as well as clinic addresses and client caseload from 2005-2013. Because this analysis focuses on teens living in Texas counties with family planning clinics, I geocode the clinic addresses to identify which Texas counties were offering family planning services before the 2011 funding cuts to serve as the treatment group. All of these counties contain at least one clinic that experienced a reduction in family planning funds due to the policy change. To identify counties with clinics outside of Texas, which form the comparison group for this study, I utilize the Guttmacher Institute's data on publicly funded family planning clinics. These data include county-level counts on the total number of federally qualified health centers, health departments, hospitals and Planned Parenthood clinics that receive government funding. A map of treatment and control counties is shown in Figure A.14. These counties represent 80% of the total number of U.S. counties, and account for 96% of the female teenage population.

To measure the effect of the funding cuts on teen births, I utilize restricted-use Natality data from the Center for Disease Control and Prevention (CDC) from 2005-2014, which contains individual-level counts of births as well as mother's age and county of residence (NCHS Natality Files, 2000-2014). Combining these data with population data from the National Cancer Institute's Surveillance, Epidemiology, and End Results Program (SEER), I construct teen birth rates (the number of teen births per 1,000 teen females) for the analysis.

While nearly all of the analysis focuses on teen birth rates, I also consider effects of family planning funding cuts on teen abortion rates and teen STD rates. Data on teen abortions is from the annual Centers for Disease Control and Prevention (CDC) Abortion surveillance, and is discussed in further detail below (CDC Abortion Surveillance, 2005-2012). STD data is from the CDC NCHHSTP Atlas (CDC NCHHSTP Atlas, 2015). My analysis of STD rates restricts attention to chlamydia and gonorrhea, because clinics are required to report cases of these diseases.

Additionally, I utilize demographic information constructed from the population data to control for time-varying county characteristics such as the fraction of teen females of each age, Black and Hispanic. To account for changing county-level economic conditions over time, I use data from the Bureau of Labor Statistics on the unemployment rate (BLS, 2000-2014). Finally, I construct two policy indicator variables using data collected from the National Conference of State Legislatures, National Survey of Family Growth, the National Women's Law Center and Zuppmann (2011) to control for other state-level contraception policies. Specifically, these policy controls are state-by-year indicator variables that account for legal over-the-counter access to emergency contraceptives and whether private insurance plans that cover prescription drugs are required to cover FDA-approved contraceptives.

Summary Statistics are shown in Table A.17. Means are separately reported for Texas counties with family planning clinics and other U.S. counties with clinics in the periods before and after the funding cuts. Before 2011 teen birth rates in Texas average nearly 69 births per 1,000 teens, compared to 45 births per 1,000 teens outside of Texas. For both groups, teen birth rates fell after 2011. As such, the analysis below can be viewed as estimating to what extent teen births rates could have declined further in the absence of family planning funding cuts.

4.3.2 Identification Strategy

I estimate the effects of the 2011 family planning funding cuts in Texas using a difference-in-differences approach. Specifically, I use all of the counties within Texas with at least one publicly funded clinic that received state and/or federal funds in 2010 as the treatment counties, since all of these 113 counties experienced budget cuts to at least one clinic after the policy change. I compare the changes in teen birth rates in these counties to all other counties in the US with publicly funded clinics. The identifying assumption underlying this approach is that changes in teen birth rates in Texas counties with family planning clinics would have matched the changes in teen birth rates in other counties with publicly funded clinics, absent the funding cuts. In the next section, I provide further discussion, as well as visual and statistical evidence, to support this assumption.[12]

Although it is typical for difference-in-differences models to estimate the average effect of a policy change across all post years, this approach is less appropriate in this context. For example, we may expect the funding cuts to have a delayed effect on teen pregnancy if clinics were slow to respond to the budget changes and/or shut down. Moreover, as the initial funding cuts took place at the end of 2011, and many clinics did not lose funding until 2012, it is feasible that there was little interruption

[12] I have also considered using several alternative comparison groups, including a broader group comprised of all US counties as well as more narrow comparison groups comprised of Texas counties, counties in Southern states, or counties in states bordering Texas. None of these groups appear to track the Texas counties' teen birth rate trend as closely as the chosen comparison counties prior to the funding cuts, suggesting they would provide a less reliable counterfactual. Additionally, I have considered using only counties that experience a clinic closure, with all other counties with a publicly funded clinic serving as the comparison group. Although the point estimates are similar to the main results (average effect of 0.047, as compared to an average effect of 0.048) but because these counties are sparsely populated, such an approach yields estimates that are much less precise. To address this concern, I alternatively limit the sample to counties with an average teen female population greater than the average teen female population of Texas counties with clinics. See Table D.1 for a replication of the main results comparing highly populated Texas counties with highly populated US counties outside of Texas.

to services until then. Finally, while there may have been a more immediate change in contraception availability, childbearing is a naturally lagged process. For these reasons, we can expect that changes in teen birth rates will be most concentrated in later years.

Since birth data is discrete and there exist some county-year cells with zero teen births, I report results from a fixed effects Poisson model.[13,14] In particular, the main results are based on estimating Poisson models of the following form:

$$E[teenbr_{ct}|fundcuts_{c,t-k}, \alpha_c, \alpha_t, X_{ct}] = exp(\sum_{k=1}^{3} \theta_k fundcuts_{c,t-k} + \alpha_c + \alpha_t + \beta X_{ct}) \tag{4.1}$$

where $teenbr_{ct}$ is the teen birth rate for county c in year t, $fundcuts_{c,t-k}$ is an indicator variable that takes a value of one for Texas counties k years after the family planning funding cuts began and zero otherwise, α_c are county fixed effects to control for any time-invariant systematic differences across counties, α_t are year fixed effects to control for shocks to teen birth rates that are common to all counties in a year, and X_{ct} includes time-varying county control variables for demographics, economics conditions, and state-level contraception policies. Importantly, this model can alternatively be expressed as one that estimates the natural log of the expected count of births while controlling for the population of teen females and constraining its coefficient to be equal to one. Therefore, estimates from the above model will be comparable to estimates from a weighted least squares model that estimates the effects of the funding cuts on logged teen birth rates and allows standard errors to

[13]Specifically, there are 191 county-year observations out of 25,200 that have a teen birth rate of zero.

[14]Like linear models, the Poisson model is not subject to the incidental parameters problem associated with fixed effects because they can be eliminated from the model. I relax the assumption of equality between the conditional mean and variance by calculating sandwiched standard errors.

be correlated within counties over time.[15]

4.4 Main Results

Before presenting regression results, I first provide a graphical analysis of the trends in teen birth rates across treatment and comparison counties. Figure A.15 Panels A and B respectively plot the logged teen birth rates and percentage change in teen birth rates for Texas counties with family planning clinics against all other US counties with family planning clinics over time. In Figure A.15 Panel A, the trends in logged teen birth rates for counties in Texas with clinics and counties outside of Texas with clinics appear to similarly track each other prior to 2011, lending some visual support to the validity of the assumption that changes in birth rates for the comparison counties provides a good counterfactual for Texas counties.

However, given that the baseline levels in birth rates are so different, it is difficult to visually distinguish if the trends diverge after 2011. Therefore, Figure A.15 Panel B presents the annual percent change in teen birth rates for Texas counties and counties outside of Texas. The percentage change in teen birth rates in Texas counties after the funding cuts increases relative to the comparison counties, providing some initial evidence that the policy change increased teen childbearing. Below I present a more rigorous statistical analysis of the apparent effects of the funding cuts on teen birth rates and provide further evidence to support the common trends assumption.

Table A.18 shows the difference-in-differences estimates from the Poisson model described in Equation 1. Column 1 presents results from a fixed effects Poisson model with no controls while Columns 2, 3 and 4 show results from models that progressively

[15] I also estimate specifications of the weighted least squares (WLS) model to control for unobserved heterogeneity in a more flexible manner, as suggested by Solon, Haider and Wooldridge (2015). When estimating WLS models, I first add one to the count of births for all county-year cells and then construct the logged birth rate. Results are statistically similar to the main estimates, and indicate that the funding cuts increased teen birth rates by 6.4%.

add controls for demographics, unemployment rate, and state-level contraception policies. Specifically, these controls include the fraction of teens of each age and race/ethnicity, the county unemployment rate, and state-level policy indicators for emergency contraception access and contraceptive insurance mandates.

As expected, given that the fraction of Hispanic teens in the comparison counties increased disproportionately in comparison counties over time, estimates are larger when controlling for demographics. In Column 1, estimates suggest a positive effect of funding cuts on teen birth rates in 2013, although they are not significant at conventional levels (p-value=0.101). Effects on birth rates across all columns one and two years after the funding cuts are statistically insignificant at the 5% level, although given the delayed nature of childbearing and the lagged rollout of the budget cuts, it is unsurprising that these effects are concentrated in later years. That said, estimates in Columns 2-4 indicate that the funding cuts to family planning services increased teen birth rates from 4.5%-5.2% three years after and 9.1%-9.8% four years after the funding cuts. Similarly, the average effect in years 3-4 ranges from 4.1%-5.0%.

In a difference-in-differences setting, changes to family planning policies in comparison counties during the sample period would result in biased estimates. To investigate the extent to which policy changes in other states affect the main estimates, Column 5 replicates estimates from Column 4, omitting counties in states outside of Texas with major funding cuts to family planning services from 2010-2012. These states include New Jersey, New Hampshire, Montana and Maine. Results are robust to the exclusion of these observations, and indicate that the Texas funding cuts increased teen birth rates by 4.6%.

Columns 6 and 7 separately add one- and two-year indicator variables for Texas counties prior to the funding cuts to check that the teen birth rate in the Texas counties closely tracked the trend in other US counties before the policy change and serve as a good comparison group for this study. Indeed, the estimates for the leads are statistically insignificant and close to zero. Moreover, the results of the estimated average effects of the funding cuts range from 4.6%-4.8% and are robust to the inclusion of the lead terms which lends further evidence to support the validity of the research design.

4.5 Differential Effects of Funding Cuts on Teen Birth Rates

In this section I discuss results from several alternative models that analyze the extent to which there were heterogeneous treatment effects across populations. Specifically, I present estimates for teens by age, and investigate effects for teens in counties without a publicly funded clinic, teens in counties with one or more Planned Parenthood clinics, and teens in low-income areas.

4.5.1 Effects by Age

While it is common practice to study birth rates for all teens aged 15-19, it may be the case that younger teens and older teens are affected differentially by changes in family planning policies. That said, it is not clear whether older or younger teens are expected to be affected more by a reduction in family planning services. For example, if teens under the age of 18 are more constrained in terms of transportation and other financial resources, this age group is more likely to be affected by a change in clinic access. However, older teens are more experienced and have sex more frequently than their younger counterparts, indicating that a potential reduction in contraception usage as a result of limited clinic access could result in a larger increase in birth rates for the older group (Martinez et al., 2011).

I replicate the main results for teens by age separately to determine if the funding cuts to family planning services affects younger teens and older teens in different ways. Table A.19 displays results for teens aged 15, 16, 17, 18 and 19 as well as the younger group, aged 15-17, and the older group, aged 18-19. Estimates across Columns 1-3 indicate that reductions in family planning spending increased birth rates for 15-17 year olds from 5.2-6% in the third and fourth years, on average, while estimates for the subgroup of 15-17 year olds, as shown in Column 6 indicates an effect of 6%. These results are much larger than the estimated 4.6% increase in birth rates for all teens.

Effects for 18 and 19 year olds are shown in Columns 4, 5, and 7, and imply that the funding cuts increased birth rates for older teens increased by 3.7-4.9% in the third and fourth years. These findings are evidence that the increase in teen birth rates overall is partially driven by the effect on the younger teens, aged 15-17. This suggests that younger teens face relatively high barriers to obtaining low-cost contraception and are more sensitive to changes in family planning clinic access.

4.5.2 Effects on Counties Without Family Planning Clinics

Given that over half of counties in Texas did not have a publicly funded clinic in 2010, it is possible that estimates from the main results understate the overall effect of the budget cuts on teen birth rates. One reason is that teens could be traveling to an adjacent county for family planning care prior to the funding cuts but are unable to visit clinics much further away. To investigate this possibility, I estimate the effects of the policy in counties without clinics, which are omitted from the main analysis, and display them in Table A.20. Specifically, I replicate Table 2 Column 5 using all clinics in Texas without publicly funded clinics as the treatment counties and U.S. counties outside of Texas without clinics as the comparison counties. In all years following the policy change the estimates statistically insignificant. However, these estimates are relatively imprecise and incapable of ruling out large effects on teen birth rates, perhaps since counties with no publicly funded clinics account for merely 4% of the population of teen females.

4.5.3 Effects on Counties with Planned Parenthood

Despite its major role in providing family planning services to thousands of Texas clients, a major motivation for the defunding of family planning services in Texas is the goal of eliminating Planned Parenthood.[16] For example, in 2012 Texas governor Rick Perry stated, "I was really proud to be able to sign into legislation that we worked with our legislature to defund Planned Parenthood in the state of Texas" (Summers, 2012). The main motivation for defunding Planned Parenthood is that although centers that receive public funding are not legally allowed to provide abortion services, publicly funded Planned Parenthood clinics are affiliated with abortion providers. State rules define abortion clinic "affiliation" as any clinic that shares an organizational name with an entity that performs abortions elsewhere. Proponents of policies to defund Planned Parenthood argue that public money can be distributed across clinics in the same organization and can indirectly fund abortions. Therefore, based on their affiliation with abortion clinics, Texas Planned Parenthood centers that provide only contraception services, STD screening and other women's health services were a primary target for 2011 funding cuts.

As a result of the Texas funding cuts, 11 Planned Parenthood facilities closed, potentially limiting low-cost contraception access for at-risk teenagers. To measure the change in birth rates in counties with Planned Parenthood facilities, I replicate Column 5 of the main results while limiting the sample to counties with Planned Parenthood clinics in 2010, which represents only 19% of the total counties. Table A.20 Column 3 displays the effects of the funding cuts on teen birth rates in Planned

[16] Of the 218,000 women receiving care through this funding, 40% obtained services through Planned Parenthood and other tier three agencies prior to 2011.

Parenthood counties. Teen birth rates in these counties increased by 4.7% in the third year and 11.3% in the fourth year, which is slightly larger than the estimated effect for teens overall.

Although several clinics closed, some Planned Parenthood centers were able to stay open as a result of an increase in private donations. See Table D.2 for an annual breakdown of donations to Texas Planned Parenthood facilities from 2011-2014. Donations approximately doubled in 2012, suggesting some substitution between public and private funding for family planning services. Therefore, estimates presented in Table A.18 may understate the true effects of defunding family planning clinics.

4.5.4 Effects on Low-Income Women

Because publicly funded family planning services mainly serve low-income women, we may expect funding cuts and clinic closures to have a larger effect on teens in counties with higher concentrations of poverty. I investigate this by separately considering effects for counties with high and low poverty rates and report these results in Table A.20. “High" poverty counties are defined as those with poverty rates above the average 2010 poverty rate for Texas counties with publicly funded clinics and “low" poverty counties are those with poverty rates below this average.[17]

As shown in Column 4, funding cuts increase teen birth rates by 9.4% in high poverty counties in 2014. In comparison, Column 5 shows estimates for counties with relatively low poverty rates. Estimates are positive, although the estimated average effects are not statistically significant, indicating that teen birth rates in relatively richer communities are less sensitive to changes in access to publicly funded family planning services.

[17]To maintain a balanced panel, I average each country’s poverty rate across the sample period, 2005-2014.

4.6 Analyzing Changes in Abortion Rates

The main political motivation for defunding family planning services is reducing abortions. Although federally funded clinics are not legally allowed to provide abortions, one argument for limiting family planning resources is that clinics affiliated with abortion providers may distribute government funding across an umbrella organization, thereby indirectly funding abortion services.

Before presenting regression-based estimates, I first provide some visual data to determine the effect of family planning funding cuts on abortion rates in Texas. Unlike most states, the Texas Department of State Health Services releases annual county-level abortion rates. Therefore, while I cannot apply the same difference-in-differences methodology described in Section 3 to present county-level estimates of the effects of funding cuts on abortion, I can provide some suggestive evidence of how abortion rates in Texas responded to the 2011 family planning budget cuts. Figure A.16 displays the trend in teen abortion rates for treated counties (Texas counties with at least one family planning clinic) from 2006-2013. Although there are no data on comparison counties to form a counterfactual for the county-level trend in abortion rates, the time series data indicate that abortion rates fell steadily in Texas from 2006-2012, and then increased in 2013.

While Figure A.16 provides some information on the abortion rate in Texas over time, it is insufficient to produce causal estimates. Therefore, I utilize state-level data from CDC on the number of abortions by age group and state of residence, which is the only existing source of annual abortion data to visually compare the changes in abortion rates in Texas with other states over time. Figure A.17 presents the trends in logged teen abortion rates over time for Texas and all other US states. The trends for the Texas and other US states visually track each other for the three years leading up to the defunding policy, although given the level differences, it is difficult to distinguish the effect on teen abortions, if any.

There are several limitations to this approach. Unfortunately, abortion data are only available up to 2012, and since funding cuts began in September, I can only observe 4 months of data post-funding cuts. Moreover, because centers are not required by law to submit annual abortion data, these data contain several omissions and inconsistencies, which are a well-documented occurrence (Blank et al., 1996).[18] Finally, these data are not available at the county level, and given that only one state, Texas, is treated in this analysis, inference is likely to be incorrect (Bertrand et al., 2004).

To overcome this limitation, I use a synthetic control design to estimate the effects of funding cuts on logged teen abortion rates, comparing the outcomes of Texas to the outcomes of a "Synthetic Texas," as suggested by Abadie et al., 2010. Synthetic control models have several advantages over traditional difference-in-differences models when estimating effects for one treatment unit. First, this procedure allows for a data-driven approach to choosing a control group. Second, unobservables remain constant over time, which minimizes the potential for bias.

Intuitively, I utilize data on teen abortion rates from 2005-2010 to identify the weighted average of comparison states that provide the best match for the teen abortion rates observed in Texas prior to the funding cuts. The identification assumption is that the synthetic Texas provides a good match for the outcomes that would have been observed in Texas absent the family planning policy change. If this assumption holds, the difference between the teen abortion rates for Texas and the teen abortion rates for the synthetic control provides an unbiased estimate of the causal effect of

[18]Because of the extent of missing data, I eliminated 15 states that omitted data for one or more years between 2005 and 2012.

the funding cuts.

To execute this strategy, I select the non-negative weights for each potential "donor state" to minimize the function:

$$(X_{TX} - X_{SC}W)'V(X_{TX} - X_{SC}W) \tag{4.2}$$

where X_{TX} is a $(K \times 1)$ vector of variables measuring outcomes from 2005-2010, X_{SC} is a $(K \times J)$ matrix containing the outcome variables for other states, W is a $(J \times 1)$ vector of weights summing to one, and the diagonal matrix V contains the "importance weights" assigned to each variable in X. I include the teen birth rates observed in 2005, 2007, and 2009 in X. These particular variables provide a good match for Texas outcomes in both levels and trends without overfitting. I report results using the data-driven regression method as described in Abadie (2010) to assign variables weights in the V matrix, noting that results are similar when assigning equal weights to each variable. One disadvantage of this approach is that it does not account for the rich set of control variables that would otherwise improve predictive power. Moreover, this approach does not allow for calculation of standard errors. To conduct statistical inference, I estimate the distribution of estimated treatment effects under the null hypothesis of a zero treatment effect and reassign treatment separately to each state in the donor pool to estimate a placebo effect for each state. I then construct p-values for the estimated effect for Texas, given its rank in this distribution. For example, if Texas had the fifth largest estimate in absolute value, then the p-value would be 5/50=0.1.[19]

Figure A.18 presents trends for both Texas teen abortion rates and the synthetic

[19] Abadie et al. (2010) suggest using the ratio of the post-intervention mean square predicted error to the pre-intervention mean square predicted error, implying that when there is a preferred pre-period match between the treated unit and synthetic control, greater weights should be placed on estimated treatment effects.

control. Visually, the trends for Texas and synthetic Texas appear to track each other fairly well from 2007-2010, although they seem to diverge in 2012, indicating a modest effect of family planning funding cuts on teen abortion rates. Panel A of Table A.21 displays estimates from the synthetic control model described above. While I note that these estimates do not indicate that a reduction in abortion rates are driving the increase in teen births, placebo estimates in Figure A.19 suggest that I cannot rule out large effects on funding cuts on teen abortion rates. Therefore, efforts to obtain more convincing estimates of the effects of the cuts on teen abortion rates could be an important avenue for future research.

4.7 Analyzing Changes in STD Rates

I also utilize the same synthetic control approach as described above to analyze teen sexually transmitted disease (STD) rates, as these data are only available at the state level. Because family planning clinic closures imply that fewer teens are able to access condoms and other devices that protect against STDs, we might expect a decrease in family planning funding to be met with increases in teen STD rates.

Figure A.20 presents trends for both Texas teen STD rates and the synthetic control. Since the implementation of the synthetic control design matches on outcomes in 2005, 2007, and 2009, the large differences between STD rates in Texas and synthetic Texas indicate that the synthetic control may not provide a good counterfactual for this outcome. Panel B of Table A.21 indicates that family planning funding cuts increase teen STD rates by about 6% in 2013, although these estimates are not statistically significant.

4.8 Conclusion

This paper analyzes the effects of defunding family planning services on teen birth rates. Using a difference-in-differences approach, I estimate that decreasing funding for family planning in Texas by 67% led to an increase in the teen birth rate by 5%. Estimates effects of funding cuts to publicly funded family planning services are more pronounced in high-poverty counties and counties with Planned Parenthood clinics, which were affected most by the policy change. Moreover, the funding cuts had a more dramatic impact on younger teens, and increased birth rates for women aged 15-17 by 6%. Although the primary stated objective of the funding cuts was to decrease abortion incidence, I find little evidence that reducing family planning funding achieved this goal.

The estimates suggest that nearly 2,200 teens would have not given birth absent the reduction in Texas family planning funding. Given that the National Campaign to Prevent Teen and Unplanned Pregnancy estimates that the average cost of teen childbearing to taxpayers is nearly $27,000 per birth, the estimated costs of the reduction in family planning funding are $81 mil, although this figure does not account for births to older women or births that occurred more recently.[20] Therefore the costs of unintended pregnancy caused by the policy change outweigh the $73 million budget cuts.

The results of this analysis show that funding cuts to family planning services can have consequences that increase costs for the public sector. As five new states are currently considering legislation to defund family planning, it is important for future research to determine to what extent government policies that reduce access to low-cost contraception can influence teen sexual behavior and unintended pregnancy.

In the past two years, the Texas state legislature has restored funding for family planning services by 19% (Texas DSHS, 2014). However, given the high fixed costs of establishing a network of health care facilities, few clinics have been able to rebuild

[20] For example, see Table D.3 for estimated effects of the Texas funding cuts on birth rates for women aged 20-24. Estimates indicate that birth rates for older women increased by 3.7% 3-4 years after the cuts, suggesting that older women are also affected by such policy changes.

and achieve funding comparable to previous levels. It is unclear how the reinstated funding will affect fertility and reproductive health in the years to come, and future work should consider the impacts of the fluctuation of funding on teen health outcomes. Finally, I note this paper provides both important insight on the connection between reductions in family planning funding and teen birth rates and offers motivation for further study of how these policies affect abortion, sexually transmitted diseases, government assistance, educational attainment and labor market outcomes.

5. CONCLUSION

Millions of dollars are at stake each year based on what sexual health programs state legislatures mandate, and this topic is often at the center of political battles. Results from a state-level analysis indicate that teen pregnancy is unresponsive to mandated changes to sex education curriculum. State policies are not effective at reducing unintended pregnancy as compared to pregnancy prevention programs or media interventions, which have been shown to cause reductions in teen birth rates from 6% to 25% (Thomas, 2012; Kearney and Levine, 2014).

Instead, I find that interventions at the clinic level are effective at reducing unintended pregnancy by lowering teen birth rates. Granting teens access to long-acting reversible contraception reduces user error compliance and prevents unintended pregnancy. While only 5% of teenagers currently use a LARC method in the US, this appears to be due in large part to a lack of awareness, misperceptions about safety, and costs. This implies that there may be scope for publicly funded clinics to address teen pregnancy rates by reducing or eliminating these financial barriers.

Finally, cutting funding for family planning services increases teen birth rates by approximately 5%. This estimates suggest that nearly 2,100 Texas teens would have not given birth absent the reduction in family planning funding. Given the potential external costs of teenage motherhood, funding cuts to family planning services can have consequences that increase long-run costs for the public sector and reduce overall social welfare.

Sections 1-3 shed light on the effectiveness of various health and education policies on teen childbearing and contribute to three separate but related literatures regarding sex education, contraception access and Title X funding. My work thus far provides clear policy implications for legislation addressing teen pregnancy as well as suggestions for future allocations of federal and state family planning funding.

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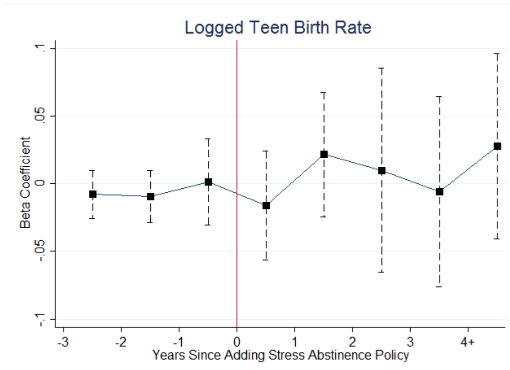
APPENDIX A

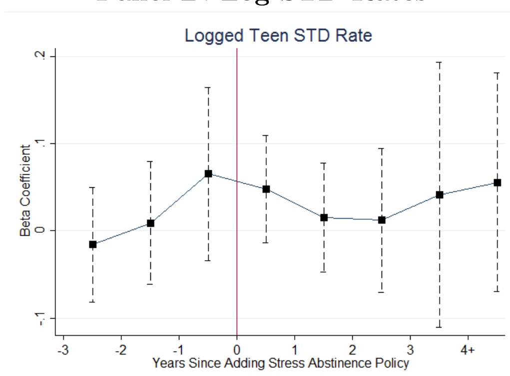
FIGURES AND TABLES

Figure A.1: Divergence in Teen Health Outcomes Before and After Adoption of a Stress-Abstinence Policy, Relative to the Difference 3 or More Years Before Adoption

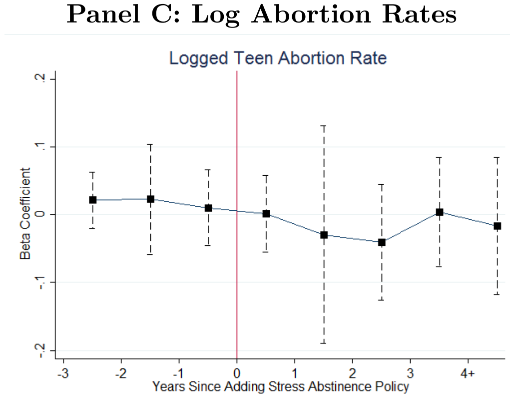
Panel A: Log Birth Rates



Panel B: Log STD Rates



Panel C: Log Abortion Rates



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 5.3, 5.4 and 5.6.

Table A.1: Policy Summary of States that Enacted a Stress Abstinence Mandate

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

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 was provided by the National Association of States Boards of Education Center for Safe and Healthy Schools.

Table A.2: Summary Statistics

	Mean	**St. Dev.**
Outcome Variables		
Births per 1,000 females aged 15-19	38.8	11.4
Births per 1,000 females aged 15-17	20.2	6.8
Births per 1,000 females aged 18-19	66.6	18.9
Births per 1,000 females aged 30-34	92.5	11.2
STDs per 1,000 Teens	20.6	7.3
Abortions Rate per 1,000 females aged 15-19	14.9	7.1
Births per 1,000 Black females aged 15-19	52.7	17.6
Births per 1,000 White females aged 15-19	23.7	11.1
Births per 1,000 Hispanic females aged 15-19	92.8	45.0
Control Variables		
Median Family Income	47,227	7,575
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 (NCHS), Division of Vital Statistics natality files via the online CDC Wonder database. State-level teen STD data, comprised of the total number of gonorrhea, chlamydia, and syphilis cases per year, are from the online, publicly available Centers for Disease Control and Prevention (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 “F"s are given to the states with the most restrictive abortion laws.

Table A.3: 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 Years 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 females ages 15-19 who are black, percent of females ages 15-19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates and median family income.

Table A.4: Effect of Stress-Abstinence Policy on Logged Teen STD 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 Years 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 females ages 15-19 who are black, percent of females ages 15-19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates and median family income.

Table A.5: Effect of Stress-Abstinence Policy on Logged Teen STD 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 Years 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 females ages 15-19 who are black, percent of females ages 15-19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates and median family income.

Table A.6: 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 females ages 15-19 who are black, percent of females 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 due to 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.

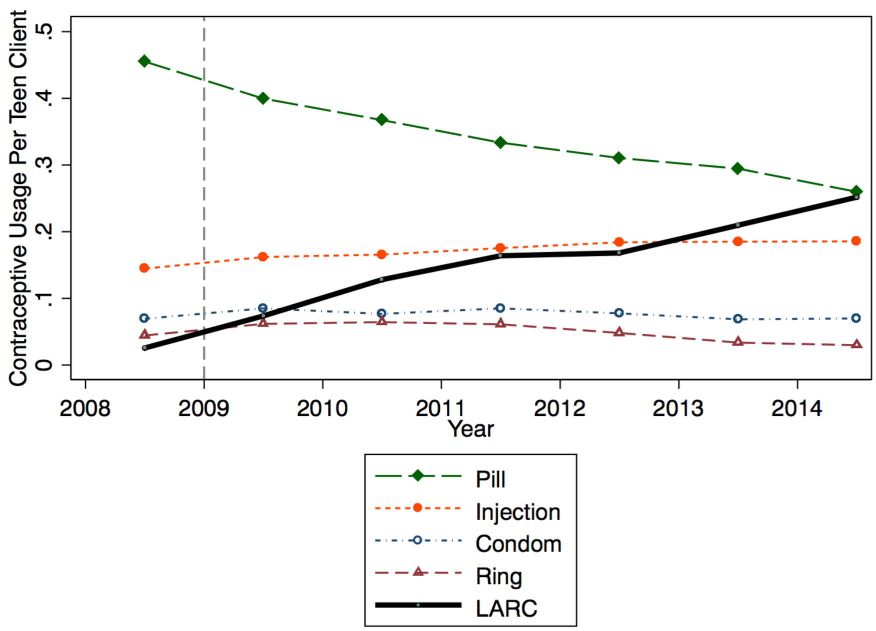
Table A.7: Effect of Stress-Abstinence Policy on Logged Teen Birth Rates for Age and Race Subgroups

	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

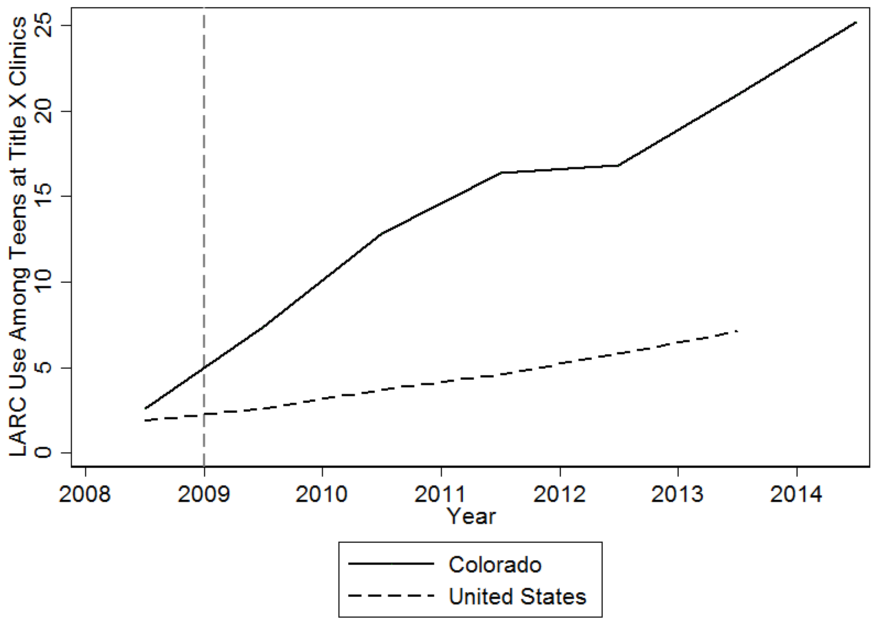
Notes: *p<0.1, **p<0.05, ***p<0.01. Estimates are based on annual data for 26 states from 2000-2011. In Panels A, B and C, the dependent variable for Columns 1-2 is the logged birth rate for females aged 15-17, for Columns 3-4 is the logged birth rate for females aged 18-19 and for Columns 5-6 is the logged birth rates for females 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 females ages 15-17 who are black and percent of females ages 15-17 who are Hispanic, the control variables for Columns 3-4 in the first three panels control for the percent of females ages 18-19 who are black and the percent of females ages 18-19 who are Hispanic, and the control variables for Columns 5-6 in the first three panels control for the percent of females ages 30-34 who are black and the percent of females ages 30-34 who are Hispanic.

Figure A.2: Primary Form of Contraceptive Used By Teens Visiting Title X Clinics in Colorado



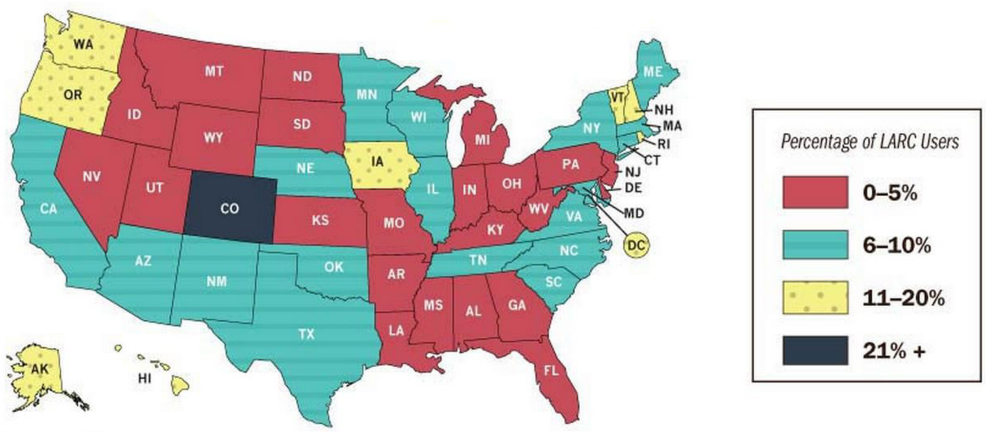
Notes: Authors' calculation based on annual data on Colorado Title X contraception usage by age and method provided by the Colorado Department of Public Health and Environment. The vertical line, drawn at 2009, represents the year Colorado's Family Planning Initiative was implemented.

Figure A.3: LARC Use Among Teens Visiting Title X Clinics, Colorado Versus United States Overall



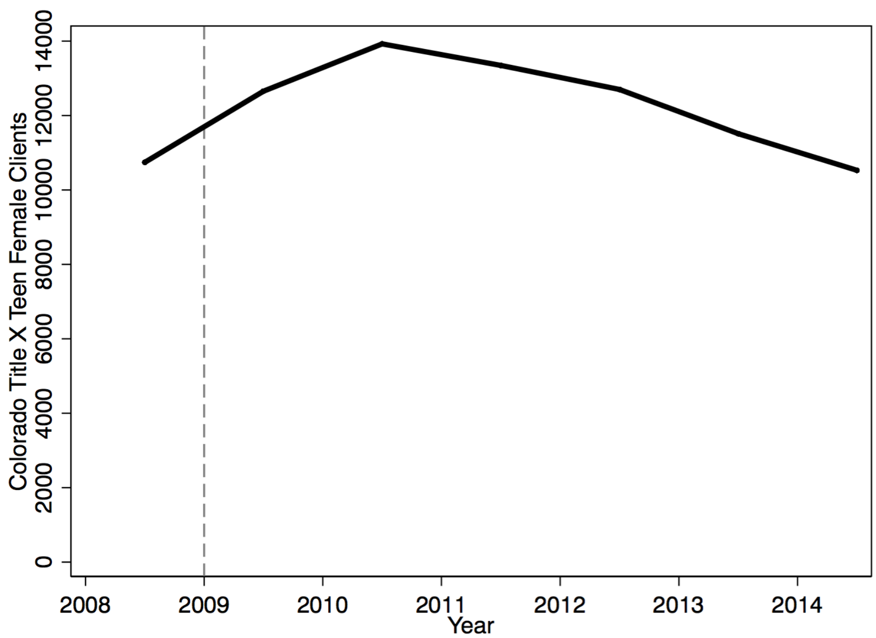
Notes: Numbers for Colorado are authors' calculation based on annual data on Colorado Title X contraception usage by age and method provided by the Colorado Department of Public Health and Environment. Numbers for the United States overall are taken from the Title X Family Planning Annual Report, United States 2013. Note that this figure shows LARC use in Colorado in 2014 for readers' information but the analysis of outcomes only extends through 2013. The vertical line, drawn at 2009, represents the year Colorado's Family Planning Initiative was implemented.

Figure A.4: LARC Use Among Teens Visiting Title X Clinics by State, 2013



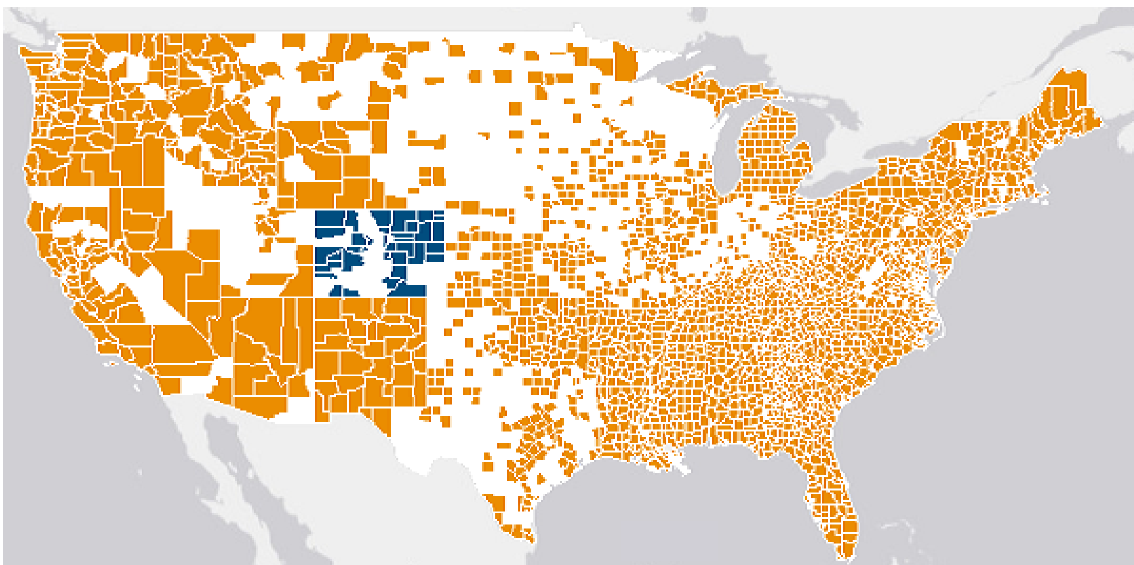
Source: Reproduced from the Title X Family Planning Annual Report, United States 2013.

Figure A.5: Teen Female Visitors to Colorado Title X Clinics Over Time



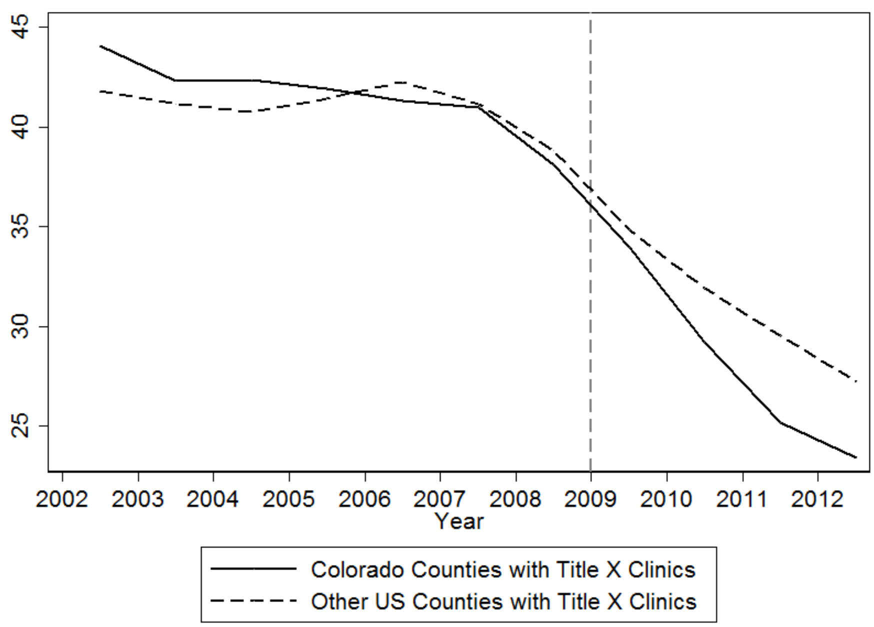
Notes: Authors' calculation based on annual data on Colorado Title X clients and contraception usage by age and method provided by the Colorado Department of Public Health and Environment.

Figure A.6: Counties With Title X Clinics



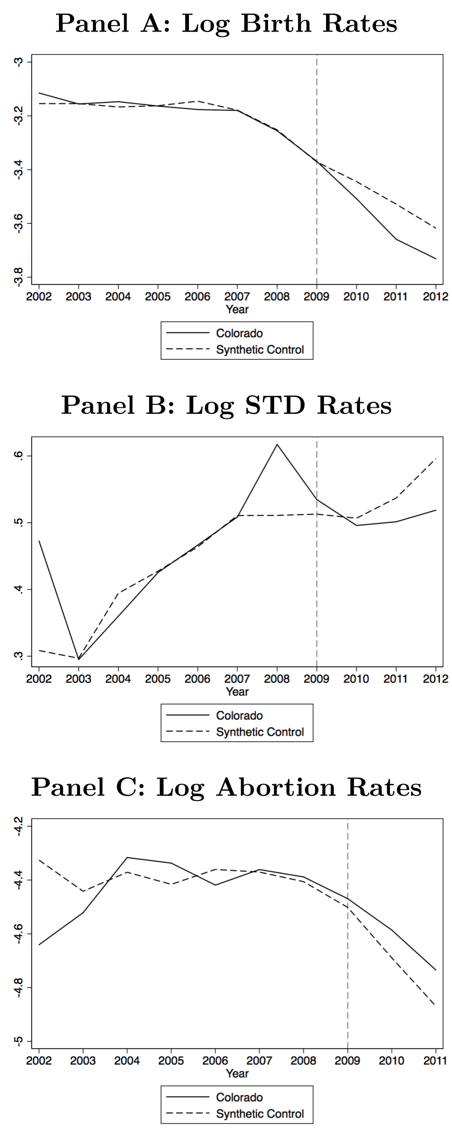
Notes: The above figure highlights counties that contain at least one Title X clinic as of 2009. The locations of Title X clinics in Colorado were obtained from Colorado's Department of Public Health and Environment's Directory of Family Planning Services. Counties with Title X clinics outside of Colorado were identified by geocoding the addresses of such clinics listed in the US Department of Health and Human Service's 340B Database. Counties in navy represent counties with Title X clinics in Colorado.

Figure A.7: Average Teen Birth Rates in Counties With Title X Clinics



Notes: Teen birth rates—with births assigned to the year of conception based on the mother's last menstrual period—for each county are constructed using the National Center for Health Statistics (NCHS), Division of Vital Statistics Natality Files and SEER population data. Counties are weighted by their teen female population. The vertical line represents the beginning of the Colorado Family Planning Initiative.

Figure A.8: Teen Outcomes in Colorado Versus Synthetic Colorado



Notes: Synthetic controls are constructed as the weighted average of states that minimize $(X_{CO} - X_{SC}W)'V(X_{CO} - X_{SC}W)$, where X_{CO} is a (3×1) vector of variables corresponding to Colorado outcomes observed in 2003, 2005, and 2007, calculated using the data-driven regression based method described in Abadie et al. (2010). The donor pool of states omits states with major funding cuts to family planning and states blocking clinics from access to Title X or Medicaid funds during the years spanned by the analysis.

Table A.8: Effectiveness of Various Methods of Contraception

Method	Typical Use	Perfect Use	Coverage Time
Sterilization*	99.9%	99.9%	Lifetime
Intrauterine Device*	99.9%	99.9%	3-12 years
Implant*	99.9%	99.9%	3 years
Injection	97%	99.9%	3 months
NuvaRing*	91%	99.7%	1 month
Oral Contraceptive	91%	99.7%	1 month
Patch	91%	99.7%	1 week
Condom	82%	98%	N/A
No Method	15%	15%	N/A

Notes: * indicates methods funded by the Colorado Family Planning Initiative. Data are from Hatcher et al. (2011).

Table A.9: Summary Statistics For Counties With Title X Clinics

	Colorado Counties *N=37*	**Comparison Counties** *N=2,219*
Pre-Treatment (2002-2008)		
Births per 1,000 females aged 15-19	41.55	41.04
Percent Teens 15 Year-Olds	19.78	19.98
Percent Teens 16 Year-Olds	19.87	20.01
Percent Teens 17 Year-Olds	19.74	19.91
Percent Teens 18 Year-Olds	20.00	20.03
Percent Teens 19 Year-Olds	20.61	20.07
Percent 15 Year-Olds Black	6.11	18.02
Percent 16 Year-Olds Black	6.01	17.88
Percent 17 Year-Olds Black	5.94	17.64
Percent 18 Year-Olds Black	5.71	17.07
Percent 19 Year-Olds Black	5.47	16.68
Percent 15 Year-Olds Hispanic	24.07	18.03
Percent 16 Year-Olds Hispanic	23.87	17.91
Percent 17 Year-Olds Hispanic	23.77	17.88
Percent 18 Year-Olds Hispanic	23.39	18.21
Percent 19 Year-Olds Hispanic	23.69	18.82
Percent 15 Year-Olds White	88.84	75.67
Percent 16 Year-Olds White	88.96	75.76
Percent 17 Year-Olds White	88.97	75.93
Percent 18 Year-Olds White	88.92	76.25
Percent 19 Year-Olds White	88.98	76.43
County Unemployment Rate	4.98	5.46
Post-Treatment (2009-2012)		
Births per 1,000 females aged 15-19	27.92	30.92
Percent Teens 15 Year-Olds	19.26	19.17
Percent Teens 16 Year-Olds	19.46	19.52
Percent Teens 17 Year-Olds	19.68	19.86
Percent Teens 18 Year-Olds	20.43	20.44
Percent 15 Year-Olds Black	6.56	18.03
Percent 16 Year-Olds Black	6.58	18.24
Percent 17 Year-Olds Black	6.74	18.38
Percent 18 Year-Olds Black	6.78	18.06
Percent 19 Year-Olds Black	6.53	17.90
Percent 15 Year-Olds Hispanic	27.89	21.54
Percent 16 Year-Olds Hispanic	27.57	21.31
Percent 17 Year-Olds Hispanic	27.46	21.12
Percent 18 Year-Olds Hispanic	26.47	21.16
Percent 19 Year-Olds Hispanic	26.25	21.40
Percent 15 Year-Olds White	87.36	74.73
Percent 16 Year-Olds White	87.41	74.56
Percent 17 Year-Olds White	87.31	74.45
Percent 18 Year-Olds White	87.26	74.65
Percent 19 Year-Olds White	87.37	74.64
County Unemployment Rate	8.02	9.21

Notes: Births are based on the National Center for Health Statistics (NCHS), Division of Vital Statistics Natality Files. Population data, including race, ethnicity, and age are from SEER. Unemployment rates are from the BLS.

Table A.10: Poisson Estimates of the Effect of the CFPI on Teen Birth Rates, Difference-in-Differences using Counties with Title X Clinics outside Colorado for Comparison

	(1)	(2)	(3)	(4)	(5)
Effect of Initiative in First Year	-0.042	0.005	-0.005	-0.006	-0.007
	(0.032)	(0.017)	(0.016)	(0.016)	(0.016)
Effect of Initiative in Second Year	-0.103***	-0.044**	-0.047**	-0.048**	-0.052**
	(0.034)	(0.022)	(0.021)	(0.021)	(0.021)
Effect of Initiative in Third Year	-0.170***	-0.101***	-0.094***	-0.098***	-0.096***
	(0.037)	(0.026)	(0.026)	(0.027)	(0.028)
Effect of Initiative in Fourth Year	-0.163***	-0.083**	-0.068*	-0.073*	-0.069*
	(0.051)	(0.037)	(0.038)	(0.039)	(0.039)
Average effect	-0.120	-0.056	-0.054	-0.056	-0.056
P-value (test average effect = 0)	0.001	0.012	0.015	0.013	0.013
Average effect in years 2-4	-0.145	-0.076	-0.070	-0.073	-0.073
P-value (test average effect in years 2-4 = 0)	0.000	0.003	0.007	0.005	0.006
Observations	24816	24816	24816	24816	19294
Counties	2256	2256	2256	2256	1754
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
County Linear Time Trends	No	Yes	Yes	Yes	Yes
Economic and Demographic Controls	No	No	Yes	Yes	Yes
Contraceptive Policy Controls	No	No	No	Yes	Yes
Restricted sample	No	No	No	No	Yes

Notes: Estimates are based on annual data on counties from 2002–2012. Births are assigned to the year of conception based on the mother's reported last menstrual period. The control for economic conditions is the county unemployment rate and demographic control variables include percent of teens who are black, percent of teens who are Hispanic, the fraction of teens by age and race. Contraceptive policy controls are state-by-year variables indicating whether over-the-counter access to emergency contraceptives are permitted and whether private insurance plans that cover prescription drugs are required to cover any FDA-approved contraceptive. The restricted sample omits counties in states with major funding cuts to family planning (TX, NJ, MT, NH, ME) and in states blocking clinics affiliated with abortion providers from access to Title X (KS, NH, NC, TN, WI, IN, TX) or Medicaid funds (IN, AZ) during the years spanned by the analysis. Robust standard errors clustered at the county level are shown in parentheses. *, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.11: Poisson Estimates of the Effect of the CFPI on Teen Birth Rates by County Poverty Rates, Difference-in-Differences using Counties with Title X Clinics outside Colorado for Comparison

	Counties with Poverty Rate > CO Median				Counties with Poverty Rate ≤ CO Median			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect of Initiative in First Year	0.000	-0.006	-0.006	-0.008	0.011	-0.002	-0.003	-0.004
	(0.019)	(0.018)	(0.018)	(0.018)	(0.030)	(0.030)	(0.030)	(0.030)
Effect of Initiative in Second Year	-0.057***	-0.059***	-0.060***	-0.066***	-0.027	-0.028	-0.030	-0.028
	(0.016)	(0.015)	(0.015)	(0.014)	(0.043)	(0.042)	(0.042)	(0.042)
Effect of Initiative in Third Year	-0.115***	-0.112***	-0.118***	-0.119***	-0.073*	-0.062	-0.048	-0.061
	(0.033)	(0.031)	(0.033)	(0.033)	(0.039)	(0.042)	(0.043)	(0.045)
Effect of Initiative in Fourth Year	-0.104**	-0.092**	-0.099**	-0.096**	-0.041	-0.027	-0.013	-0.028
	(0.042)	(0.043)	(0.045)	(0.045)	(0.059)	(0.060)	(0.061)	(0.062)
Average effect	-0.069	-0.067	-0.071	-0.073	-0.032	-0.030	-0.024	-0.030
P-value (test average effect = 0)	0.001	0.002	0.001	0.001	0.411	0.452	0.553	0.457
Average effect in years 2-4	-0.092	-0.088	-0.093	-0.094	-0.047	-0.039	-0.030	-0.039
P-value (test average effect in years 2-4 = 0)	0.001	0.001	0.001	0.001	0.284	0.377	0.494	0.393
Observations	18139	18139	18139	13981	6677	6677	6677	5313
Counties	1649	1649	1649	1271	607	607	607	483
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic and Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Contraceptive Policy Controls	No	No	Yes	Yes	No	No	Yes	Yes
Restricted sample	No	No	No	Yes	No	No	No	Yes

Notes: See Table A.10.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.12: Poisson Estimates of Lead Terms in Difference-in-Differences Model using Counties with Title X Clinics outside Colorado for Comparison

	All Counties				Counties with Poverty Rate > CO Median				Counties with Poverty Rate ≤ CO Median			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Effect of Initiative in First Year	-0.007	-0.003	0.008	-0.016	-0.008	0.003	0.003	-0.025	-0.004	-0.006	0.023	0.005
	(0.016)	(0.020)	(0.030)	(0.037)	(0.018)	(0.020)	(0.032)	(0.041)	(0.030)	(0.039)	(0.054)	(0.067)
Effect of Initiative in Second Year	-0.052**	-0.047**	-0.034	-0.062*	-0.066***	-0.052***	-0.053*	-0.085*	-0.028	-0.031	0.003	-0.018
	(0.021)	(0.023)	(0.032)	(0.038)	(0.014)	(0.015)	(0.028)	(0.044)	(0.042)	(0.048)	(0.059)	(0.063)
Effect of Initiative in Third Year	-0.096***	-0.091***	-0.076*	-0.108**	-0.119***	-0.103***	-0.103**	-0.140**	-0.061	-0.064	-0.025	-0.049
	(0.028)	(0.029)	(0.044)	(0.053)	(0.033)	(0.028)	(0.048)	(0.063)	(0.045)	(0.056)	(0.076)	(0.088)
Effect of Initiative in Fourth Year	-0.069*	-0.063*	-0.047	-0.082	-0.096**	-0.078**	-0.078	-0.119*	-0.028	-0.032	0.011	-0.015
	(0.039)	(0.038)	(0.056)	(0.060)	(0.045)	(0.035)	(0.064)	(0.070)	(0.062)	(0.073)	(0.090)	(0.094)
One Year Before Initiative		0.007	0.016	-0.004		0.021	0.021	-0.002		-0.005	0.019	0.004
		(0.015)	(0.018)	(0.028)		(0.020)	(0.022)	(0.037)		(0.019)	(0.032)	(0.047)
Two Years Before Initiative			0.013	-0.003			-0.000	-0.019			0.036	0.024
			(0.021)	(0.026)			(0.028)	(0.031)			(0.025)	(0.039)
Three Years Before Initiative				-0.020				-0.023				-0.015
				(0.018)				(0.025)				(0.025)
Average effect	-0.056	-0.051	-0.037	-0.067	-0.073	-0.057	-0.058	-0.092	-0.030	-0.033	0.003	-0.019
P-value (test average effect = 0)	0.013	0.032	0.328	0.135	0.001	0.000	0.146	0.072	0.457	0.509	0.965	0.800
Average effect in years 2-4	-0.073	-0.067	-0.052	-0.084	-0.094	-0.078	-0.078	-0.115	-0.039	-0.042	-0.004	-0.027
P-value (test average effect in years 2-4 = 0)	0.006	0.013	0.214	0.084	0.001	0.000	0.081	0.043	0.393	0.446	0.959	0.731
Observations	19294	19294	19294	19294	13981	13981	13981	13981	5313	5313	5313	5313
Counties	1754	1754	1754	1754	1271	1271	1271	1271	483	483	483	483
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic and Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Contraceptive Policy Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Restricted sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: See Table A.10.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.13: WLS Estimates of the Effect of the CFPI on Teen Birth Rates, Difference-in-Differences using Counties with Title X
Clinics outside Colorado for Comparison

	All Counties				Counties with Poverty Rate > CO Median				Counties with Poverty Rate ≤ CO Median			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Effect of Initiative in First Year	0.010	0.000	-0.001	-0.002	-0.005	-0.012	-0.013	-0.015	0.018	0.011	0.010	0.009
	(0.025)	(0.024)	(0.024)	(0.024)	(0.019)	(0.018)	(0.018)	(0.018)	(0.037)	(0.037)	(0.037)	(0.037)
Effect of Initiative in Second Year	-0.033	-0.035	-0.037	-0.039	-0.060***	-0.062***	-0.064***	-0.071***	-0.016	-0.019	-0.019	-0.018
	(0.029)	(0.029)	(0.029)	(0.029)	(0.017)	(0.017)	(0.017)	(0.016)	(0.045)	(0.045)	(0.045)	(0.045)
Effect of Initiative in Third Year	-0.089***	-0.083***	-0.077**	-0.087***	-0.112***	-0.109***	-0.113***	-0.121***	-0.067*	-0.063	-0.034	-0.062
	(0.029)	(0.030)	(0.031)	(0.032)	(0.040)	(0.038)	(0.039)	(0.040)	(0.040)	(0.043)	(0.045)	(0.046)
Effect of Initiative in Fourth Year	-0.067	-0.054	-0.049	-0.058	-0.096*	-0.085*	-0.090*	-0.097*	-0.036	-0.033	-0.004	-0.031
	(0.043)	(0.044)	(0.044)	(0.045)	(0.050)	(0.051)	(0.052)	(0.053)	(0.060)	(0.061)	(0.063)	(0.064)
Average effect	-0.044	-0.043	-0.041	-0.046	-0.068	-0.067	-0.070	-0.076	-0.025	-0.026	-0.012	-0.025
P-value (test average effect = 0)	0.106	0.122	0.146	0.105	0.010	0.009	0.008	0.005	0.528	0.531	0.780	0.553
Average effect in years 2-4	-0.063	-0.057	-0.055	-0.061	-0.089	-0.085	-0.089	-0.097	-0.040	-0.038	-0.019	-0.037
P-value (test average effect in years 2-4 = 0)	0.036	0.058	0.077	0.051	0.005	0.007	0.006	0.004	0.352	0.386	0.673	0.427
Observations	24816	24816	24816	19294	18139	18139	18139	13981	6677	6677	6677	5313
Counties	2256	2256	2256	1754	1649	1649	1649	1271	607	607	607	483
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic and Demographic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Contraceptive Policy Controls	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Restricted sample	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

Notes: See Table A.10 but note that these estimates are based on the weighted least squares analogue to the Poisson model. Cells are weighted by the population of female teens.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.14: Poisson Estimates of the Effect of the CFPI on Teen Birth Rates, Difference-in-Differences using Colorado Counties without Title X Clinics for Comparison

	All Counties		Poverty Rate > CO Median		Poverty Rate ≤ CO Median	
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of Initiative in First Year	0.011	0.041	-0.018	0.053	0.090*	0.217**
	(0.058)	(0.057)	(0.073)	(0.077)	(0.052)	(0.110)
Effect of Initiative in Second Year	-0.115	-0.136*	-0.179*	-0.172*	0.058	0.168
	(0.085)	(0.072)	(0.095)	(0.089)	(0.091)	(0.168)
Effect of Initiative in Third Year	-0.060	-0.081	-0.079	-0.081	-0.021	0.063
	(0.087)	(0.077)	(0.114)	(0.098)	(0.090)	(0.156)
Effect of Initiative in Fourth Year	-0.053	-0.104	-0.067	-0.130	-0.027	0.101
	(0.134)	(0.112)	(0.177)	(0.150)	(0.107)	(0.168)
Average effect	-0.054	-0.070	-0.086	-0.082	0.025	0.137
P-value (test average effect = 0)	0.486	0.307	0.394	0.360	0.703	0.292
Average effect in years 2-4	-0.076	-0.107	-0.108	-0.128	0.004	0.111
P-value (test average effect in years 2-4 = 0)	0.423	0.181	0.383	0.227	0.961	0.439
Observations	704	704	407	407	297	297
Counties	64	64	37	37	27	27
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
County Linear Time Trends	Yes	Yes	Yes	Yes	Yes	Yes
Economic and Demographic Controls	No	Yes	No	Yes	No	Yes
Contraceptive Policy Controls	n/a	n/a	n/a	n/a	n/a	n/a
Restricted Sample	n/a	n/a	n/a	n/a	n/a	n/a

Notes: See Table A.10 but note that the comparison group used for these estimates is counties in Colorado without Title X clinics.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.15: Triple Difference Estimates of the Effect of the CFPI on Teen Birth Rates

	All Counties			Poverty Rate > CO Median			Poverty Rate ≤ CO Median		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Estimates Without County Linear Trends									
Effect of Initiative in First Year	-0.013	0.003	-0.014	-0.020	0.010	-0.006	0.017	0.052	0.037
	(0.062)	(0.053)	(0.055)	(0.084)	(0.073)	(0.074)	(0.042)	(0.049)	(0.051)
Effect of Initiative in Second Year	-0.133**	-0.130**	-0.124**	-0.189***	-0.177***	-0.156**	0.001	0.002	-0.012
	(0.063)	(0.058)	(0.059)	(0.065)	(0.061)	(0.063)	(0.090)	(0.099)	(0.097)
Effect of Initiative in Third Year	-0.118	-0.122*	-0.141**	-0.115	-0.124	-0.155*	-0.143***	-0.148**	-0.152**
	(0.072)	(0.072)	(0.071)	(0.099)	(0.093)	(0.093)	(0.051)	(0.060)	(0.066)
Effect of Initiative in Fourth Year	-0.124	-0.132	-0.156	-0.116	-0.125	-0.127	-0.146**	-0.166**	-0.202**
	(0.091)	(0.095)	(0.096)	(0.124)	(0.121)	(0.121)	(0.064)	(0.076)	(0.082)
Average effect	-0.097	-0.095	-0.109	-0.110	-0.104	-0.111	-0.068	-0.065	-0.082
P-value (test average effect = 0)	0.071	0.069	0.039	0.134	0.136	0.111	0.063	0.160	0.091
Average effect in years 2-4	-0.125	-0.128	-0.140	-0.140	-0.142	-0.146	-0.096	-0.104	-0.122
P-value (test average effect in years 2-4 = 0)	0.054	0.045	0.028	0.113	0.090	0.079	0.018	0.044	0.025
Observations	34439	34429	25233	23098	23091	16711	11341	11338	8522
Counties	3132	3131	2295	2101	2100	1520	1031	1031	775
Economic and Demographic Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Restricted Sample	No	No	Yes	No	No	Yes	No	No	Yes
Panel B: Estimates With County Linear Trends									
Effect of Initiative in First Year	0.032	0.034	0.019	0.020	0.023	0.009	0.084	0.092	0.077
	(0.059)	(0.058)	(0.060)	(0.074)	(0.072)	(0.076)	(0.055)	(0.057)	(0.059)
Effect of Initiative in Second Year	-0.076	-0.084	-0.072	-0.139	-0.148	-0.118	0.086	0.066	0.051
	(0.086)	(0.084)	(0.087)	(0.096)	(0.092)	(0.095)	(0.093)	(0.092)	(0.095)
Effect of Initiative in Third Year	-0.048	-0.051	-0.059	-0.054	-0.065	-0.073	-0.040	-0.052	-0.064
	(0.089)	(0.085)	(0.086)	(0.115)	(0.109)	(0.111)	(0.093)	(0.096)	(0.099)
Effect of Initiative in Fourth Year	-0.041	-0.058	-0.070	-0.043	-0.068	-0.047	-0.025	-0.051	-0.096
	(0.135)	(0.130)	(0.132)	(0.178)	(0.170)	(0.172)	(0.111)	(0.116)	(0.119)
Average effect	-0.033	-0.040	-0.046	-0.054	-0.065	-0.057	0.026	0.014	-0.008
P-value (test average effect = 0)	0.673	0.607	0.565	0.594	0.508	0.566	0.704	0.844	0.910
Average effect in years 2-4	-0.055	-0.064	-0.067	-0.079	-0.094	-0.080	0.007	-0.012	-0.037
P-value (test average effect in years 2-4 = 0)	0.566	0.486	0.476	0.528	0.429	0.509	0.928	0.878	0.655
Observations	34439	34429	25233	23098	23091	16711	11341	11338	8522
Counties	3132	3131	2295	2101	2100	1520	1031	1031	775
Economic and Demographic Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Restricted Sample	No	No	Yes	No	No	Yes	No	No	Yes

Notes: See Table A.10 but note that these estimates are based on a triple-difference model, comparing counties with and without Title X clinics in Colorado and outside of Colorado. Specifically, all estimates are based on a model that includes county fixed effects, year fixed effects, state-by-year fixed effects, and year effects specific to counties with Title X clinics.

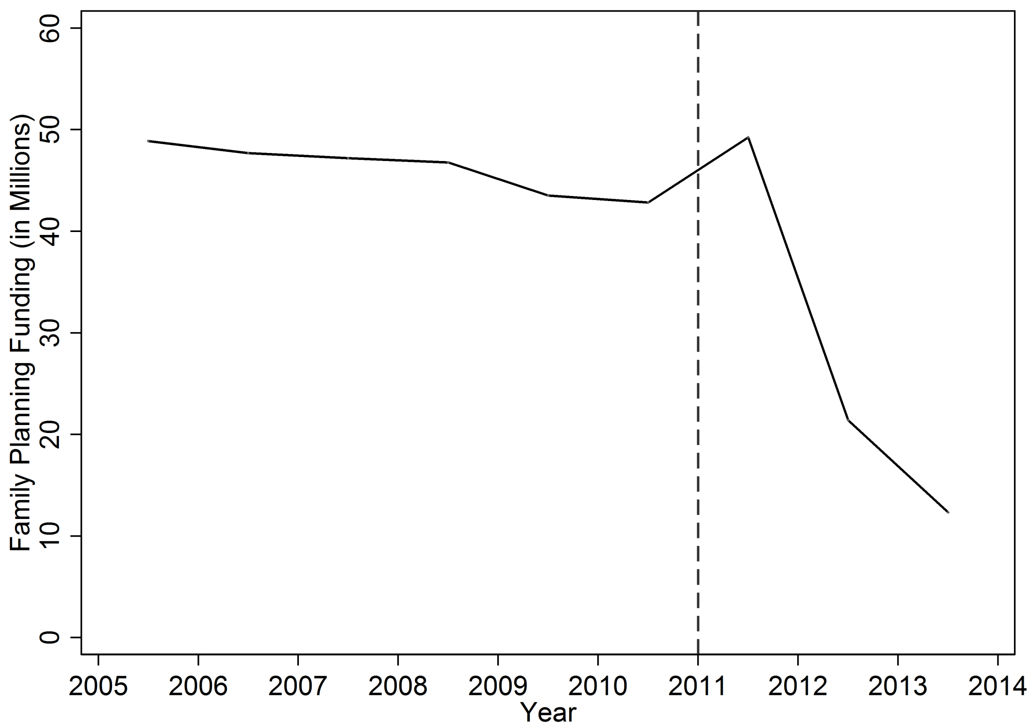
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.16: State-level Synthetic Control Estimates of the Effects of the CFPI on Log Teen Birth Rates, STDs, and Abortions

Panel A: Log Teen Birth Rates	Estimate	P-value
Effect of Initiative in First Year	-0.008	0.950
Effect of Initiative in Second Year	-0.067	0.275
Effect of Initiative in Third Year	-0.134	0.050
Effect of Initiative in Fourth Year	-0.117	0.200
Average Effect in Years 1-4	-0.079	0.200
Average Effect in Years 2-4	-0.106	0.200
Panel B: Log Teen STD Rates	Estimate	P-value
Effect of Initiative in First Year	-0.011	0.950
Effect of Initiative in Second Year	-0.044	0.775
Effect of Initiative in Third Year	-0.069	0.725
Effect of Initiative in Fourth Year	-0.110	0.475
Average Effect in Years 1-4	-0.059	0.900
Average Effect in Years 2-4	-0.074	0.775
Panel C: Log Teen Abortion Rates	Estimate	P-value
Effect of Initiative in First Year	0.074	0.583
Effect of Initiative in Second Year	0.145	0.416
Effect of Initiative in Third Year	0.176	0.500
Average Effect in Years 1-3	0.132	0.583
Average Effect in Years 2-3	0.161	0.583

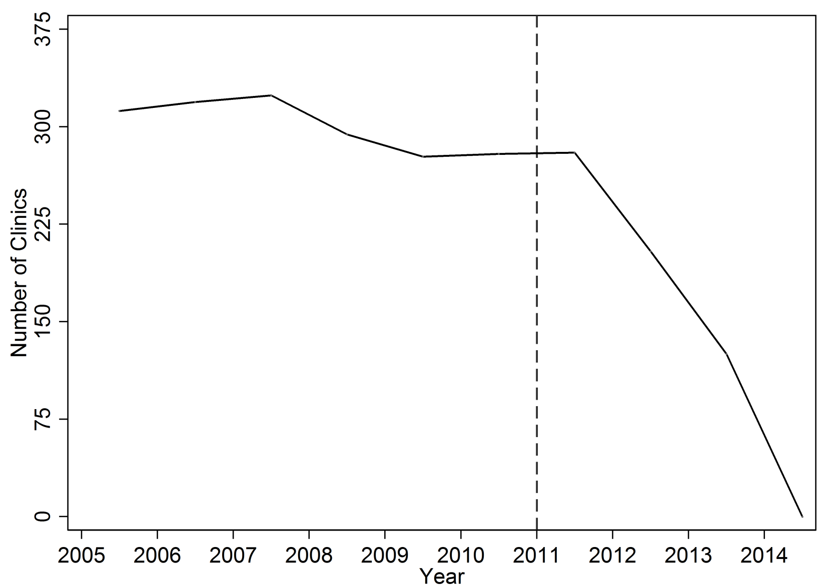
Notes: The synthetic control for Colorado for estimating the effect on each outcome is constructed as the weighted average of states that minimize $(X_{CO} - X_{SC}W)'V(X_{CO} - X_{SC}W)$, where X_{CO} is a (3×1) vector of variables corresponding to Colorado outcomes observed in 2003, 2005, and 2007; X_{SC} is a (3×39) matrix containing the same variables for states in the donor pool; for the synthetic control; W contains the weight for each state; and the diagonal matrix V contains the "importance weights" assigned to each variable in X based on the data-driven regression based method described in Abadie et al. (2010). The donor pool of states includes all states included in the "restricted sample" of our other analyses. That is, it omits states with major funding cuts to family planning (Texas, New Jersey, Montana, New Hampshire, Maine) and states blocking clinics affiliated with abortion providers from access to Title X (Kansas, New Hampshire, North Carolina, Tennessee, Wisconsin, Indiana, Texas) or Medicaid funds (Indiana, Arizona) during the years spanned by the analysis. The analysis of abortions additionally omits the 18 states that have no annual data for any year between 2003 and 2011. Permutation-based p-values are based on the distribution of estimated treatment effects obtained by reassigning treatment to each state in the donor pool, estimating the effects using the same synthetic control approach, and calculating the ratio of the post-intervention mean square predicted error to the pre-intervention mean square predicted error. The estimated effects for each state in each period from this process are shown in Appendix Figure C.1.

Figure A.9: Texas Family Planning Funding Over Time



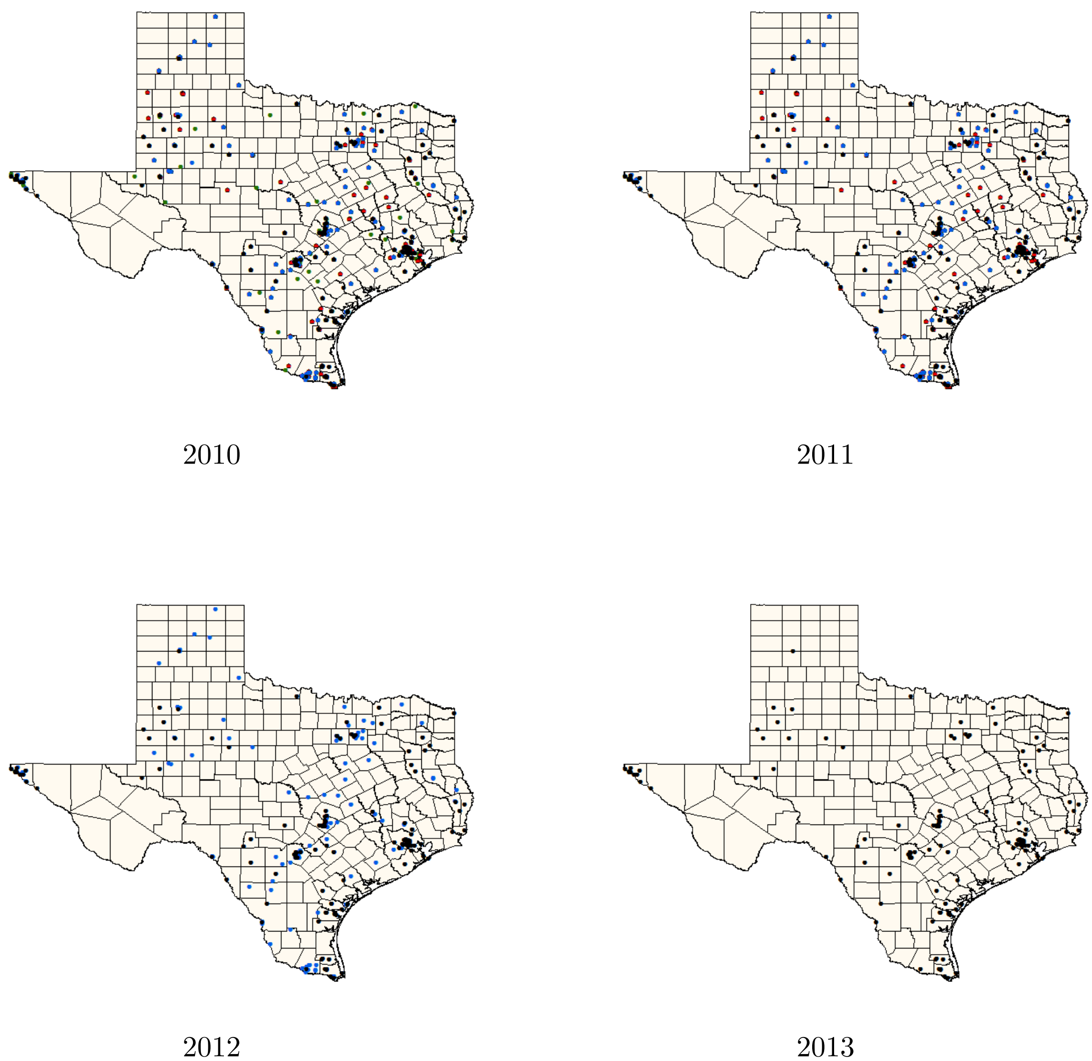
Notes: Author's calculation based on annual funding contract data provided by the Texas Department of State Health Services.

Figure A.10: Texas Publicly Funded Family Planning Clinics Over Time



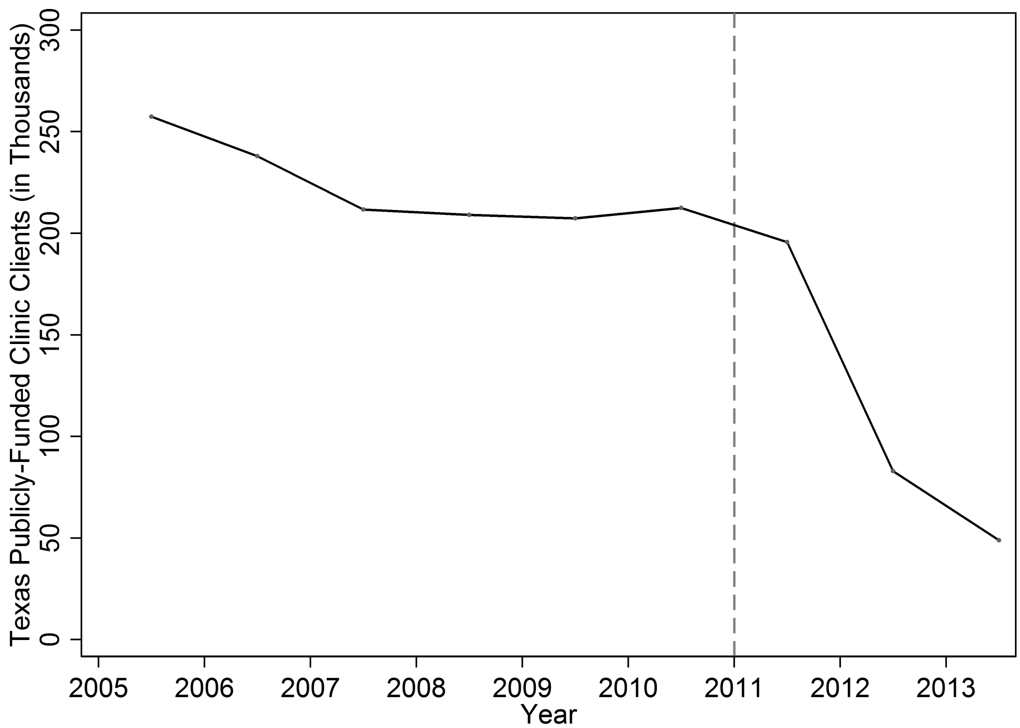
Notes: Author's calculation based on clinic location data provided by the Texas Department of State Health Services.

Figure A.11: Texas Publicly Funded Family Planning Clinic Locations Over Time



Notes: Author's calculation based on geocoded clinic location data provided by the Texas Department of State Health Services.

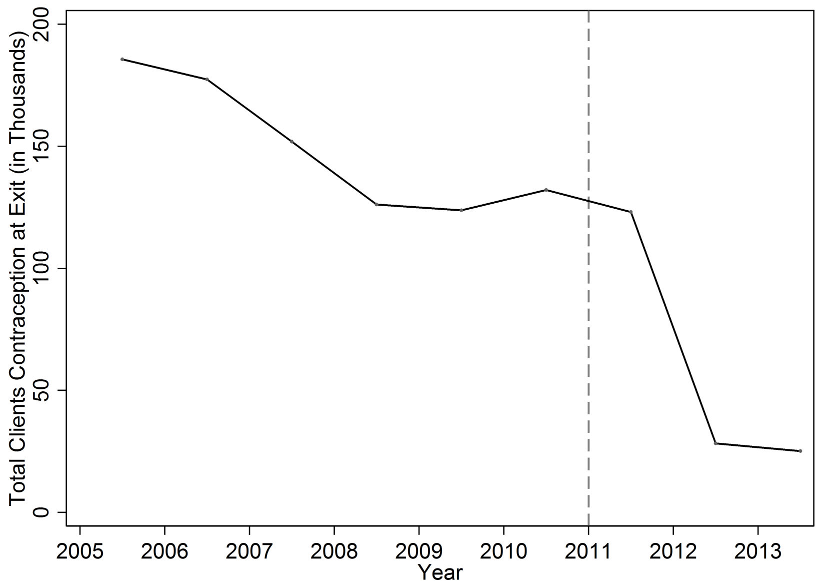
Figure A.12: Texas Family Planning Clinic Clients Over Time



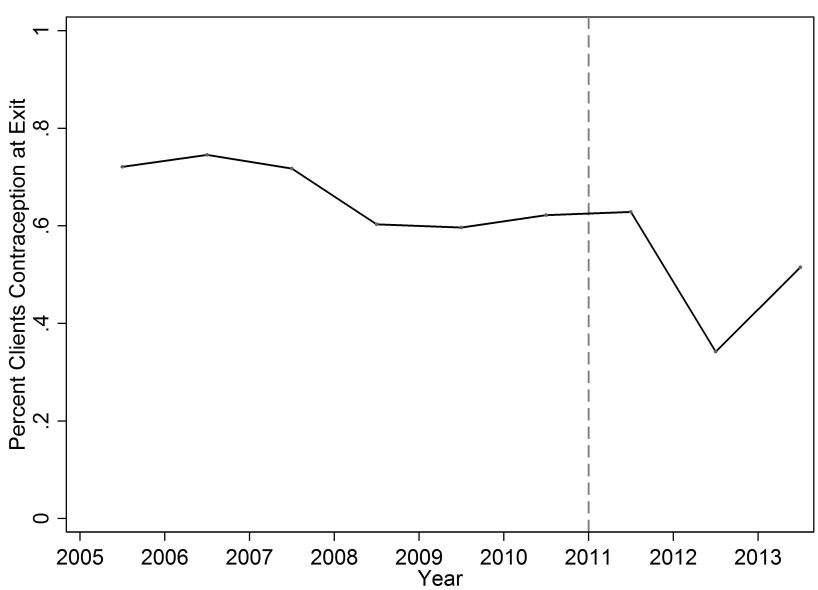
Notes: Author's calculation of the total number of clients based on annual data provided by the Texas Department of State Health Services.

Figure A.13: Contraception Use By Clinic Clients

Panel A. Total Number of Clients Using Contraception at Exit

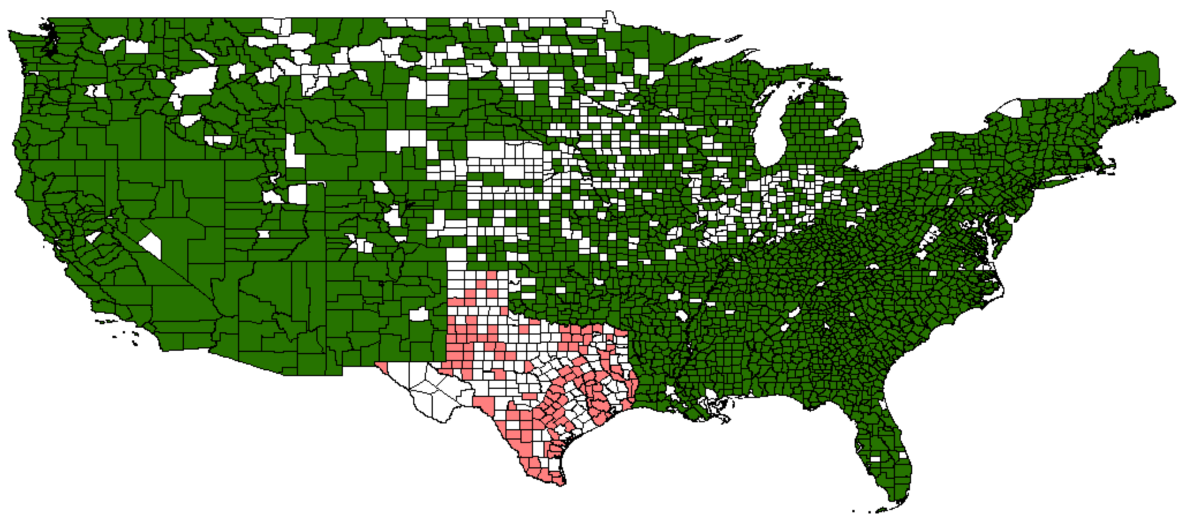


Panel B. Percent of Clients Using Contraception at Exit



Notes: Author's calculation of family planning clients using or obtaining contraceptive devices (intrauterine devices, implants, injections, oral contraceptives, patches, rings, cervical caps) at exit, based on annual data provided by the Texas Department of State Health Services.

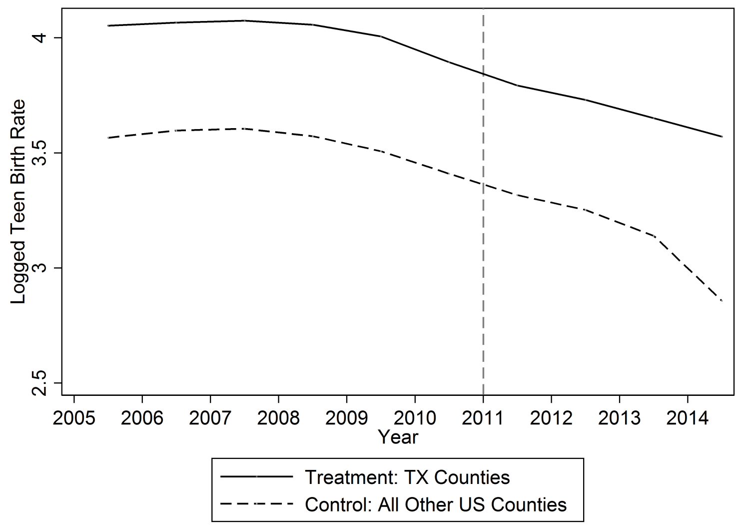
Figure A.14: Counties with Publicly Funded Family Planning Clinics



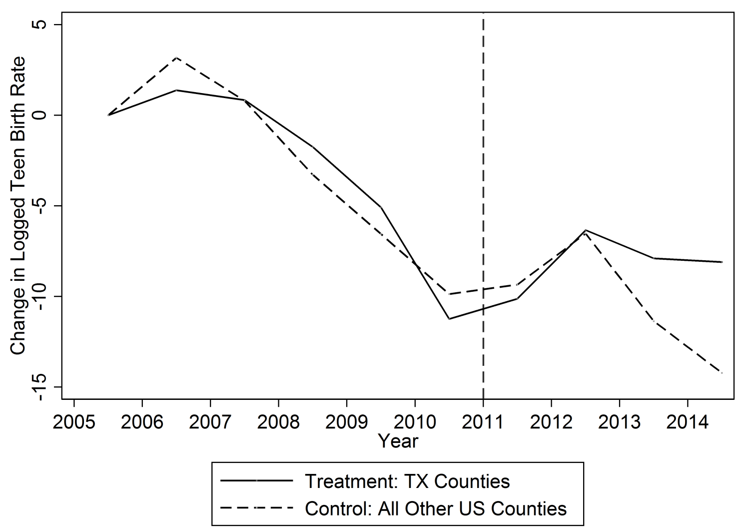
Notes: Highlighted above are all U.S. counties that contain one or more publicly funded family planning clinics as of 2010. Texas counties comprise the treatment group for the main analysis and are highlighted in red. Clinic locations for Texas counties is identified from geocoded data provided by the Texas Department of State Health Services. Clinic data for U.S. counties outside Texas is from the Guttmacher Institute.

Figure A.15: Trends in Teen Birth Rates in Counties with Publicly Funded Clinics

Panel A. Logged Teen Birth Rates in Counties with Publicly Funded Clinics

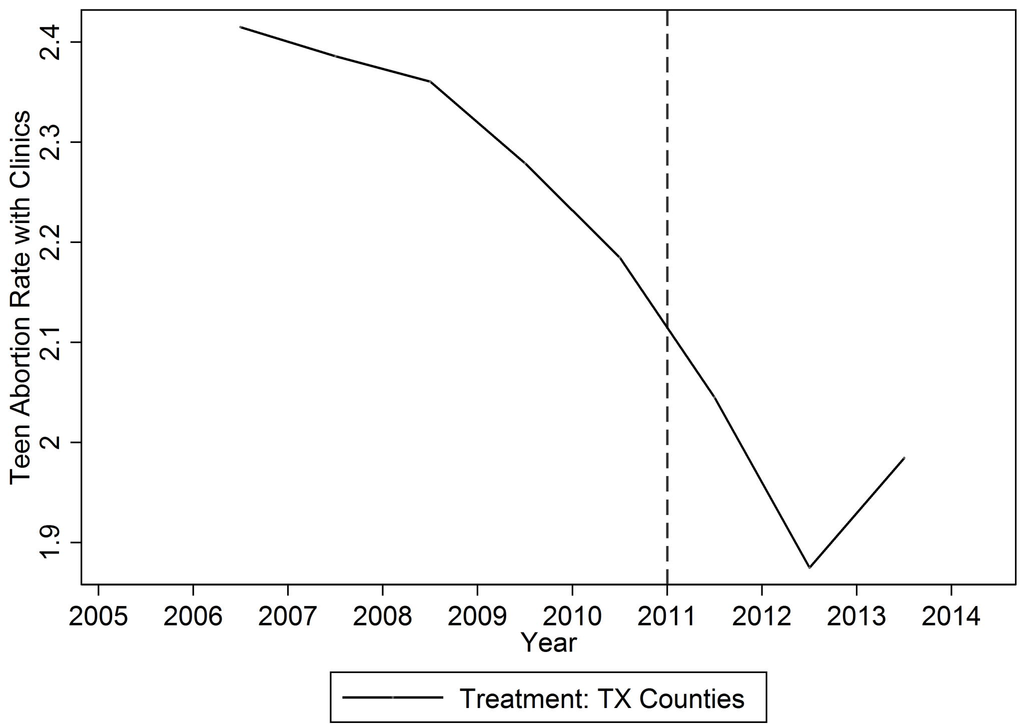


Panel B. Percent Change in Teen Birth Rates



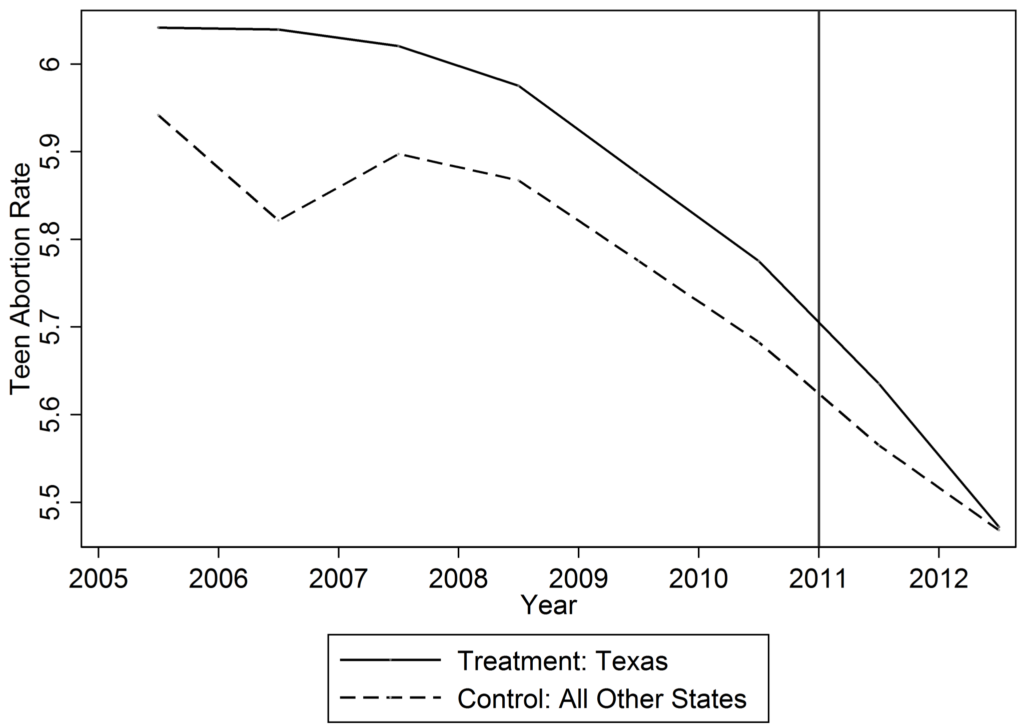
Notes: Teen birth rates are constructed using the National Center for Health Statistics, Division of Vital Satistics Natlity Files and SEER population data. The vertical line represents the beginning of funding cuts to Texas family planning clinics.

Figure A.16: Logged Teen Abortion Rates in Texas Counties with a Publicly Funded Clinic



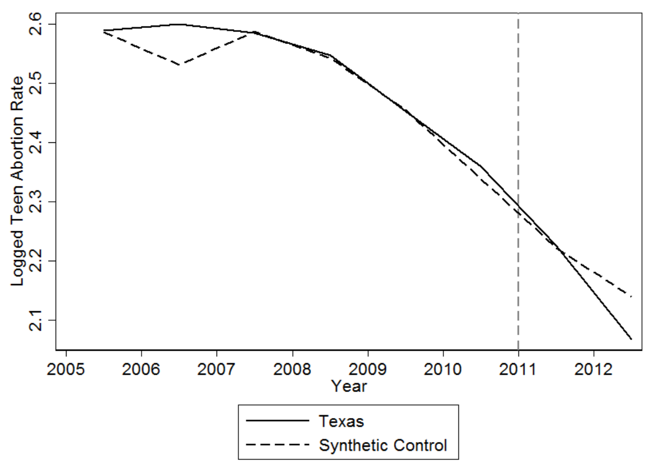
Notes: Teen abortion rates are constructed using annual county-level data from the Texas Department of State Health Services. The vertical line represents the beginning of funding cuts to Texas family planning clinics.

Figure A.17: Logged Teen Abortion Rates by State



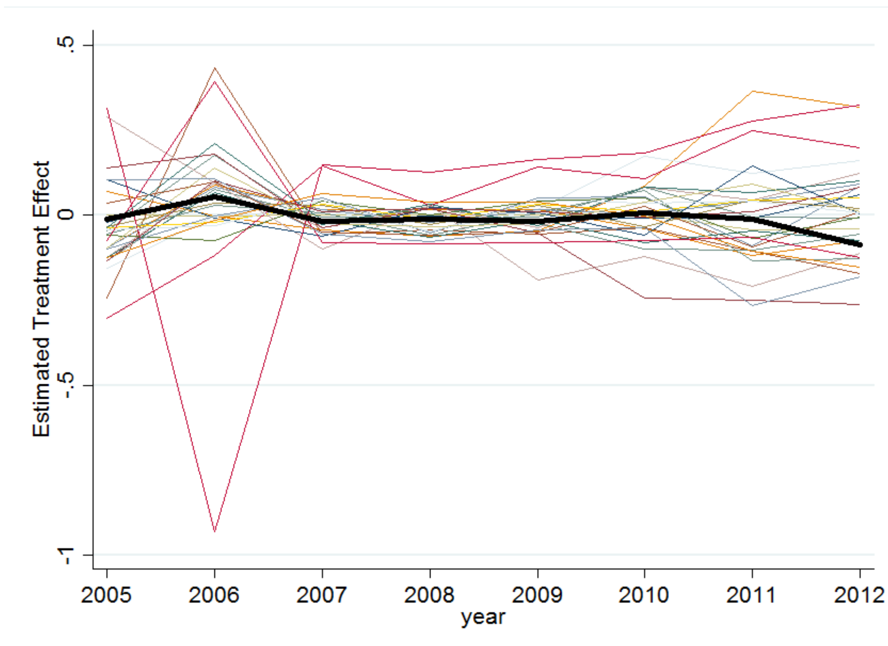
Notes: Teen abortion rates are constructed using the annual Center for Disease Control and Prevention Abortion Surveillance and SEER population data. The vertical line represents the beginning of funding cuts to Texas family planning clinics.

Figure A.18: Teen Abortion Rates in Texas Versus Synthetic Texas



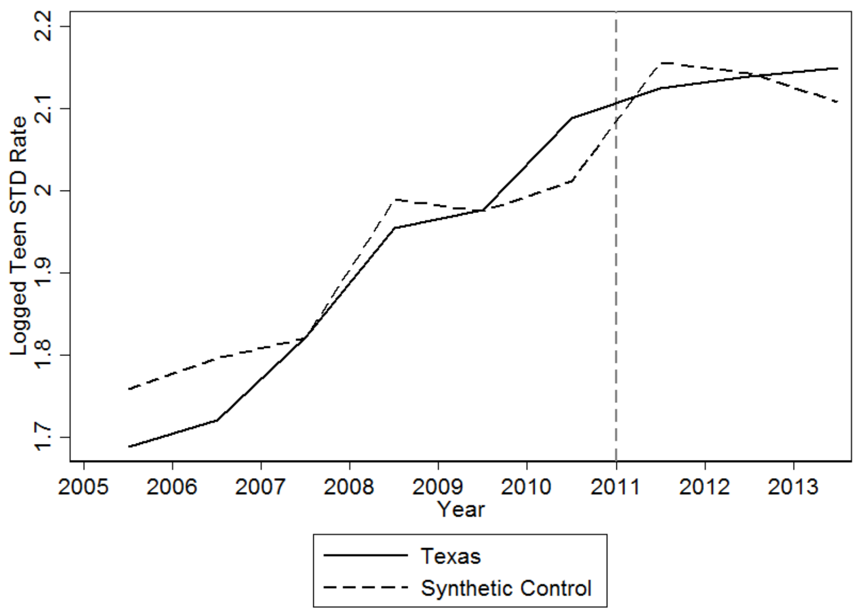
Notes: Teen abortion rates are constructed using the annual Center for Disease Control and Prevention Abortion Surveillance and SEER population data. The vertical line represents the beginning of funding cuts to Texas family planning clinics. I omit states with major funding cuts to family planning services: New Jersey, New Hampshire, Montana, and Maine, as well as 17 states with missing abortion data.

Figure A.19: Synthetic Control Placebo Estimates- Teen Abortion Rates



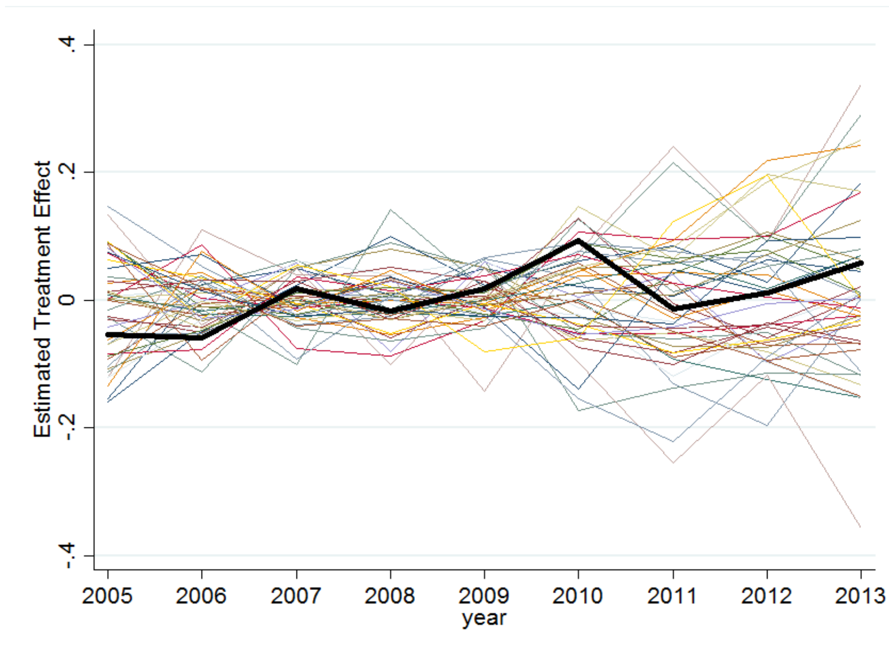
Notes: The above figure graphs the root mean squared predicted error (RMSPE) for all states from a synthetic control model. The solid black line represents the RMSPE for Texas. The vertical line represents the beginning of funding cuts to Texas family planning clinics. Teen abortion rates are constructed using the annual Center for Disease Control and Prevention Abortion Surveillance and SEER population data. I omit states with major funding cuts to family planning services: New Jersey, New Hampshire, Montana, and Maine, as well as 17 states with missing abortion data.

Figure A.20: Teen STD Rates in Texas Versus Synthetic Texas



Notes: State-level teen sexually transmitted disease rates are constructed using the Center for Disease Control and Prevention's NCHHSTP Atlas and SEER population data. Teen STD rates include the total number of chlamydia and gonorrhea cases per 1,000 teens. The vertical line represents the beginning of funding cuts to Texas family planning clinics. I omit states with major funding cuts to family planning services: New Jersey, New Hampshire, Montana, and Maine.

Figure A.21: Synthetic Control Placebo Estimates- Teen STD Rates



Notes: The above figure graphs the root mean squared predicted error (RMSPE) for all states from a synthetic control model. The solid black line represents the RMSPE for Texas. The vertical line represents the beginning of funding cuts to Texas family planning clinics. State-level teen sexually transmitted disease rates are constructed using the Center for Disease Control and Prevention's NCHHSTP Atlas and SEER population data.

Table A.17: Summary Statistics For Counties with Publicly Funded Clinics

	Treated Counties	**Comparison Counties**
Pre-Treatment (2005-2010)		
Births per 1,000 females aged 15-19	69.30	45.22
Fraction Teens 15 Year-Olds	0.20	0.20
Fraction Teens 16 Year-Olds	0.20	0.20
Fraction Teens 17 Year-Olds	0.20	0.21
Fraction Teens 18 Year-Olds	0.20	0.20
Fraction Teens 19 Year-Olds	0.20	0.19
Fraction Black Teens	0.10	0.14
Fraction Hispanic Teens	0.44	0.08
County Unemployment Rate	5.91	6.92
Post-Treatment (2011-2013)		
Births per 1,000 females aged 15-19	53.34	35.05
Fraction Teens 15 Year-Olds	0.20	0.20
Fraction Teens 16 Year-Olds	0.20	0.20
Fraction Teens 17 Year-Olds	0.20	0.20
Fraction Teens 18 Year-Olds	0.20	0.20
Fraction Teens 19 Year-Olds	0.21	0.20
Fraction Black Teens	0.10	0.14
Fraction Hispanic Teens	0.48	0.09
County Unemployment Rate	6.50	8.02

Notes: Births per 1,000 teen females are based on data from the National Center for Health Statistics, Division of Vital Statistics Natality Files and SEER population data. Population data including race, ethnicity, sex and age are from SEER. County-level unemployment rates are from the Bureau of Labor Statistics. Column 1 presents means for treated counties, which include the counties in Texas that have publicly funded health clinics and experienced funding cuts in 2011. Column 2 shows the means for counties outside of Texas that have family planning clinics, which represent the comparison group for this analysis.

Table A.18: Effect of Funding Cuts on Teen Birth Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect of Cuts in First Year	-0.011	0.008	0.001	0.001	0.001	0.003	0.007
	(0.010)	(0.015)	(0.014)	(0.014)	(0.014)	(0.017)	(0.020)
Effect of Cuts in Second Year	-0.001	0.022	0.014	0.013	0.013	0.016	0.020
	(0.013)	(0.017)	(0.017)	(0.017)	(0.017)	(0.019)	(0.022)
Effect of Cuts in Third Year	0.028	0.055***	0.049**	0.048**	0.049**	0.051**	0.055**
	(0.019)	(0.021)	(0.021)	(0.021)	(0.021)	(0.023)	(0.026)
Effect of Cuts in Fourth Year	0.065***	0.100***	0.095***	0.094***	0.095***	0.097***	0.102***
	(0.020)	(0.022)	(0.022)	(0.022)	(0.022)	(0.024)	(0.027)
One-Year Lead						0.013	0.017
						(0.015)	(0.018)
Two-Year Lead							0.017
							(0.014)
Average effect	0.020	0.046	0.040	0.039	0.039	0.042	0.046
P-value (test average effect = 0)	0.142	0.006	0.018	0.021	0.021	0.032	0.040
Average effect in years 3-4	0.031	0.052	0.048	0.048	0.048	0.049	0.052
P-value (test average effect in years 3-4 = 0)	0.009	0.000	0.000	0.000	0.000	0.001	0.002
Observations	25200	25200	25200	25200	24410	24410	24410
Demographic Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	No	No	Yes	Yes	Yes	Yes	Yes
Policy Controls	No	No	No	Yes	Yes	Yes	Yes
Restricted Sample	No	No	No	No	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Estimates are based on annual county-level Natality data from 2005-2014. Demographic controls include the fraction of teens aged 15-19 by age, ethnicity and race, economic controls include county unemployment rates, and policy controls include state-by-year indicator variables for over-the-counter emergency contraception access and private insurance mandates for contraceptive coverage. The restricted sample omits counties in states with major funding cuts to family planning services: New Jersey, New Hampshire, Montana, and Maine. Robust standard errors are clustered at the county level and are shown in parenthesis.

Table A.19: Effect of Funding Cuts on Birth Rates by Age Subgroup

	15 Yr Olds	**16 Yr Olds**	**17 Yr Olds**	**18 Yr Olds**	**19 Yr Olds**	**15-17 Yr Olds**	**18-19 Yr Olds**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect of Cuts in First Year	0.068*	0.005	0.026	0.007	-0.002	0.025	-0.000
	(0.038)	(0.028)	(0.026)	(0.015)	(0.014)	(0.024)	(0.012)
Effect of Cuts in Second Year	0.052	0.040	0.018	0.041**	0.004	0.030	0.017
	(0.046)	(0.029)	(0.025)	(0.018)	(0.017)	(0.023)	(0.016)
Effect of Cuts in Third Year	0.073	0.105***	0.066**	0.043*	0.057**	0.079***	0.049**
	(0.045)	(0.029)	(0.030)	(0.023)	(0.023)	(0.028)	(0.021)
Effect of Cuts in Fourth Year	0.110**	0.104***	0.096***	0.109***	0.065***	0.100***	0.080***
	(0.053)	(0.035)	(0.030)	(0.022)	(0.018)	(0.031)	(0.019)
Average effect	0.076	0.064	0.051	0.050	0.031	0.059	0.036
P-value (test average effect = 0)	0.044	0.010	0.035	0.003	0.051	0.015	0.018
Average effect in years 3-4	0.061	0.070	0.054	0.051	0.041	0.060	0.043
P-value (test average effect in years 3-4 = 0)	0.041	0.000	0.005	0.000	0.001	0.002	0.001
Observations	23362	24046	24191	24207	24212	24197	24214
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Policy Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Restricted Sample	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Estimates are based on annual county-level Natality data from 2005-2014. The outcome variables for Columns 1-5 are births to teens by age from 15-19. The outcome variables for Columns 6 and 7 are the births to teens aged 15-17 and 18-19, respectively. Demographic controls include the fraction of teens by age, ethnicity and race. Economic controls include county unemployment rates, and policy controls include state-by-year indicator variables for over-the-counter emergency contraception access and private insurance mandates for contraceptive coverage. The restricted sample omits counties in states with major funding cuts to family planning services: New Jersey, New Hampshire, Montana, and Maine. Robust standard errors are clustered at the county level and are shown in parenthesis.

Table A.20: Differential Effects of Funding Cuts on Teen Birth Rates

	Counties With Clinics (1)	Counties Without Clinics (2)	Counties With Planned Parenthood (3)	Counties with Poverty Rate >TX Avg (4)	Counties with Poverty Rate < TX Avg (5)
Effect of Cuts in First Year	0.001	-0.062	0.008	-0.033	0.004
	(0.014)	(0.049)	(0.017)	(0.020)	(0.017)
Effect of Cuts in Second Year	0.013	0.019	0.016	-0.009	0.002
	(0.017)	(0.097)	(0.022)	(0.024)	(0.021)
Effect of Cuts in Third Year	0.049**	-0.061	0.053*	0.027	0.029
	(0.021)	(0.071)	(0.027)	(0.023)	(0.024)
Effect of Cuts in Fourth Year	0.095***	0.022	0.118***	0.090***	0.064**
	(0.022)	(0.070)	(0.030)	(0.024)	(0.026)
Average effect	0.039	-0.020	0.049	0.019	0.025
P-value (test average effect = 0)	0.021	0.731	0.027	0.202	0.238
Average effect in years 3-4	0.048	-0.013	0.057	0.039	0.031
P-value (test average effect in years 3-4 = 0)	0.000	0.763	0.001	0.000	0.057
Observations	24410	6020	4860	9440	21780
Controls	Yes	Yes	Yes	Yes	Yes
Restricted Sample	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes

Notes: *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Estimates are based on annual county-level Natality data from 2005-2014. Estimates in Column 1 include all US counties with a publicly funded family planning clinic, which Column 2 includes all counties without a publicly funded clinic. Estimates in Column 3 include counties containing a Planned Parenthood clinic in 2010, and estimates from Column 4 and Column 5, respectively, are from a subset of counties that have average poverty rates higher, and lower, than the treated Texas counties' average poverty rate in 2010. Controls include the fraction of teens aged 15-19 by age, ethnicity and race, unemployment rates, and state-by-year indicator variables for over-the-counter emergency contraception access and private insurance mandates for contraceptive coverage. The restricted sample omits counties in states with major funding cuts to family planning services: New Jersey, New Hampshire, Montana, and Maine. Robust standard errors are clustered at the county level and are shown in parenthesis.

Table A.21: State-Level Synthetic Control Estimates of the Effects of Family Planning Funding Cuts on Log Teen Abortion Rates and STD Rates

Panel A: Log Teen Abortion Rates	Estimate	P-Value
Effect of Funding Cuts in First Year	-0.002	0.867
Effect of Funding Cuts in Second Year	-0.088	0.100
Panel B: Log Teen STD Rates	Estimate	P-Value
Effect of Funding Cuts in First Year	-0.014	0.933
Effect of Funding Cuts in Second Year	0.012	0.933
Effect of Funding Cuts in Third Year	0.058	0.533

Notes: State-level data on teen STDs and abortions are from the Center for Disease Control and Prevention (CDC) NCHHSTP Atlas, and CDC Abortion Surveillance data, respectively. Rates are calculated using SEER population data. Synthetic control estimates represent Texas' post/pre root mean squared predicted error (RMSPE). P-values are computed from an empirical distribution of all state units' ratio of post/pre RMSPE.

APPENDIX B

SECTION 2 APPENDIX

Table B.1: Effect of Stress-Abstinence Policy on Logged Teen Birth Rates (Using All Untreated States as Controls)

	(1) OLS	(2) OLS	(3) OLS	(4) WLS	(5) WLS	(6) WLS	(7) Poisson	(8) Poisson	(9) Poisson
Panel A. Average Treatment Effect									
Abstinence Mandate in Effect	-0.026	-0.016	-0.023	0.003	0.004	-0.000	-0.007	-0.004	-0.010
	(0.023)	(0.025)	(0.027)	(0.020)	(0.021)	(0.022)	(0.019)	(0.020)	(0.021)
1 Year Prior to Enactment			-0.018			-0.008			-0.013
			(0.015)			(0.013)			(0.013)
2 Years Prior to Enactment			-0.016			-0.015			-0.020**
			(0.018)			(0.009)			(0.009)
Panel B. Dynamic Treatment Effect									
Effect of Policy in Year of Enactment	-0.040**	-0.030*	-0.037*	-0.024	-0.019	-0.023	-0.031**	-0.027*	-0.033**
	(0.016)	(0.017)	(0.020)	(0.015)	(0.016)	(0.016)	(0.014)	(0.015)	(0.015)
1 Years After Enactment	-0.015	0.005	-0.002	0.006	0.016	0.012	-0.002	0.008	0.002
	(0.019)	(0.018)	(0.020)	(0.019)	(0.018)	(0.018)	(0.018)	(0.017)	(0.016)
2 Years After Enactment	-0.037	-0.023	-0.030	-0.005	-0.000	-0.004	-0.016	-0.010	-0.016
	(0.029)	(0.029)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.029)	(0.029)
3 Years After Enactment	-0.043	-0.031	-0.038	-0.014	-0.013	-0.017	-0.024	-0.021	-0.027
	(0.029)	(0.030)	(0.031)	(0.026)	(0.027)	(0.027)	(0.025)	(0.025)	(0.025)
4+ Years After Enactment	-0.010	-0.009	-0.016	0.028	0.021	0.017	0.020	0.018	0.012
	(0.028)	(0.037)	(0.038)	(0.025)	(0.026)	(0.028)	(0.026)	(0.028)	(0.030)
1 Year Prior to Enactment			-0.018			-0.008			-0.013
			(0.016)			(0.013)			(0.013)
2 Years Prior to Enactment			-0.016			-0.015			-0.020**
			(0.019)			(0.009)			(0.010)
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 46 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 females ages 15-19 who are black, percent of females ages 15-19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates and median family income. Four states which dropped stress-abstinence policies during the sample period are excluded this analysis. These states include California, New Jersey, Maryland and West Virginia.

Table B.2: Effect of Stress-Abstinence Policy on Logged Teen STD Rates (Using All Untreated States as Controls)

	(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	-0.001	0.020	0.025	0.011	0.020	0.038	0.020	0.038	0.061
	(0.043)	(0.044)	(0.050)	(0.048)	(0.048)	(0.054)	(0.047)	(0.041)	(0.049)
1 Year Prior to Enactment			0.013			0.078*			0.101**
			(0.047)			(0.046)			(0.044)
2 Years Prior to Enactment			0.010			0.018			0.014
			(0.022)			(0.021)			(0.019)
Panel B. Dynamic Effects									
Effect of Policy in Year of Enactment	0.028	0.038	0.043	0.040	0.037	0.056	0.048*	0.043*	0.067**
	(0.029)	(0.030)	(0.036)	(0.030)	(0.030)	(0.037)	(0.027)	(0.025)	(0.034)
1 Years After Enactment	0.011	0.023	0.028	0.001	-0.003	0.015	-0.000	-0.003	0.020
	(0.038)	(0.037)	(0.040)	(0.033)	(0.031)	(0.034)	(0.029)	(0.026)	(0.030)
2 Years After Enactment	0.001	0.021	0.026	-0.004	0.008	0.026	-0.009	0.008	0.031
	(0.040)	(0.035)	(0.040)	(0.036)	(0.031)	(0.036)	(0.033)	(0.026)	(0.031)
3 Years After Enactment	-0.009	0.018	0.023	0.010	0.030	0.047	0.027	0.058	0.079
	(0.059)	(0.062)	(0.068)	(0.076)	(0.078)	(0.084)	(0.077)	(0.075)	(0.082)
4+ Years After Enactment	-0.017	0.007	0.012	0.008	0.025	0.043	0.025	0.062	0.085
	(0.057)	(0.067)	(0.073)	(0.075)	(0.079)	(0.085)	(0.067)	(0.067)	(0.074)
1 Year Prior to Enactment			0.013			0.078*			0.101**
			(0.048)			(0.045)			(0.044)
2 Years Prior to Enactment			0.010			0.018			0.014
			(0.022)			(0.021)			(0.020)
N	600	600	600	600	600	600	600	600	600
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 46 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 females ages 15-19 who are black, percent of females ages 15-19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates and median family income. Four states which dropped stress-abstinence policies during the sample period are excluded this analysis. These states include California, New Jersey, Maryland and West Virginia.

Table B.3: Effect of Stress-Abstinence Policy on Logged Teen Abortion Rates (Using All Untreated States as Controls)

	(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	-0.032	-0.034	-0.026	-0.022	-0.022	-0.021	-0.004	-0.003	0.000
	(0.041)	(0.031)	(0.044)	(0.039)	(0.035)	(0.041)	(0.024)	(0.026)	(0.028)
1 Year Prior to Enactment			0.010			0.001			0.008
			(0.028)			(0.020)			(0.015)
2 Years Prior to Enactment			-0.001			0.003			0.008
			(0.035)			(0.034)			(0.029)
Panel B. Dynamic Effects									
Effect of Policy in Year of Enactment	-0.036	-0.039	-0.038	-0.016	-0.013	-0.012	-0.010	-0.004	-0.000
	(0.026)	(0.028)	(0.040)	(0.014)	(0.018)	(0.024)	(0.012)	(0.014)	(0.018)
1 Years After Enactment	-0.064	-0.061	-0.061	-0.043	-0.036	-0.036	-0.010	-0.001	0.003
	(0.059)	(0.055)	(0.062)	(0.059)	(0.063)	(0.068)	(0.040)	(0.044)	(0.046)
2 Years After Enactment	-0.048	-0.036	-0.035	-0.056	-0.045	-0.044	-0.032	-0.023	-0.020
	(0.048)	(0.033)	(0.041)	(0.045)	(0.039)	(0.043)	(0.032)	(0.033)	(0.033)
3 Years After Enactment	-0.005	-0.009	-0.009	0.003	-0.000	0.000	0.018	0.012	0.015
	(0.052)	(0.036)	(0.048)	(0.053)	(0.043)	(0.051)	(0.037)	(0.031)	(0.037)
4+ Years After Enactment	0.015	0.008	0.008	0.031	0.009	0.010	0.046**	0.014	0.017
	(0.043)	(0.039)	(0.043)	(0.028)	(0.039)	(0.039)	(0.019)	(0.031)	(0.030)
1 Year Prior to Enactment			0.004			0.001			0.008
			(0.031)			(0.020)			(0.015)
2 Years Prior to Enactment			-0.002			0.003			0.009
			(0.036)			(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 33 states from 2000-2009. 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 females ages 15-19 who are black, percent of females ages 15-19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates and median family income. Four states which dropped stress-abstinence policies during the sample period are excluded this analysis. These states include California, New Jersey, Maryland and West Virginia. Additionally, 13 states were removed from the analysis due to missing or inconsistent data. The states that were dropped for these regressions include Arizona, Colorado, Delaware, Florida, Illinois, Kentucky, Louisiana, Nevada, New Hampshire, New York, Rhode Island, Vermont and Wyoming.

Table B.4: Effect of Stress-Abstinence Policy on Teen Birth Rates

	(1) OLS	(2) OLS	(3) OLS	(4) WLS	(5) WLS	(6) WLS
Panel A. Average Treatment Effect						
Abstinence Mandate in Effect	0.086	0.099	-0.007	0.879	0.739	0.884
	(1.138)	(1.149)	(1.422)	(0.927)	(1.082)	(1.254)
1 Year Prior to Enactment			-0.043			0.690
			(0.991)			(0.832)
2 Years Prior to Enactment			-0.447			0.073
			(0.569)			(0.477)
Panel B. Dynamic Treatment Effect						
Effect of Policy in Year of Enactment	-0.951	-0.908	-1.055	-0.475	-0.469	-0.324
	(0.680)	(0.834)	(1.168)	(0.570)	(0.677)	(0.869)
1 Years After Enactment	-0.018	0.442	0.299	0.520	0.655	0.786
	(0.857)	(0.760)	(1.002)	(0.779)	(0.725)	(0.847)
2 Years After Enactment	-0.644	-0.589	-0.748	0.284	0.338	0.476
	(1.325)	(1.299)	(1.578)	(1.287)	(1.290)	(1.417)
3 Years After Enactment	-0.606	-0.631	-0.773	0.209	0.066	0.187
	(1.346)	(1.398)	(1.642)	(1.204)	(1.381)	(1.541)
4+ Years After Enactment	1.449	1.361	1.224	2.352*	2.237	2.364
	(1.336)	(1.595)	(1.858)	(1.190)	(1.641)	(1.861)
1 Year Prior to Enactment			-0.150			0.644
			(1.017)			(0.856)
2 Years Prior to Enactment			-0.516			0.054
			(0.613)			(0.510)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	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 females ages 15-19 who are black, percent of females ages 15-19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates and median family income.

Table B.5: Effect of Stress-Abstinence Policy on Teen STD Rates

	(1) OLS	(2) OLS	(3) OLS	(4) WLS	(5) WLS	(6) WLS
Panel A. Average Treatment Effect						
Abstinence Mandate in Effect	0.383	1.019	1.519	0.480	0.542	0.860
	(1.188)	(0.840)	(0.932)	(1.630)	(0.812)	(0.928)
1 Year Prior to Enactment			1.373			1.460
			(0.904)			(1.103)
2 Years Prior to Enactment			0.911			0.217
			(0.585)			(0.638)
Panel B. Dynamic Treatment Effect						
Effect of Policy in Year of Enactment	0.866	1.206**	1.738**	0.859	0.696	1.039
	(0.810)	(0.563)	(0.657)	(0.829)	(0.600)	(0.737)
1 Years After Enactment	0.486	0.918	1.421*	-0.175	-0.236	0.074
	(0.849)	(0.761)	(0.764)	(0.750)	(0.708)	(0.618)
2 Years After Enactment	0.293	0.880	1.429*	-0.316	-0.297	0.029
	(0.961)	(0.602)	(0.744)	(1.080)	(0.699)	(0.779)
3 Years After Enactment	0.448	1.421	1.903	0.750	1.232	1.522
	(1.725)	(1.482)	(1.588)	(2.504)	(1.908)	(2.023)
4+ Years After Enactment	0.071	0.831	1.297	0.847	1.093	1.395
	(1.804)	(1.575)	(1.605)	(2.604)	(1.472)	(1.562)
1 Year Prior to Enactment			1.381			1.437
			(0.865)			(1.090)
2 Years Prior to Enactment			0.914			0.217
			(0.617)			(0.644)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	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 females ages 15-19 who are black, percent of females ages 15-19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates and median family income.

Table B.6: Effect of Stress-Abstinence Policy on Teen Abortion Rates

	(1) OLS	(2) OLS	(3) OLS	(4) WLS	(5) WLS	(6) WLS
Panel A. Average Treatment Effect						
Abstinence Mandate in Effect	-0.123	-0.168	-0.047	-0.176	-0.068	0.011
	(0.397)	(0.298)	(0.415)	(0.469)	(0.312)	(0.381)
1 Year Prior to Enactment			0.273			0.079
			(0.531)			(0.454)
2 Years Prior to Enactment			0.208			0.301
			(0.565)			(0.546)
Panel B. Dynamic Treatment Effect						
Effect of Policy in Year of Enactment	-0.356	-0.175	-0.047	-0.100	0.103	0.181
	(0.344)	(0.262)	(0.417)	(0.381)	(0.229)	(0.358)
1 Years After Enactment	-0.428	-0.327	-0.200	-0.186	0.032	0.112
	(0.427)	(0.606)	(0.609)	(0.585)	(0.657)	(0.662)
2 Years After Enactment	-0.245	0.069	0.207	-0.668	-0.318	-0.237
	(0.468)	(0.563)	(0.646)	(0.454)	(0.449)	(0.466)
3 Years After Enactment	0.261	-0.078	0.039	-0.043	0.025	0.106
	(0.725)	(0.509)	(0.638)	(0.746)	(0.492)	(0.572)
4+ Years After Enactment	0.649	-0.998	-0.883	0.600	-0.642	-0.557
	(0.475)	(0.902)	(0.868)	(0.512)	(0.804)	(0.806)
1 Year Prior to Enactment			0.305			0.091
			(0.520)			(0.452)
2 Years Prior to Enactment			0.201			0.294
			(0.542)			(0.545)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	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 females ages 15-19 who are black, percent of females 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 due to 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.

Table B.7: Effect of Stress-Abstinence Policy on Teen Abortion Rates without Dropping States with Missing or Inconsistent Abortion Data

	(1) OLS	(2) OLS	(3) OLS	(4) WLS	(5) WLS	(6) WLS	(7) Poisson	(8) Poisson	(9) Poisson
Panel A. Average Treatment Effect									
Abstinence Mandate in Effect	0.034	0.035	0.076	0.045	0.034	0.059	0.066	0.052	0.065
	(0.064)	(0.062)	(0.086)	(0.060)	(0.047)	(0.067)	(0.044)	(0.036)	(0.044)
1 Year Prior to Enactment			0.146*			0.066			0.043
			(0.078)			(0.079)			(0.065)
2 Years Prior to Enactment			0.123			0.050			0.023
			(0.090)			(0.055)			(0.046)
Panel B. Dynamic Treatment Effect									
Effect of Policy in Year of Enactment	0.066	0.069	0.128	0.112	0.097	0.124	0.136	0.120	0.137
	(0.070)	(0.057)	(0.090)	(0.099)	(0.091)	(0.098)	(0.107)	(0.105)	(0.101)
1 Years After Enactment	0.033	0.062	0.116	0.082	0.105	0.128	0.115	0.135	0.150
	(0.089)	(0.121)	(0.151)	(0.113)	(0.125)	(0.140)	(0.094)	(0.092)	(0.100)
2 Years After Enactment	0.014	0.014	0.077	-0.019	-0.035	-0.009	-0.006	-0.020	-0.005
	(0.078)	(0.059)	(0.102)	(0.068)	(0.046)	(0.066)	(0.044)	(0.042)	(0.050)
3 Years After Enactment	0.006	-0.011	0.044	-0.017	-0.039	-0.017	0.003	-0.036	-0.023
	(0.053)	(0.039)	(0.066)	(0.054)	(0.047)	(0.058)	(0.038)	(0.039)	(0.047)
4+ Years After Enactment	0.049	-0.051	0.006	0.029	-0.079	-0.058	0.051	-0.070	-0.060
	(0.044)	(0.060)	(0.075)	(0.042)	(0.078)	(0.086)	(0.036)	(0.069)	(0.072)
1 Year Prior to Enactment			0.125			0.071			0.050
			(0.089)			(0.078)			(0.061)
2 Years Prior to Enactment			0.119			0.048			0.023
			(0.091)			(0.053)			(0.043)
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 2000-2009. 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 females ages 15-19 who are black, percent of females ages 15-19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates and median family income.

Table B.8: Effect of Stress-Abstinence Policy on Logged Teen Birth Rate Using a Triple Difference Model

	(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	-0.006	-0.002	0.021	0.021	0.026	0.010	0.012	0.015
	(0.021)	(0.021)	(0.026)	(0.018)	(0.018)	(0.023)	(0.017)	(0.017)	(0.021)
1 Year Prior to Enactment			0.002			0.015			0.011
			(0.017)			(0.019)			(0.017)
2 Years Prior to Enactment			0.014			0.008			0.005
			(0.017)			(0.011)			(0.010)
Panel B. Dynamic Effects									
Effect of Policy in Year of Enactment	-0.014	-0.014	-0.011	0.005	0.005	0.009	-0.002	-0.002	0.002
	(0.017)	(0.017)	(0.019)	(0.011)	(0.011)	(0.013)	(0.010)	(0.010)	(0.013)
1 Years After Enactment	0.004	0.004	0.007	0.030*	0.030*	0.035**	0.021	0.022*	0.025*
	(0.018)	(0.018)	(0.020)	(0.015)	(0.015)	(0.016)	(0.013)	(0.013)	(0.014)
2 Years After Enactment	-0.018	-0.018	-0.014	0.020	0.020	0.024	0.007	0.008	0.011
	(0.026)	(0.026)	(0.027)	(0.023)	(0.023)	(0.024)	(0.021)	(0.021)	(0.022)
3 Years After Enactment	-0.026	-0.026	-0.022	0.005	0.005	0.010	-0.006	-0.005	-0.001
	(0.023)	(0.023)	(0.026)	(0.019)	(0.020)	(0.023)	(0.017)	(0.017)	(0.020)
4+ Years After Enactment	0.011	0.011	0.015	0.033	0.033	0.038	0.025	0.026	0.030
	(0.032)	(0.032)	(0.040)	(0.037)	(0.037)	(0.043)	(0.034)	(0.035)	(0.039)
1 Year Prior to Enactment			0.003			0.015			0.011
			(0.017)			(0.019)			(0.017)
2 Years Prior to Enactment			0.015			0.009			0.005
			(0.020)			(0.012)			(0.011)
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

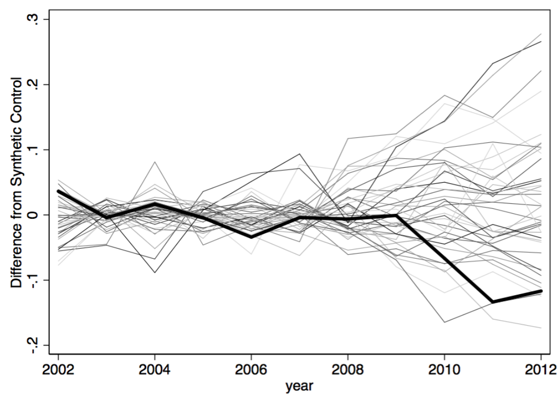
Notes: *p<0.1, **p<0.05, ***p<0.01. Estimates are based on annual data for 26 states from 2000-2011. Each column by panel represents coefficients from a separate triple-difference regression using the female 30-34 year old population as a within-state control group, and the reported coefficients are for the teen group. Robust standard errors are clustered at the state level and are shown in parentheses. The control variables include percent of females ages 15-19 who are black, percent of females ages 15-19 who are Hispanic, a policy-based measure of abortion access, state unemployment rates and median family income.

APPENDIX C

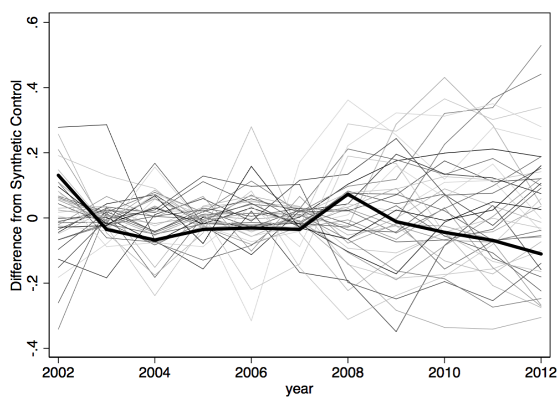
SECTION 3 APPENDIX

Figure C.1: Results of Permutation Tests for Synthetic Control Estimates

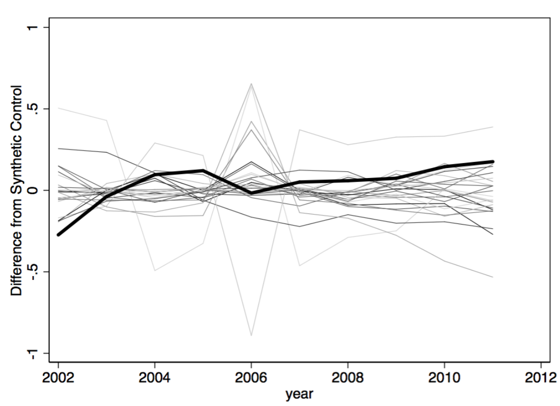
Panel A: Log Birth Rates



Panel B: Log STD Rates



Panel C: Log Abortion Rates

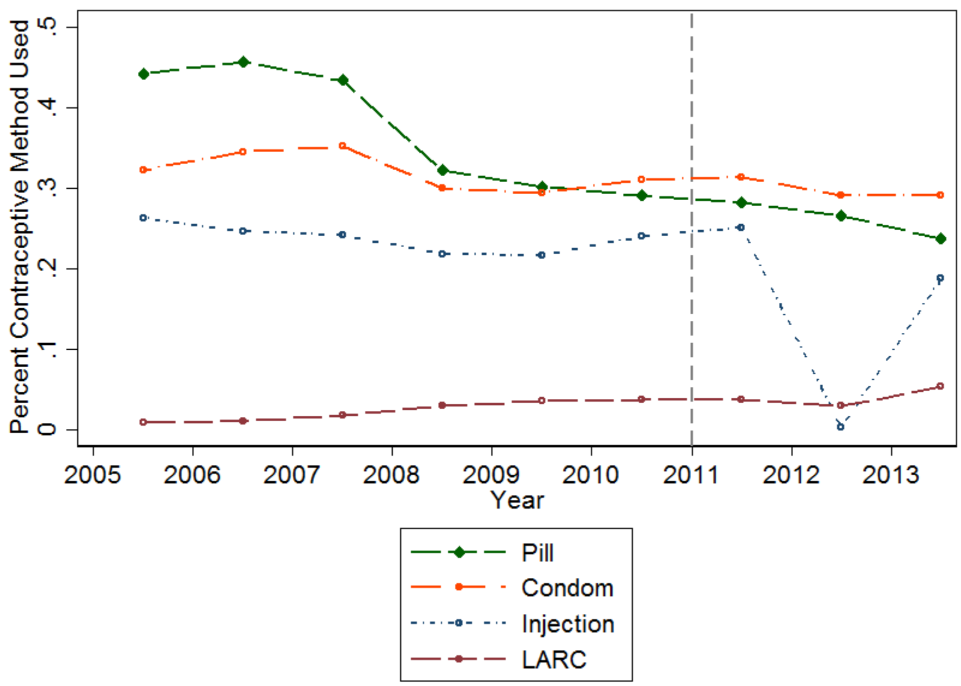


Notes: This figure shows the difference between each state and its "synthetic control" in each period, which is used to construct p-values as described in Table A.16. The estimates for Colorado are bolded.

APPENDIX D

SECTION 4 APPENDIX

Figure D.1: Percent Using Contraception at Exit, by Contraceptive Type



Notes: Author's calculation of family planning clients using or obtaining contraceptive devices (intrauterine devices, implants, injections, oral contraceptives, patches, rings, cervical caps) at exit, based on annual data provided by the Texas Department of State Health Services.

Table D.1: Effect of Funding Cuts on Teen Birth Rates in Highly Populated Counties

	(1)	(2)	(3)	(4)	(5)	(6)
Effect of Cuts in First Year	-0.012	0.008	0.000	-0.000	0.002	0.006
	(0.010)	(0.015)	(0.014)	(0.015)	(0.017)	(0.020)
Effect of Cuts in Second Year	-0.003	0.022	0.014	0.013	0.015	0.019
	(0.013)	(0.017)	(0.017)	(0.017)	(0.019)	(0.022)
Effect of Cuts in Third Year	0.026	0.053***	0.048**	0.047**	0.049**	0.053**
	(0.018)	(0.021)	(0.021)	(0.021)	(0.023)	(0.026)
Effect of Cuts in Fourth Year	0.061***	0.097***	0.092***	0.091***	0.093***	0.097***
	(0.020)	(0.022)	(0.022)	(0.022)	(0.024)	(0.027)
One-Year Lead					0.011	0.015
					(0.015)	(0.018)
Two-Year Lead						0.015
						(0.014)
Average effect	0.018	0.045	0.038	0.038	0.040	0.044
P-value (test average effect = 0)	0.194	0.008	0.023	0.027	0.043	0.053
Average effect in years 3-4	0.029	0.050	0.046	0.046	0.047	0.050
P-value (test average effect in years 3-4 = 0)	0.015	0.000	0.000	0.001	0.002	0.003
Observations	31220	31220	31220	31220	31220	31220
Demographic Controls	No	Yes	Yes	Yes	Yes	Yes
Economic Controls	No	No	Yes	Yes	Yes	Yes
Policy Controls	No	No	No	Yes	Yes	Yes
Restricted Sample	No	No	No	No	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Estimates are based on annual county-level Natality data from 2005-2014. Highly populated counties include those with an average teen female population greater than 7,135 during the sample period. Demographic controls include the fraction of teens aged 15-19 by age, ethnicity and race, economic controls include county unemployment rates, and policy controls include state-by-year indicator variables for over-the-counter emergency contraception access and private insurance mandates for contraceptive coverage. The restricted sample omits counties in states with major funding cuts to family planning services: New Jersey, New Hampshire, Montana, and Maine. Robust standard errors are clustered at the county level and are shown in parenthesis.

Table D.2: Annual Donations to Texas Planned Parenthood Facilities

Year	Donation
2011	\$2,081,122
2012	\$4,118,405
2013	\$3,733,981
2014	\$3,846,217

Data on annual donation by Planned Parenthood region is from yearly, public Form 990s. Donation data are aggregated from Lubbock, Houston, Dallas, Midland, San Antonio, Waco and McAllen facilities.

Table D.3: Effect of Funding Cuts on Birth Rates 20-24 Year Olds

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Effect of Cuts in First Year	-0.015	-0.008	-0.016	-0.017*	-0.016	-0.017	-0.017
	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.012)
Effect of Cuts in Second Year	-0.003	0.012	0.003	0.002	0.002	0.002	0.001
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	(0.013)
Effect of Cuts in Third Year	0.023**	0.039***	0.032***	0.031***	0.031***	0.031**	0.030**
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.014)
Effect of Cuts in Fourth Year	0.069***	0.088***	0.081***	0.080***	0.080***	0.079***	0.079***
	(0.020)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.017)
One-Year Lead						-0.003	-0.004
						(0.010)	(0.011)
Two-Year Lead							-0.002
							(0.011)
Average effect	0.019	0.033	0.025	0.024	0.024	0.024	0.023
P-value (test average effect = 0)	0.100	0.001	0.011	0.013	0.012	0.029	0.054
Average effect in years 3-4	0.031	0.042	0.038	0.037	0.037	0.037	0.036
P-value (test average effect in years 3-4 = 0)	0.002	0.000	0.000	0.000	0.000	0.000	0.000
Observations	25200	25200	25200	25200	24410	24410	24410
Demographic Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Economic Controls	No	No	Yes	Yes	Yes	Yes	Yes
Policy Controls	No	No	No	Yes	Yes	Yes	Yes
Restricted Sample	No	No	No	No	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *, **, and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively. Estimates are based on annual county-level Natality data from 2005-2014. Demographic controls include the fraction of women aged 20-24 by age, ethnicity and race, economic controls include county unemployment rates, and policy controls include state-by-year indicator variables for over-the-counter emergency contraception access and private insurance mandates for contraceptive coverage. The restricted sample omits counties in states with major funding cuts to family planning services: New Jersey, New Hampshire, Montana, and Maine. Robust standard errors are clustered at the county level and are shown in parenthesis.