Unintended Consequences of Insurance: ACA's Free Contraception Mandate and Risky Sex

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Abstract

Health insurance is a primary driver of rising medical expenditures. I examine insurance's effect on risky sex, a behavior with quick, meaningful negative results. Leveraging mandated zero cost-sharing for contraception and pre-policy insured rates as a measure of treatment intensity, I find this 2012 policy reduced fertility but caused unintended consequences: decreased prevention and increased sexually transmitted infections. I discuss imperfections of controlling for pre-trends using state-trends in difference-in-differences and suggest approaches to control for pre-trends directly. I use the 2010 dependent coverage mandate to examine the overall effect of insurance and find protective net effects of insurance on STIs.

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1. Introduction

Health insurance has figured prominently in medical spending's rapid growth (Finkelstein, 2007; Finkelstein et al., 2012; Manning et al., 1987). While health insurance increases utility by smoothing consumption, insurance can also have unintended consequences. Determining the effect of insurance on risky behavior is important because these distortions can cause increased illness and medical spending. However, insurance's effect on risky behaviors receives less attention than the increased quantity of care demanded due to lower out-of-pocket cost (Zwiefel and Manning, 2000).[1] Additionally, increased risky behaviors cause negative externalities; the financial burden is spread across the insurance risk pool, and infections can be transmitted to individuals who have not changed their behavior.

Most research examining health insurance's effect on risky behavior has focused on ex ante moral hazard, which is the effect cheaper treatment in the future has on risky behaviors today. In this paper, I examine a different pathway between health insurance and risky behaviors: substitution from broad behavior-based prevention to narrow medical-based prevention. Specifically, I investigate if insurance lowering prescription contraception prices impacts condom sales and sexually transmitted infections (STI). Studies that test for ex ante moral hazard find little or inconsistent evidence that health insurance changes smoking, diet, exercise, and drinking (Dave and Kaestner, 2009; De Preux, 2011; Barbaresco et al., 2015; Simon et al., 2017).

[1] There is a large literature on distortions, including moral hazard, in other types of insurance markets. See Cummins and Tennyson (1996) for a car insurance example and Chetty (2008) for an unemployment insurance example. However, health insurance has important distinguishing characteristics. Health insurance covers maintenance and prevention (such as prescription contraception), while car insurance does not cover maintenance such as oil changes. The distinction between treatment and prevention is blurry in health; for instance, statins treat high cholesterol but also prevent heart attack. Most importantly for changes in prevention, other types of insurance can provide replacements (cars, houses, income), while health insurance can often only provide access to treatment that may not completely cure the illness or repair the injury.

Observing ex ante moral hazard is difficult because many health shocks occur only after years or decades of poor health behaviors (e.g., smoking and cancer) (Department of Health and Human Services, 2010). As a result, future, rather than current, health insurance covers the eventual consequences of many current risky behaviors.

This paper empirically tests for unintended effects on risky sex decisions and the ensuing health consequences. The risky behavior I consider is sex between a man and a woman without a condom.[2] Focusing on risky sex has two advantages. First, health consequences of risky sex such as unplanned pregnancy and STIs occur quickly, so decisions related to sex should be responsive to current insurance. Few risky health behaviors cause adverse health shocks as quickly as unprotected sex. One exception is drug-use can result in overdoses and mortality almost immediately, and recent research suggests drug-use is responsive to ex ante moral hazard (Doleac and Mukherjee, 2018).

Second, focusing on risky sex provides a unique opportunity to isolate an unintended effect of insurance by examining a policy in the Patient Protection and Affordable Care Act (ACA). The zero cost-sharing for prescription contraception mandate of 2012 made prescription birth control free for insured women, increasing health insurance on the intensive margin or the degree to which each person is insured. The zero cost-sharing mandate allows me to isolate an effect on risky sex, because it affects only a single aspect of health insurance: the cost of prescription contraception.

[2] I acknowledge that risky sex can occur in other contexts, such as with men who have unprotected sex with men. Here I focus on heterosexuals who should respond to the zero cost-sharing mandate.

This policy was implemented in all states simultaneously, so its effect cannot be determined by comparing treated and untreated states. To overcome this obstacle to identification, I use pre-policy insurance rates as a measure of treatment intensity, similar to the approach employed by Finkelstein (2007) using uninsured rates. Specifically, I use the pre-policy insured rate among 25-to-29-year-olds for each state in order to protect against confounding from the 2010 policy allowing young adults to enroll on parents' insurance. The insured rate represents the percent of 25-to-29-year-olds exposed to the zero cost-sharing mandate, because the mandate only applies to insured individuals. I also perform robustness tests using 30-to-34-year-olds to address young adults aging into the 25-to-29 age range as well as several other robustness and falsification tests. I use state-year level data and dose-response difference-in-differences to determine the effect of this policy on several outcomes related to risky sex, such as condom sales, STI incidence, and fertility.

I estimate five difference-in-differences models to address potential bias from any differential pre-trends. First, I estimate simple difference-in-differences models, which are biased in the presence of differential pre-trends. Second, I control for state-level trends and show that state-trends do not completely remove bias from pre-trends, and state-trends can introduce bias in situations with flat pre-trends and time-varying treatment effects. Meer and West (2016) discuss bias caused by unit-trends in the context of binary treatment, but I extend the discussion to dose-response difference-in-differences and suggest alternative approaches. Third, I control for state-specific pre-period trends, mechanically forcing flat pre-trends. Fourth, by interacting time and treatment dose for the pre-period, I directly control for any pre-trend, but this method requires only one additional control instead of 50 state pre-trends. Lastly, I adjust the difference-in-differences model to estimate a linear time-varying treatment effect. In this analysis, I model the pre-policy trend and intercept as well as the change in trend and one-time jump that occur in the year of policy implementation.

I also use event study analysis to identify any change in relative trends that occur in the treatment year by interacting the treatment intensity in each state with year dummies. I also perform two pre-trend adjustments to the event studies to reflect the third through fifth difference-in-differences estimates. First, I follow Wolfers (2006) by controlling for state pre-trends, which fixes the pre-period to zero, and non-parametrically estimates the post-period deviations from the pre-trends. Second, instead of using 50 state-level pre-trends, I use the pre-period slope from the fifth difference-in-differences model to net-out the pre-period slope from the standard event studies.

Economic theory provides clear predictions about the unintended consequences from the zero cost-sharing mandate. Prescription contraception decreases the cost of having sex without a condom and should decrease demand for condoms. Condoms and prescription birth control are substitutes for preventing pregnancy, but condoms also prevent STI transmission. Substitution to prescription contraception should lead to increased incidence of STIs. I find that the estimated effects of the zero cost-sharing mandate are consistent with theory: a reduction in condom sales and increased incidence of chlamydia.

An increase in STIs is not a consequence of all health insurance expansions. As an extension, I examine the young adult dependent coverage mandate of 2010. This mandate allowed children 25 and under to join their parents' health insurance and caused an exogenous shock to the extensive margin of health insurance. I leverage this shock to determine the overall effect of health insurance. I use the same empirical strategy, but now the measure of treatment intensity is the uninsured rate among people in their early 20s. I find a protective effect of health insurance on net; the dependent coverage mandate resulted in fewer STIs.

This study makes three main contributions to the literature. First, it empirically tests for unintended consequences of insurance on risky sex. Several studies have examined the effect of health insurance on health behaviors, but largely in contexts where health behaviors are slow to result in disease and are less likely to respond to health insurance coverage (Dave and Kaestner, 2009; De Preux, 2011; Barbaresco et al., 2015; Simon et al., 2017). Second, I show controlling for pre-trends is a more appropriate method than state-trends. This approach completely removes any differential pre-trend and does not introduces bias, which state-trends do with flat pre-trends and time-varying treatment effects.

The third contribution is an evaluation of an important and contentious aspect of the ACA. For instance, almost 90 outside groups submitted briefs during *Burwell v. Hobby Lobby*, which determined that certain corporations do not have to pay for insurance plans that cover prescription birth control (Supreme Court of the United States, 2014). Furthermore, information on the effect of this mandate provides insight on recent and current policy proposals. The executive branch has already issued a rule limiting the zero cost-sharing mandate (Wolf, 2017), greatly expanding employers' ability to obtain an exemption from the requirement to offer insurance that covers birth control at no out-of-pocket cost. However, several states and non-profit organizations have sued to prevent implementation of the new regulation, and the legal battle is ongoing (Sobel et al., 2018). Additionally, Congress repeatedly considered repealing the ACA, which would have ended this mandate (Kaplan and Pear, 2017). Evidence from the implementation of this policy offers insight about the effect of proposals that would eliminate or reduce it.

There is also great policy interest in the outcomes I examine: fertility and STIs. Fertility, especially unintended pregnancy, is a very expensive consequence of unprotected sex and is often paid for by public insurance. The government spends an estimated $12 billion on unwanted pregnancies each year (Thomas and Monea, 2011). Additionally, some STIs are becoming increasingly resistant to treatment. Bacterial STIs were previously easy to treat, but complications from STIs, such as pelvic inflammatory disease, are an increasing concern (Hersher, 2016). In fact, the World Health Organization (2017) prioritized gonorrhea as one of the eleven most important antibiotic resistant bacteria. This study also produces insight on an important outcome missing from many analyses of risky sex: condoms. By analyzing condom sales, I provide evidence on one mechanism through which insurance affects STI incidence and fertility. Finally, in contrast to the many studies on Medicaid expansion and Medicare that focus on low-income or older populations, I focus on a largely understudied group; the marginal individual in this context is a middle-class young adult with private insurance or with privately-insured parents.

The zero cost-sharing mandate lowered the cost of prescription contraception but not of condoms, which resulted in a reduction in condom sales. One way to counteract the increase of risky sex would be to subsidize condoms. Another policy concern is that while increased risky sex causes increased health care utilization, spending on medical care does not reflect the full economic loss of reduced prevention. Risky behavior results in more health shocks, so people lose utility directly from illness and injury.

The remainder of this paper proceeds as follows: Section 2 provides background including a review of the existing literature and information on the zero cost-sharing mandate. Section 3 details the data sources and research method. Empirical results are presented in Section 4. In Section 5, I examine the extensive margin of health insurance to determine the overall effect of health insurance using another policy in the ACA, the mandate that adult children under the age of 26 be allowed on their parents' insurance. Robustness and falsification tests are discussed in Section 6. Finally, Section 7 concludes with a discussion.

2. Background

2.1: Literature Review

This study sits at the intersection of two literatures: (1) responses to health insurance, particularly effects on risky behaviors and prevention, and (2) the economics of sexual activity. While not the focus of this study, the theoretical work on ex post moral hazard in health insurance starts with Pauly (1968). Both the RAND and Oregon health insurance experiments showed strong empirical evidence insurance increases use of medical care (Manning et al., 1987; Finkelstein et al., 2012). Finkelstein (2015) summarizes this literature. The literature on ex ante moral hazard closely relates to this study. The theoretical work on ex ante moral hazard in health insurance starts with Ehrlich and Becker (1972). However, the empirical evidence of ex ante moral hazard is much less consistent than the evidence of ex post moral hazard.

Generally, researchers find weak or mixed empirical evidence of ex ante moral hazard; many studies find effects on a small subset of examined health behaviors or find effects only among certain demographic groups. A common strategy to examine the causal effect of insurance on health behaviors is to leverage the exogenous change in insurance status caused by aging into Medicare eligibility (Dave and Kaestner, 2009; De Preux, 2011). While most people entering Medicare are 65 years old, other studies examine policies that affect younger populations. For example, Barbaresco et al. (2015) study the population targeted by the 2010 requirement that insurers cover adult dependents under 26 years of age, comparing changes in 23-to-25-year-olds to 27-to-29-year-olds. Simon et al. (2017) compare states that did and did not expand Medicaid coverage to low-income childless adults.

The studies on the effect of health insurance on risky health behaviors primarily examine effects on smoking, exercise, and drinking. The lack of evidence may in part be related to the outcomes studied, because these health behaviors often do not result in health shocks for many years. To overcome this obstacle I examine risky sex, which has a short lag before resulting in health shocks such as pregnancy and STIs.

In contrast to the lack of consensus about the effect of insurance coverage on risky behaviors, the literature on the economics of sex generally finds that lowering the cost of sex without a condom increases health shocks, particularly STIs (Chesson, 2012). Klick and Stratmann (2007) and Levine (2003) examine state laws that require minors to inform or involve their parents in order to obtain an abortion. These studies show that such laws resulted in fewer abortions, fewer cases of gonorrhea, and fewer pregnancies. Ressler et al. (2006) find that increasing cash welfare payments, which decreased the cost of having a child, increased rates of sexually transmitted infection. Similarly, Ahituv et al. (1996) determine that condom use increased when the cost of unprotected sex (AIDS prevalence/risk of infection) increased.

However, not all studies find that lowering the cost of sex without a condom increased sex-related health shocks. For instance, easier access to emergency contraception did not affect fertility or abortion rates (Gross et al, 2013). In certain contexts, even easier access to condoms did not reduce the number of pregnancies or STIs. Looking at school-based programs that distributed condoms to teens, Buckles and Hungerman (2016) find that these programs increased teen pregnancy, particularly if additional information was not provided with condoms. Conversely, Lovenheim et al. (2016) find that expansion of school-based health centers, which provide access to prescription birth control and often condoms, led to lower teen fertility. I add to the literature on risky sex by examining a different source of exogenous variation in the cost of risky sex: health insurance expansion.

With some exceptions, existing research on the cost of risky sex focuses on births and diseases, while ignoring the first-order effects on behavior such as condom usage and purchase. Understanding the effect on condom purchases helps confirm that changes in fertility and infection result from changes in risky sexual behavior and not from an unobserved contemporaneous shock. The lack of evidence on these outcomes is primarily driven by data limitations. Questions about use of condoms and prescription contraception are not even included in the surveys most likely to ask about these behaviors, such as recent waves of the Behavioral Risk Factor Surveillance System. I address this gap with proprietary data on condom sales.

2.2: Policy Background

President Obama signed the ACA into law in March 2010. The ACA was the most significant legislative change to the health care system in the 50 years since passage of Medicare and Medicaid (Oberlander, 2010). Unlike Medicare and Medicaid, the ACA is primarily a market-based health insurance expansion.[3] For instance, two of the most well-known aspects of the ACA are the individual mandate and the health insurance exchanges, which caused major changes to the private health insurance system (Kaiser Family Foundation, 2013). The individual mandate requires that every individual have comprehensive health insurance. The insurance exchanges are online marketplaces to compare plans and purchase health insurance. Neither of these policies involve the government directly providing health insurance; instead, they leverage and expand the existing private health insurance market.[4]

This study uses an adjustment to private health insurance markets made by the ACA to test for unintended distortions in risky behavior. The zero cost-sharing mandate requires insurance plans to cover prescription contraception with no out-of-pocket cost starting in August 2012 (Health Resources & Service Administration, 2017). At least one version of each form of prescription contraception (e.g., oral, injectable, intrauterine device) must be covered with no out-of-pocket expense, but there is no requirement that branded versions be covered with no cost-sharing if a generic option of the method is available (Centers for Medicare & Medicaid Services, 2015). This policy ensures 47 million women can access prescription contraception and other preventive care with no deductible, co-pay, or co-insurance (Simmons and Skopec, 2012).

[3] One major aspect of the ACA that does involve expansion of government-based health insurance is Medicaid coverage of childless adults, a group generally not eligible for Medicaid pre-ACA.
[4] For more details on these and other aspects of the ACA, see Kaiser Family Foundation (2013).

The zero cost-sharing mandate affects the intensive margin of health insurance, because it changes the degree of coverage by requiring zero cost-sharing for certain benefits. Importantly, this policy went into effect before much of the ACA, such as the establishment of the insurance exchanges, the requirement that individuals have insurance, or the bulk of Medicaid expansion to childless adults (Senate.gov, 2010),[5] which reduces concern about contemporaneous policy shocks. While the requirement that young adults be allowed on their parents' insurance began in 2010, I focus my analysis on an older population unaffected by the dependent coverage mandate. Additionally, there is less concern about policy timing endogeneity, because I use a change in federal law instead of state-level policies.

The zero cost-sharing mandate had a meaningful effect on both the out-of-pocket cost of prescription contraception and contraception use. Between 2012 and 2014, the percent of privately-insured women who paid $0 out-of-pocket for contraception increased by 30-50 percentage points across methods (oral, injectable, ring, intrauterine device) (Bearak et al., 2016; Sonfield et al., 2015). The median out-of-pocket cost fell from $10 to $0 for oral contraception and from $20 to $0 for intrauterine devices (IUD). Even a few hundred dollars can be meaningful to low-income women, but the reduction in cost was much higher for many women. For example, the cost of an IUD at the 90th percentile dropped from $844 to $15, though some uninsured women and women working for religiously-exempt employers still bear at least some financial burden (Bearak et al., 2016; Sonfield et al., 2015). An analysis of women working in 499 Midwest firms that provide health insurance found this policy caused a 2.3 percentage point

[5] In fact, many other early aspects of the ACA did not directly affect patients, and instead focused on health care institutions and infrastructure. For more details, see the implementation timeline provided by the U.S. Senate (Senate.gov, 2010).

or 7.6% increase in prescription contraception use (Carlin et al., 2016).[6] Becker (2018) finds similar increases in contraception use, disproportionately on long-acting forms of contraception.

3. Data and Method

3.1: Data

The data for this study come from several sources and are at the state-year level.[7] Each state's insured rate for 25- to 29-year-olds in 2011-12, which serves as the measure of treatment intensity, is derived from the Behavioral Risk Factor Surveillance System (BRFSS). Each year the BRFSS surveys over 400,000 adults and is representative at the state level (Centers for Disease Control and Prevention, 2013). The main benefit of BRFSS is that each state-year has sufficient sample size to precisely estimate the insured rate for 25- to 29-year-olds. I also perform robustness tests using American Community Survey data to focus on the rate of private insurance (Ruggles et al, 2018).

Condom sales for each state-year come from Nielsen Retail Scanner data, which contain sales information provided to Nielsen by retailers.[8] These data have important advantages over many surveys. First, since information is not self-reported, it does not suffer from reporting error, including social desirability bias. Second, Nielsen Retail Scanner data provide information on more condoms in a state-year than any survey. While these data do not cover 100 percent of sales, a large fraction of food, drug, and big-box stores' sales are covered. These data capture

[6] There is evidence in the behavioral economics literature that reducing the price to $0 can be significantly more effective than reductions to small, non-zero prices (e.g., Shampanier et al., 2007).
[7] Ideally, a panel data set would contain insurance status, demographics, prevention (prescription contraception and condoms), fertility, and STIs. However, no individual-level data set contains the requisite data elements for this analysis.
[8] This is in contrast to Nielsen Consumer Panel Dataset (known as HomeScan), where consumers report purchases to Nielsen.

over 50% of sales at grocery and drug stores as well as about a third of mass merchandise stores from 35,000 locations (Kilts Center for Marketing, 2017). If changes in store inclusion are uncorrelated with treatment intensity, incomplete coverage will not bias estimated effects. While these data are the most appropriate source available on condoms for my analysis, there are two main limitations: (1) they contain condom sales instead of condom use and (2) sales to certain age groups cannot be isolated. However, no survey appropriate for longitudinal analysis or with sufficient sample size in each state-year contains information on condom use.

Less immediate outcomes, such as STI incidence and number of births, come from federal administrative data sources. The National Center for HIV/AIDS, Viral Hepatitis, STD, and TB Preventions AtlasPlus provides information on STIs including chlamydia and gonorrhea (Centers for Disease Control and Prevention, 2017c). These data are a limiting factor for unit of analysis, because sub-state information is not available on sub-populations such as 25- to 29-year-olds. I focus on chlamydia and gonorrhea because these STIs are primarily found in heterosexuals who may respond to the cost of prescription contraception; HIV and syphilis are concentrated in men who have sex with men (Centers for Disease Control and Prevention, 2016; Centers for Disease Control and Prevention, 2017b). State or local regulations require doctors, laboratories, and hospitals to report diagnosed cases of certain illness including STIs to local health departments, who then relay this information to the CDC (Centers for Disease Control and Prevention, 2015). National Vital Statistics provide counts of births in each state and year.

Each outcome is collapsed to the state-year level for 25- to 29-year-olds. While the ACA's 2010 dependent coverage mandate applies to young adults up to 26 (through 25) years of age, STI data are only available for pre-determined age groupings (20-24, 25-29, 30-34, etc.).

Analyses are performed on 25- to 29-year-olds to isolate the effect of the zero cost-sharing mandate from the earlier policy, though 25-year-olds may contaminate the analysis somewhat. Robustness tests on an older group (30- to 34-year-olds) provide additional evidence that this data limitation is not driving results.

I control for a set of time-varying state-level characteristics: the unemployment rate (total and age-specific) and population (total and age-specific) provided by the Bureau of Labor Statistics;[9] income per capita data from the Bureau of Economic Analysis; a binary measure of strict abortion regulation based on information from the Guttmacher Institute;[10] and state mandates of adult dependent health insurance coverage and of required coverage of prescription contraception from Collins and Nicholson (2010) and Raissian and Lopoo (2015).

The years of analysis are 2006 to 2014. I start the analysis in 2006 because emergency contraception became available over-the-counter for adults starting in that year (National Conference of State Legislatures, 2012). Over-the-counter emergency contraception could interact with health insurance (the measure of treatment intensity in this study) in important ways. Over-the-counter emergency contraception eliminated the need to interact with a health provider, which was a greater burden to women without insurance. To isolate my analysis from the effect of emergency contraception, I exclude years before the introduction of over-the-counter emergency contraception.

[9] "Age-specific" indicates that analyses on 25- to 29-year-olds include controls for the population and unemployment rate of 25- to 29-year-olds.

[10] Data were requested from the Guttmacher Institute. States are assigned to one of four categories – supportive, middle ground, hostile, extremely hostile – based on the number of major abortion restrictions in place during a year. For three examples, see www.guttmacher.org/sites/default/files/images/2000-2014-maps-states.png. I dichotomized categories into hostile (hostile or extremely hostile) or not (supportive or middle ground). Data were unavailable for 2007 and 2009. For the very few states that switched from not hostile to hostile between 2006 and 2008 or 2008 and 2010, I assigned hostile; otherwise 2007 and 2009 values were set to the values of the neighboring years.

3.2: Difference-in-differences Simulations

Parallel trends is a primary assumption of difference-in-differences, and examining event study pre-trends is a tool to assess the plausibility of this assumption. In the absences of flat pre-trends, researchers often include state-trends (or other unit-specific trends) to address this source of bias. However, state-trends often do not fully remove bias, and can actually introduce bias. The latter point has been made in the context of difference-in-differences with binary treatment and variation in treatment timing (Meer and West, 2016; Wolfers, 2006). Here I perform simple simulations to illustrate these points in the context of dose-response difference-in-differences and provide support for alternative approaches, specifically controlling for only pre-trend and not overall trend.

The first simulation shows incomplete correction provided by including state-trends, and demonstrates the superior performance of pre-trend controls. In this simulation, I constructed data such that the event study has a pre-trend slope of one, and a post-trend slope of negative one. This event study is in Figure 1, Panel A. The difference-in-difference estimate is 2.5, but the event study clearly shows the treatment effect is negative: violation of the parallel trends assumption biases the estimate. To create an event study controlling for state-trends, I regress the outcome on state-trends, and use the residuals for the event study. While the pre-trend is flatter in Figure 1, Panel B, an upward slope remains in the pre-period. Additionally, the average difference between the counter-factual and the observed values in the event study is -2, but the difference-in-differences estimate is only -1.1.

In Figure 1, Panel C, I show that including state-trends, but the trend variables are zero in the post-period, results in residuals with flat pre-trends. In Figure 1, Panel D, I achieve the same adjustment using an overall pre-trend variable – the interaction of the treatment intensity and time in the pre-period, zero in the post-period. Both of these controls provide correct difference-in-difference estimates of -2.

The second simulation shows state-trends can introduce bias. Meer and West (2016) discuss the case of binary treatment, variation in treatment timing, and time-varying treatment effects. Here I show the case of dose-response difference-in-differences. I constructed data such that the event study will have a flat pre-trend, and a post-trend slope of one. Figure 2, Panel A shows the event study, and the difference-in-differences estimate is 1. In Figure 2, Panel B, I show an event study for the residuals after regressing the outcome on state-trends, which introduces a downward slope in the pre-period and attenuates the difference-in-differences estimate to 0.54. The source of the attenuation is the state-trend controls force the overall slope of the residuals to be zero. Since the event study's slope is different in the pre- and post-period, and the average slope is zero, the residuals' pre-trend slope cannot be zero. In Figure 2, Panels C and D, I show that neither state pre-trend controls nor an overall pre-trend control introduces pre-slopes, and neither biases the difference-in-differences estimate.

If pre-trends and post-trends have the same slope, controlling for state trends, state pre-trends, or overall pre-trend result in the same difference-in-differences estimate (results available upon request). If the event study trends smoothly through the treatment year, all three approaches provide unbiased estimates of 0. If the pre-trends and post-trends have the same slope and there is a jump or drop in the treatment year, all three methods provide estimates equal to the size of the jump or drop.

3.3: Method

Identifying the effect of national policies can be difficult, because all states simultaneously experience the policy shock. To identify the effect of the zero cost-sharing mandate, I use a continuous measure of treatment intensity based on the pre-mandate insurance level, specifically the insured rate for 25- to 29-year-olds in 2011-12. The zero cost-sharing mandate will have stronger behavioral effects in states with high insured rates, because the mandate only applies to people with insurance. Consider the extreme cases: a hypothetical state with no insured 25- to 29-year-olds in 2011-12 would have no potential for an exogenous change in the cost of prescription birth control, while a state where every person is insured would have the potential for a large exogenous change in the cost and use of prescription contraception.

By focusing on 25- to 29-year-olds, I am less likely to conflate the estimated effect of the zero cost-sharing mandate with the earlier dependent coverage policy. Some states implemented contraception coverage mandates before 2012, but they tend to be weaker, and I control for state mandates in all regressions. I also perform robustness tests using the rate of privately-insured 25- to 29-year-olds.

To determine the effect of the zero cost-sharing mandate, I perform difference-in-differences and event study analyses. The strengths of event studies are that they reveal all changes that occur in the event year as well as providing a compelling visual representation. However, by adding parametric assumptions I can derive causal estimates with meaningful interpretations, perform statistical inference, and gain statistical power.

I estimate the four difference-in-difference models described in the simulation subsection. However, the zero cost-sharing mandate could cause an immediate effect as well as a time-varying effect, both of which merit capturing. The time-varying effect could result from more people learning about the policy over time. Another potential reason for a time-varying effect is the compounding effect of STI infection: an initial transmission has the potential to spread to future partners. To capture both effects, I fit a line for the pre-period, and then estimate both a one-time jump/drop that occurs in the year of policy implementation as well as any change in slope.[11] See Figure 3 for a stylized event study with a visual representation of this analysis.

First, I estimate event study models. The estimating equation is

$$\log(Y_{st}) = \beta_0 + \sum_{\substack{t=2006 \\ \neq 2011}}^{2014} \beta_t \left(InsureRate_s * \mathbf{1}(Year_t)\right) + \alpha_s * \mathbf{1}(State_s) + \delta_t * \mathbf{1}(Year_t) + \beta_x * \boldsymbol{X}_{st} + \epsilon_{st}. \qquad \text{(Eq. 1)}$$

State and year fixed effects are represented by $\mathbf{1}(State_s)$ and $\mathbf{1}(Year_t)$. By including these fixed effects, I control for time-invariant state characteristics and national year-specific changes. The insured rate for 25- to 29-year-olds in 2011-12 is $InsureRate_s$. The relationship between treatment intensity and outcomes in year *t* is β_t, and these coefficients show the pattern in the

[11] This is similar to post-estimation in Finkelstein (2007). After estimating event studies, she compares the difference in the event studies between 1970 (five years after Medicare introduction) and 1965 (the year of Medicare introduction) to the difference between 1965 and 1960 (five years before Medicare introduction). This approximately compares the slope in the event study before the policy to the slope in the event study after the study. For an example of a similar parameterization, see Levy et al. (2016) and Wolfers (2006) for a partial parameterization.

outcome between states with high and low uninsured rates. Since Eq. 1 is a log-linear regression and $InsureRate_s$ is a rate between 0 and 1, each percentage point increase in the treatment intensity (insured rate) corresponds to a β_t percent increase in the outcome in year *t* compared to the base year of 2011.

I also control for a set of time-varying state-specific covariates, $\boldsymbol{X}_{st}$. Controls include the unemployment rate (total and age-specific), population (total and age-specific), income per capita, a dummy for strict regulation of abortion, and dummy variables for state-level mandates similar to the ACA's dependent coverage mandate and mandates of contraception coverage.

Since the main difference-in-differences estimates control for pre-trends, I provide two additional event study graphs that also account for any pre-trends. Generally, both of these approaches produce very similar graphs. First, I follow Wolfers (2006) by completely controlling for any pre-trend (the pre-period is forced to 0) and estimating the post-period deviations from that pre-trend. The estimating equation for this method is:

$$\log(Y_{st}) = \beta_0 + \sum_{t=2012}^{2014} \beta_t \left(InsureRate_s * \mathbf{1}(Year_t)\right) \qquad \text{(Eq. 2)}$$

$$+ \alpha_s * \mathbf{1}(State_s) + \delta_t * \mathbf{1}(Year_t) + [\theta_s * \mathbf{1}(State_s) * Year_t] + \beta_x * \boldsymbol{X}_{st} + \epsilon_{st}.$$

The coefficient on the interaction of state dummies and continuous time, θ_s, represents state-trends. By including the post-period interaction of insured rate and year, the state-trends are identified using only the pre-period data. The β_t plot the time-varying treatment effect in the post-period.

Second, I adjust my traditional event studies to reflect the difference-in-difference estimate with the overall pre-trend control. I estimate the pre-period slope, and net that slope from all β_t's, basically rotating the event study to have a flat pre-trend. Then I subtract the mean of the pre-period β_t's, so the pre-period is centered around the x-axis. This approach reflects a valid difference-in-differences estimate and shows the pre-period variation.

I estimate five difference-in-difference models. First, I estimate difference-in-differences with no trend adjustment (Eq. 3), and second, I control for state-trends (Eq. 4). These models correspond to the simulations in Panels A and B of Figures 1 and 2. As mentioned, both of these estimates are biased in the presence of pre-trends.

$$\log(Y_{st}) = \beta_0 + \beta_{DD}\big(InsureRate_s * \mathbf{1}(t \geq 2012)\big) \qquad \text{(Eq. 3)}$$
$$+\alpha_s * \mathbf{1}(State_s) + \delta_t * \mathbf{1}(Year_t) + \gamma_x * \boldsymbol{X}_{st} + \epsilon_{st}\ .$$

$$\log(Y_{st}) = \beta_0 + \beta_{DD}\big(InsureRate_s * \mathbf{1}(t \geq 2012)\big) \qquad \text{(Eq. 4)}$$
$$+\alpha_s * \mathbf{1}(State_s) + \delta_t * \mathbf{1}(Year_t) + [\phi_s * \mathbf{1}(State_s) * Year_t] + \gamma_x * \boldsymbol{X}_{st} + \epsilon_{st}\ .$$

Third, I control for state pre-trends (Eq. 5), and fourth, I control for an overall pre-trend (Eq. 6). These models correspond to the simulations Panels C and D of Figures 1 and 2. In Eq 5, θ_s captures the state pre-trends, and state pre-trends are the interaction of state dummies ($\mathbf{1}(State_s)$), continuous time centered in the mid-point of the post-period ($t - 2013$), and an indicator for the pre-period ($\mathbf{1}(t < 2012)$). In Eq. 6, η controls for the overall pre-trend, and the pre-policy insured rate replaces the state dummies.

$$\log(Y_{st}) = \beta_0 + \beta_{DD}\big(InsureRate_s * \mathbf{1}(t \geq 2012)\big) + \alpha_s * \mathbf{1}(State_s) + \delta_t * \mathbf{1}(Year_t) \quad \text{(Eq. 5)}$$
$$+ \delta_t * \mathbf{1}(Year_t) + [\theta_s * \mathbf{1}(State_s) * (t - 2013) * \mathbf{1}(t < 2012)] + \gamma_x * \boldsymbol{X}_{st} + \epsilon_{st}\ .$$

$$\log(Y_{st}) = \beta_0 + \beta_{DD}\big(InsureRate_s * \mathbf{1}(t \geq 2012)\big) + \alpha_s * \mathbf{1}(State_s) + \delta_t * \mathbf{1}(Year_t) \quad \text{(Eq. 6)}$$
$$+ \delta_t * \mathbf{1}(Year_t) + [\eta * InsureRate_s * (t - 2013) * \mathbf{1}(t < 2012)] + \gamma_x * \boldsymbol{X}_{st} + \epsilon_{st}\ .$$

The coefficient of interest is β_{DD}. By including state and year fixed effects, I control for cross-sectional, time-invariant differences in outcomes. The binary variable $\mathbf{1}(t \geq 2012)$ indicates whether the mandate is in effect. Since $InsureRate_s$ is a rate between 0 and 1, for a one percentage point increase in the insured rate, there is a β_{DD} percent increase in the outcome. The model includes the same vector of time-varying state-specific covariates as in Eq. 1, $\boldsymbol{X}_{st}$.

To estimate both a one-time jump/drop and a linear time-varying treatment effect, I estimate models of the form:

$$\log(Y_{st}) = \beta_0 + \beta_1(InsureRate_s * (t - 2012))$$
$$+ \beta_2\big(InsureRate_s * \mathbf{1}(t \geq 2012)\big) + \beta_3\big(InsureRate_s * (t - 2012) * \mathbf{1}(t \geq 2012)\big)$$
$$+ \alpha_s * \mathbf{1}(State_s) + \delta_t * \mathbf{1}(Year_t) + \gamma_x * \boldsymbol{X}_{st} + \epsilon_{st}\ . \quad \text{(Eq. 7)}$$

A pre-period line is modeled by β_0 and β_1, while β_2 and β_3 represent the post-period deviation from that trend. For a one percentage point increase in the insured rate, there is a one-time change in the outcome of β_2 percent and an annual increase of β_3 percent. See Figure 3 for a stylized event study with a visual representation of this analysis. The estimated β_2 and β_3 apply to the years in the analysis but may not persist indefinitely, especially as additional ACA policies were implemented.

The main identifying assumption of these models cannot be directly verified. Since the counterfactual is unobservable, I must rely on an ocular test: the adjusted pre-trends should be approximately linear. Linearity is important because higher order functions will have different slopes across the domain, and in those cases, the methods I use could find spurious effects. Non-parallel pre-trends represent a similar concern in the traditional difference-in-differences. Additionally, I must assume that no contemporaneous shocks are correlated with the pre-policy insured rate and the outcomes, which cannot be tested directly. However, I perform falsification tests by estimating the effect of the zero cost-sharing mandate on state characteristics that should be unaffected by this policy change. If both assumptions are met, then any observed changes are due to the policy.

All standard errors are clustered at the state level, and regressions are weighted by the 2011 age-specific population. After weighting by population, estimates reflect the national average treatment effect.

4. Results

4.1: Summary Statistics

Table 1 contains the mean and standard deviation of measures of treatment intensity and outcome variables, weighted by age-specific state populations. In addition to summary statistics for 25- to 29-year-olds, I also present information on 20- to 24-year-olds who are the age group analyzed for the dependent coverage mandate in Section 5, and 30- to 34-year-olds who are analyzed in a robustness check in Section 6. The first panel reports treatment measures. About two-thirds of those age 25 to 29 had insurance in 2011-12. Importantly, there is substantial variation across states, with standard deviations in insured rates around 7 percentage points. The second panel of Table 1 reports summary statistics for outcome variables. Chlamydia, a very common STI, primarily infects women and is often contracted through heterosexual intercourse (Centers for Disease Control and Prevention, 2016), so should respond to policies that affect birth control use. Condom sales and births are more common than the diseases examined here.

4.2: Effects on STIs and Births

Appendix Figure A1, Panels A-C, show unadjusted event studies for STIs and births. Figure 4 shows adjusted event studies. From Figure 4, post-period estimates from both the pre-trend adjusted event studies (black dots) and Eq. 2 (similar to Wolfers, 2006, grey dots) are generally very similar. Both methods show an increase in STIs and a decrease in births. This is consistent with my hypotheses and an unintended consequence of insurance; treatable STI incidence increased more in higher treated states beginning in 2012. Chlamydia is the most common STI in this age-group, and so the most likely to be affected by this policy. The zero cost-sharing mandate has a positive but less conclusive estimated effect on gonorrhea. While these effects are evident in the unadjusted event studies in Appendix Figure A1, the pre-trend slopes show the problem with estimating simple difference-in-differences models. All three post-period points are also very linear and sloping for STIs and births, indicating a dynamic time-varying treatment effect. I model this effect in the fifth column of Table 2.

Table 2 presents difference-in-differences results. For the effects on chlamydia in Panel A, it is clear that the simple difference-in-differences estimate in the first column is biased down. This estimate reflects the downward pre-trend in Appendix Figure A1. Controlling for state-trends in the second column is more consistent with the adjusted event studies, but is still closer to zero than the average post-period effect in the adjusted event studies. After appropriately controlling for pre-trends in the third and fourth columns, the estimated effect is consistent with the adjusted event studies. A percentage point increase in treatment intensity for 25- to 29-year-olds caused a 0.777-0.847% increase chlamydia incidence. Both of these estimates are approximately the average of the post-period $\beta_t's$ in the adjusted event studies. When I model the dynamic effect of the policy in the fifth column, a percentage point increase in treatment intensity caused a one-time 0.530% increase and a yearly increase (change in slope) of 0.248% in chlamydia incidence. Higher chlamydia incidence reflects an unintended consequence of this insurance expansion. The estimated effect on gonorrhea represents a meaningful increase, but the estimates have larger standard errors and are not consistently statistically significant.

Panel C of Appendix Figure A1 shows the unadjusted event study for births. Panel C of Figure 4 shows the adjusted event study. Both event studies show a sharp downward shift after policy implementation, likely reflecting increased use of prescription contraception and a decrease in unintended pregnancy. From the difference-in-differences results in Panel C of Table 2, the zero cost-sharing mandate did have a large and often statistically significant effect on births. The estimate should be interpreted as a 0.11-0.14 percent decrease in births for each percentage point increase in the insured rate. Most of the effect is time varying, with a statistically significant 0.094 decrease in slope.

In summary, both the difference-in-differences and event studies show evidence that the zero cost-sharing mandate caused an increase in chlamydia and a decrease in births. Next, I provide suggestive evidence the increase in STIs resulted from lower investment in prevention as measured by condom sales. Since this policy lacks any countervailing protection against STIs, this unintended consequence resulted in increased cases of chlamydia. However, increased access to prescription contraception reduced total births, likely due to fewer unintended pregnancies.

4.3: Effects on Prevention Investment (Condom Sales)

There are two main concerns with the Nielsen data I use for analyzing condom sales, so the results in this subsection provide suggestive evidence the effects on STIs operate through reduced condom sales. First, while fertility and STI data are the universe of diagnoses/births, a non-random sample of retailers provide condom sales data. Any correlation between store openings or closings with insured rates and condom sales would bias these estimates.

Second, I cannot isolate age groups, so I start the analysis for condoms in 2010 to prevent contamination from the 2010 dependent coverage mandate. With only two years of pre-period, I use the Nielsen data to perform analysis at the quarter level. Since the analysis starts after the dependent coverage mandate of 2010, and since the policy and outcome cover all ages, I use the average insured rate for 20- to 29-year-olds as the measure of treatment intensity. Note that a large fraction of people in this age range use condoms. Almost 40% of people in their early 20s and over a quarter of 25- to 29-year-olds use condoms (Reece et al., 2010), which is consistent with this policy noticeably affecting total condom sales.

Panel D of Appendix Figure A1 shows the unadjusted event study for condom sales. Panel D of Figure 4 shows the adjusted event study. The adjusted event study provides evidence the zero cost-sharing mandate reduced condom sales. This event study includes quarter-by-year fixed effects, so the remaining cyclicality is due to seasonal heterogeneous treatment effects. Table 3 presents estimates for the zero cost-sharing mandate on condom sales. Since the pre-trends in the unadjusted event study are quite flat, the simple difference-in-differences are consistent with both pre-trend adjusted difference-in-differences. For each percentage point increase in the insured rate, condom sales fall by 0.1-0.2 percent. Most of the effect is time varying, a 0.13 percent annual decrease in condom sales after 2012 for each percentage point increase in the 2008-09 insured rate. Decreased condom sales in response to health insurance coverage suggests condoms are a mechanism by which lower-cost contraception affected STIs and is consistent with the theoretical predictions.

5. Extensive Margin of Health Insurance: Dependent Coverage Mandate

While the zero cost-sharing mandate affected the intensive margin of health insurance, it is important to determine whether an increase in STIs is a feature of other insurance expansions or if comprehensive coverage protects against the spread of disease. To investigate this question I exploit the young adult dependent coverage mandate of 2010. This policy caused an exogenous shock on the extensive margin – the number of people insured – by allowing young adults to join their parents' health insurance.

The dependent coverage mandate required that, starting in September 2010, all insurance plans covering dependents of the primary policyholder must offer coverage to children of the policyholder up to age 26 (Department of Labor, 2017). Prior to implementation, close to 14 million people in their 20s were uninsured (Collins and Nicholson, 2010). The dependent coverage mandate had an economically meaningful and statistically significant effect on the insured rate for young adults. Appendix Figure A2 shows the pattern of uninsured rates for 18- to 24-year-olds and 25- to 34-year-olds. Before 2010, 18- to 24-year-olds consistently had higher uninsured rates, but experienced a sharp decrease in their uninsured rate starting in 2010. Sommers et al. (2013) estimate that this mandate increased the percent of adults under the age of 26 who are insured by 6.7 percentage points.[12]

The net impact of the extensive margin of health insurance on STIs and pregnancy is ambiguous due to countervailing effects of insurance. On the one hand, the dependent coverage mandate increased the probability that a potential sexual partner has insurance, permitting quick and effective treatment of STIs. If a sexual partner is STI-free, sex without a condom will not result in infection transmission. On the other hand, insurance lowers the cost of prescription contraception, which can increase STI transmission. Additionally, insurance lowers the cost of childbirth, so the dependent coverage mandate may result in an increase in intended pregnancies among people who otherwise could not afford pregnancy-associated medical expenses. So the dependent coverage mandate may cause an increase in intended births but a decrease in unintended births, with an ambiguous effect on net births.

[12] This is a large change compared to other recent policies aimed at increasing insurance rates. For instance, the State Children's Health Insurance Program (SCHIP), which offers public health insurance to low-income but Medicaid-ineligible children, increased coverage by 5.7 percentage points in the target population. However, the net effect on childhood insurance rates was much smaller because of strict income eligibility criteria (LoSasso and Buchmueller, 2004).

I use the same empirical strategy as in my main analysis to examine the effect of the 2010 dependent coverage mandate, but now the treatment intensity is the percent of uninsured 20- to 24-year-olds in 2008-09. Intuitively I expect more uninsured young adults in a state will correspond to larger potential increases in the insured rate from this policy. While previous studies have used older adults as a control group to perform binary treatment difference-in-differences, Slusky (2017) suggests this approach has significant problems. Again, the data are collapsed to the state-year level for 20- to 24-year-olds. While this policy applies to 25-year-olds, most of the data on the outcomes I examine are only available in five-year age groupings (20-24, 25-29), so I focus on 20- to 24-year-olds.

If my empirical strategy shows that the dependent coverage mandate affects outcomes through insurance coverage, then the mandate must increase coverage more in states with lower pre-mandate coverage. To test this hypothesis, I regress the change in insured rate (2011-12 rate minus 2008-09 rate) on the 2008-09 uninsured rate for ages exposed to the dependent coverage mandate.[13] These estimates are in Appendix Table A1. Importantly, the effect is large and statistically significant for young adults exposed to the policy. The dependent coverage mandate reduced the uninsured rate for young adults by 4.3 percentage points or about 6 percent.[14] This is comparable to the 6.7 percentage point effect in Sommers et al. (2013). Some uninsured young adults may not gain coverage from the dependent coverage mandate because of uninsured parents or parents' unwillingness to add a child to their plan.

[13] The regression takes the form: $\Delta\, InsureRate_s = \beta_0 + \beta_{change} UninsureRate0809_s + \epsilon_{st}$

[14] From Appendix Table A1, 0.139*(1-0.688)=0.043

As a falsification test, I conduct the same analysis for older groups who should be unaffected by the dependent coverage mandate and report these results in Appendix Table A1. I show that the dependent coverage mandate did not affect insurance coverage for these groups; estimates are closer to zero and not statistically significant. The percent change for young adults is at least twice the magnitude as for other age groups.

I conduct similar difference-in-differences analyses as for the zero cost-sharing mandate (event studies available upon request) with two notable differences: 1) the measure of treatment intensity is now the pre-policy insured rate for 20- to 24-year-olds, and 2) the years of analysis are 2006 to 2012, to isolate the effect of the dependent coverage mandate from the effect of the zero cost-sharing mandate.[15]

Table 4 presents estimates for the effect on STIs and births. The estimates for the effect of the dependent coverage mandate indicate that the overall effect of insurance reduces STIs. For both chlamydia and gonorrhea, a percentage point increase in the uninsured rate leads to a ½ to ¾ percent decrease in STIs after adjusting for pre-trends. After adjusting for pre-trends, there is a precisely estimated zero effect on births. This is somewhat consistent with previous research of the dependent coverage mandate's effect on fertility, which find modest or mixed effects (Dills and Grecu, 2017; Heim, Lurie, and Simon, 2017).

Table 5 reports results for condoms sales. The pre-trend adjusted difference-in-differences results indicate a moderate-sized but not consistently significant decrease. In the dynamic model, the time-varying effect is meaningful and statistically significant, perhaps due to a more accurate

[15] Like the zero cost-sharing mandate, the dependent coverage mandate is a national policy, which addresses concerns about policy timing endogeneity or regression to the mean driving results.

model specification. The dependent coverage mandate might have a smaller effect on condom sales than the zero cost-sharing mandate for two reasons. First, a price of $0 is more salient (Shampanier et al., 2007), and while this policy lowers the price of contraception, the effect might be smaller because birth control was still not free to many newly insured young adults. Second, ability to be on a parent's insurance plan could cause an income effect of young adults. Average insurance premiums for people age 18-24 on the individual market were $1,429 in 2009 and could be much higher based on state (America's Health Insurance Plans, 2009). The protective effects of the dependent coverage mandate on STIs more than compensated for any potential reduction in prevention, suggesting an increase in STIs is not endemic to all insurance expansions.

6. Robustness and Falsification Tests

One possible threat to identification is health insurance's effect on frequency of doctors' visits, including for STI testing. This response would change the number of STI diagnoses even if risky behavior remained unchanged. Appendix Figure A3 and Appendix Figure A4 show adjusted event studies for the effect of the mandates on routine medical services. Both figures are inconsistent with changes in interactions with health professionals, and thus testing, driving the STI results. In fact, the change in this outcome runs in the opposite direction as the change for STIs. Women may have less frequent contact with doctors after the zero cost-sharing mandate because they gained access to free long-acting contraception that can last multiple years (e.g., IUDs), which may lead to skipped annual wellness visits.

In terms of actual measures of testing, the National Ambulatory Medical Care Survey, which samples doctors' offices and visits, contains information on chlamydia testing. Since this data source is not designed for state-level analysis, I can only provide suggestive evidence for the dependent coverage mandate based on a comparison between 20- to 25-year-olds (treated group) and 26- 30-year-olds (control group). Appendix Figure A5 shows that both groups generally trend together through the whole period.

I also conduct robustness checks with unweighted regressions, excluding early Medicaid expansion states, and on an older group (30- to 34-year-olds). Generally, the results are robust to different specifications, with similar magnitudes and direction. Results for unweighted regressions in Appendix Table A2 and Appendix Table A3 are similar; however, results for the dependent coverage mandate are smaller, and fewer estimates are statistically significant for both mandates. Smaller effects for the unweighted models indicate that more populous states are more responsive. Though most of the expansion of Medicaid to childless adults occurs after both mandates, some states expanded coverage early. Evidence for California suggests Medicaid expansion might not have a meaningful impact on contraception use (Early et al., 2018). I check if estimates are robust to excluding early expansion states and present these results in Appendix Table A4 and Appendix Table A5. Results are similar in magnitude, but estimates for the dependent coverage mandate are less frequently significant.

In Appendix Table A6, I show the estimated effect of the zero cost-sharing mandate on an older group, 30- to 34-year-olds. This analysis is an important robustness test of the zero cost-sharing mandate, because 25-year-olds are targeted by the 2010 dependent coverage mandate but included in the zero cost-sharing mandate analysis due to data limitations. In addition, many people who are in their early 20s in 2010 age into the 25- to 29-year-old group before 2014. The 30- to 34-year-olds sample does not suffer from either of these concerns. Additionally, it provides insight into heterogeneous effects by age. Comparing the main results for 25- to 29-year-olds in Table 2 to 30- to 34-year-olds in Appendix Table A6, effects for both age groups are similar. The effect on chlamydia is approximately the same and highly significant, while the estimates for gonorrhea are similar in magnitude but less significant than for chlamydia. However, the estimate for fertility is smaller and no longer significant for the older group.[16]

I perform two additional robustness tests for the zero cost-sharing mandate. First, I use the privately-insured rate as the measure of treatment intensity instead of the overall insured rate. This addresses concerns that young adults on public insurance, namely Medicaid, are likely to have low cost sharing for all prescriptions and should not drive the main results. I use data from the ACS to construct privately-insured rates, and estimated effects in Table A7 are consistent with the main results. The second robustness test lags births by a year, because pregnancies last for a large fraction of a year. Table A8 reports these results, which are similar or larger than the main estimates.

Falsification tests of whether the mandates impact other state-level characteristics are reported in Appendix Tables A9-A10. Only one of 56 (1.8%) coefficients are statistically significant at a 10% level, and only one at a 5% level. Both coefficients are quite small, and we would expect to reject some null hypotheses due to type I error. Importantly, HIV and syphilis are not statistically significant, because these two diseases are concentrated in men who have sex

[16] One reason for the difference in fertility response between 25- to 29-year-olds and 30- to 34-year-olds is that during these age ranges, probability of pregnancy from unprotected sex declines (Dunson et al., 2004). Therefore, even though both groups appear to have similar increases in risky sex based on STIs, fertility of the older group is less responsive to this change.

with men (Centers for Disease Control and Prevention, 2016; Centers for Disease Control and Prevention, 2017a) and should be less responsive to the mandates, particularly related to female contraception.

7. Conclusion

This study contributes to the literature by testing unintended consequences of health insurance with respect to risky sex. Increased risky sex in response to lower expected costs is consistent with the previous literature on risky sex and the rational choice model of behavior. While previous empirical research generally finds mixed or weak evidence of ex ante moral hazard, there is reason to believe many forms of prevention are responsive to future, not current, insurance status.

I find the zero cost-sharing mandate resulted in fewer condoms purchased and more cases of chlamydia. However, the protective effect of insurance from the dependent coverage mandate caused a meaningful reduction in STI incidence. While I cannot directly quantify the effect of these policies on net utility without making strong assumptions about utility functions, it seems likely both had positive net impacts. While the zero cost-sharing mandate did cause an increase in STIs, decreased unintended births are likely much more meaningful both in financial and non-monetary terms. The benefit of the dependent coverage mandate is more definitive: this mandate reduced STIs. Additionally, based on the reduction in fertility at the intensive margin, the null effect on birth from the dependent coverage mandate is likely due to an increase in intended fertility and a reduction in unintended pregnancies.

An important policy implication of my findings is that insurance has unintended consequences, but in some cases comprehensive insurance coverage can mitigate these problems. Similarly, since lowering the cost of prescription contraception causes substitution away from condoms, one way to prevent risky sex could be to subsidize condoms. Additionally, repeal of one or both policies in this study is a real possibility, and this analysis provides suggestive evidence on the effect of policy termination.

Future work that leverages changes in expectations about future insurance status could reveal distortions in other health behaviors. Empirically testing the effect of subsidizing condoms on condom use and STI transmission could be important to determine if condom subsidies are a potential tool to counteract the unintended consequences found in this study. While women generally receive information on STI risk and consequences at initiation of prescription contraception use, male partners may be less well informed; examining the impact of informing both partners about STI risk is another important question for future research.

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Figures

Figure 1: Simulated Analysis - Pre-Trend of One, Post-Trend of Negative One

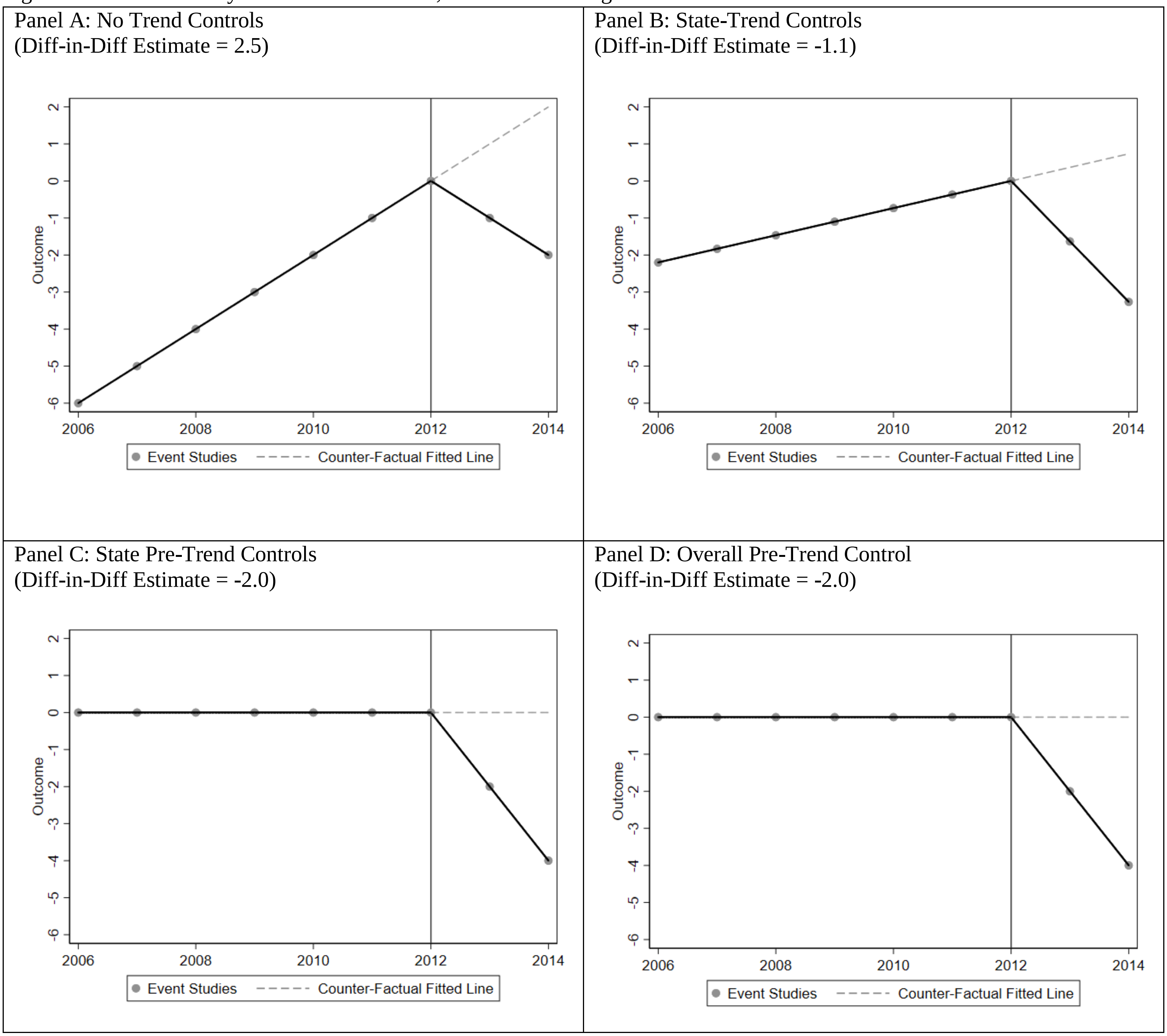


Figure 2: Simulated Analysis - Pre-Trend of 0, Post-Trend of One

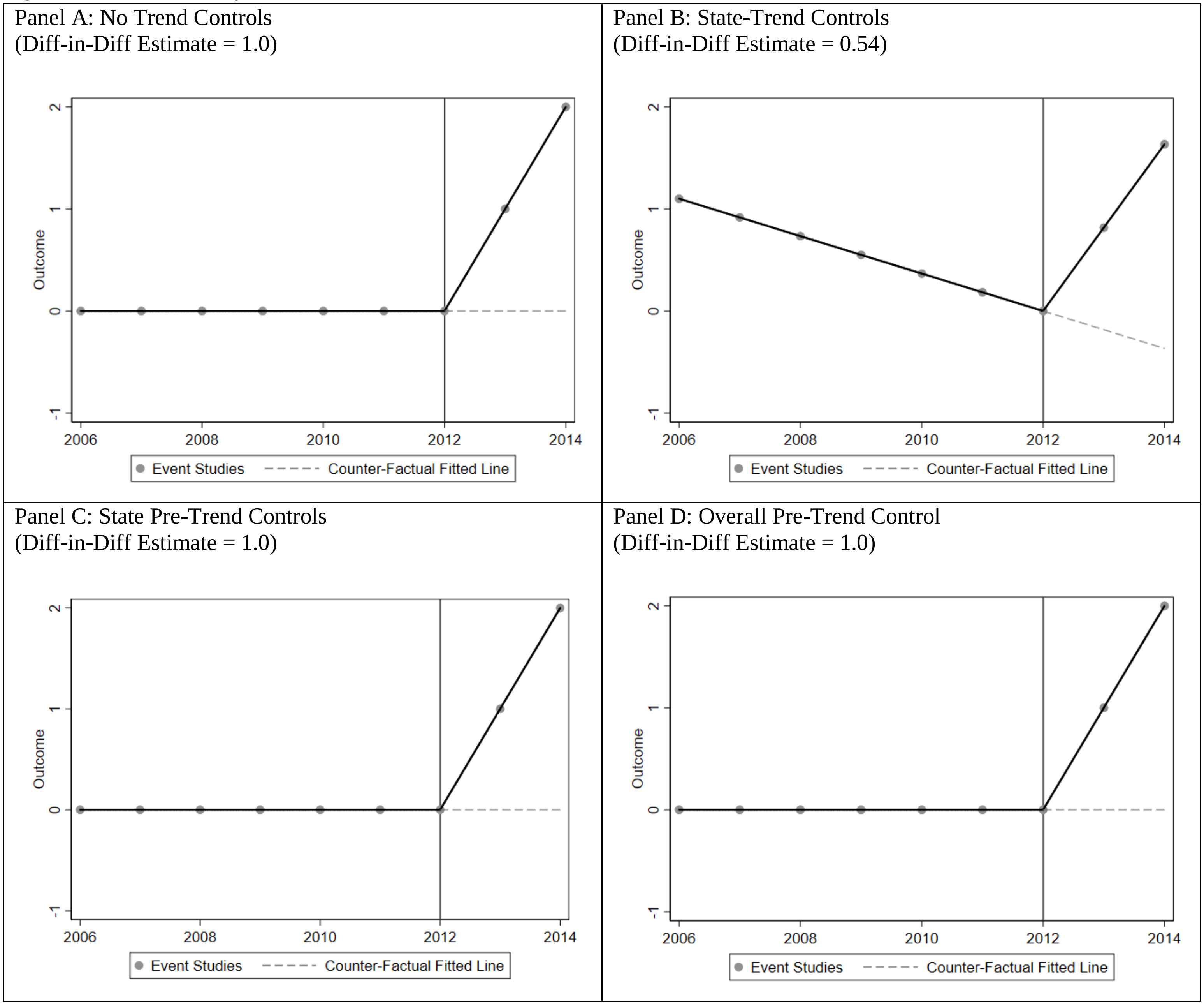
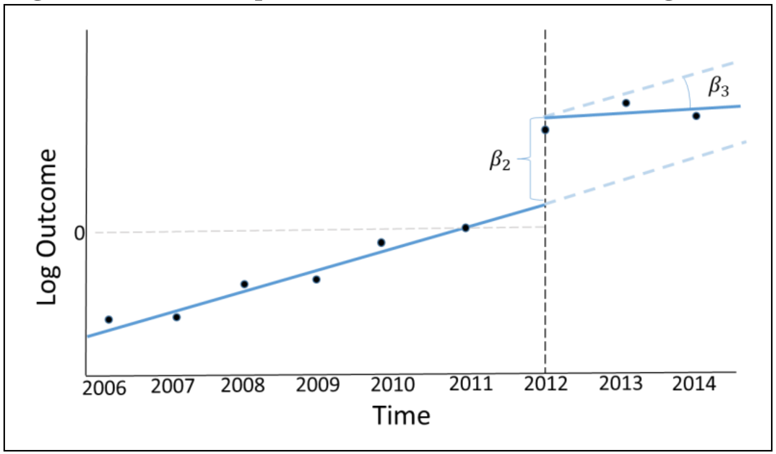
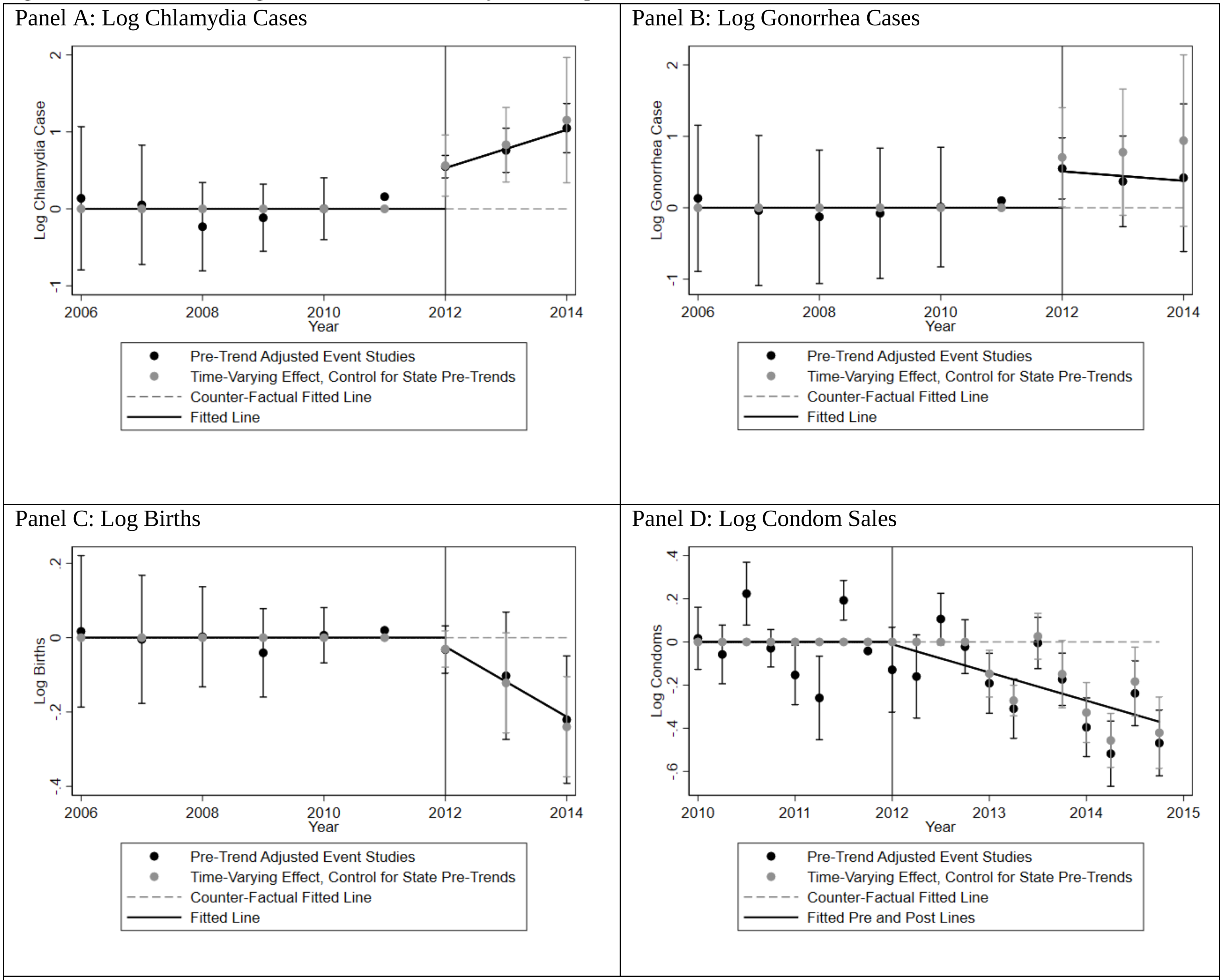


Figure 3: Visual Representation of One-Time Change and Time-Varying Effect (Eq. 7)



Note: In this analysis, the counter factual is based on assuming the solid line on the left (before policy implementation) would continue on the same path as the lower dotted line if there had been no policy shocks. The deviation from the lower dotted line is the causal effect of the policy.

Figure 4: Zero Cost-Sharing Mandate Pre-Trend Adjusted Graphs



Note: Whiskers 95% confidence intervals; Clustered at state-level, weighted by 2011 state-age population
Panel A-C treatment intensity: 2011-12 insured rate, 25-29 year olds
Panel D treatment intensity: 2011-12 insured rate, 20-29 year olds
Controls: unemployment rate (total and age-specific), population (total and age-specific), income per capita, strict state regulation of abortion, and state mandates

Tables

Table 1: Summary Statistics

Variable	20-24 Year Olds	25-29 Year Olds	30-34 Year Olds
	Treatment Intensity[17]		
Uninsured Rate	0.328	-	
(Avg. 2008-09; age-specific)	(0.083)	-	
Insured Rate	-	0.677	0.730
(Avg. 2011-12; age-specific)	-	(0.072)	(0.080)
	Outcomes		
Condoms	10,030,114	10,030,114	10,030,114
(Total)	(9,574,394)	(9,574,394)	(9,574,394)
Log Condoms	15.656	15.656	15.656
(Total)	(1.035)	(1.035)	(1.035)
Births	43,594	52,369	46,899
	(36,555)	(43,213)	(40,928)
Log Births	10.314	10.508	10.350
	(0.911)	(0.895)	(0.963)
Chlamydia Cases	22,056	9,755	4,257
	(18,113)	(8,817)	(4,111)
Log Chlamydia Cases	9.615	8.749	7.875
	(0.963)	(1.003)	(1.050)
Gonorrhea Cases	4,708	2,640	1,423
	(3,360)	(2,125)	(1,277)
Log Gonorrhea Cases	8.026	7.419	6.762
	(1.173)	(1.161)	(1.190)

Notes: Weighted by age-specific state populations
Standard deviation in parentheses (SD)

[17] Correlation between insured rates for 20- to 24-year-olds and 25- to 29-year-olds is 0.79.

Table 2: Effect of Zero Cost-Sharing Mandate on STIs and Births (25-29 Year Olds)

	No Pre-Trend Adjustment	State Trends	State Pre-trends	Overall Pre-trend	Dynamic Model
Panel A: Log Chlamydia Cases					
Difference-in-Differences	-0.071	0.666***	0.847***	0.777***	0.530***
	(0.195)	(0.214)	(0.275)	(0.258)	(0.171)
Change in Slope					0.248**
					(0.122)
Panel B: Log Gonorrhea Cases					
Difference-in-Differences	0.728	0.736**	0.883**	0.443	0.510
	(0.439)	(0.360)	(0.434)	(0.483)	(0.345)
Change in Slope					-0.066
					(0.206)
Panel C: Log Births					
Difference-in-Differences	-0.195***	-0.066*	-0.139**	-0.111	-0.024
	(0.067)	(0.035)	(0.052)	(0.074)	(0.043)
Change in Slope					-0.094**
					(0.037)

*p-value<0.10, **p-value<0.05, ***p-value<0.01
Standard errors in parentheses (SE), clustered at state-level, weighted by 2011 state-age population
Controls: unemployment rate (total and age-specific), population (total and age-specific), income per capita, strict state regulation of abortion, and state mandates
Years: 2010-2014; 450 observations at the state-year level

Table 3: Effect of Zero Cost-Sharing Mandate on Log Condoms Sales (20-29 Year Olds)

	No Pre-Trend Adjustment	State Trends	State Pre-trends	Overall Pre-trend	Dynamic Model
Difference-in-Differences	-0.150**	0.115	-0.199***	-0.163	-0.011
	(0.064)	(0.072)	(0.049)	(0.104)	(0.083)
Change in Slope					-0.131**
					(0.050)

*p-value<0.10, **p-value<0.05, ***p-value<0.01
Standard errors in parentheses (SE), clustered at state-level, weighted by 2011 state-age population
Controls: unemployment rate (total and age-specific), population (total and age-specific),
income per capita, strict state regulation of abortion, and state mandates
Years: 2010-2014; 960 observations at the state-quarter level

Table 4: Effect of Dependent Coverage Mandate on STIs and Births (20-24 Year Olds)

Panel A: Log Chlamydia Cases

	No Pre-Trend Adjustment	State Trends	State Pre-trends	Overall Pre-trend	Dynamic Model
Difference-in-Differences	0.068 (0.176)	-0.289 (0.199)	-0.610** (0.261)	-0.484 (0.295)	-0.224 (0.215)
Change in Slope					-0.256* (0.136)

Panel B: Log Gonorrhea Cases

	No Pre-Trend Adjustment	State Trends	State Pre-trends	Overall Pre-trend	Dynamic Model
Difference-in-Differences	-0.043 (0.185)	-0.127 (0.249)	-0.695* (0.365)	-0.743** (0.350)	-0.324 (0.221)
Change in Slope					-0.410** (0.202)

Panel C: Log Births

	No Pre-Trend Adjustment	State Trends	State Pre-trends	Overall Pre-trend	Dynamic Model
Difference-in-Differences	0.087** (0.042)	-0.009 (0.034)	0.028 (0.047)	-0.014 (0.053)	-0.031 (0.038)
Change in Slope					0.015 (0.021)

*p-value<0.10, **p-value<0.05, ***p-value<0.01
Standard errors in parentheses (SE), clustered at state-level, weighted by 2011 state-age population
Controls: unemployment rate (total and age-specific), population (total and age-specific), income per capita, strict state regulation of abortion, and state mandates
Years: 2006-2012; 350 observations at the state-year level

Table 5: Effect of Dependent Coverage Mandate on Log Condom Sales

	No Pre-Trend Adjustment	State Trends	State Pre-trends	Overall Pre-trend	Dynamic Model
Difference-in-Differences	0.313***	0.074*	-0.046	-0.078	0.031
	(0.103)	(0.044)	(0.081)	(0.079)	(0.053)
Change in Slope					-0.109***
					(0.037)

*p-value<0.10, **p-value<0.05, ***p-value<0.01
Standard errors in parentheses (SE), clustered at state-level, weighted by 2011 state-age population
Controls: unemployment rate (total and age-specific), population (total and age-specific),
income per capita, strict state regulation of abortion, and state mandates
Years: 2006-2012; 336 observations at the state-quarter level