Ex Ante Moral Hazard in Health Insurance: Lessons from the ACA and Risky Sex

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Abstract

Given current levels of health spending, distortions from insurance are of utmost importance. Ex ante moral hazard (insurance's impact on risky behavior) receives much less attention than ex post moral hazard. I examine effects of insurance on risky sex, a behavior with quick, meaningful consequences. Exploiting the zero cost-sharing for contraception mandate and pre-policy insured rates as a measure of treatment intensity, I find ex ante moral hazard decreases prevention and increases STIs. Using the dependent coverage mandate to determine insurance's overall effect, I find the protective effect of insurance on STIs more than compensates for the reduction in prevention.

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1. Introduction

Health insurance has played a prominent role in the rapid growth of medical spending over the past half century (Finkelstein, 2007; Finkelstein et al., 2012; Manning et al., 1987). While health insurance increases utility by smoothing consumption, insurance can also have unintended consequences, known as moral hazard. Ex ante moral hazard is the effect health insurance has on risky health behaviors and investment in prevention. This kind of moral hazard receives less attention than ex post moral hazard, the increased quantity of care demanded due to lower out-of-pocket cost (Zwiefel and Manning, 2000).[1] However, determining the effect of insurance on risky behavior is important because these distortions can cause increased illness and medical spending. Additionally, ex ante moral hazard causes 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 studies that do examine 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).[2] A major obstacle to observing ex ante moral hazard is that many health shocks occur only after years or decades of poor health

[1] There is a large literature on moral hazard in other types of insurance markets as well. 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. For instance, health insurance covers maintenance and prevention (such as prescription contraception), while car insurance does not cover maintenance such as oil changes. Additionally, 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.

[2] Dave and Kaestner (2009) examine exercise, smoking, alcohol consumption. De Preux (2011) examines exercise, smoking, alcohol consumption. Barbaresco et al. (2015) examine exercise, smoking, alcohol consumption, BMI/obesity, pregnancy. Simon et al. (2017) examine exercise, smoking, alcohol consumption, BMI/obesity.

behaviors, for example 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 ex ante moral hazard in risky sex decisions and the ensuing health consequences. The risky sex behavior I consider is sex between a man and a woman without a condom.[3] There are two advantages of focusing on risky sex. First, many health consequences of risky sex such as unplanned pregnancy and sexually transmitted infection (STI) occur quickly, so changes to insurance status should affect risky sex. Few risky health behaviors cause adverse health shocks as quickly as unprotected sex. One exception is drug-use can result in an overdose and mortality almost immediately, and a recent working paper suggests drug-use is another risky behavior that is responsive to ex ante moral hazard (Doleac and Mukherjee, 2018).

The second benefit of focusing on risky sex is that a recent policy change in the Patient Protection and Affordable Care Act (ACA) provides a unique opportunity to isolate an ex ante moral hazard effect. 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 ex ante moral hazard effect, because it affects only a single aspect of health insurance: the cost of prescription contraception. This policy was implemented in all states simultaneously, so its effect cannot be determined by comparing treated and untreated states. To

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

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).

I use state-year level data and dose-response event studies to determine the effect of this policy on several outcomes related to risky sex, such as condom sales, STI incidence, and fertility. The measure of treatment intensity I use is the pre-policy insured rate among 25-to-29-year-olds in each state. 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.[4] Using non-parametric event study analysis, I identify any change in relative trends that occur in the treatment year by interacting the treatment intensity in each state with year dummies. I then impose a linear parameterization on the event studies. In this analysis, I model the pre-policy trend and intercept in the event studies as well as the change in trend and one-time jump that occur in the year of policy implementation. The benefits of the parametrization are increased statistical power and clearer estimates, but it requires the assumption that trends are approximately linear.

Economic theory provides clear predictions of the ex ante moral hazard effects of 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 when it comes to preventing pregnancy, but condoms also prevent the transmission of STIs. Based on the dual role of condoms, substitution to prescription contraception should lead to increased incidence of STIs. While much of the ex ante moral hazard literature considers the

[4] See Courtemanche et al. (2017) and Finkelstein (2007) for two examples using similar sources of exogenous variation in other contexts.

substitution from prevention to treatment, I am examining a different pathway between health insurance and risky behaviors: substitution from broad behavior-based prevention to narrow medical-based prevention. I find that the estimated effects of the zero cost-sharing mandate are consistent with ex ante moral hazard: a reduction in condom sales and increased incidence of chlamydia.

An increase in STIs is not consequence of all health insurance expansions. As an extension, I examine the young adult dependent coverage mandate of 2010 that allowed children 25 and under to join their parents' health insurance. This mandate caused an exogenous shock to the extensive margin of health insurance, which I leverage 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 that this policy also reduced investment in prevention. However, the protective effect of health insurance more than offset the reduction in prevention; the dependent coverage mandate resulted in fewer STIs.

This study makes two main contributions to the literature. First, it empirically tests for ex ante moral hazard in 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).

The second main contribution is an evaluation of 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 recently drafted a rule limiting the zero cost-sharing mandate (Wolf, 2017), greatly expanding the ability of employers to obtain an exemption from the requirement to offer insurance that covers birth control at no out-of-pocket cost. Additionally, Congress recently considered repealing the ACA, which may have ended this mandate (Kaplan and Pear, 2017). Evidence from the implementation of this policy offers suggestive evidence 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 also subsidize condoms. Another policy concern is that while ex ante moral hazard causes increased health care utilization, spending on medical care does not reflect the full economic loss of reduced prevention. Ex ante moral hazard results in more health shocks, so people experience utility loss 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 empirical 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 ex ante moral hazard 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 of ex post moral hazard (Manning et al., 1987; Finkelstein et al., 2012). Finkelstein (2015) provides an overview of this literature. 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 of ex ante moral hazard may in part be related to the set of 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 effects on actual behaviors such as use and purchases of condoms. Understanding the effect on condom purchases helps confirm that changes in fertility and infection are due to changes in risky sexual behavior and not due to 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.[5] 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.[6]

This study uses an adjustment to private health insurance markets made by the ACA to test for ex ante moral hazard. 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

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[5] 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.
[6] For more details on these and other aspects of the ACA, see Kaiser Family Foundation (2013).

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).

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),[7] 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

[7] 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).

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 or 7.6% increase in prescription contraception use (Carlin et al., 2016).[8]

3. Data and Method

3.1: Data

The data for this study come from several sources and are at the state-year level.[9] 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.

Condom sales for each state-year come from Nielsen Retail Scanner data, which contains sales information provided to Nielsen by retailers.[10] 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

[8] 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).

[9] 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.

[10] This is in contrast to Nielsen Consumer Panel Dataset (known as HomeScan), where consumers report purchases to Nielsen. Calculated (or Derived) based on data from The Nielsen Company (US), LLC and marketing databases provided by the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are those of the researchers and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

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 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 which stores are included 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 sales of condoms 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. Information on STIs including chlamydia and gonorrhea are available from the National Center for HIV/AIDS, Viral Hepatitis, STD, and TB Preventions AtlasPlus (Centers for Disease Control and Prevention, 2017c). I focus on chlamydia and gonorrhea because these STIs are primarily found in heterosexuals who may respond to the cost of prescription contraception, while 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 an earlier mandate in the ACA allows dependents to use their parents' insurance 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. Event study analysis and 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;[11] income per capita data from the Bureau of Economic Analysis; a binary measure of strict abortion regulation based on information from the Guttmacher Institute;[12] 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

[11] "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.
[12] 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.

the effect of emergency contraception, I exclude years before the introduction of over-the-counter emergency contraception.

3.2: 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. 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.

The rationale for the treatment measure is that the potential effect of the zero cost-sharing mandate varies with the percent of the population that is insured. The effect of the zero cost-sharing mandate on behavior is stronger in states with high rates of insurance, because the mandate only applies to people who are insured. 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.

To determine the effect of the zero cost-sharing mandate, I perform non-parametric and parametric event study analyses. The strengths of non-parametric 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 a linear parametric assumption I can derive causal estimates with meaningful interpretations, perform statistical inference, and gain statistical power.

Additionally, by clearly stating the parametric method's assumptions, I create transparency about when the method is appropriate.

The zero cost-sharing mandate could cause an immediate effect as well as a time-varying effect, both of which are important to capture. The time-varying effect could be due to more people learning about the policy over time. Another potential reason for a time-varying effect is the compounding effect of STI infection: when one case is transmitted, that infection now has the potential to be spread to future partners. See Figure 1 for a stylized event study with a visual representation of the parametric analysis. To capture both the immediate and time-varying effects in the parametric event study analysis, I fit a line for the pre-period represented by the solid blue line before 2012 in Figure 1. I then estimate both a one-time jump/drop that occurs in the year of policy implantation as well as any change in slope.[13]

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

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

By including state and year 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

[13] 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.

coefficients show the pattern in the outcome between states with high and low uninsured rates. Since equation (1) is a log-linear regression and $InsureRate_s$ is a rate between 0 and 1, for a percentage point increase in the treatment intensity (insured rate) there is 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.

In addition to revealing any change that occurs in the treatment year, the non-parametric event studies provide information on the identifying assumptions of the parametric event studies. The two identifying assumptions are: (1) pre- and post-trends are approximately linear, and (2) there are no contemporaneous shocks. The non-parametric event study graphs show that the β_t's progress linearly. The second assumption that there are no contemporaneous changes related to states' pre-policy insurance levels 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.

The research design for statistical inference is a parametric event study analysis estimating both a one-time immediate effect and a linear time-varying treatment effect using a continuous measure of treatment intensity based on the pre-policy insured rate. To identify the effect of zero cost-sharing, I estimate models of the form:

$$\log(Y_{st}) = \beta_0 + \beta_1(InsureRate_s * (t - 2012))$$
$$+ \beta_2\big(InsureRate_s * \mathbf{1}(t \geq 2012)\big) + \beta_3\big(InsureRate_s * (t - 2012) * \mathbf{1}(t \geq 2012)\big)$$
$$+\alpha_s * \mathbf{1}(State_s) + \delta_t * \mathbf{1}(Year_t) + \gamma_x * \boldsymbol{X}_{st} + \epsilon_{st} \text{ .} \qquad \text{(Eq. 2)}$$

β_2 and β_3 are the parameters of interest and represent the estimated effect of the zero cost-sharing mandate on the intercept and the slope, respectively. By including state and year fixed effects, I control for cross-sectional, non-time-varying differences in outcomes; any remaining variation in outcomes is attributed to policy variation.

A pre-period trend is modeled by β_0 and β_1, while β_2 and β_3 represent the deviation from that trend in the post period. 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 one-time change in the outcome of β_2 percent and an annual increase of β_3 percent. The model includes the same vector of time-varying state-specific covariates as in Eq. 1, $\boldsymbol{X}_{st}$. The estimated β_2 and β_3 apply to the years in the analysis timeframe but may not persist indefinitely, especially as other aspects of the ACA went into effect.

There are important similarities and distinctions between my parametric event study approach and the traditional difference-in-differences method. Both use different levels of exposure to a treatment (either binary or in my case dose-response) to compare changes before and after policy implementation. In fact, the difference-in-differences method is a special case of my parametric event study analysis with flat pre-trends and no time-varying treatment effects. However, in addition to estimating a one-time change, my approach controls for a linear pre-treatment trend and estimates a linear time-varying treatment effect relative to this trend. This allows for a relaxed identification assumption: instead of requiring groups with different levels of treatment intensity to have the same trend in the absence of the policy, they need only continue on their relative pre-treatment trends. In a stylized event study found in Figure 1, this can be visualized as assuming the solid line on the left before policy implementation would remain 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.

As with difference-in-differences, the main identifying assumption cannot be directly verified in the parametric event study analysis used in this study. Since the counterfactual is unobservable, I must rely on an ocular test: the pre-trends should be approximately linear.[14] Linearity is important because higher order functions will have different slopes across the domain, and in those cases, the parametric event study method I am using could find spurious effects. This is similar to the concern with non-parallel pre-trends in the traditional difference-in-differences. Additionally, I must assume there are no contemporaneous shocks that are correlated with the pre-policy insured rate and the outcomes. If both assumptions are met, then any observed changes in intercept or slope in the event studies are due to the policy.

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

[14] Though it might be tempting to extend this parametric approach to higher degrees, an ocular test could be difficult to implement with quadratic or higher order functions, especially in terms of detecting causal effects. In such a context, the method used in Wolfers (2006) might be more appropriate.

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. Treatment measures are in the first panel. 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. Figure 2 shows the geographic distribution of the treatment intensities. Geographic variation in insured rates is dispersed. As expected, there are fewer insured people in the South, which has lower average education and income, while there are more insured people in the Northeast and upper Midwest. Summary statistics for outcome variables are shown in the second panel of Table 1. Chlamydia is a very common STI. It is primarily found in women and is often contracted through heterosexual intercourse (Centers for Disease Control and Prevention, 2016), so should be responsive to policies that affect birth control use. Condom sales and births are more common than the diseases examined here.

4.2: Effects on Prevention Investment (Condom Sales)

Figure 3 shows the non-parametric event study analysis for condom sales. Since condom data are not available by age group, I start the analysis for condoms in 2010 to prevent contamination from the 2010 dependent coverage mandate. This leaves only two years of pre-period, so I take advantage of the fact that Nielsen data allow for analysis at the quarter-level. Since the analysis starts after the dependent coverage mandate of 2010, and since the policy and the outcome cover all ages, I use the average insured rate for 20- to 29-year-olds as the measure of treatment intensity. However, the results are robust to using yearly data as well as using the insured rate for people in their late 20s as the measure of treatment intensity (see Section 6). Note that a large fraction of people in this age range are condom users. 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 causing a noticeable effect on total condom sales.

The figure provides evidence for ex ante moral hazard causing a reduction in prevention due to the zero cost-sharing mandate. Comparing before and after the third quarter of 2012 in Figure 3, the trend for log condoms is fairly flat (if noisy) before the policy and drops starting in 2012; the effect of zero cost-sharing is a relative decrease in the purchases of condoms in states that were more treated or had higher insured rates. As I show in Section 6, the results for annual data or using the insured rate for 25- to 29-year-olds are robust. While the quarterly results are noisier, I can more accurately separate the pre- and post-periods caused by the policy change in the second half of 2012.

Table 2 presents estimates for the zero cost-sharing mandate on condom sales. The pre-period is 2010 to the second quarter of 2012, and the post-period is the third quarter of 2012 through 2014. The effect of the zero cost-sharing mandate was a 0.13 percent annual decrease in condom sales after 2012 for each percentage point increase in the 2008-09 insured rate. Since the effects are mainly changes in slopes, the impact compounded over time; the zero cost-sharing mandate causes an approximate 0.13 percent decrease in condom sales the first year after policy implementation for a percentage point increase in treatment intensity, and a 0.26 percent decrease in the second year. The point estimate on the change in intercept is smaller and not statistically significant. A standard deviation increase in treatment intensity would result in about 7.5 million fewer condoms sold in the third year after implementation of the zero cost-sharing mandate. The fact that condom sales decreased in response to health insurance coverage is evidence of ex ante moral hazard and is consistent with the theoretical predictions.

4.3: Effects on Births and STIs

Non-parametric event studies for births and STIs are shown in Figure 4 to Figure 6, and results from the parametric event study analysis are presented in Table 3. Importantly for statistical inference, the figures are consistent with assumption that pre- and post-trends are approximately linear. Figure 4 shows the non-parametric event study analysis for births. There is a sharp decrease in births starting in 2012, which likely reflects increased use of prescription contraception and a decrease in unintended pregnancy. From the parametric event study results in Table 3, the zero cost-sharing mandate did have a statistically significant effect on births. Since the intensity of treatment is a proportion ranging from 0 to 1 and the outcome is a log, the estimate should be interpreted as a 1/10 of percent annual decrease in births for each percentage point increase in the insured rate. If the pre-policy insured rate among 25- to 29-year-olds was one standard deviation higher, I predict there would have been 25,000 fewer births the third year after the zero cost-sharing mandate went into effect.

Figure 5 and Figure 6 show the non-parametric event studies for chlamydia and gonorrhea, respectively. Again, the figures are consistent with the assumption that pre- and post-trends are approximately linear. The effect of zero cost-sharing on STIs is an important indication of ex ante moral hazard. The non-parametric event study for chlamydia in Figure 5 reverses a monotonic downward trend, with the event study reaching the bottom of a valley the year before policy implementation. This is consistent with my hypotheses and an ex ante moral hazard effect; treatable STI incidence increased more in higher treated states beginning in 2012. The effect of the zero cost-sharing mandate on gonorrhea shown in Figure 6 is less conclusive. However, the pre-trend is fairly flat, and the figure does show a meaningful jump after policy implementation.

The results from a parametric event study analysis of STIs are also in Table 3. The zero cost-sharing mandate caused an increase in chlamydia. A percentage point increase in treatment intensity for 25- to 29-year-olds caused a one-time 0.530% increase and a yearly increase (change in slope) of 0.248% in chlamydia incidence. This implies a standard deviation increase in the insured rate for 25- to 29-year-olds would result in 20,000 more cases of chlamydia three years after the zero cost-sharing mandate. The fact that the zero cost-sharing mandate increased chlamydia reflects the decrease in prevention as measured by condom sales and the effect of ex ante moral hazard. The estimated effect of the zero cost-sharing mandate on gonorrhea was a large but not statistically significant one-time jump and very little change in slope.

In summary, both the non-parametric and parametric event studies show evidence that the zero cost-sharing mandate caused ex ante moral hazard; investment in prevention as measured by condom sales decreased due to this policy. Since this policy does not offer any countervailing protection against STIs, ex ante moral hazard resulted in increased cases of chlamydia. However, increased access to prescription contraception reduced total births, likely due to fewer unintended pregnancies.

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). Before implementation of the dependent coverage mandate, 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 A1 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.[15]

[15] 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).

While changes at the extensive margin of health insurance should also cause a reduction in prevention, the net impacts on STIs and pregnancy are more ambiguous due to countervailing effects of insurance. One source of ambiguity from the dependent coverage mandate is due to the increased probability that a potential sexual partner has insurance, permitting quick and effective treatment of STIs. If a sexual partner is STI-free, then reductions in prevention in the form of sex without a condom will not result in infection transmission. In the notation of the economic model, if $STI\,prevalence$ decreases and $f'(STI\,prevalence) > 0$, then Pr(STI) decreases. Additionally, insurance lowers the cost of having a baby, so the dependent coverage mandate may result in reduction in condom-use and an increase in intended pregnancies among people who would like but could not afford the medical expenses associated with a pregnancy. 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.

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 20- to 24-year-olds who are uninsured in 2008-09. The intuition is that the more uninsured young adults in a state, the larger the potential increase in the insured rate from this policy. Again, the data are collapsed to the state-year level for 20- to 24-year-olds. While 25-year-olds are eligible to use their parents' insurance due to this policy, 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 using the following model:

$$\Delta\, InsureRate_s = \beta_0 + \beta_{change} UninsureRate0809_s + \epsilon_{st}$$

Estimates from this model 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.[16] This is comparable to the 6.7 percentage point effect in Sommers et al. (2013). There are several reasons why not all uninsured young adults gain coverage from the dependent coverage mandate, such as uninsured parents or unwillingness for parents to add a child to their plan. As a falsification test, I conduct the same analysis for older groups who should not be affected by the dependent coverage mandate. Results for older groups are also in Appendix Table A1 and 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 non-parametric and parametric event study analyses as for the zero cost-sharing mandate. However, there are two changes worth noting: 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. The omitted year in the non-parametric event studies is 2009.

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

Effects of the dependent coverage mandate on condom sales are in Appendix Figure A2 and Table 4. Comparing 2006-2009 and 2010-2012 in Appendix Figure A2, condom sales trend upward but flatten out starting in 2010. This indicates that states with high uninsured levels purchased more condoms over time compared to low uninsured states before 2010. In 2010 when high uninsured states experienced larger increases in insurance rates, the relative gains in condom sales stopped. Consistent with the non-parametric event study analysis, the dependent coverage mandate caused a statistically significant negative change in the slope, as shown in Table 4. The effect of the dependent coverage mandate of 2010 was a 0.109 percent annual decrease in condom sales after 2010 for each percentage point increase in the 2008-09 uninsured rate. A standard deviation increase in treatment intensity would result in about 5 million fewer condoms sold in the third year after implementation of the dependent coverage mandate. Like the zero cost-sharing mandate, the dependent coverage mandate caused an ex ante moral hazard effect by reducing investment in prevention.

Estimates for the effect on births and STIs are presented in Table 5 and Appendix Figure A3-A5. The non-parametric event study for births in Appendix Figure A3 and the point estimates in Table 5 indicate the dependent coverage mandate had no net effect on fertility. Since the theory provides ambiguous predictions of the effect of the dependent coverage mandate on STIs, the non-parametric event studies give an impression of which effect is stronger, the protective effect of health care or the reduction in prevention caused by ex ante moral hazard. The estimates for the effect of the dependent coverage mandate indicate that the protective effect of insurance more than makes-up for the reduction in prevention. Appendix Figure A4 and Appendix Figure A5 show the non-parametric event studies for the effect on chlamydia and gonorrhea, respectively. The graph for the effect of the dependent coverage mandate on chlamydia cases in Appendix Figure A4 has an increasing trend before the policy goes into effect, peaks the year of policy implementation, and the trend reverses in the post period. High-uninsured states had increasing incidence of this STI compared to low uninsured states in the pre-period, but this trend reversed in the post-period. As in the graph for the effect on chlamydia, Appendix Figure A5 shows an upward trend in gonorrhea that peaks the year before the policy goes into effect and decreases in the post period.[17]

The results from the parametric event study analysis in Table 5 confirm the results from the non-parametric event study. The dependent coverage mandate caused a downward change in slope for log chlamydia; a percentage point increase in the treatment intensity caused a yearly decrease of 0.256% in chlamydia incidence. An increase of one standard deviation in the uninsured rate for 20- to 24-year-olds would result in almost 38,000 fewer chlamydia cases in the third year after the implementation of the dependent coverage mandate. Likewise, for the effect on gonorrhea, the parametric event study analysis shows a downward change in slope after policy implementation; a percentage point increase in the treatment intensity caused a yearly decrease of 0.410% in gonorrhea cases. A standard deviation increase in treatment intensity would prevent 14,000 gonorrhea cases in the third year after the dependent coverage mandate among 20- to 24-year-olds.

The dependent coverage mandate caused ex ante moral hazard and countervailing effects. This policy resulted in lower investment in prevention as measured by condom sales. However,

[17] 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.

because the dependent coverage mandate caused a shock to the extensive margin of health insurance, other aspects of insurance protected against STI infection. The net effect on STIs was a reduction in illness. While both the zero cost-sharing and dependent coverage mandate caused ex ante moral hazard, an increase in STIs is not endemic to all insurance expansions.

6. Robustness and Falsification Tests

One possible threat to identification is that health insurance changes how often people interact with healthcare providers, including the frequency of STI testing. Such a response would change the number of STI diagnoses even if risky behavior remained the same. Appendix Figure A6 and Appendix Figure A7 show non-parametric 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 results for STIs. In fact the direction of the change in this outcome is the opposite as the direction for STIs. One potential factor leading to less frequent contact with doctors after the zero cost-sharing mandate is that women gain access to free long-acting contraception from this policy that can last multiple years (e.g. IUDs), and so they may skip 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 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 A8 shows that both groups generally trend together through the whole period.

I also conduct robustness checks with unweighted regressions, Poisson 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. 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 largely robust to excluding states that expanded Medicaid coverage to childless adults early.

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 3 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 both not significant and similar magnitudes. However, the estimate for fertility is smaller and no longer significant for the older group.[18]

I present robustness tests for the effect of the zero cost-sharing mandate on condom sales in Appendix Figure A9 and Appendix Table A7 using the insured rate for 25- to 29-year-olds (Panel A) and using annual data (Panel B). In Appendix Figure A9, the patterns in the pre-period are quite flat, while there is a clear downward trend in the post-period for both graphs. The estimated effects from the parametric analysis in Appendix Table A7 show small changes in intercept and meaningful downward changes in slope, which is consistent with the main effects presented in Table 2.

Falsification tests of whether the mandates impact other state-level characteristics are reported in Appendix Tables A8-A9. The only coefficient that is statistically significant is the change in slope for the 2012 zero cost-sharing policy on percent of the state that is women. However, the coefficient is quite small, and we would expect to incorrectly reject the null hypothesis at least once due to type I error. Importantly, HIV and syphilis are not statistically significant, because these two diseases are concentrated in men who have sex 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. Another important falsification test is that the non-parametric event studies for the 25- to 29-year-olds and 30- to 34-year-olds[19] trend smoothly through 2010. This indicates that the

[18] 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 increase in risky sex based on STIs, fertility of the older group is less responsive to this change.

[19] Event studies for 30- to 34-year-olds are available upon request and are similar to event studies for 25- to 29-year-olds.

dependent coverage mandate, which should only affect people under 25, did not affect the older group.

7. Conclusion

This study contributes to the literature by testing for ex ante moral hazard 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 that a standard deviation increase in treatment intensity results in several million fewer condoms purchased three years after insurance expansions, both at the intensive and extensive margins of insurance. When I isolate the effect of ex ante moral hazard using the zero cost-sharing mandate, a standard deviation increase in pre-policy insured rate results in 25,000 more cases of chlamydia three years after the policy went into effect. However, the protective effect of insurance on STI transmission more than makes up for any negative effect of ex ante moral hazard; the dependent coverage mandate caused meaningful reductions 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 shed light on 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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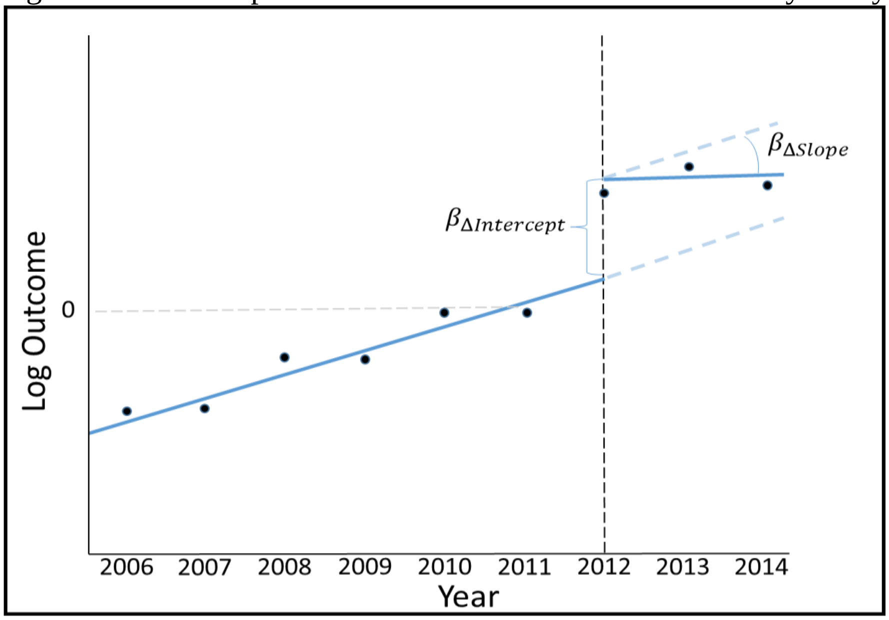
Figures

Figure 1: Visual Representation of the Parametric Event Study Analysis



Note: In this stylized event study, the continuous trends assumption can be visualized by 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 2: Geographic Representation of Insured Rates, 25-29 Year Olds, 2011-12

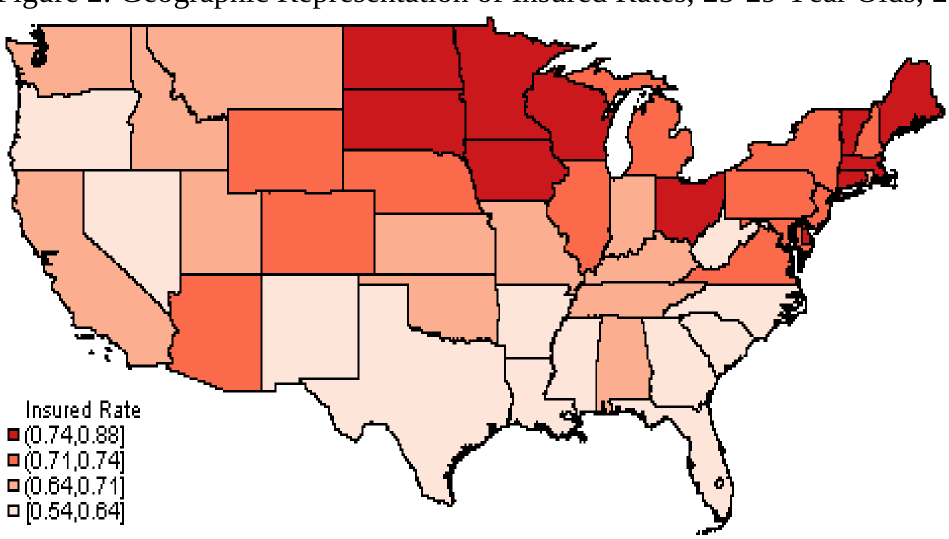
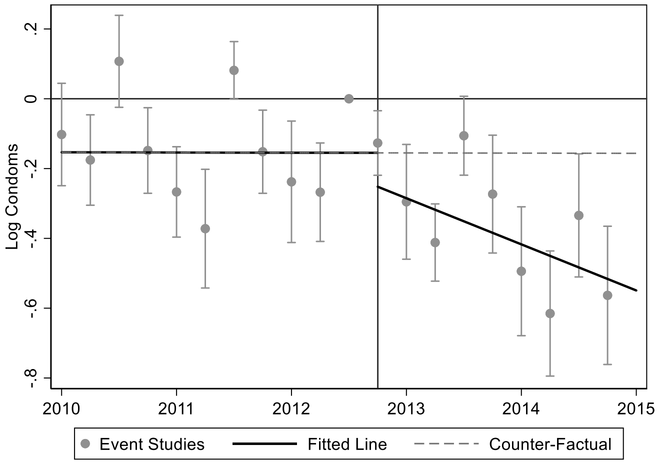
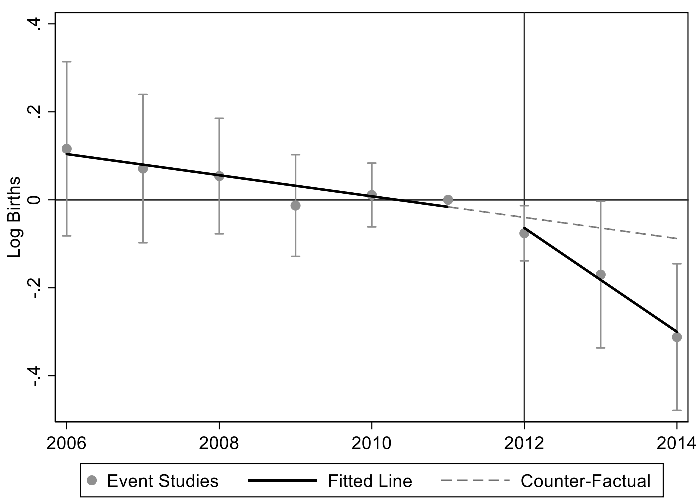


Figure 3: Zero Cost-Sharing Mandate –
Log Condoms



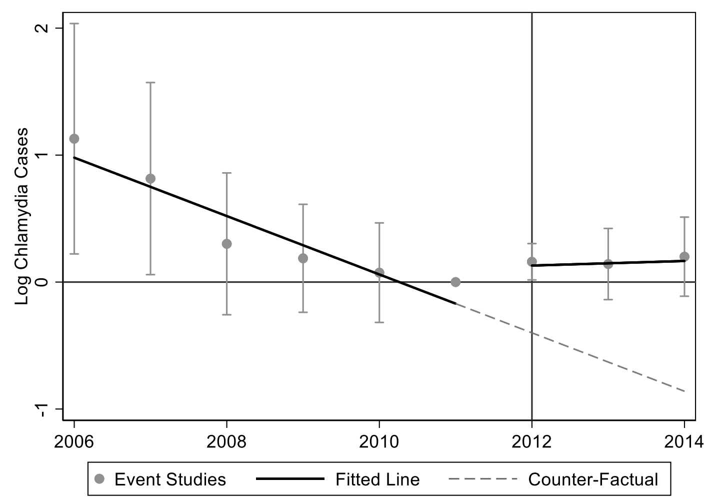
Note: Whiskers 95% confidence intervals
Treatment intensity: 2011-12 insured rate, 20-29 year olds
Cluster at state-level, weighted by 2011 state-age population
Controls: unemployment rate (total and age-specific), population (total and age-specific), income per capita, strict state regulation of abortion,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

Figure 4: Zero Cost-Sharing Mandate –
Log Births



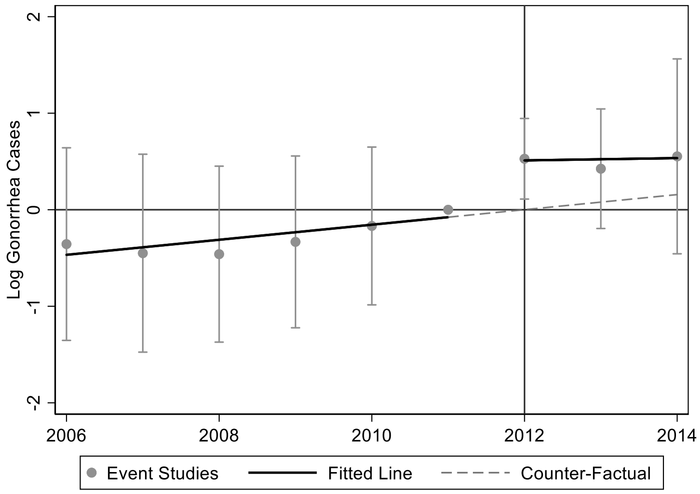
Note: Whiskers 95% confidence intervals
Treatment intensity: 2011-12 insured rate, 25-29 year olds
Cluster at state-level, weighted by 2011 state-age population
Controls: unemployment rate (total and age-specific), population (total and age-specific), income per capita, strict state regulation of abortion,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

Figure 5: Zero Cost-Sharing Mandate –
Log Chlamydia Cases



Note: Whiskers 95% confidence intervals
Treatment intensity: 2011-12 insured rate, 25-29 year olds
Cluster 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

Figure 6: Zero Cost-Sharing Mandate –
Log Gonorrhea Cases



Note: Whiskers 95% confidence intervals
Treatment intensity: 2011-12 insured rate, 25-29 year olds
Cluster 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

Tables

Table 1: Summary Statistics

Variable	20-24 Year Olds	25-29 Year Olds	30-34 Year Olds
	Treatment Intensity[20]		
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)

[20] 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 Log Condom Sales

	Log Condom Sales
Pre-Period Slope	-0. 001 (0.033)
Change in Intercept	-0.097 (0.067)
Change in Slope	-0.132** (0.062)
Predicted effect of SD increase in treatment in third year of policy	- 7,489,627

*p-value<0.10, **p-value<0.05, ***p-value<0.01
Standard errors in parentheses (SE), cluster 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
Predicted effect = Treatment intensity SD
* (Avg. yearly occurrences)
* ($\beta_{Intercept\Delta}$ + (3 * $\beta_{slope\Delta}$))

Table 3: Effect of Zero Cost-Sharing Mandate (25-29 Year Olds)

	Log Birth Cases	Log Chlamydia Cases	Log Gonorrhea Cases
Pre-Period Slope	-0.024 (0.020)	-0.230*** (0.098)	0.078 (0.101)
Change in Intercept	-0.024 (0.043)	0.530*** (0.171)	0.510 (0.345)
Change in Slope	-0.094** (0.037)	0.248** (0.122)	-0.066 (0.206)
Predicted effect of SD increase in treatment in third year of policy	- 25,550	18,652	1,297

*p-value<0.10, **p-value<0.05, ***p-value<0.01
Standard errors in parentheses (SE), cluster 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
Predicted effect = Treatment intensity SD * (Avg. yearly occurrences) * ($\beta_{Intercept\Delta}$ + (3 * $\beta_{slope\Delta}$))

Table 4: Effect of Dependent Coverage Mandate on Log Condom Sales

	Log Condom Sales
Pre-Period Slope	0. 131 (0.044)
Change in Intercept	0.031 (0.053)
Change in Slope	-0.109*** (0.037)
Predicted effect of SD increase in treatment in third year of policy	-5,225,326

*p-value<0.10, **p-value<0.05, ***p-value<0.01
Standard errors in parentheses (SE), cluster 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
Predicted effect = Treatment intensity SD
* (Avg. yearly occurrences)
* ($\beta_{Intercept\Delta}$ + (3 * $\beta_{slope\Delta}$))

Table 5: Effect of Dependent Coverage Mandate (20-24 Year Olds)

	Log Birth Cases	Log Chlamydia Cases	Log Gonorrhea Cases
Pre-Period Slope	0.036 (0.024)	0.178 (0.112)	0.221* (0.127)
Change in Intercept	-0.031 (0.038)	-0.224 (0.215)	-0.324 (0.221)
Change in Slope	0.015 (0.021)	-0.256* (0.136)	-0.410** (0.202)
Predicted effect of SD increase in treatment in third year of policy	1,194	- 38,174	-13,990

*p-value<0.10, **p-value<0.05, ***p-value<0.01
Standard errors in parentheses (SE), cluster 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
Predicted effect = Treatment intensity SD * (Avg. yearly occurrences) * ($\beta_{Intercept\Delta}$ + (3 * $\beta_{slope\Delta}$))