THREE ESSAYS IN HEALTH ECONOMICS

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ABSTRACT

This dissertation consists of three essays in health economics. The first chapter estimates changes in sexually transmitted disease rates for young adults in the United States following the Affordable Care Act's dependent coverage mandate; a provision that allows dependents to remain covered under their parents' health insurance plans until the age of 26. This study is the first to analyze changes in reported chlamydia and gonorrhea rates resulting from the dependent coverage mandate. Utilizing a difference-in-differences framework coupled with administrative data from the Centers for Disease Control and Prevention, I find that reported chlamydia rates increased for males and females ages 20-24 relative to comparison groups of males and females ages 15-19 and 25-29 following the mandate. I also find evidence of an increase in gonorrhea rates for females in this age group. I find no evidence that the mandate induced *ex ante* moral hazard.

The second chapter estimates the relationship between state-level factors and the passage of electronic cigarette regulation. E-cigarettes are controversial products. They may help addicted smokers to consume nicotine in a less harmful manner or to quit tobacco cigarettes entirely, but these products may also entice youth into smoking. This controversy complicates e-cigarette regulation as any regulation may lead to health improvements for some populations and health declines for other populations. Using data from 2007 to 2016, we examine factors that are plausibly linked with U.S. state e-cigarette regulations. We find that less conservative states are more likely to regulate e-cigarettes and that states with stronger tobacco lobbies are less likely to regulate e-

cigarettes. This information can help policymakers as they determine how best to promote public health through regulation.

The third chapter estimates the effect of changes in the number of family planning clinics on county-level fertility rates. Results suggest that increasing the number of clinics in a county decreases the fertility rate by .3 percent. These results are likely biased downward due to the inclusion of multiple types of clinics (i.e., fertility and contraceptive).

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1.1 Introduction

The Patient Protection and Affordable Care Act's (ACA) dependent coverage mandate of 2010 requires that insurers offering dependent coverage allow dependents to remain covered on their parents' employer-sponsored health insurance plans until the age of 26. The mandate was particularly relevant as young adults were the least likely to be covered by health insurance, with 28.6 percent of adults ages 18-24 uninsured in 2008 (Monheit *et al.*, 2011). Early evidence suggests that the dependent coverage mandate was effective in addressing this concern, prompting an increase in dependent coverage by over two million among the target population (Akosa Antwi *et al.*, 2013).

In this study, I evaluate the effect of the dependent coverage mandate on the two most commonly reported infectious diseases in the United States – chlamydia and gonorrhea (Centers for Disease Control and Prevention, 2011). In 2014 young adults ages 20-24 had the highest rates of chlamydia and gonorrhea of any age group, with 39 and 33 percent of all cases reported, respectively (Centers for Disease Control and Prevention, 2015a). If left untreated, these sexually transmitted diseases (STDs) can lead to serious long-term reproductive health consequences, including infertility. The Centers for Disease Control and Prevention (CDC) estimates that more than 20,000 women become infertile annually due to untreated STDs (CDC, 2016b; 2016c). Furthermore, STDs lead to an estimated $16 billion in direct costs (i.e. the product of the number of newly diagnosed STD cases and the estimated lifetime cost, discounted to present values) each year in the United States (Owusu-Edusei *et al.*, 2013), making analysis of policies affecting reported STD rates important to both the fields of public health and health economics.

The effect of any insurance expansion, including the dependent coverage mandate, on STD rates is unclear. Various mechanisms may lead to an increase in STD rates (e.g., *ex-ante* moral hazard or pent up demand) or a decrease (e.g., a perceived increase in parental access to sensitive healthcare records), therefore, the impact of the mandate on STD rates is *ex-ante* ambiguous, making empirical analysis necessary. Given the breadth of the current literature on the effects of the mandate, there is surprisingly little work that analyzes communicable diseases. In addition to being the first to estimate changes in reported STD rates resulting from the mandate, this study also contributes to the literature on *ex ante* moral hazard with an auxiliary analysis of changes in risky sexual behavior.

Exploiting changes in the age cutoff for dependent coverage eligibility with a difference-in-differences approach, I estimate the effect of insurance expansion on reported STD rates for young adults ages 20-24. The findings suggest that the mandate led to an increase in reported chlamydia rates by 13.78 percent for males ages 20-24, and an increase in chlamydia and gonorrhea rates by 15.01 and 10.31 percent respectively for females ages 20-24. Additionally, I find no evidence of *ex-ante* moral hazard in terms of changes in risky sexual behavior (i.e., having sex without a condom).

1.2 Related Literature, Background on STDs, and Conceptual Framework

1.2.1 Background on the Dependent Coverage Mandate

The ACA's dependent coverage mandate requires group and individual health insurers that provide dependent coverage to extend this coverage until the dependent's 26th birthday, and applies to any existing health insurance plans offering dependent coverage effective on or after September 23rd, 2010. Plans that took effect before September 23rd, 2010 were considered grandfathered, meaning that dependents ineligible for dependent coverage prior to the mandate must wait (up to six months) for the next plan renewal date, and may only gain coverage if ineligible for their own employer-based health benefits until 2014. The dependent coverage mandate prohibits insurers from denying dependent coverage based on student or marital status, employment, parental co-residency, and financial dependency. Before the mandate, the federal age cut-off for dependent coverage eligibility for the Veteran's Administration, and federal programs including the Children's Health Insurance Program and Medicaid was 19 (Levine et al, 2011; Antwi et al, 2013).

Though 25 states had enacted some form of dependent coverage extension prior to the ACA (Levine *et al.*, 2011), the eligibility requirements for extended coverage varied by state. Unlike the ACA's dependent coverage mandate, nearly all states required individuals to be unmarried to qualify as dependents, and some required dependents to be current students. In states with pre-ACA provisions, the average age cut-off for dependent coverage eligibility was 25, with an overall range of 24-30. Research suggests that these state-level provisions led to a small increase in coverage for young adults, offset by decreases in public insurance coverage (Levine et al., 2011) and own employer-sponsored insurance coverage (Monheit et al., 2011). Additionally, self-funded insurers are exempt from state law due to a provision under the Employee Retirement Income Security Act (ERISA) such that in 2009 56.1 percent of workers were exempt (Amuedo-Dorantes and Yaya, 2013), up from 50 percent in 1998 (Jensen and Morrisey, 1999). This distinction in requirements suggests that insurance expansion at the federal level is likely to have a larger impact on the target population than expansion at the state level, making empirical analysis necessary. Furthermore, this distinction coupled with a lack of individual-level data prevents me from utilizing a difference-in-difference-in-differences (DDD) design commonly found in the literature (Antwi *et al.*, 2013).

1.2.2 Sexually Transmitted Diseases

Sexually transmitted diseases are most often transmitted through sexual contact. The CDC provides administrative data on reported chlamydia, gonorrhea, and syphilis rates, but I excluded syphilis from this analysis because of differences in epidemiology (more specifically, syphilis has distinctly different symptoms and is much less prevalent than chlamydia and gonorrhea) (CDC, 2016d). Chlamydia tends to lack symptoms in infected men and women in up to 90 and 95 percent of cases, respectively (Planned Parenthood, 2014a). Similarly, men and women infected with gonorrhea lack symptoms in roughly 10 and 80 percent of cases, respectively (Planned Parenthood, 2014b). Symptoms of both diseases include discharge and a painful burning sensation while urinating (CDC, 2016b; 2016c). If left untreated, both diseases can have long-term and adverse health effects including infertility and an increased risk of contracting HIV (CDC, 2016b; 2016c), as well as ectopic pregnancy, which further increases the existing risk of infertility (CDC, 2016b; 2016c).

Antibiotics cure chlamydia and gonorrhea, though research suggests that gonorrhea is growing increasingly resistant to treatments using only a single type of antibiotic, including azithromycin (CDC, 2016a) and ceftriaxone (Unemo and Shafer, 2011). Gonorrhea is therefore treated with a combination-dose of ceftriaxone and azithromycin (CDC, 2016e), and chlamydia is treated with a seven-day course of antibiotics (CDC, 2016f). Having sex without a condom increases the risk of contracting STDs (CDC, 2016c), therefore condom use is an important factor in preventing infection. Another important factor in prevention is screening. Under the ACA, private insurance plans are required to cover, without cost-sharing, all preventive services deemed grade A or B by the United States Preventive Services Task Force (USPSTF) (Loosier *et al.*, 2014). Based on the USPSTF's recommendations, covered services include chlamydia and gonorrhea screening for sexually active women under the age of 25, and older women who may be at increased risk. However, the USPSTF concludes that there is insufficient evidence to justify recommending screening for sexually active men (USPSTF, 2014). Because treatment of these diseases does not protect against future infection (CDC, 2016b; 2016c), untreated sexual partners put treated individuals at risk of reinfection.

1.2.3 Economic Evidence on the Impact of Health Insurance Expansion

A multitude of studies have focused specifically on the impact of the dependent coverage mandate on insurance coverage for the young adult population, finding that young adult uninsurance rates fell after the mandate's implementation (Antwi *et al.*, 2013; Cantor *et al.*, 2012a), often with greater effects seen among men (Barbaresco *et al.*, 2015; Sommers *et al.*, 2013). Fewer studies, however, have considered the effect of the mandate on preventive care, and sexual and reproductive health.

Using a pre-post design in a clinical setting, Lau et al (2014) estimate the effect of the dependent coverage mandate on the use of preventive services, finding that young adults were more likely to receive routine examinations including blood pressure and cholesterol screening, and annual dental visits, but were not, however, more likely to receive influenza vaccinations. Using data from the National Survey of Family Growth (NSFG), an epidemiological study by Arora and Desai (2016) also uses a pre-post design to estimate the effect of the ACA's 2012 expansion of covered women's preventive services, finding no evidence of changes in the use of prescription birth control, birth control counseling, sterilization counseling, STD counseling/testing/treatment, or HIV screening for women ages 15-44. However, because the NSFG's 2011-2013 wave was completed by September 2013, respondents reported contraceptive use for at most only the first year following the August 2012 expansion. Furthermore, the pre-post design does not account for national trends, potentially biasing the estimates.

Simon *et al.* (2016) find that the ACA's 2014 Medicaid expansion increased the probability of having insurance coverage by nine percent, and HIV screening by five percent for the target population. In addition to providing evidence on the relationship between changes in insurance coverage and STD screening, their findings also provide evidence of disparities in reproductive healthcare utilization across sexes, with the largest effects on HIV screening seen among men. Finally, Trudeau and Conway (2017) estimate both the individual and joint effects of the ACA's dependent coverage mandate and expansion of covered preventive services for women (i.e. the contraceptive mandate) on various outcomes. Their findings suggest a negative relationship between the contraceptive mandate and reported pregnancy.

1.2.4 *Ex-ante* Moral Hazard and Health Insurance Expansions

To date, there are only two papers that explore *ex-ante* moral hazard (i.e., riskier behavior associated with insurance coverage reducing financial vulnerability to health shocks) around the dependent coverage mandate, and their results are mixed. Barbaresco *et al.* (2015) find that the probability of risky drinking (as defined by monthly excessive or binge drinking), but not smoking or unwed pregnancy, increased after the mandate. Conversely, Breslau *et al.* (2017) find no evidence of *ex-ante* moral hazard in the context of risky substance use. In terms of reproductive health, moral hazard may not occur because the costs of illness associated with risky behaviors are not purely financial (Ehrlich and Becker, 1972). Furthermore, moral hazard may only impact behaviors associated with more deliberate decision-making (Breslau *et al.,* 2017). However, ambiguity surrounding the mechanisms through which the dependent coverage mandate influences STD rates motivates auxiliary analyses (Section 5. B.), in which I test for evidence of *ex-ante* moral hazard by estimating changes in condom use (a proxy for risky sexual behavior).

1.2.5 Insurance Coverage and Demand for STD Screening

I follow a standard economic model of the demand for health (Grossman, 1972), in which consumers maximize a utility function of the following general form: $U = U(H_t, Z_t)$, where H_t is the stock of health at time t, and Z_t is the consumption of other goods. The endowment of health stock is received at birth and depreciates with age, but can be increased through health investment. The demand for health investment is derived indirectly from the consumer's demand for health stock. Insurance reduces the cost of healthcare investments by reducing the point-of-service cost of covered healthcare services, leading to an increase in the quantity of these services demanded.

Despite the clear prediction generated by demand-theory, the impact of a large health insurance expansion remains *ex-ante* ambiguous as there are various other offsetting factors that may link insurance coverage with STD rates. For example, following the mandate young adults may fear that their parents have access to sensitive medical records through the detailing of services rendered in insurance providers' explanation of benefits (Guttmacher, 2012), decreasing STD screening and rates. Conversely, rates may increase due to pent up demand from foregone healthcare utilization while uninsured, or moral hazard in terms of an increase in risky behavior (i.e., *ex-ante* moral hazard). Finally, there may be no change to rates following insurance uptake as STD treatment can be obtained without visiting a physician in states that permit the clinical practice of expedited partner therapy (which allows physicians to prescribe treatment for sexual partners of infected patients without a diagnosis) (CDC, 2017a), and because screening is not necessarily covered on all insurance plans (Owusu-Edusei and Gift, 2010; Loosier *et al.*, 2014). Because of this ambiguity, theory cannot clearly predict the results, making empirical analysis necessary.

1.3. Data, Variables, and Methods

1.3.1 Data

The primary data source is the CDC's Wide-ranging Online Data for Epidemiologic Research Database, which provides administrative data on the annual number of STD cases by state and population category (i.e., age, sex, and race/ethnicity). The CDC receives information on STD cases from a variety of public and private sources including health clinics, private physicians, hospitals, correctional facilities, and the military. State and local health departments report the number of STD cases to the CDC's National Center for HIV/AIDS, Viral Hepatitis, STD, and TB Prevention. Because the ACA's 2014 expansion in Medicaid eligibility requirements may confound estimated effects of the dependent coverage mandate, I conducted these analyses using data from 2007-2013. Additionally, I ran a robustness check with 2014 included in the sample, and a robustness check controlling for states that expanded their Medicaid programs before the ACA (i.e., Delaware, DC, Massachusetts, New York, and Vermont) by dropping them from the analyses. The results from both robustness checks are statistically indistinguishable across specifications, and available upon request. After excluding data for other age groups and those of unknown sex and race, the resulting dataset contains 9180 observations.

1.3.2 Outcome Variables

The data is aggregated by age group, race/ethnicity, sex, state, and year. Chlamydia and gonorrhea rates are expressed as the state's annual number of cases per 100,000 individuals in each population category. Population data from the United States Census was combined with demographic information from the American Community Survey (ACS) 3-year estimates (Ruggles *et al.*, 2015) to produce the denominator of individuals in each population category. The ACS is a large, nationally representative survey conducted annually by the United States Census Bureau through mailings, telephone interviews, and in-person interviews, collecting information on social, economic, and demographic characteristics.

To capture factors that may contribute to changes in STD rates, I augmented the CDC data to include state-level demographic measures from the ACS. Control variables included indicators for minority status (Native American or Alaska Native, Asian or Pacific Islander, African American, and Hispanic) to account for differences in insurance uptake across race and ethnicity (Sommers *et al.*, 2013), the proportion of the population category that is a current student, and the proportion that is married. Student and marital status were often used to determine state-level dependent coverage eligibility prior to the mandate (Levine *et al.*, 2011), potentially making these variables important predictors of access to reproductive healthcare services. Average family income (adjusted for inflation using the 2013 Consumer Price Index-Urban Consumers) accounts for access to financial resources. I also controlled for the economic climate using age-specific state-level annual average unemployment rates from the ACS 3-year estimates. Unemployment is negatively associated with the probability that men have health insurance (Cawley *et al.*, 2015), therefore unemployment may indirectly affect STD rates by limiting access to STD services. Standard to the literature, I included an indicator variable to control for state-level dependent coverage mandates enacted prior to the ACA (Cantor *et al.*, 2012b). By the end of the study period a total of 31 states had enacted some form of dependent coverage expansion; more specifically, ten states implemented such policies by 2007, 11 in 2008, 6 in 2009, 3 in 2010, and 1 state in 2011.

1.3.3 Methods

I utilized a difference-in-differences (DD) framework to estimate the average effect of the mandate on young adults ages 20-24 (i.e., those individuals who are eligible for dependent coverage under their parents' health insurance plans under the mandate) relative to a comparison group of adults ages 15-19 and 25-29. Because of the CDC's age categories, adults ages 19 and 25 were included in the comparison group, though they are eligible for extended parental health insurance coverage under the mandate and may therefore experience similar effects to those in the treatment group. Because this potentially biases the estimates downward, I also conducted a sensitivity check using adults ages 30-34 as the comparison group in Section 5. B.

The impact of the mandate on STD rates was estimated using the following DD regression model (1):

$$(1)\ Y_{grst} = \beta_0 + \beta_1 Treat_g + \beta_2 (Treat_g * Post_t) + \beta_3{}' X_{grst} + \gamma_t + \delta_s + \varepsilon_{grst}$$

Y_{grst} represents the reported STD rate for age group *g*, of race/ethnicity *r*, in state *s*, and year *t*, $Treat_g$ is a dummy variable denoting the treatment group (ages 20-24), and X_{grst} is a vector of other, time-varying controls that may affect STD rates. The interaction of $Treat_g$ and $Post_t$ captures the effect of the mandate on reported STD rates for the treatment group (ages 20-24) relative to the comparison group (ages 15-19 and 25-29). State and year fixed effects (δ_s and γ_t respectively) account for time invariant state-level policies and characteristics, as well as trends occurring nationally over time. All models were estimated via least squares methods, with standard errors clustered at the state level. I also ran models using weighted least squares methods, which produced similar results (available upon request). Additionally, I conducted analyses across sex to account for differences in reproductive healthcare utilization (Kalmuss and Tatum, 2007) and insurance uptake under the mandate (Barbaresco *et al.*, 2015; O'Hara and Brault, 2013; Sommers *et al*., 2013). The year 2010 was dropped from the sample to account for any delays in insurance uptake, and because annual data may not accurately match rates for 2010 due to the mandate's staggered implementation.

1.4 Results

1.4.1 Summary Statistics

Summary statistics by sex for the treatment and comparison groups before the mandate's implementation are presented in Table 1.1. Prior to the mandate, the average chlamydia rate for males ages 20-24 was 1591.6 cases per 100,000, while the equivalent rate for females in this age group was 3169.5 cases per 100,000. The pre-mandate average gonorrhea rate for males in the treatment group was 591.7 cases per 100,000, while the equivalent rate for females was 628.0 cases per 100,000. These estimates are comparable to those provided by the CDC for this period. The average chlamydia and gonorrhea rates reported by the CDC for the period of 2007-2009 for males ages 20-24 were 979.83 and 431.70 cases per 100,000, respectively. The equivalent average rates for females of this age group and period were 3013.83 and 589.48 cases of chlamydia and gonorrhea per 100,000, respectively (CDC, 2017b).

Table 1.1 Summary Statistics by Age Group and Sex – Pre-Mandate: 2007-2009

Sample:	Males		Difference from t-test	Females		Difference from t-test
Age group:	*20-24*	*15-19 & 25-29*	*p*-value	*20-24*	*15-19 & 25-29*	*p*-value
Chlamydia rate per 100,000	1591.6	928.7	0.000	3169.5	2291.8	0.000
Gonorrhea rate per 100,000	591.7	363.0	0.000	628.0	464.4	0.000
Age-specific unemployment rate (%)	10.92	13.66	0.000	10.92	13.66	0.000
Student	0.359	0.501	0.000	0.410	0.527	0.000
Non-student	0.641	0.499	0.000	0.590	0.473	0.000
Married	0.116	0.166	0.000	0.189	0.218	0.005
Non-married	0.884	0.834	0.000	0.811	0.782	0.005
State level dependent coverage mandates	0.379	0.379	1	0.379	0.379	1
Family income (1,000s)	58.29	67.19	0.000	52.12	65.25	0.000
Observations	765	1530	--	765	1530	--

Notes: All variables are at the state/year/race/sex/group level and unweighted. Sample is young adults ages 15-29 in the United States. Family income has been adjusted for inflation using the 2013 Consumer Price Index Urban Consumers.

1.4.2 Validity of Study Design

A necessary assumption for the DD model to recover causal effects is that in the absence of the mandate, the outcomes for the treatment and comparison groups would have moved in parallel in the post-treatment period (i.e., the parallel trends assumption). In the pre-mandate period, chlamydia and gonorrhea rates trended similarly for the treatment and comparison groups for both sexes, which is suggestive evidence that the parallel trends assumption has been satisfied. Additionally, I provide further suggestive evidence by estimating differences in trends across groups in the pre-treatment period. More specifically, I estimated the following regression model for the pre-mandate period:

$$(2)\ Y_{grst} = \alpha_0 + \alpha_1 Treat_g + \alpha_2\left(Treat_g * LTT_t\right) + \alpha_3{}' X_{grst} + \gamma_t + \delta_s + \mu_{grst}$$

As in model (1), Y_{grst} represents the reported STD rate, X_{grst} is a vector of control variables, and δ_s and γ_t are the state and year fixed effects, respectively. In model (2), the treatment group indicator ($Treat_g$) is interacted with a linear time trend (LTT_t), such that the resulting coefficient of interest (α_2) provides an estimate of the difference in trends between treatment and comparison groups for the pre-mandate period. The coefficients (Table 1.2), are not statistically significant, therefore I fail to reject the hypothesis that the treatment and comparison groups moved in parallel in the pre-mandate period, which again supports the parallel trends assumption.

Table 1.2 Tests for Parallel Trends – Results by Sex: 2007-2009

Sample:	Males		Females	
Outcome:	*Chlamydia*	*Gonorrhea*	*Chlamydia*	*Gonorrhea*
Pre-mandate proportion:	1591.6	591.7	3169.5	628.0
Treatment group-time trend	34.334	-14.052	45.495	9.313
	(28.079)	(9.788)	(29.501)	(11.567)
Observations	2219	2219	2221	2221

Notes: Observations are at the state-year-sex-race-group level for the United States. All models are estimated with OLS and control for treatment group indicator, age-specific unemployment rate, state demographics, and state and year fixed effects. Standard errors are clustered around the state and reported in parentheses. Significance levels denoted by * 0.10 ** 0.05 *** 0.01. Family income has been adjusted for inflation using the 2013 Consumer Price Index Urban Consumers. The treatment group is adults ages 20-24, and the comparison group is adults ages 15-19 and 25-29.

1.4.3 DD Estimates

The DD regression results (Table 1.3) suggest that chlamydia rates for males ages 20-24 increased by 219.24 cases per 100,000, relative to males ages 15-19 and 25-29 following the mandate, representing a 13.78 percent increase over the pre-mandate mean (1591.6 cases per 100,000). The estimates also suggest that chlamydia and gonorrhea rates for females in the treatment group increased by 475.62 and 64.76 cases per 100,000, respectively, relative to females in the comparison group, representing a 15.01 percent increase in the chlamydia rate (from 3169.5 cases per 100,000), and a 10.31 percent increase in the gonorrhea rate (from 628.0 cases per 100,000).

Table 1.3 Effect of the Dependent Coverage Mandate on STD Rates in the United States, DD Results by Sex: 2007-2013

Sample:	Males		Females	
Outcome:	*Chlamydia*	*Gonorrhea*	*Chlamydia*	*Gonorrhea*
Pre-mandate proportion:	1591.590	591.657	3169.530	628.024
DD estimate	219.243*** (39.265)	23.023 (17.366)	475.622*** (54.392)	64.755*** (12.863)
Observations	4447	4447	4437	4437

Notes: Observations are at the state-year-sex-race-group level. All models are estimated with OLS and control for treatment group indicator, age-specific unemployment rate, state demographics, and state and year fixed effects. Standard errors are clustered around the state and reported in parentheses. Significance levels denoted by * 0.10 ** 0.05 *** 0.01. Family income has been adjusted for inflation using the 2013 Consumer Price Index Urban Consumers. The treatment group is adults ages 20-24, and the comparison group is adults ages 15-19 and 25-29. The year 2010 is excluded.

1.4.4 Policy Endogeneity and Dynamics

To test for policy endogeneity, I modified model (1) to include a set of policy leads and lags (i.e., indicator variables for each year interacted with the treatment group indicator variable, with 2009 excluded as the omitted category), as is standard in the DD literature (Autor, 2003). In the context of the dependent coverage mandate, policy endogeneity could occur if policy makers sought to address increasing STD rates among the target population by extending access to health insurance. In terms of regression output, a positive, statistically significant coefficient on a policy lead (i.e., 2007 and 2008) would indicate the presence of policy endogeneity by suggesting that events occurring before the mandate's implementation influenced the estimated increase in STD rates following the mandate.

The coefficients on the policy leads (reported in Table 1.4) in both male regressions and the female gonorrhea regression are not statistically different from zero.

Because the sign of the coefficient on the lead of 2007 in the female chlamydia regressions suggests that female chlamydia rates were negatively associated with policy implementation, I fail to reject the hypothesis of no policy endogeneity. However, the statistically significant coefficients on the policy lags (i.e., interactions between the treatment group and year indicators for 2011 through 2013) suggest that the effect of the mandate was not a discrete change, but instead varied over time. In the context of the dependent coverage mandate, pent-up demand (which occurs when individuals forgo utilizing healthcare while uninsured) is evidenced by an initial surge in STD rates following the mandate, reflecting changes in the quantity of tests demanded following gains in insurance coverage. For females, the largest effects are seen in the first year after the mandate (i.e., 2011), which is a pattern suggestive of pent up demand.

Table 1. 4 Tests for Policy Endogeneity – Event Study Results by Sex: 2007-2013

Sample:	Males		Females	
Outcome:	*Chlamydia*	*Gonorrhea*	*Chlamydia*	*Gonorrhea*
Pre-mandate proportion:	1591.590	591.657	3169.530	628.024
2007-treatment group interaction	-69.696	27.553	-96.129*	-19.385
	(54.955)	(19.407)	(55.651)	(22.776)
2008-treatment group interaction	-21.226	44.094	-14.991	-0.810
	(61.261)	(29.597)	(69.754)	(24.805)
2011-treatment group interaction	176.880**	53.176**	482.903***	89.071***
	(82.174)	(23.817)	(77.668)	(23.404)
2012-treatment group interaction	226.747***	61.177***	434.730***	40.178
	(54.924)	(20.030)	(85.194)	(24.575)
2013-treatment group interaction	162.903***	26.480	399.579***	45.478*
	(46.082)	(22.792)	(83.691)	(23.722)
F-test on joint significance of policy leads: *p*-value	0.335	0.217	0.201	0.647
Observations	4447	4447	4437	4437

Notes: Observations are at the state-year-sex-race-group level for the United States. All models are estimated with OLS and control for treatment group indicator, age-specific unemployment rate, state demographics, and state and year fixed effects. Standard errors are clustered around the state and reported in parentheses. Significance levels denoted by * 0.10 ** 0.05 *** 0.01. The treatment group is adults ages 20-24, and the comparison group is adults ages 15-19 and 25-29. The year 2010 is again excluded, and the 2009 indicator is omitted as the reference year.

1.5 Robustness Checks and Extensions

1.5.1 Alternative Comparison Group Specification

As a robustness check, I re-estimated model (1) using adults ages 30-34 as the comparison group, an age group ineligible for dependent coverage under the mandate and commonly used in the current literature estimating the effect of the mandate on risky behaviors (Saloner *et al.*, 2017). The resulting DD estimates capture the effect of the mandate on males and females ages 20-24 relative to those ages 30-34. The results (Table 1.5) suggest that the effect of the mandate on chlamydia rates is not appreciably different across specifications. The results for the gonorrhea regressions, however, change in significance for both sexes, and the sign of the DD estimate changes in the male regression. The estimated effect is slightly larger in the alternative specification, which may reflect the hypothesized, downward effect of including eligible adults (i.e. those ages 19 and 25) in the comparison group. Overall, the early twenties and early thirties age groups may not be as comparable in terms of sexual behaviors and the use of reproductive healthcare services; men ages 15-24 are more than twice as likely to receive STD/HIV services than men ages 34-44 (Chabot *et al.*, 2011). Furthermore, specifications using the 15-19, 25-29, and 30-34 age groups individually as comparison groups did not satisfy the parallel trends assumption, precluding their use as comparison groups in the primary analyses (results available upon request).

Table 1.5 Effect of the Dependent Coverage Mandate on STD Rates in the United States, DD Results by Sex, using an Alternative Comparison Group Specification: 2007-2013

Sample:	Males		Females	
Outcome:	*Chlamydia*	*Gonorrhea*	*Chlamydia*	*Gonorrhea*
Pre-mandate proportion:	1591.590	591.657	3169.530	628.024
DD estimate	251.033*** (55.730)	-44.559*** (16.484)	525.195*** (84.864)	15.299 (19.940)
Observations	2985	2985	2963	2963

Notes: Observations are at the state-year-sex-race-group level. All models are estimated with OLS and control for treatment group indicator, age-specific unemployment rate, state demographics, and state and year fixed effects. Standard errors are clustered around the state and reported in parentheses. Significance levels denoted by * 0.10 ** 0.05 *** 0.01. The treatment group is adults ages 20-24, and the comparison group is adults ages 30-34. The year 2010 is excluded.

1.5.2 Testing for *Ex-Ante* Moral Hazard

To explore the potential mechanisms driving the estimated changes in STD rates, I tested for evidence of *ex-ante* moral hazard by estimating changes in condom use among young adults following the dependent coverage mandate. Condom use acts as a proxy for risky sexual behavior because unprotected sex (i.e., sex without a condom) greatly increases the risk of transmitting STDs (CDC, 2015b). In the context of *ex-ante* moral hazard, insurance expansion may promote risky sexual behavior by providing coverage for STD treatment, thereby mitigating the financial cost of infection. I estimated the effect of the mandate on the probability that young adults had unprotected sex using data from the National Center for Health Statistics' National Survey of Family Growth (NSFG). The NSFG gathers information on topics including reproductive healthcare utilization from a nationally representative sample of men and women ages 15-44. I used data from 2007-2013, with 2010 again omitted from the analysis. Additionally, the first eight months of 2011 are missing due to a gap in data collection (NSFG, 2015). Because sex without a condom between a married couple does not necessarily constitute risky sexual behavior, I removed individuals with the legal status of married from the sample. After also removing respondents under the age of 15 and over the age of 29, the resulting male dataset contains 6627 observations, and the resulting female dataset contains 6969 observations.

The outcome variable in these analyses is the probability that a respondent did not use a condom at their last sexual intercourse. The NSFG asks respondents what type of contraceptive method was used during their last sexual intercourse, with response options varying by sex and including condoms, birth control pills, and emergency contraception (NSFG, 2015). Respondents that had not used a condom at their last encounter were coded as having had unprotected sex. Control variables included indicators for student status, employment status, race/ethnicity, and whether the individual had multiple sex partners in the last twelve months. Because the NSFG does not make state identifiers available for public use, I augmented model (1) to include year fixed effects only, estimating models via weighted least squares methods with heteroscedasticity-robust standard errors.

In both specifications, the coefficients of interest (Table 1.6) are statistically indistinguishable from zero. These results suggest that relative to a comparison group of males and females ages 15-17 and 26-29, adults ages 18-25 are not engaging in riskier sexual behavior following the mandate. These age ranges were chosen to be more precise than those provided by the CDC, while still maintaining comparability in treatment and comparison group specifications across analyses. To confirm that these estimates do not reflect conditional on positive bias (a form of selection bias which may occur if the mandate affects the population who has sex, and the resulting population has a different propensity to use condoms), I used the same set of covariates and methods as in the mechanism analyses to estimate the effect of the mandate on changes in the probability that young adults engaged in sexual intercourse in the past four weeks. The results (Table 1.7) are statistically indistinguishable from zero, suggesting that the mandate did not influence changes in condom use by affecting the composition of the population engaging in sexual activity.

Table 1.6 Effect of the Dependent Coverage Mandate on Risky Sexual Behavior in the United States, DD Results by Sex: 2006-2013

Sample:	Unmarried Males	Unmarried Females
Outcome:	*Probability of Unprotected Sex*	*Probability of Unprotected Sex*
Pre-mandate proportion:	0.368	0.558
ACA	-0.069	0.074
	(0.046)	(0.046)
Observations	4067	4539

Notes: Observations are at the individual/year level. All models are estimated with OLS and control for year fixed effects. Standard errors are clustered around the state and reported in parentheses. Significance levels denoted by * 0.10 ** 0.05 *** 0.01. The treatment group is adults ages 18-25, the comparison group is adults ages 15-17 and 26-29. All analyses are weighted with final weights for the corresponding four-year datasets.

Table 1.7 Effect of the Dependent Coverage Mandate on Sexual Frequency in the United States, DD Results by Sex: 2006-2013

Sample:	Unmarried Males	Unmarried Females
Outcome:	*Probability of Sex in last 4 Weeks*	*Probability of Sex in last 4 Weeks*
Pre-mandate proportion:	0.632	0.690
DD estimate	0.051	0.027
	(0.042)	(0.040)
Observations	4528	4990

Notes: Observations are at the individual-year level. All models are estimated with weighted least squares methods, control for treatment group indicator, state demographics, year fixed effects, and robust standard errors (reported in parentheses). Significance levels denoted by * 0.10 ** 0.05 *** 0.01. Sample means are unweighted. The treatment group is adults ages 18-25, and the comparison group is adults ages 15-17 and 26-29. All analyses are weighted with final weights for the corresponding four-year datasets.

There are several limitations to the preceding tests for *ex-ante* moral hazard. First, the lack of state identifiers precluded my ability to control for any state-specific policies or trends that may affect condom use. Second, the sample size may have been too small to precisely identify changes in behavior for the proportion of the young adult population that gained dependent coverage under the mandate. Per Cooper and Pesko (2017), I estimated a back-of-the-envelope calculation of power by comparing the width of the estimate’s 95 percent confidence intervals to the pre-mandate, treatment-group mean, with results suggesting that the mechanism analyses likely lack sufficient power (details available upon request). Third, due to the design of these questions, the resulting data accounts for sexual intercourse (i.e., penile penetration) between opposite-sex partners only. Lastly, due to the sensitive nature of these questions, survey data may not accurately reflect behavior, however I do not suspect that reporting error changed around the mandate, or that such error may be heterogeneous across treatment and comparison groups. Given these limitations, results from the mechanism analyses should be interpreted with caution.

1.6 Discussion

I used a difference-in-differences framework to estimate changes in sexually transmitted disease (STD) rates following the Affordable Care Act's dependent coverage mandate, which allows young adults to remain covered under employer-sponsored parental health insurance plans offering dependent coverage for up to an additional seven years. I analyzed changes in chlamydia and gonorrhea rates for young adults ages 20-24, which is an age group with the highest incidences of these diseases (CDC, 2011), and that has historically had high uninsurance rates (Monheit *et al.*, 2011). Additionally, I contribute to the literature on moral hazard by estimating changes in risky sexual behavior following the mandate.

Empirical results suggest that after the mandate, reported chlamydia rates for males and females ages 20-24 increased by 13.78 and 15.01 percent, respectively. Similarly, reported gonorrhea rates increased by 10.31 percent for females in this age group. Given that the mandate increased non-spousal dependent coverage by more than 25 percent among the young adult population (Cantor *et al.*, 2012a), the empirical results likely reflect improved access to STD screening through out-of-pocket cost reduction. Additionally, in the context of insurance expansion and STD testing, these results are supported by other findings in the literature, which suggest that the ACA's 2014 Medicaid expansion also increased the probability of having insurance and receiving HIV screening by somewhat equivalent proportions (Simon *et al.*, 2016). It is also worth noting that (except in cases where expedited partner therapy is used) when an infected patient notifies their partner, that partner must also seek treatment from a physician regardless of insurance status, which suggests that the effects of the mandate may reach farther than the treated population. Furthermore, unweighted estimates from the NSFG suggest that over the study period, 36 percent of males and 24 percent of females ages 18-25 had multiple sex partners in the past 12 months. Given that receiving treatment for an STD does not prevent reinfection (CDC, 2016b; 2016c), reported STD rates may rise by a larger proportion than the population able to gain insurance coverage under the mandate due to reinfection from uninsured to insured partners.

In auxiliary analyses, I find no evidence of changes in risky sexual behavior (i.e., the probability of having unprotected sex), which differs from others in the current literature suggesting that some risky behaviors (i.e., excessive or binge drinking) increased after the mandate (Barbaresco *et al.*, 2015). Given the previously discussed limitations of the data used in the mechanism analyses (including a small sample size and a lack of state-identifiers), the results of the tests for *ex-ante* moral hazard should be interpreted with caution. Researchers seeking to definitively assess changes in risky sexual behavior resulting from the ACA's dependent coverage mandate should use a more comprehensive dataset.

A limitation to the primary analyses of this study is the inability to distinguish the number of STD tests conducted from reported cases. In the CDC's data, the number of cases is a direct function of both the incidence of disease and the degree of testing conducted. I offer evidence suggesting that young adults' demand for reproductive healthcare services changed following the mandate, however, changes in the number of tests conducted may also be the result of *ex-post* moral hazard. *Ex-post* moral hazard results in increased demand for healthcare services, which is induced either by the patient following reductions in price afforded by insurance, or by physician recommendations incentivized by insurance's guaranteed reimbursement. Therefore, future research comparing data on STD tests conducted with reported cases would contribute to the literature on *ex-post* moral hazard in the context of reproductive health (Gruber and Owings, 1996).

CHAPTER 2. WHAT FACTORS PREDICT THE PASSAGE OF STATE LEVEL E-CIGARETTE REGULATIONS?

2.1 Introduction

In this study we provide the first analysis of the factors that lead U.S. states to regulate e-cigarettes; emerging and controversial products in tobacco markets. E-cigarettes are battery-operated, often cigarette-shaped, devices containing a liquid which typically includes nicotine along with other components such as propylene glycol and flavorings. A heating element vaporizes the liquid and the resulting vapor is inhaled. Unlike tobacco cigarettes, many e-cigarettes do not contain tobacco.[1] E-cigarettes were developed in China in 2003 and entered the U.S. in 2007 (Riker et al., 2012). Since that time, e-cigarette use has proliferated among Americans; 3.6% of adults (Schoenborn and Gindi, 2015) and 16% of high school students (Singh, 2016) use these products.

Although e-cigarette use is rapidly increasing among both adults and youth, state governments have only recently begun to impose regulations on these products. Moreover, the majority of regulations passed to date have focused on youth access (e.g., minimum purchase ages) with only a few states implementing standard tobacco control regulations (e.g., taxation and bans on use in public places). Despite the potential importance of regulating e-cigarettes and previous studies exploring factors driving tobacco cigarette, marijuana, and alcohol regulation (Bradford and Bradford, 2016, Sloan

[1] We note that many e-cigarettes contain nicotine. Tobacco is a primary source, but not the only source, of nicotine. Hence, we note that some e-cigarettes may contain trace amounts of tobacco through nicotine.

et al., 2005, Macinko and Silver, 2015), economists have not investigated which factors drive e-cigarette regulations. However, health scholars note that this information is critical to promote effective regulation (Bradford and Bradford, 2016). The goal of this paper is to address this critical gap in the literature. The paper proceeds as follows: Section 2 discusses controversy surrounding e-cigarettes and public health; data, variables, and methods are outlined in Section 3; Section 4 presents the results; and Section 5 concludes.

2.2 Controversy Surrounding E-Cigarette Use and Public Health

The public health community has reached a consensus that tobacco cigarette use, which has been irrefutably linked with cancer and is a leading cause of morbidity and mortality (U.S. Department of Health and Human Services, 2014), should be mitigated. However, there is controversy as to whether e-cigarette use should be supported or curtailed. Indeed, the extent to which expanded e-cigarette use will enhance or harm overall health is arguably one of the most fiercely debated questions within the public health community at this time (Riker et al 2012).

Two key factors propagate this contentious debate. First, the clinical literature on e-cigarette health effects is limited due to the newness of these products. In particular, there is insufficient evidence from which to draw strong conclusions on whether expanded e-cigarette use will improve or harm public health. The available evidence is generally descriptive in nature or captures short-term health effects (e.g., through randomized control trials) and cannot, without strong and likely untenable assumptions, shed light on the causal role of e-cigarette use in overall health production (Glasser et al., 2017). Second, the health effects, whatever they maybe, likely vary across consumers due to the different reasons that lead to e-cigarette use. Such potential heterogeneity across consumers implies that expanded e-cigarette use may improve health for some groups and harm health for other groups, leaving the net health effect ambiguous.

In terms of the potential health effects of e-cigarettes, because tobacco is not burned, and therefore cancer-causing toxins are not released, e-cigarettes are generally considered to be healthier than tobacco cigarettes based on current medical information (Pisinger and Døssing, 2014). E-cigarettes may therefore offer addicted smokers, who are unlikely to quit otherwise (Centers for Disease Control and Prevention, 2011), a less harmful way to consume nicotine (the addictive component of tobacco cigarettes). Such a harm reduction pathway will likely enhance overall public health.[2] This pathway is plausibly important given that, in 2016, 16% of U.S. adults smoke tobacco cigarettes and 56% of smokers unsuccessfully attempted to quit smoking in the past year.[3] However, while e-cigarettes are believed to be less harmful to health than tobacco cigarettes, e-cigarettes are not harmless to users and recent evidence suggests that e-cigarettes could be more harmful than initially believed. For example, e-cigarette ingredients may cause

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[2] Harm reduction is an important component of the Food and Drug Administration (FDA) official position on regulation of tobacco products. This agency has the authority to regulate e-cigarettes at the federal level. For example, in July 2017, FDA Commissioner Scott Gottlieb argued for the importance of harm reduction in the FDA's regulatory efforts to reduce the health consequences of tobacco addition: 'Envisioning a world where [tobacco] cigarettes would no longer create or sustain addiction, and where adults who still need or want nicotine could get it from alternative and less harmful sources, needs to be the cornerstone of our efforts – and we believe it's vital that we pursue this common ground' (https://www.fda.gov/newsevents/newsroom/pressannouncements/ucm568923.htm; accessed September 30th, 2017).

[3] Authors' analyses of the 2016 Centers for Disease Control and Prevention Behavioral Risk Factor Surveillance Survey data. More details available on request.

cancer (Yu et al., 2016) and serious problems with lung function (Reidel et al., 2017). Moreover, e-cigarette use is linked with a range of adverse health outcomes such as coughing, nausea, chest pain, and increased blood pressure[4] (Grana et al., 2014, Orellana-Barrios et al., 2015). Within the field of economics, a recent study by Viscusi (2016) documents that the e-cigarette-attributable mortality risky is only 1/100 to 1/1000 the tobacco cigarette-attributable mortality risk. These estimates suggest that, while both products harm health, e-cigarettes are substantially less harmful than tobacco cigarettes and that expanded e-cigarette use, if it follows from decreased tobacco cigarette use, will increase overall health. In addition, e-cigarettes can serve as a cessation device and may therefore help some smokers quit entirely (Brown et al., 2014, Bullen et al., 2013, Caponnetto et al., 2013), which should improve public health.

However, while e-cigarettes may serve as an effective cessation device for some groups of tobacco cigarette smokers, there is mixed evidence on the extent to which e-cigarettes help all smokers quit (Pearson et al., 2014, Harrell et al., 2014) suggesting a more limited link between expanded e-cigarette use and smoking cessation.

Smokers may also use e-cigarettes to circumvent tobacco cigarette smoking bans in public places (McKee and Capewell, 2015). Such use may harm public health by reducing the motivation to quit (as the full costs, in particular hassle costs, of smoking have declined) and by increasing the locations in which a smoker can use cigarettes (both electronic and tobacco). Thus, expanded e-cigarette use may reduce cessation and lead to increases in nicotine addiction overall. Finally, public health advocates argue that e-

[4] We note that increased blood pressure is likely concentrated among those who use e-cigarette containing nicotine.

cigarettes encourage youth, who would not otherwise use any cigarettes, to take up tobacco cigarette smoking through gateway effects (Fairchild et al., 2014).

Thus, the net effects of expanded e-cigarette use on public health overall are unclear and complicated by both limited information on e-cigarette health effects and the complex set of reasons motivating consumers to use e-cigarettes. While our study does not address these thorny issues, we attempt to address why states opt to regulate e-cigarettes.

2.3 Data and Methods

2.3.1 E-cigarette Regulations

Our outcomes are state e-cigarette regulations implemented between 2007 and 2016, and extracted from the Centers for Disease Control and Prevention (CDC) (2016). We construct an indicator variable indicating whether a state has one or more of the following regulations: e-cigarette tax, minimum purchase age, or ban on use in public places (worksites, restaurants, bars, or schools). We chose these regulations as they are the most common e-cigarette regulations. We construct indicators for each specific regulation we study: taxation, minimum purchase age, and public use ban.[5]

2.3.2 State-Level Factors

The study of factors that determine how states regulate is longstanding and encompasses a wide-range of disciplines, including, but not limited to, political science,

[5] For regulations that are passed within a year, we code the fraction of the year in which the law is in place. More details are available on request.

public health, sociology, legal studies, and economics (e.g., Bradford and Bradford (2016), Sloan et al. (2005), Snyder et al. (2004), Berry and Berry (1990), and Marlow (2008)). While it is beyond the scope of our study to comprehensively review this large body of inter-disciplinary research, we use insight developed from this literature to select factors that could impact states' e-cigarette regulation decisions. We examine the following factors: voter preferences, diffusion of regulations across state borders, special interest groups, previous regulatory experience with related products, fiscal health, and consumer tastes.

To proxy for voter preference, we use a measure of state citizen ideology developed by Berry et al. (1998). In particular, we use the revised 1960 to 2013 citizen ideology series.[6] Broadly, for each state this index reflects the ideological ranking of each member of Congress and each district. Lower scores indicate more conservative ideology within the state. We refer interested readers to Berry et al. (1998) for more details on this index. Conservative ideology is correlated with less regulation of markets in general while progressive ideology is correlated with support for health-related regulations in particular (Beland, 2015). Unfortunately, the ideology data are only available through 2013. To avoid excluding 2015 and 2016 (our right hand side variables are lagged one year), during which time numerous states implemented e-cigarette regulations (Centers for Disease Control and Prevention, 2016), we linearly impute values for these years.[7] Moreover, there is no ideology data for the District of Columbia. To address these data limitations, we have estimated an alternative set of regressions in

[6] https://rcfording.wordpress.com/state-ideology-data/; accessed October 27th, 2017.
[7] More details on imputation are available on request.

which we utilize the Governor's political party, specifically whether the Governor is a Democrat using data drawn from the University of Kentucky Center for Poverty Research (2016), to proxy for political preferences. We treat the mayor of DC as the *de facto* Governor of this locality (Maclean and Saloner, 2017). Results generated in this auxiliary specification are not appreciably different from our core results (reported later in the manuscript), although somewhat less precise (results available upon request).

Regulations have been documented to 'diffuse' from state to state (Macinko and Silver, 2015, Bradford and Bradford, 2016). In particular, a state government learns from the experiences of its geographic neighbors and adopts similar regulations. In our context, diffusion predicts that if a neighbor implements an e-cigarette regulation, then a state would be more likely to also implement this regulation. We measure the fraction of geographic neighboring states with an e-cigarette regulation. We develop separate diffusion variables for each regulation we study (e.g., a taxation diffusion variable is used in the taxation regression).[8]

We proxy for the strength of the tobacco lobby using annual financial contributions to political campaigns from the tobacco industry. We include the tobacco industry lobbying as health policy experts predict that this industry may either support the growth of the e-cigarette market (Lempert et al., 2016). Many tobacco cigarette companies are now selling e-cigarettes or this industry may oppose the growth of the e-cigarette market as tobacco companies may fear losing market share as consumers substitute from tobacco cigarettes to e-cigarettes (Lempert et al., 2016). We also include

[8] We exclude Alaska and Hawaii as they have no geographic neighbors.

financial contributions to political campaigns from the public health community. The public health community has mixed opinions as to whether increased use of e-cigarettes will help or harm health overall. We use data from FollowtheMoney.org[9] to construct financial contribution variables. As an additional measure of lobbying/special interest efforts, we include state tobacco control funding from the Centers for Disease Control and Prevention STATE system (Centers for Disease Control and Prevention, 2016).[10] Such funding reflects state government tobacco control efforts to alter tobacco product use and associated health effects, which may be positively or negatively correlated with passage of e-cigarette regulations depending on whether states seek to expand or curtail e-cigarette use.

A state's experience with regulations of related goods may impact future regulations (Bae et al., 2014). We measure the tobacco cigarette tax per pack to proxy for related goods as tobacco cigarettes have been identified as e-cigarette substitutes for some individuals (Friedman, 2015). Data on tobacco cigarette taxation is drawn from the CDC.

During periods of poor state fiscal health, public interest tends to shift towards regulations targeting economic growth and government austerity, and away from other regulations; e.g., e-cigarette regulation. On the other hand, during periods of poor fiscal health, states may favor relatively 'costless' regulations, such as the e-cigarette regulations we examine. To proxy for state fiscal health we include the annual

[9] Accessed December 14th, 2016.

[10] We note that tobacco control expenditures would be preferable to funding, but the former variable is not available for all years of our study. More details available on request.

unemployment rate (Macinko and Silver, 2015, Bradford and Bradford, 2016) using data from the U.S. Bureau of Labor Statistics.

We control for the proportion of the adult population that smokes tobacco cigarettes using data from the CDC's Behavioral Risk Factor Surveillance Survey to reflect consumer tastes for tobacco cigarette smoking and demand for e-cigarettes for harm reduction, dual use, and/or cessation purposes.

Finally, we include state demographics from the American Community Survey (Ruggles et al., 2015) to proxy for factors not captured by other controls. Specifically, we include the percentage of the population that: is male, is less than 19 years, and has less than a high school education in regression models.

We convert financial variables to 2016 dollars using the Consumer Price Index.

2.3.3 Methods

We follow Bradford and Bradford (2016) and estimate the duration regression model outlined in Equation (1):

$$(1) \qquad \boldsymbol{L_{st} = \alpha_0 + \alpha_1 X'_{st} + \gamma_s + \tau_t + \varepsilon_{st}}$$

This model is formally referred to as the instantaneous hazard of adoption with state-year data (Bradford and Bradford, 2016). $\boldsymbol{L_{st}}$ is a state e-cigarette regulation. This variable is coded 0 in all years prior to the passage of the law, 1 in the law passage year, and missing thereafter. This coding structure incorporates the fact that a state is only 'at risk' for an event (an e-cigarette law passage) prior to the event actually occurring. $\boldsymbol{X_{st}}$ is a vector of state-level factors that are allowed to vary across time, $\boldsymbol{\gamma_s}$ is a vector of state fixed effects, $\boldsymbol{\tau_t}$ is a vector of year fixed effects,[11] and $\boldsymbol{\varepsilon_{st}}$ is the error term. We estimate linear probability models (LPM), lag our state-level factors by one year, and cluster standard errors by state (Bertrand et al., 2004). We select the LPM over a probit or logit model as these alternative models are vulnerable to the incidental parameters problem when state fixed effects are included in the regression (Greene, 2004). All analyses are unweighted (Solon et al., 2015).

2.4 Results

The vast majority of states (45) have passed an e-cigarette regulation by 2016. Minimum purchase age regulations are the most common (44 states) and taxes are the least common (3 states). There is no obvious regional clustering in terms of minimum purchase ages or taxes, however, public use bans appear to be clustered to some extent in the Northeast (13 states).

Table 1 reports summary statistics overall and by year for our analysis sample (all U.S. states and localities with the exception of Alaska, the District of Columbia, and Hawaii); we report both percentages and the number of states that have passed each year.[12] 45 states had implemented any e-cigarette regulation overall; 0 in 2007, 6 in 2011, and 45 in 2016. 3 states implemented a tax over the full study period; 0 in 2007, 1 in 2011, and 3 in 2016. In terms of minimum purchase ages, 44 states had implemented

[11] We followed Bradford and Bradford (2016) and estimated a series of regressions which employed different methods to controlling for time effects (e.g., a linear time trend, polynomials in time, and year splines). We compared goodness-of-fit metrics (likelihood ratio tests and r-squared values) and determined that the year fixed effects offered the best fit. More details available on request.

[12] Summary statistics are not reported in duration format. That is we code laws as 1 in the years following the law passage rather than coding these observations as missing.

such a regulation over the full study period; 0 in 2007, 6 in 2011, and 44 in 2016. Finally, 13 states passed a ban on e-cigarette use in public places between 2007 and 2016; 0 in 2007, 4 in 2011, and 13 in 2016.

Selected regression results are reported in Table 2 (we suppress coefficient estimates on demographic variables for brevity). We have also estimated models without state fixed effects; results, which are not appreciably different (although somewhat more precise; results available upon request). Overall, and somewhat surprisingly, factors that the literature suggests may lead states to implement e-cigarette regulations are generally not statistically significant predictors of the regulations we examine. Moreover, the coefficient estimates are generally small in magnitude and imprecise.

There are two deviations from this pattern of null results. (i) Less conservative states are more likely to pass a ban on e-cigarette use in public places. (ii) A stronger tobacco lobby, as proxied by tobacco lobbying expenditures, reduces the probability that a state will regulate e-cigarettes. These findings are in line with our expectations (see Section 3.2).

2.5 Discussion

In this study we explore state-level factors that are potentially related to the passage of state-level e-cigarette regulations among U.S. states. We contribute to two complementary literatures. First, we add to the small literature that examines e-cigarette regulations. While previous studies evaluated the impact of e-cigarette regulations on use of e-cigarettes and tobacco cigarettes (Friedman, 2015, Pesko et al., 2016), we explore factors that drive regulation implementation. Second, our study contributes to the large literature that seeks to understand the factors that determine state regulations more broadly, e.g., Bradford and Bradford (2016). Our study adds information on a new and controversial topic: e-cigarettes.

By far the most common regulation during our study period was a minimum purchase age. This pattern suggests that policymakers have been most concerned with minimizing e-cigarette use among youth and, in turn, the potential health effects for this population. States have also begun to pass regulations that protect non-users and increase the hassle costs of e-cigarettes (public place bans), and increase the financial costs of e-cigarettes (taxation).

Overall, our findings do not suggest that factors emphasized by the large and inter-disciplinary literature on regulation determinants are important for the emergence of e-cigarette regulations, at least among states that implemented such regulations over the period 2007 to 2016. In particular, we find no statistically significant evidence that diffusion, the public health lobby, tobacco control efforts by the state, previous regulatory experience, fiscal health, or consumer tastes predict e-cigarette regulation passage.

However, in line with our hypothesis, we find that less conservative states are more likely to prohibit e-cigarette use in public places. We find that states characterized by stronger tobacco lobbies are less likely to regulate e-cigarettes. While our data do not allow us to fully explore this finding, a negative relationship between tobacco lobby strength and the probability of implementing e-cigarette regulations is in line with the hypothesis that tobacco companies are entering the e-cigarette market and do not wish to curtail e-cigarette use, and hence profits.

In summary, our findings are often not statistically different from zero and, in some sense, contradict predictions from theory and previous work which suggests that the factors we study should predict state e-cigarette regulation passage. Whether differences are attributable to our focus on early adopting states, fundamental differences between e-cigarettes and other goods (e.g., tobacco cigarettes), or some other factor is not clear. However, further study, once the U.S. e-cigarette market becomes more established, may be able to offer additional insights on the regulation of these controversial products.

Table 2.1 State summary statistics: 2007-2016

Sample:	All years	2007	2011	2016
E-cigarette regulations				
Any regulation [%, (N)]	28.96 (45)	0 (0)	12.50 (6)	93.75 (45)
E-cigarette tax [%, (N)]	1.67 (3)	0 (0)	2.08 (1)	6.25 (3)
Minimum purchase age [%, (N)]	27.50 (44)	0 (0)	12.50 (6)	91.67 (44)
Public place ban [%, (N)]	8.96 (13)	0 (0)	8.33 (4)	27.08 (13)
Voter preference				
State ideology index (%)	46.69	54.52	41.36	--
Regulation diffusion				
Neighboring states with any regulation (%)	28.81	0	12.50	92.22
Neighboring states with an e-cigarette tax (%)	1.57	0	1.91	6.13
Neighboring states with a minimum purchase age (%)	27.50	0	12.50	90.76
Neighboring states with ban in public place (%)	9.32	0	9.13	26.11
Special interest				
Tobacco lobbying dollars (100,000s)	4.63	--	--	--
Health lobbying dollars (100,000s)	26.95	--	--	--
State tobacco control funding dollars (millions)	13.22	--	--	--
Tobacco cigarette regulation				
Cigarette tax ($ per pack)	1.42	--	--	--
Fiscal health				
Unemployment rate (%)	6.67	--	--	--
Demographics				
Smoke (%)	19.20	--	--	--
Observations	480	48	48	48

Notes: All variables are at the state-year level and unweighted. AK, HI, and DC excluded. Data are not in duration format; that is we code laws as 1 in the years following the law passage rather than missing.

Table 2.2 State factors predicting passage of e-cigarette regulations, controlling for state and year fixed-effects: 2007-2016

Outcome:	Any regulation	E-cigarette tax	Minimum purchase age	Public place ban
Duration sample proportion (Number of states that have passed a law):	0.1166 (45)	0.0063 (3)	0.1122 (44)	0.0289 (13)
State ideology index (%)	-0.0002	-0.0007	-0.0010	0.0016*
	(0.0016)	(0.0005)	(0.0015)	(0.0008)
Regulation diffusion				
Neighboring states with any regulation (%)	-0.0018	--	--	--
	(0.0019)			
Neighboring states with an e-cigarette tax (%)	--	-0.0009	--	--
		(0.0008)		
Neighboring states with a minimum purchase age (%)	--	--	-0.0027	--
			(0.0018)	
Neighboring states with ban in public place (%)	--	--	--	-0.0001
				(0.0009)
Special interest				
Tobacco lobbying dollars (100,000)	-0.0003**	0.0000	-0.0002	-0.0000
	(0.0001)	(0.0000)	(0.0002)	(0.0001)
Health lobbying dollars (100,000)	-0.0001	0.0000	-0.0002	0.0000
	(0.0004)	(0.0000)	(0.0004)	(0.0001)
State tobacco control funding dollars (millions)	-0.0004	-0.0005	-0.0014	0.0000
	(0.0026)	(0.0005)	(0.0024)	(0.0010)
Tobacco cigarette regulation				
Cigarette tax ($ per pack)	0.0424	0.0050	0.0647	0.0170
	(0.0585)	(0.0149)	(0.0559)	(0.0367)
Fiscal health				
Unemployment rate (%)	0.0129	0.0017	0.0181	-0.0031
	(0.0221)	(0.0091)	(0.0207)	(0.0097)
Observations	386	475	392	450

Notes: All models estimated with a duration model. Dependent variables are coded as 0 in the years prior to law passage, 1 in the year of law passage, and missing thereafter; hence sample sizes vary across outcomes. Observations are at the state-year level. All models are unweighted and control for demographics, and state and year fixed effects. Standard errors are clustered around the state and reported in parentheses. *, **, *** = statistically significant at the 10%, 5%, 1% confidence level.

CHAPTER 3. FAMILY PLANNING CLINICS AND FERTILITY

3.1 Introduction

In 2014, an estimated 38 million women of reproductive age required contraceptive services and supplies; over 20 million of these women required publicly funded services, meaning that they were either living in poverty or below the age of 20 (Frost *et al.*, 2016). In 2012, fertility rates among unemployed women were higher than their employed counterparts (74 births per 1000 women compared to 64 births per 1000, respectively), and the fertility rate for women living in poverty was higher than for those in any other income bracket (at 82 births per 1000 women) (Monte & Ellis, 2014). Contraceptives provide women with the ability to time pregnancy, which has important economic implications in terms of educational attainment and earnings. While the delayed entry into parenthood as a result of increased educational attainment has been well documented (Wu and MacNeill, 2002), research suggests that there is also a reciprocal effect: becoming a parent during an educational program reduces a woman's probability of completion (Marini, 1984), especially among high school students (Upchurch & McCarthy, 1990). Childbearing among teens is of particular concern, with mixed results suggesting that teen mothers may spend up to 2.6 fewer years in school (Kane *et al.*, 2013).

Because delaying fertility allows women to increase their investment in human capital through increases in educational attainment and work experience, women who delay first births to relatively later in life tend to earn higher wages (Blackburn *et al.*, 1993). More specifically, research suggests that delaying motherhood leads to an increase in earnings of nine percent per year, as well as a three percent increase in wages and a six percent increase in hours worked (Miller, 2011). Furthermore, some research suggests that among college-educated mothers, increased wages may potentially be due to female workers seeking positions with firms that provide better support for parents and improved opportunities for career advancement (Amuedo-Dorantes & Kimmel, 2005).

As providers of contraceptive services, family planning clinics play an important role in timing fertility, however these clinics also provide a variety of other services, including pregnancy testing, screening for sexually transmitted diseases, and HPV vaccinations (Guttmacher, 2016). As such, family planning clinics are important providers in the safety-net healthcare market, offering more affordable healthcare services to low-income women. In 2014, 7.8 million women were relying on care from publicly-funded family planning clinics, with an estimated forty percent of users relying on these clinics as their only source of recent care (Frost *et al.*, 2012). For all family planning clinic users, health outcomes may improve following family planning clinic openings due to increased healthcare utilization, particularly contraceptive services (Kavanaugh and Anderson, 2013).

This study is the first to estimate the relationship between fertility rates and nationwide changes in the total number of family planning clinics, which acts as a proxy for access to reproductive healthcare. Based on estimated elasticities of demand for healthcare services (Manning *et al.*, 1987; and Ringel *et al.*, 2002), clinic openings may increase the utilization of reproductive healthcare through reductions in cost in two ways.

First, the opening of a clinic may reduce the opportunity cost of receiving care (e.g., reducing hassle costs of travel) and second, it may reduce the direct cost of receiving care (in the case that a patient receives publicly funded services). Demand theory suggests that the decrease in costs associated with increasing access to care may increase the demand for care, potentially increasing utilization of contraceptive services and reducing the number of births. To test this hypothesis, I employ a weighted least squares regression model, which includes fixed effects to control for county-specific characteristics as well as national trends across time. Results suggest that increasing the number of family planning clinics in a county reduces the birth rate by .3 percent. These results are consistent with the current literature, which suggests that decreasing access to family planning services increases the fertility rate (Fischer *et al.*, 2017).

The paper is organized as follows: Section 2 details the literature and mechanisms through which changes in clinic access may affect fertility, Section 3 discusses the data and methods used in this study, Section 4 presents the results, Section 5 discusses an extension to the analysis, and Section 6 concludes with a discussion of the results and future work.

3.2 Related Literature and Conceptual Framework

3.2.1 Literature on Access to Care, and Fertility

Several studies have considered the relationship between distance to care and utilization as well as health outcomes. More specifically, Goodman *et al.* (1997) find that hospitalization is significantly lower for patients that live farther than 30 minutes from a hospital. Similarly, Buchmueller *et al.*, (2006) find that increased distance to the nearest hospital due to urban hospital closures increases deaths from heart attacks and unintentional injuries.

While these studies have shown that access is an important factor in healthcare utilization and health outcomes, research also suggests that government policy may play an important role, particularly in the context of reproductive health. Historically, fertility rates have been on the decline due to various factors including increased female labor force participation and the introduction of contraceptives, but research has also shown that government policy has contributed to this decline as well. More specifically, federally funded family planning programs may have reduced fertility among poor women by up to 30 percent, which is driven by reductions in the number of births as well as delayed childbearing (Bailey, 2012).

More relevant to this analysis is research that has focused on the effect of changes in government policy on access to family planning services, including abortions and contraceptives. Leveraging changes in state policy that restricted funding for family planning clinics, several researchers have sought to estimate these effects, with similar results. At the state-level, research has found that restricting access to contraceptive services through restricted funding decreased the Medicaid claims for contraceptives by 35.5 percent (Stevenson *et al.*, 2016). Fischer *et al.* (2017) focus on policy changes in the state of Texas, estimating that following restrictions in funding for non-abortion family planning providers, births in counties with no publicly funded family planning clinic within 25 miles increased by 1.5 percent.

Fischer *et al.* (2017) also estimate the change in abortions following more stringent requirements on abortion providers in Texas using a difference-in-differences estimation approach. Several other studies focus specifically on changes in access to abortion clinics and abortions (a type of reproductive healthcare service). Grossman *et al*. (2017) estimate that abortions in Texas declined by 14 percent in the year following the enactment of restrictive abortion laws which reduced the number of facilities providing these services. Similarly, Lindo *et al*. (2017) use a difference-in-differences approach to estimate the effect of these closures on several outcomes, including abortions and births. Their results suggest that for women living very close to clinics, closures that increased distance by 25 miles reduced abortions by 10 percent, with smaller effects seen among women living further away. Additionally, they found that the increased congestion at remaining clinics both reduced and delayed abortions, as measured by increased gestational age at the time of procedure.

Because contraceptives are considered to be a form of preventive care (HRSA, 2017), most relevant to this study is research that assesses the relationship between access and the use of both reproductive and preventive care. Lu and Slusky (2016) find that an increase in distance to the nearest publicly funded health clinic of 100 miles reduces the annual utilization of clinical breast exams, mammograms, and PAP tests. In complementary work, Slusky (2017) finds larger effects on utilization of preventive care among Hispanic women. This study contributes to this literature in several ways. First, this study is expansive in scope, covering 45 states. Second, instead of focusing on restrictions in access (particularly for abortion clinics), the estimation is designed to show the effect of increases in access to all family planning services. Finally, this study considers clinics that are both publicly and privately funded, where the current research focuses on publicly funded facilities.

3.2.2 Access and the Demand for Family Planning Services

This study follows Grossman's (1972) model of the demand for healthcare services, in which consumers maximize a generalized utility function of the following form: $\boldsymbol{U = U(H_t, Z_t)}$, where $\boldsymbol{H_t}$ represents the stock of health at time t, and $\boldsymbol{Z_t}$ represents consumption of other goods. Individuals receive and endowment of health stock at birth, which depreciates with age but can be increased through health investment. Theoretically, increasing the number of clinics in a county increases the accessibility of services to patients, and reduces the transportation and hassle costs to patients in that county. Additionally, publicly-funded family planning clinics offer services at a reduced cost for low-income patients (Guttmacher, 2016), improving access to reproductive healthcare services by reducing the out-of-pocket cost for eligible patients. The decrease in costs should lead to an increase in family planning service utilization, increasing the use of contraceptive services and thereby reducing the fertility rate.

3.3 Data, Variables, and Methods

3.3.1 Data and Variables

The Centers for Disease Control and Prevention's (CDC) National Center for Health Statistics (NCHS), Division of Vital Statistics (DVS) provides annual birth counts by mother's county of residence for counties with a total population of 250,000 or more. I combine these birth counts with county level population estimates of the number females ages 15-49 from the National Cancer Institute's Surveillance Epidemiology and End Results (Cancer-SEER) program (Swenson, 2014; Stevens *et al.*, 2011) to produce the number of births per 1000 women ages 15-49 in a county.

Data on the annual number of family planning clinics in a county is sourced from the U.S. Census Bureau's County Business Patterns (CBP). CBP data report the number of facilities in a county annually for the period of 1998-2015. Family planning clinics (North American Industry Classification System (NAICS) code 621410) include abortion clinics, birth control clinics, childbirth preparation classes, family planning counseling services, family planning centers, fertility clinics, pregnancy counseling centers, and reproductive health services centers.[13] This data includes both publicly and privately funded facilities.

State-level demographic and economic controls are sourced from the University of Kentucky's Center for Poverty Research. I control for whether the governor is a democrat, treating the mayor of the District of Columbia as the de-facto governor. The county unemployment rate is sourced from the Bureau of Labor Statistics' (BLS) Local

[13] https://siccode.com/en/naicscodes/621410/family-planning-centers

Area Unemployment Statistics. Other county-level controls include the poverty rate and the median household income, which is adjusted for inflation using the 2015 consumer price index (CPI) and reported in thousands of dollars. These variables are sourced from the US Census Bureau's Small Area Income and Poverty Estimates (SAIPE). Additionally, I use the SEER dataset to calculate the percent of the county population that is a minority (i.e., non-White). Combining these data produces a dataset that covers 263[14] counties in 45 states for the period of 1998-2015.

3.3.2 Methods

This study identifies the effects of increasing the number of family planning facilities using within county year-to-year variation in the number of clinics. I estimate a weighted least squares regression model that relates a county's fertility rate to its number of family planning clinics using the following regression model (1):

$$(\mathbf{1}) \qquad \boldsymbol{Fertility\ Rate_{ct}} = \boldsymbol{\beta_0} + \boldsymbol{\beta_1 FPC_{c,t-1}} + \boldsymbol{\beta_2{}' X_{ct}} + \boldsymbol{\gamma_t} + \boldsymbol{\delta_c} + \boldsymbol{\varepsilon_{ct}}$$

where $\boldsymbol{Fertili}ty\ Rate_{ct}$ is the annual number of births per 1000 women ages 15-49 in a county, $FPC_{c,t-1}$ is the annual number of family planning clinics in a county in period t-1, X_{ct} is a set of state and county level demographic and economic controls, γ_t is a vector of year fixed effects, and δ_c is a vector of county fixed effects. The lagged number of clinics accounts for the time from conception to birth, implicitly assuming that previous

[14] This estimate was confirmed using 2015 American Community Survey 5-Year Estimates:
U.S. Census Bureau; American Community Survey, 2010 American Community Survey 1-Year Estimates, generated by Melissa Oney; using American FactFinder; <http://factfinder2.census.gov>; (12 September 2018).

years' clinic counts effect current fertility rates. County fixed effects account for time-invariant factors specific to each county that may affect fertility rates, and year fixed effects control for national level changes across time. Standard errors are clustered at the county level, however I also conduct these analyses with standard errors clustered at the state level to allow for spatial correlation of errors across counties that may be due to state-level factors including reproductive health policies. The results are similar, and available upon request.

3.4 Results

3.4.1 Summary Statistics

Table 3.1 reports summary statistics for the period of 1998-2015. The average county level fertility rate for this period is roughly 55 births per 1000 women ages 15-49. These estimates are comparable to those provided by the Centers for Disease Control and Prevention.[15] From 1998 to 2015, 1400 clinics opened, and 1173 clinics closed, for a net reduction of 227 clinics over the study period. The average number of clinics in a county is just under 5 during this period. The average county unemployment rate is 6.14 percent, the average poverty rate is 12.82 percent, and the average median family income is $61,850 (in 2015 dollars). At the state-year level, nearly 45 percent of counties have a democrat governor over this period.

[15] United States Department of Health and Human Services (US DHHS), Centers for Disease Control and Prevention (CDC), National Center for Health Statistics (NCHS), Division of Vital Statistics, Natality public-use data on CDC WONDER Online Database, for years 1995-2002 available November 2005, for years 2003-2006 available March 2009, and for years 2007-2016 available February 2018. Accessed: 9/12/2018.

Table 3.1 Summary statistics (1998-2015)

Sample:	(1) All Counties	(2) Counties with higher than average clinic counts	(3) Counties with lower than average clinic counts	Difference from *t*- test (*p*-value)
Outcome variable				
Fertility rate (births per 1000 women ages 15-49)	55.12	56.48	54.42	0.000
Number of family planning clinics				
Number of family planning clinics	4.905	9.550	2.675	0.000
Demographics				
Unemployment rate (%)	6.140	6.159	6.131	0.725
Median household income (thousands, 2015 dollars)	61.85	61.05	62.23	0.018
Poverty (%)	12.82	13.90	12.30	0.000
Democrat governor	0.448	0.442	0.451	0.584
Minority (% non-White)	21.77	23.81	20.79	0.000
Observations	4286	1390	2896	--

Notes: All variables are at the county/year level and unweighted.

3.4.2 Primary Results and Validity Checks

Table 3.2 presents the results of the analysis estimating the relationship between the number of family planning clinics and county-level fertility rates. These results suggest that increasing the number of family planning clinics in a county by one decreases the fertility rate by .177 births per 1000 women ages 15-49. This is equivalent to a .3 percent reduction in the fertility rate over the mean (55.123). Additionally, results suggest that a one percent increase in the unemployment rate is associated with a reduction in the fertility rate, equivalent to .443 fewer births per 1000 women ages 15-49. Finally, there is a statistically significant relationship between the poverty rate and fertility, indicating that a one percent increase in a county's poverty rate is associated with an increase in fertility of .257 births per 1000 women ages 15-49. There is no evidence to suggest that the percent of the county population that is non-White, the median household income, or living in a state with a democrat governor are significant determinants of fertility.

Table 3.2 Fertility Rates & Family Planning Clinics, Results using Population-Specific Weights (1998-2015)

	(1) County & year FEs	(2) Adding Demos
Sample mean:	55.123	55.123
Number of family planning clinics		
Clinics (t-1)	-0.183***	-0.177***
	(0.0397)	(0.0403)
Demographics		
Unemployment rate (%)	--	-0.443**
		(0.137)
Median household income	--	0.0661
(thousands, 2015 dollars)		(0.0543)
Poverty (%)	--	0.257*
		(0.130)
Democrat governor	--	-0.397
		(0.271)
Minority (% non-White)	--	-0.0311
		(0.160)
N	3813	3809

Notes: All models control for county and year fixed effects. Standard errors are clustered at the county level and reported in parentheses. Models are weighted using the county population of females ages 15-49. Significance denoted as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

3.4.3 Robustness Checks

Next, I conduct several robustness checks, including tests for endogeneity and estimation using different weighting schemes. Per Swenson (2015), I test for evidence of policy endogeneity by including in the regression analysis a set of leads for the number of family planning clinics in a county. Theoretically, the number of births in a county may induce changes in the number of clinics in a county, leading to issues of reverse causality in the primary analysis. The results (presented in Table 3.3) suggest that the previous year's (period $t\text{-}1$) clinic count significantly reduces the fertility rate in a county (period t). These results also show no evidence of endogeneity, as evidenced by the lack of significance on the coefficients estimating the relationship between fertility rates and clinic counts in both current (period t) and future ($t+1$) periods.

In additional robustness checks, I re-estimate equation (1) using different weighting schemes. Because the fertility data is administrative and covers all births in counties with population sizes greater than 250,000, endogenous sampling is not a cause for concern, however it may be necessary to correct for heteroscedasticity in the county/year error term related to population size (Solon *et al.*, 2015). In the primary analysis I use population-specific weights (number of females ages 15-49), however this is not the entire population served by family planning clinics. More specifically, many publicly funded facilities provide reproductive services to males as well (Guttmacher, 2016), such that counties with sufficiently large male populations may warrant the opening of a facility. As such, I run analyses using total county population weights with similar results, though the size of the estimates is slightly smaller (see column (2) of Table 3.4).

Table 3.3 Fertility Rates & Family Planning Clinics, Test for Endogeneity using Population-Specific Weights (1998-2015)

	(1)	(2)	(3)
Sample mean:	55.123	55.123	55.123
Number of family planning clinics			
Clinics (t-1)	-0.177***	-0.118**	-0.123***
	(0.0403)	(0.0372)	(0.0332)
Clinics	--	-0.0888	-0.0311
		(0.0577)	(0.0274)
Clinics (t+1)	--	--	-0.0874
			(0.0496)
Demographics			
Unemployment rate (%)	-0.443**	-0.444**	-0.474***
	(0.137)	(0.136)	(0.136)
Median household income	0.0661	0.0636	0.0549
(thousands, 2015 dollars)	(0.0543)	(0.0545)	(0.0550)
Poverty (%)	0.257*	0.250*	0.214
	(0.130)	(0.125)	(0.112)
Democrat governor	-0.397	-0.414	-0.369
	(0.271)	(0.273)	(0.279)
Minority (% non-White)	-0.0311	-0.0348	-0.0437
	(0.160)	(0.157)	(0.151)
N	3809	3809	3546

Notes: All models control for county and year fixed effects. Standard errors are clustered at the county level and reported in parentheses. Models are weighted using the county population of females ages 15-49. Significance denoted as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 3.4 Fertility Rates & Family Planning Clinics, Results by Weighting Specification (1998-2015)

	(1) Population-Specific Weights	(2) County Population	(3) Unweighted
Sample mean:	55.123	55.123	55.123
Number of family planning clinics			
Clinics (t-1)	-0.177***	-0.176***	-0.147**
	(0.0403)	(0.0407)	(0.0543)
Demographics			
Unemployment rate (%)	-0.443**	-0.447**	-0.282*
	(0.137)	(0.135)	(0.116)
Median household income	0.0661	0.0626	0.0831
(thousands, 2015 dollars)	(0.0543)	(0.0540)	(0.0456)
Poverty (%)	0.257*	0.243	0.197*
	(0.130)	(0.129)	(0.0901)
Democrat governor	-0.397	-0.417	-0.672***
	(0.271)	(0.274)	(0.188)
Minority (% non-White)	-0.0311	-0.0339	-0.0207
	(0.160)	(0.161)	(0.141)
N	3809	3809	3809

Notes: All models control for county and year fixed effects. Standard errors are clustered at the county level and reported in parentheses. Models are weighted as follows: (1) county population of females ages 15-49, (2) total county population, and (3) unweighted. Significance denoted as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Unweighted results are also comparable to the primary results using population-specific weights, however somewhat smaller than those found in either population-based weighting scheme (see column (3) of Table 3.4). When comparing all three weighting schemes, the standard errors on the variable of interest (the lagged number of clinics) are smallest in the primary specification, suggesting that the population-specific weighting scheme produces the most precise estimates. Overall, results from both the test for endogeneity and the various weighting schemes are similar to those found in the primary specification, which lends credibility to the results of the primary analysis.

3.5 Extension

In an extension to this study, I aggregate the data to the state level and conduct the regression analysis again. While more disaggregated analyses have the advantage of producing more precise estimates by using variation driven by changes in local economic conditions, research has shown that health analyses are sensitive to the level of aggregation (Lindo, 2015). As such, I repeat my analysis using data aggregated to the state-level, following regression model (1) but replacing the county fixed effects with state fixed effects. Aggregated analyses have the advantage of leveraging spillover effects that occur at the lower level of aggregation (i.e. county effects are included in state effects). The results (presented in Table 3.5) are similar to those found in the primary analysis, however the size of the coefficient on the lagged number of clinics is somewhat smaller. Because adding an additional family planning clinic to a state only affects the portion of the population that lives within a reasonable distance of the new clinic, access to reproductive healthcare services for many in the state would remain unchanged.

Table 3.5 Fertility Rates & Family Planning Clinics, State Level Analyses (1998-2015)

	(1) Population-Specific Weights	(2) State Population	(3) Unweighted
Sample mean:	55.263	55.263	55.263
Number of family planning clinics			
Clinics (t-1)	-0.0913***	-0.0872**	-0.122***
	(0.0232)	(0.0253)	(0.0343)
Demographics			
Unemployment rate (%)	0.298	0.276	-0.250
	(0.386)	(0.380)	(0.336)
Median household income (thousands, 2015 dollars)	-0.00356	0.135	-0.144
	(0.185)	(0.271)	(0.120)
Poverty (%)	-1.052*	-0.896	-1.269**
	(0.443)	(0.512)	(0.366)
Democrat governor	-0.494	-0.977	-0.160
	(0.522)	(0.866)	(0.724)
Minority (% non-White)	-0.842***	-1.108***	-0.543
	(0.191)	(0.284)	(0.350)
N	747	747	747

Notes: All models control for state and year fixed effects. Standard errors are clustered at the state level and reported in parentheses. Models are weighted as follows: (1) state population of females ages 15-49, (2) total state population, and (3) unweighted. Significance denoted as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

3.6 Conclusion

Family planning clinics are facilities that may provide a variety of reproductive healthcare services, including fertility treatments, contraceptive services, abortions, and testing for sexually transmitted diseases (Guttmacher, 2016). Most relevant to this study is the availability of contraceptive services provided at these clinics, which allow women to time or prevent pregnancy. The ability to delay pregnancy has been shown to increase women's investment in human capital (Blackburn *et al.*, 1993) and in turn, increase women's earnings (Miller, 2011).

Previous literature has largely focused on the state of Texas and primarily relies on variation driven by changes in government funding intended to restrict access to abortion services (Fischer *et al.*, 2017; Grossman *et al.*, 2017; Lindo *et al.*, 2017; Lu and Slusky, 2016; Slusky, 2017). Fischer *et al.* (2017) find that increasing the driving distance to the nearest clinic by 25 miles reduces the fertility rate by 1.5 percent in the state of Texas.

To my knowledge this study is the first to consider the relationship between family planning clinics and fertility rates nationwide. In this study, I estimate the effect of adding an additional family planning clinic at the county level for counties with population sizes of at least 250,000. Unlike the approach used in the current literature which typically considers changes in driving distance to the nearest clinic, the approach used in this study inherently accounts for congestion (i.e., increased patient load at remaining facilities due to closures). The results suggest that the addition of a single family planning clinic reduces the county fertility rate by .3 percent on average.

An estimated reduction in fertility rates of .3 percent is notably smaller than the estimated reduction of 1.5 percent found in the literature. The difference in the size of these estimates may be due to the differences in scope, however it may also be due to the limitations of this analysis. The first of these limitations is that the data in this study only covers counties with population sizes of 250,000 or more. As a result, this study may not be considering particularly vulnerable populations that lack access to sufficient care. The second limitation is that the clinic counts include both contraceptive and fertility clinics.

Including fertility clinics is likely to bias the estimates downwards as fertility clinics serve to increase the number of births in a county.

In robustness checks for endogeneity, I find no evidence that fertility rates are driving changes in the number of family planning clinics at the county level. Furthermore, the results are not particularly sensitive to the weighting scheme, lending credibility to the analysis. In unreported robustness checks, I run the estimation after taking the log of the fertility rate (adjusting for zero counts), producing similar results (Table 3.6).

Table 3.6 Fertility Rates & Family Planning Clinics, Results by Model Specification (1998-2015)

Model:	(1) Linear	(2) Log-linear
Sample mean:	55.123	4.017
Number of family planning clinics		
Clinics (t-1)	-0.177***	-0.00301***
	(0.0403)	(0.000673)
Demographics		
Unemployment rate (%)	-0.443**	-0.00866***
	(0.137)	(0.00221)
Median household income	0.0661	0.00119
(thousands, 2015 dollars)	(0.0543)	(0.000961)
Poverty (%)	0.257*	0.00505*
	(0.130)	(0.00217)
Democrat governor	-0.397	-0.00800
	(0.271)	(0.00424)
Minority (% non-White)	-0.0311	-0.00104
	(0.160)	(0.00273)
N	3809	3809

Notes: All models control for county and year fixed effects. Standard errors are clustered at the state level and reported in parentheses. Models are weighted as follows: (1) county population of females ages 15-49, (2) total county population, and (3) unweighted. Significance denoted as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Because some family planning clinics also provide prenatal care, changes in access to family planning clinics may affect both infant and maternal health, particularly for populations that rely on family planning clinics as their primary care provider (Frost *et al.*, 2012). In particular, limiting access to care may put infants at increased risk of preterm birth or low birth weight, and women with high-risk pregnancies may be at increased risk for maternal mortality. In future research, I will include these outcomes in my analysis in an effort to provide a more comprehensive look at the relationship between access to family planning clinics and both infant and maternal health outcomes.

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