The Dissertation Committee for Karen Michelle Mulligan
certifies that this is the approved version of the following dissertation:

Essays in Health Economics

Committee:

Jason Abrevaya, Supervisor

Sandra Black

Daniel Hamermesh

Stephen Trejo

Mark Hayward

Essays in Health Economics

by

Karen Michelle Mulligan, B.S.; M.S.; M.S.Eco.

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To Chris, you've been more supportive than you'll never know.

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Essays in Health Economics

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Karen Michelle Mulligan, Ph.D.
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Supervisor: Jason Abrevaya

This dissertation consists of three chapters on health economics, two of which focus on contraception and the third on vaccination. Chapter one examines the impact of state-level contraception insurance coverage mandates on women's fertility outcomes. It utilizes variation in mandated insurance coverage for contraception across states and over time to determine the causal impact of insurance coverage of contraception on fertility outcomes, specifically abortion rates and birth rates. State-level results indicate that a mandate decreases abortion rates by 6% in the year of introduction and decreases birth rates by 3% two years following introduction, with the magnitude of both effects remaining steady over the long run.

Chapter two utilizes longitudinal data on varicella (chicken pox) immunizations in order to estimate the causal effects of state-level school-entry and daycare-entry immunization mandates within the United States.[1] We find significant causal effects of mandates upon vaccination rates among preschool children aged 19-35 months; these effects appear in the year of mandate adoption, peak two years after adoption,

[1]Co-authored with Jason Abrevaya

and show a minimal difference from the aggregate trend about six years after adoption. For a mandate enacted in 2000, the model and estimates imply that roughly 20% of the short-run increase in state-level immunization rates was caused by the mandate introduction. We find no evidence of differential effects for different socioeconomic groups. Combined with the previous cost-benefit analyses of the varicella vaccine, the estimates suggest that state-level mandates have been effective from an economic standpoint.

Chapter three utilizes variations in access to emergency contraception (EC) across states to determine the impact of over the counter access on abortion rates, birth rates, and risky sexual behavior. Using state-level data, a flexible time specification finds that giving individuals over the counter access to EC reduces births and increases risky behavior, which is captured by STD rates. These effects are larger for adults compared with teenagers, however, there are not significant differential effects by race. Finally, the effects are increasing over time following the legislation.

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Chapter 1

Contraception and fertility: the role of insurance mandates

Despite the Centers for Disease Control and Prevention (CDC) listing family planning as one of the ten great public health achievements of the 20th century (CDC, 1999), the U.S. Department of Health and Human Services listed "increasing the proportion of health insurance plans that cover contraceptive supplies and services" as one of its goals for Healthy People 2010, highlighting a key lingering issue related to family planning. In a study of 492 insurers across the U.S., Sonfield et al. (2004) found that 78-97% of typical employer-based insurance plans in 2002 covered some form of contraception, up from 32-59% in 1993. These estimates only include HMO, PPO, and POS plans, and additionally the authors point out that "some lives, and perhaps many, are not represented by these typical plans". The Kaiser Family Foundation (KFF) surveys employers annually regarding their health benefits, and their 2003 estimates indicate that 58-80% of small firms and 58-78% of large firms had insurance plans which covered the five leading reversible contraceptives.[1]

In an attempt to increase insurance coverage for contraception, the Equity in Prescription Insurance and Contraception Coverage Act (EIPCC) has been introduced in Congress four times since 1997. Although the federal government has not passed contraception coverage legislation, many states have passed EIPCC laws of

[1] The five leading methods are the diaphragm, one- and three-month injectibles, IUDs, and oral contraceptives.

their own, effectively mandating insurance coverage for contraception at the state-level. These mandates require employer-provided insurance plans to cover contraceptives if they provide prescription drug coverage. Based on the 2003 KFF surveys, 84-100% of small firms and 99-100% of large firms included prescription drug coverage in their insurance plans.[2] The disparity between prescription drug and contraception coverage in insurance plans indicates that the mandates should lead to significant increases in contraception coverage, at least in states with mandates.

The earliest contraception insurance coverage mandate was enacted in 1998, with a total of 20 states passing mandates by 2002; currently 28 states have mandates, and two others (Michigan and Montana) require contraception coverage as a result of Attorney General opinions. Between 1998 and 2009, 61-69% of individuals in the U.S. obtained insurance coverage through their employers, such that the contraception coverage insurance mandates impact a significant proportion of the population. This impact could in theory go in either direction. Since mandates work to decrease the cost of contraceptives, we expect them to impact fertility outcomes among those with employer-based insurance. Holding sexual activity fixed, pregnancy rates should decrease, but the lower price of contraception is also likely to increase sexual activity.

Given that mandates operate through employer-provided insurance, they may lead to greater disparities within the population. The most obvious disparity would occur between employed and insured relative to unemployed and uninsured, meaning disparities between certain demographic groups could arise due to their correlation with insurance status. Based on estimates from the KFF, white individuals are more likely to have employer-based insurance than black individuals (68.5-74% versus 51.4-58%), which implies mandates might have a larger impact on whites. Similarly,

[2]The estimates from Sonfield et. al. (2004) find similar levels of coverage (93-96%).

individuals with more education are also more likely to have employer-based insurance: 30.9% of individuals with less than a high school diploma receive insurance through their employer, compared with 57.2% of high school graduates and 80.6% of college graduates. Individuals who are 35-54 are more likely to have employer-provided coverage, however, they are also outside of their prime childbearing years, so it is unclear whether fertility levels or insurance levels will have the predominate effect for various age groups. Higher income individuals are also more likely to receive insurance through their employer: 86.6% of individuals with incomes greater than 300% of the poverty level have employer insurance, compared with 70.7% of individuals with incomes between 200-299% of the poverty level. (Individuals with incomes below 200% are more likely to be covered by Medicaid, which is required to provide free contraception services.)

The majority of the existing research on health insurance mandates focuses on the potential labor market costs of the mandates, measured in terms of decreases in wage, employment, or the probability of insurance being offered. For example, Gruber (1994) finds that regulations mandating benefits actually have very little effect on firms dropping coverage for their employees, and postulates that this might be the case because mandates are not binding (i.e., most firms already provided the mandated benefits prior to legislation). Kaestner and Simon (2002) find little evidence that state health insurance regulation, specifically mandated benefits, impacts labor market outcomes such as wages or hours worked. A somewhat related branch of research examines the impact of state-mandated insurance coverage of infertility treatments on treatment utilization and birth rates. Bitler and Schmidt (2006) find no evidence that the mandates increase treatment utilization, but Henne (2008) finds evidence to the contrary. Schmidt (2007) finds that infertility coverage mandates lead to an eight percent increase in birth rates among women over 35, but that the mandates do not reduce racial disparities in access to treatment.

While contraception-coverage mandates have not previously been studied in the economics literature, there have been several studies looking at the effect of insurance coverage on contraception use and fertility outcomes. While their results are not causal, Culwell and Feinglass (2007) use the 2002 Behavioral Risk Factor Surveillance System and find insured women are more likely to use prescription contraceptives compared with uninsured women. Kearney and Levine (2009) find that Medicaid waivers that increase income limits for eligibility increase the probability of contraception use among sexually active women. Moreover, they find these waivers decrease non-teen births by 2% and teen births by 4%. Postlethwaite et al. (2007) utilize a change in insurance coverage for members of the Kaiser Foundation Health Plan in California to determine the impact of providing 100% coverage (i.e., no copays) for the most effective contraceptive types.[3] The change in coverage was found to be associated with an 132% increase in couple-year protection for IUDs and a 32% increase in couple-year protection for injectibles.

The purpose of this study is to estimate the causal effect of state mandated insurance coverage of contraception on fertility outcomes. It takes advantage of variation in mandated contraception coverage legislation across states over time to determine the effect of mandated insurance coverage of contraception in the U.S. on state-level abortion rates and birth rates. Additionally, this paper examines whether there are differential effects across various age and race groups in an attempt to determine whether mandates impact certain demographic subgroups disproportionately which could potentially exacerbate disparities in fertility outcomes. In addition to

[3]These methods include intrauterine devices (IUD), injectibles, and implants. The implant was discontinued in 2002, and so was excluded from the final analysis. Copays remained in place for oral contraceptives in an attempt to incentivize switching to contraception methods with the lowest failure rates.

the aggregate data, individual-level analysis is performed with the NLSY and CPS Fertility Supplement, which have the benefit of being able to link fertility outcomes to other individual characteristics. Using a flexible time specification, in addition to the effect in the year of mandate introduction, the effects in years subsequent to the mandates are estimated.

The aggregate results indicate that mandates lead to a 6.3% (3.8%) decrease in abortion (pregnancy) rates in the year of mandate introduction, and the magnitude of these effects remain steady over the long run. Mandates lead to a decrease in birth rates of 3% beginning two years following mandate introduction. The aggregate results are used to make a back-of-the-envelope calculation of a lower bound estimate of the impact of the Affordable Care Act, which effectively extends contraception insurance mandates to all states. This legislation takes effect in August 2012, and is estimated to result in approximately 108,000 fewer abortions and 37,000 fewer births.

The outline of the paper is as follows. Section 1.2 provides additional background on contraception and insurance coverage mandates. Section 1.3 describes the data used for the analysis. Descriptive statistics for state-level fertility outcomes, the CPS sample, and the NLSY sample (as well as mandate and non-mandate subsamples) are provided. Section 1.4 presents the estimation strategy. The effect of mandates on fertility outcomes is initially modeled as a structural break and then extended to a more flexible dynamic framework. Section 1.5 presents the results, and section 1.6 concludes.

1.1 Background

1.1.1 Contraception

The availability of a wide range of contraception options allows women to choose the method that best suits their needs. However, when the full range of options is not available or affordable, some women might choose less appropriate methods or forgo birth control altogether, therefore increasing the probability of an unplanned pregnancy. While the estimated out-of-pocket costs for birth control are substantially less than pregnancy itself, they are not trivial. For example, Planned Parenthood lists the following as cost estimates: the pill costs between $15-50 per month, shots cost between $35-75 per injection and last three months, implants cost between $400-800 up front but last three years, and IUDs cost between $500-1000 up front but last twelve years (Planned Parenthood, 2011). None of these costs include the exam fees which are needed to obtain birth control.

Ensuring prescription contraceptives are available and reasonably priced for women is important since these methods of contraceptives provide the lowest failure rates under typical usage. Trussell (2011) estimates the percentage of women experiencing unintended pregnancy under typical use of various types of contraception.[4] He finds the unintended pregnancy percentages during the first year of use are lowest for prescription contraceptives: 0.05% of women using implants experience an unintended pregnancy, 0.8% for IUDs, 6% for injectibles, and 9% for the pill, patch, and ring.[5] Non-prescription contraceptives have lower percentage of unintended pregnancy than no method (85%), but are less effective than prescription methods: 18% of women using a condom experience an unintended pregnancy, 24% for fertility awareness methods, and 28% for spermicides. Despite lower failure rates for prescription contraception, approximately 14% of all women use non-prescription contraceptives. The 2006-08 National Survey of Family Growth (NSFG) estimates indicate that approximately 5% of women who discontinued use of contraceptives did so because it was too expensive or their insurance did not cover it, which is similar

[4]Typical usage refers to actual use, which includes both inconsistent and incorrect use.
[5]Trussell's estimates are derived using the 1995 and 2002 National Survey of Family Growth.

to estimates found in Jones et al. (2002).

Table 1.1 shows contraception usage estimates from the NSFG by age groups. Individuals who do not use contraception can be broken down into four types: women who are sexually active, women who are either sexually inactive, women who actively trying to become pregnant, or women who cannot become pregnant (but did not get sterilized as a means of contraception). The last three types do not need to use contraception (for birth control purposes) and are combined into a single category. Overall contraception use has not changed much between 1995 and 2006-08, although it has decreased slightly among 20-29 year olds. The percentage of individuals that use prescription contraception has increased slightly between 1995 and 2006-08 among all age groups, with the exception of women bewteen the ages of 20-24. The increase (decrease) in prescription (non-prescription) use among teenagers was considerably larger compared to other age groups between 1995 and 2002.[6]

1.1.2 Contraception Insurance Coverage Mandates

Maryland enacted the first contraception insurance coverage mandate in 1998, and since then 27 other states have enacted mandates. Additionally, Michigan and Montana require insurance coverage of contraception as a result of an Attorney General opinion. Table 1.2 gives full details year of mandate introduction and exemptions by state.

These data were obtained from the National Conference of State Legislatures (NCSL) as well as the Guttmacher Institute. The mandates apply only to

[6] I am unaware of any specific legislation that might have been responsible for such large changes in the composition of teenage contraception use relative to adults, and determining whether changes in social norms played a role is beyond the scope of this paper. The change in composition is mainly driven by a decrease in condom usage coupled with an increase in pill and injectable usage.

Table 1.1: Contraception use by age, 1995-2008

Year	Age Group	Prescription	Non-Prescription	Sterilization	No Contraception Sexually Active	No Contraception Other
1995	15-19	16.7	12.8	0.1	7.1	63.1
	20-24	40.0	20.5	3.2	6.0	30.2
	25-29	32.4	22.0	14.9	4.7	25.7
	30-44	14.9	17.2	40.6	4.3	22.1
2002	15-19	21.6	9.9	0.0	6.9	61.6
	20-24	40.1	18.1	2.7	8.4	30.9
	25-29	34.5	20.4	13.1	8.0	24.1
	30-44	17.7	14.3	37.7	7.1	23.1
2006-08	15-19	20.3	7.5	0.0	6.5	65.3
	20-24	36.4	16.4	1.7	9.1	36.2
	25-29	33.2	19.3	11.7	8.6	27.2
	30-44	19.6	13.5	41.1	6.6	18.8

Source: Use of Contraception in the United States, 1995-2008, NSFG.

Table 1.2: Contraception Insurance Coverage Mandates, by State

State	Year in Effect	Exemption Allowed	State	Year in Effect	Exemption Allowed
Arizona	2002	X	Montana	2006	
Arkansas	2005	X	Nevada	1999	X
California	1999	X	New Hampshire	1999	
Colorado	2010		New Jersey	2005	X
Connecticut	1999	X	New Mexico	2001	X
Delaware	2000	X	New York	2002	X
Georgia	1999		North Carolina	1999	X
Hawaii	1999	X	Oregon	2007	X
Illinois	2003	X	Rhode Island	2000	X
Iowa	2000		Texas	2001	
Maine	1999	X	Vermont	1999	
Maryland	1998	X	Virginia	2001	
Massachusetts	2002	X	Washington	2007	
Michigan	2006	X	West Virginia	2005	X
Missouri	2001	X	Wisconsin	2009	

Source: National Conference of State Legislatures and Alan Guttmacher Institute employment-based health plans, and employers who self-insure are exempt from the legislation since they fall under federal jurisdiction, and EIPCC legislation has not been passed at the federal level. These mandates still impact a significant portion of the population with employer-based insurance: the KFF estimated that in 2011, sixty percent of covered workers are employed by a firm that self-insures, compared with fifty percent in 2000. Twenty states have some form of an exemption clause (either allowing the employer to refuse to offer coverage or the insurer to refuse to write a plan that includes coverage), most of which are religious. While these mandates still work to increase access to coverage among the affected population, the insurance plans might still employ some form of cost-sharing, which may result in inadequate decreases in out-of-pocket costs for some individuals. As an example, a 2010 study found that privately insured women using oral contraceptives whose plan covered prescription drugs paid approximately half (53%) of the cost of pills (Liang, et. al, 2010).

The potential effect of the mandates could be small under several scenarios: if the majority of firms claim exemptions, if a majority of firms provided contraception coverage prior to the mandates, or if the majority of individuals receive insurance from an employer who self-insures. With respect to the religious exemptions, the finding by Sonfield et al. (2004) seem to indicate that religious exemptions might not be common since they find a large disparity between coverage of contraceptives in mandate and non-mandate states in plans determined at the local level.[7] Specifically, they find 47-61% of plans in non-mandate states cover the five leading contraception methods, compared with 87-92% of plans in mandate states. The high level of coverage in mandate states would likely not be present if a substantial amount of

[7]Differences for plans at the local level are used for comparison since plans determined at the national level are more likely to adhere to state mandates.

religious exemptions were claimed. Additionally, the wording of the exemption in the legislation in the majority states is narrow enough such that many religious-based organizations such as schools and hospitals are not eligibe for exemption.

Given estimates from the KFF employer insurance coverage survey, it is also unlikely that mandates will not be binding in the sense that many plans already provided coverage pre-mandate. Although the KFF estimates are not broken down by state, their 1999 estimates (when only 10 states had mandates) find low coverage levels for reversible contraception, ranging from 32-57% of small firms depending on the plan type and ranging from 42-68% for large firms depending on plan type. These numbers trended downward in 2001 and increased in subsequent years; slighly more than half of the mandates were enacted prior to 2001, so it is unlikely that coverage levels were high even in mandate states. The final concern for whether mandates affected enough individuals to have a noticeable impact is that the mandates are limited in the sense that they do not affect individuals who work for employers that self-insure. As a conservative estimate, the mandates have the potential to impact at least 24% of the total population in any given year since 40% of covered individuals who have employer-based insurance (based on the low-end KFF estimates) and based on 2009 CPS estimates, on average 61% of individuals receive insurance through their employer in states with mandates.

1.2 Data

Aggregate data represent state-level data for the years 1993-2007. The two dependent variables considered are the birth rate and abortion rate. Birth rates are collected from the Center for Disease Control (CDC) vital statistics website, and can be separated by various age and race categories. The birth rate is given as the number of births per 1000 women aged 15-44.[8] The abortion rate represents the number of abortions performed in a state per 1000 women aged 15-44 who reside in that state. These data are collected annually by the CDC, however some states do not report abortion statistics to the government.[9] As with the birth data, abortion data can be separated by various age and race categories. Population estimates from the Census Bureau are used to calculate rates for subgroups since these data are reported in totals rather than rates. Population-weighted averages for abortion and birth rates for various mandate definition subsamples are given in Table 1.3.

Since mandates operate through employer-based insurance coverage, controls for the percentage of the population in each state with employer-provided coverage are added to the aggregate data. These estimates are obtained from the Bureau of the Census. Unfortunately, state-level estimates do not separate the data by employers who self-insure and those who do not.

The first individual-level dataset utilized in this analysis is the June Current Population Survey (CPS) Fertility Supplement, which is available in 1995 and even years between 1998 and 2010. The survey asks all women when their most recent child was born, but this analysis limits the sample to women between the ages of 15 and 44 for consistency across aggregate and individual level data. This question is used to construct an indicator variable for whether the individual has given birth in the past year. The analysis also utilizes individual level controls, including race/ethnicity, marital status, education, family income, employment status, and age. Summary statistics are presented in Table 1.4.

[8]For regressions involving age or race subgroups, the rate is calculated as the number of birth per 1000 women within that subgroup.

[9]Specifically, starting in 1998, California and New Hampshire do not report abortion statistics. Alaska, Louisiana, Oklahoma, and West Virginia do not report in all years, but they report in a majority of the years.

Table 1.3: Abortion and Birth Rates (per 1000 women aged 15-44) by State Mandate Status, 1993-2007 (CDC data)

	Full Sample	No Mandate	Mandate (Any Type)
Abortion Rate	17.6	18.0	16.6
	(0.57)	(0.71)	(0.78)
Pre-Mandate			20.7
Abortion Rate			(1.1)
Post-Mandate			16.6
Abortion Rate			(0.80)
Difference			-4.1***
			(1.4)
Birth Rates	14.3	14.2	14.5
	(0.09)	(0.11)	(0.15)
Pre-Mandate			14.7
Birth Rate			(0.23)
Post-Mandate			14.5
Birth Rate			(0.15)
Difference			-0.23
			(0.23)

Means are population weighted, with standard deviations in parenthesis.
The "Mandate" column defines mandates most broadly.
Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$

Table 1.4: Descriptive statistics, CPS Fertility Supplement (1995-2010)

		Full sample (mean)	Did not give birth in last year subsample (mean)	Gave birth in last year subsample (mean)
Individual	Gave Birth	0.093	0	1
	Black	0.110	0.110	0.112
	Hispanic	0.128	0.124	0.167
	Other non-white race	0.067	0.067	0.071
	White	0.695	0.699	0.649
	Age (continuous)	30.05	30.25	28.11
	Age: 15-19	0.163	0.170	0.091
	Age: 20-29	0.304	0.285	0.488
	Age: 30-39	0.347	0.343	0.385
	Age: 40-44	0.185	0.201	0.036
	Married	0.472	0.450	0.680
	Single	0.528	0.550	0.320
	Less than high school	0.206	0.208	0.180
	High school graduate	0.264	0.262	0.280
	Some college	0.291	0.294	0.268
	College graduate	0.239	0.236	0.272
	Employed	0.666	0.680	0.532
	Private	0.524	0.535	0.415
	Income: <$10K	0.083	0.080	0.113
	Income: $10-20K	0.108	0.106	0.125
	Income: $20-30K	0.129	0.127	0.139
	Income: $30-40K	0.130	0.130	0.125
	Income: $40-50K	0.100	0.101	0.091
	Income: $50-60K	0.096	0.097	0.090
	Income: >$60K	0.355	0.359	0.317
Mandate Type	Any Mandate	0.367	0.368	0.363
	No exemptions	0.070	0.070	0.069
	Exemptions	0.264	0.264	0.259
	No Mandate	0.633	0.632	0.637
Sample Size		200,993	182,116	18,817

Table 1.5: Fertility Outcomes, NLSY (1997-2009)

	Full Sample	Mandate	No Mandate
Abortion Rate	12.7	16.5	10.7
Birth Rate	77.4	99.0	66.5
Pregnancy Rate	98.5	122.9	86.2
Pregnancy Loss Rate	18.5	20.8	17.4
Sample Size	57,005	37,849	19,156

Rates are per 1000 women in the sample in the NLSY data.

The second individual-level dataset utilized in this analysis is the 1997 National Longitudinal Survey of Youth (NLSY), which interviewed 4385 women aged 12 to 18 in 1997 and has reinterviewed them annually through 2009. The data provides complete family histories which are used to construct whether a woman was pregnant, experienced a pregnancy loss, gave birth, or aborted a pregnancy for each year between 1997 and 2009. The total number of abortions in the full sample is relatively small (722 abortions over the fifteen year period), and additionally abortions are most likely under-reported. Table 1.5 presents summary statistics for fertility outcomes in the NLSY.

Comparing these rates to the aggregate level data, the abortion rate is lower in the NLSY, which is attributable to under-reporting. The birth rate in the NLSY is higher than in the aggregate data, which makes sense given the young age of the NLSY sample. The rate is similar to the birth rate for 15-30 year olds as computed using data from Vital Statistics. The number of pregnancies is slightly less than the sum of births, losses, and abortions due to the fact that some women experience more than one pregnancy per year. If a woman becomes pregnant and loses the pregnancy and then becomes pregnant again in the same year but the second pregnancy results in a birth, each outcome is recorded in the year, but only one pregnancy is recorded.

1.3 Estimation Strategy

A simple framework to determine the impact of the mandates is given by

$$Y_{st} = \beta Mandate_{st} + X_{st}\gamma + \theta_s + \theta_t + \epsilon_{st} \tag{1.1}$$

where s indexes state, t indexes time, Y_{st} represents the abortion rate or birth rate, $Mandate_{st}$ is an indicator equal to one if there is a mandate present for contraception insurance coverage in state s at time t, and X_{st} are state-level controls.[10]

The model in (1.1) implicitly assumes the effect of the mandates are captured by a structural break. An effect on pregnancies and abortions might occur in the year of the mandate itself, however, an outcome of a birth would be delayed such that an impact on birth rates might not occur for at least one year following a mandate. The model in (1.1) can confound pre-existing trends with the response of the outcomes to contraception insurance coverage mandates. To address these issues, this paper also estimates a flexible time specification as proposed by Wolfers (2006). Specifically, the following model is estimated for aggregate outcomes:

$$Y_{st} = \sum_{k \in \mathcal{K}} \beta_k M^k_{st} + X_{st}\gamma + \theta_s + \theta_t + \epsilon_{st}, \tag{1.2}$$

where s indexes state, t indexes time, Y_{st} represents the abortion rate, birth rate, or pregnancy rate, M^k_{st} is a dummy equal to one in the k^{th} period following the contraception insurance coverage mandate ($k = 0$ implies the mandate introduction took place that period), θ_t are year indicator variables, θ_s are state indicator variables, and X_{st} are state-level controls. The state indicator variables (θ_s) allow for (time-invariant) unobservable state characteristics to affect fertility outcomes, whereas the

[10]Controls include the percentage of the population that has employer insurance, median household income, and whether emergency contraception is available over the counter.

year indicator variables (θ_t) allow for a flexible specification of the aggregate trend in fertility outcomes. β_k is interpreted as the impact of a mandate for insurance coverage for contraception in state s at time t on the abortion/birth rate k periods following the mandate (relative to states without a mandate at time t). The set $\mathcal{K}$ indicates the number of leads and lags included in the model. For the results presented below, the set $\mathcal{K} = \{-4, -3, -2, -1, 0, 1, 2, 3, 4, 5, 6, 7\}$ is used.

Even with the dynamic specification in (1.2), there might still be concern that states experiencing downward trends in fertility outcomes are more likely to enact mandates, such that the estimates in (1.2) would simply be picking up the pre-existing trends. To further rule out potential endogeneity of whether a state enacts a mandate or the timing of mandates, models are estimated which regress whether a state has a mandate on state characteristics as well as the trends in birth, abortion, and insurance rates. Additionally, for states with mandates, I estimate a model which regresses the year of mandate on state characteristics and the average birth, abortion, and insurance rates in the years leading up to mandate introduction. The only significant predictor of the presence of a mandate is the percentage of the population that voted Democrat in the 1996 presidential election (significant at the 10% level), which should be captured by state-level fixed effects.[11] Among states with mandates, none of the variables were significant predictors of mandate timing. The results from these regressions are shown in Table 1.6. While these results indicate that pre-existing trends in fertility can be ruled out as a source of endogeneity, it is not possible to rule out state-level shocks in a given year as a potential source of bias.

[11]This result is similar if an indicator for blue states is included instead of the percentage of the votes in the 1996 presidential election.

Table 1.6: Endogeneity Check: Mandate Presence

	Dependent variable = 1 if state ever has mandate		Dependent variable = year of mandate introduction	
Variable	Estimate	Std. Error	Estimate	Std. Error
population (hundred thousands)	-0.001	0.002	0.014	0.011
pct. with high school diploma	0.015	0.017	0.243	0.161
pct. with college degree	0.025	0.031	-0.006	0.394
household income	-0.00001	0.00002	-0.0001	0.0003
pct. vote for Clinton in 1996	0.033*	0.020	0.023	0.094
trend in birth rate	1.945	1.444		
trend in abortion rate	0.332	0.398		
trend in pct. insured	0.161	1.132		
average lagged birth rate			-0.394	0.528
average lagged abortion rate			-0.138	0.101
average lagged insurance rate			-0.112	0.098

The variables are averages for the 1990s. Results are similar using data from the 2000s. The first regression includes all 51 states, and the second regression only includes the 30 states with mandates. The lagged rates are calculated as the average rates of the three years prior to mandate introduction. Results are similar using various combinations of years to generate lagged rates.

1.3.1 Individual-level models

For both the CPS and NLSY datasets, linear probability models are estimated that are analogous to the model in (2) augmented with individual controls:

$$Pr(Y_i = 1|X_i, s(i), t(i)) = \sum_{k \in \mathcal{K}} \alpha_k M^k_{s(i)} + X_i\gamma + \delta_{t(i)} + \delta_{s(i)}, \tag{1.3}$$

where i indexes individuals, Y_i is an indicator variable for abortion, birth, pregnancy, or pregnancy loss (for the NLSY data) or an indicator equal to one if the individual has given birth in the past year (for the CPS data), X_i is a vector of individual controls, $s(i)$ is individual i's state, and $t(i)$ is the year in which individual i was sampled.[12] Due to the small sample size of the NLSY leading to the relatively small number of outcomes, this analysis does not exploit the panel nature of the data, but rather pools all available observations.

[12]Similar results are obtained using Probit estimation.

Table 1.7: Effect of contraception insurance coverage mandates on state-level outcomes

	Aggregate	Teenager	Aged 20-29	Aged 30-39
Abortion Rate	-1.38***	-0.37*	-2.23***	-0.50***
	(0.34)	(0.14)	(0.65)	(0.18)
Baseline	19.80	5.30	30.70	10.10
Implied pct.	6.96	6.94	7.27	4.98
Birth Rate	-0.44**	-0.19	-4.36*	0.14
	(0.22)	(0.24)	(2.25)	(0.59)
Baseline	14.20	13.01	111.3	59.20
Implied pct.	3.11	1.43	3.92	0.23

Each cell represents the coefficient on an indicator of whether a mandate is in place. State and year fixed effects are included, as well as controls for the percentage of the population within each state that obtains insurance through their employer, median income, and whether emergency contraception is available over the counter. Estimates are reported in rates per 1000. The regressions are population-weighted, and standard errors are clustered at the state level. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$

1.4 Results

The mandate indicator $Mandate_{st}$ is defined as equal to one if state s has any form of a mandate requiring insurance coverage for contraceptives at time t. This broad definition of mandates leads to the inclusion of both Michigan and Montana as states with mandates, even though their mandated coverage was enacted through Attorney General opinion. Several definitions of mandates were used as a robustness check, but the results do not change much by how broadly mandates are defined. The state-level estimation results for the single mandate indicator specification are presented in Table 1.7. The table also includes the baseline abortion and birth rates for each subgroup, as well as the implied percentage change in the abortion and birth rates.

The mandate decreases abortion rates for all subgroups, with the mandate leading to a 6.9% decrease in abortion rates relative to baseline.[13] Mandates also decrease birth rates, however these effects are only significiant at the aggregate level and for women between the ages of 20 and 29. The decrease in birth rates at the aggregate level (20-29 age group) is approximately 3.1% (3.9%) compared to baseline. (The effect for adults is also significant, but this is being driven primarily by the 20-29 year olds.)[14]

The results for the flexible time specification and subsample estimations are presented in Tables 1.8 and 1.9.

At the state-level (i.e., for the full sample), there is evidence that mandates have an effect on fertility outcomes beyond the initial year of introduction, however the timing of these effects is slightly different depending on the outcome. The effect on the abortion rate occurs immediately following the mandate enactment and remains steady over time. Not surprisingly, the impact of births does not appear until two years following the mandate.[15] For the year of mandate introduction, the causal effect is calculated as the difference from the average effect of the 2nd-4th years leading up to the year of introduction estimate, which is an approximately -1.249 decrease in the abortion rate, or 6.3% from baseline. The post-mandate effects are not cumulative. Therefore, the 1-year post-mandate estimate of -1.324 represents an additional 0.075 decrease in the abortion rate relative to the initial year effect.

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[13]The baseline fertility outcomes are calculated as the average in 1997, which was the year prior to the first mandate introduction. The baseline abortion (birth) rate is 19.8 (14.2).

[14]Additionally, following Levine et al. (1996), models were estimated using the "pregnancy rate" as the outcome variable. The pregnancy rate is a constructed measure defined as the sum of the abortion rate and the birth rate, and serves as an approximation to the number of pregnancies per 1000 women aged 15-44. The results are very similar to results using abortion and birth rates as outcome variables, and are available upon request.

[15]A test of the null that the effects on the abortion rate are constant over time cannot be rejected at any level of significance. The same null for birth rates can be rejected at the 10% level of significance.

Table 1.8: Effect of contraception insurance coverage mandates on abortion rates

	Full Sample		Aged 15-19		Aged 20-29		Aged 30-39	
	Estimate	Implied Pct.	Estimate	Implied Pct.	Estimate	Implied Pct.	Estimate	Implied Pct.
Year of	-1.25***	6.31	-0.347***	6.55	-2.29***	7.46	-0.62***	6.14
mandate	(0.30)		(0.10)		(0.62)		(0.20)	
1-year	-1.32***	6.67	-0.40***	7.55	-2.92***	9.51	-0.73***	7.23
post-mandate	(0.38)		(0.13)		(0.70)		(0.23)	
2-year	-1.20**	6.06	-0.40**	7.55	-2.45***	7.98	-0.53**	5.25
post-mandate	(0.47)		(0.19)		(0.87)		(0.25)	
3-year	-1.48**	7.47	-0.43**	8.11	-2.84***	9.25	-0.64**	6.34
post-mandate	(0.56)		(0.19)		(0.99)		(0.29)	
4-year	-1.45***	7.32	-0.41**	7.74	-2.53**	8.24	-0.49	4.85
post-mandate	(0.53)		(0.19)		(0.99)		(0.30)	
5-year	-1.29**	6.52	-0.36*	6.79	-2.05*	6.68	-0.37	3.66
post-mandate	(0.54)		(0.21)		(1.17)		(0.38)	
6-year	-1.27*	6.41	-0.37	6.98	-1.97	6.42	0.39	3.86
post-mandate	(0.70)		(0.26)		(1.44)		(0.47)	
7-year	-1.50	7.58	-0.46	8.68	-2.49	8.11	-0.22	2.18
post-mandate	(1.05)		(0.34)		(2.09)		(0.69)	

Estimates are from a flexible time specification. Each column reports results from a separate regression on different subsamples. Each estimate corresponds to the marginal effect in the k^{th} year following the mandate calculated as the difference from the average effect of the $2^{nd} - 4^{th}$ years leading up to the mandate. Estimates are reported in rates per per 1000. All regressions are population weighted by subgroup and include state and year indicator variables and controls for percentage of the population with private insurance, median income, and whether emergency contraception is available over the counter. Standard errors are clustered at the state level. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$.

Table 1.9: Effect of contraception insurance coverage mandates on birth rates

	Full Sample		Aged 15-19		Aged 20-29		Aged 30-39	
	Estimate	Implied Pct.	Estimate	Implied Pct.	Estimate	Implied Pct.	Estimate	Implied Pct.
Year of	-0.29	2.04	-0.15	1.15	-3.49	3.14	-0.49	0.83
mandate	(0.19)		(0.20)		(2.15)		(0.70)	
1-year	-0.31	2.18	-0.11	0.85	-4.24*	3.81	-0.10	0.17
post-mandate	(0.23)		(0.27)		(2.36)		(0.57)	
2-year	-0.43*	3.01	-0.21	1.61	-4.79*	4.30	0.09	0.15
post-mandate	(0.26)		(0.29)		(2.71)		(0.75)	
3-year	-0.50*	3.52	-0.31	2.38	-5.87*	5.27	-0.10	0.17
post-mandate	(0.30)		(0.33)		(2.99)		(0.77)	
4-year	-0.60*	4.23	-0.40	3.07	-6.60**	5.93	0.28	0.47
post-mandate	(0.33)		(0.33)		(3.26)		(0.75)	
5-year	-0.65*	4.58	-0.45	3.46	-6.52*	5.86	0.55	0.93
post-mandate	(0.33)		(0.35)		(3.74)		(0.74)	
6-year	-0.64*	4.51	-0.46	3.54	-6.18	5.55	1.58	2.67
post-mandate	(0.36)		(0.36)		(4.43)		(1.78)	
7-year	-0.80*	5.63	-0.45	3.46	-7.97	7.16	1.97	3.33
post-mandate	(0.48)		(0.42)		(5.53)		(1.68)	

Estimates are from a flexible time specification. Each column reports results from a separate regression on different subsamples. Each estimate corresponds to the marginal effect in the k^{th} year following the mandate calculated as the difference from the average effect of the $2^{nd} - 4^{th}$ years leading up to the mandate. Estimates are reported in rates per per 1000. All regressions are population weighted by subgroup and include state and year indicator variables and controls for percentage of the population with private insurance, median income, and whether emergency contraception is available over the counter. Standard errors are clustered at the state level. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$.

To determine whether mandates requiring insurance coverage for contraceptives had differential effects on various age subgroups, estimation was conducted using various age subsamples. Four age categories are considered: teenagers (defined as between the ages of 15 and 19), adults (defined as between the ages of 20 and 44), adults in their twenties, and adults in their thirties. Mandates decrease abortion rates across all age subgroups, and with the exception of women in their thirties, the effects of mandates remain significant until five years following the year of introduction. The effects are slightly higher for women in their twenties, with mandates leading to a 7.5% decrease in the abortion rate in the year of introduction, compared to 6.5% and 6.2% for teens and women in their thirties, respectively.

1.4.1 CPS Fertility Supplement Results

The individual-level model includes all the controls listed in Table 1.4. While the coefficients on these controls are not of direct interest and therefore not reported, it is worth nothing that the probability of giving birth in the past year is lower among individuals with higher levels of education, higher levels of income, single individuals, employed individuals, and white individuals. Given these results, models are estimated with interaction terms to determine whether mandated insurance coverage for contraception impacts certain demographic groups disproportionately. Specifically, the models estimate differences between whites and non-whites, non-college graduates and college graduates, married and single individuals, individuals less than 30 years old and individuals greater than or equal to thirty, and low income and high income households (defined as households earning greater than $50,000 per year.) The results for the CPS estimation are presented in Table 1.10.

Since the CPS data is only available in even years, the flexible time specification includes indicator variables pooled over two years rather than an indicator

Table 1.10: Effect of contraception insurance coverage mandates on birth rates, CPS Sample

		Estimated differences between demographic subgroups				
	(1)	White/ Non-White	Age <30/ Age $\geq$30	Married/ Single	Low/High Income	College/Non-college Grad
Mandate Indicator	-2.78	2.88	-14.9***	11.6***	-5.38**	2.70
	(2.44)	(3.74)	(4.47)	(3.93)	(2.58)	(4.24)
0-1 years post-mandate	-0.21	-1.77	-9.64	6.32	0.11	-8.62
	(2.94)	(5.97)	(6.44)	(6.72)	(6.06)	(7.35)
2-3 years post-mandate	-8.61***	1.25	-8.38	-7.76	-7.51	-9.41
	(3.09)	(6.74)	(6.80)	(7.19)	(6.67)	(7.51)
4-5 years post-mandate	-5.27*	-0.04	-8.34	-3.40	1.77	-13.4
	(3.02)	(5.63)	(6.04)	(6.76)	(7.28)	(8.74)
6-7 years post-mandate	-7.87**	-0.80	-11.3*	3.08	-4.19	-2.43
	(3.96)	(5.63)	(6.38)	(4.91)	(6.80)	(8.68)
8-9 years post-mandate	-2.76	4.62	-15.9**	1.93	5.39	-11.8
	(4.37)	(5.45)	(6.94)	(6.52)	(7.46)	(8.34)
10+ years post-mandate	-0.69	-0.66	-26.5***	5.06	-4.67	-14.8
	(4.53)	(5.65)	(7.44)	(6.04)	(7.05)	(10.1)

The first row represents results from a regression with a single mandate indicator, and the subsequent rows are estimates from the flexible time specification. The first column is the effect of the mandate on the full sample. The 2nd-6th columns are the estimated differences in the effect of mandates for various demographic subgroups. For the flexible time specifications, the marginal effect in the kth year following the mandate is calculated as the difference from the average effect of the 2nd-4th years leading up to the mandate multiplied by 1000 (i.e., the estimates are interpreted as changes in birth rates. All regressions are population weighted and include year and state indicator variables as well as the following controls: education, age, income, marital status, employment status and race/ethnicity. Standard errors are clustered at the state level. N=200,933.
Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$.

variable for each year. The effects for the CPS results are marginal effects multiplied by 1000, which can be interpreted as the effect on the birth rate. For example, in the first cell, the effect would be interpreted as the mandate leading to a 2.8 decrease in births per 1000 women in the past year (although this effect is insignificant). Similar to the aggregate results, the effect of mandates on the birth rate does not become significant until 2-3 years following the mandate introduction, and remains relatively stable in the long run. Since more years of data are available in the CPS, the model is able to identify lag terms 8-10 years post-mandate (as opposed to 7 years post-mandate in the aggregate model). The estimates for 8-10 years post-mandate are insignificant, which indicates that once more recent data become available at the aggregate level, it is worth estimating updated models to determine whether the aggregate effects disappear after more than 7 years post-mandate. Given that the birth rate in the full CPS sample is 92.2, the effect 2-3 years following the mandate represents a 7.1% change in the birth rate. While this result is slightly more than double the effect found using aggregate data, it represents a two-year effect. Columns two through seven present differences in effects for various demographic subgroups. For the most part, there are not significant differences by subgroup. Consistent with the aggregate results, the difference between women under the age of 30 and women 30 or older is significant, but this does not show up until 6-7 years following the mandate introduction.

1.4.2 NLSY 1997 Results

Table 1.11 presents the results from the NLSY data. Given the relatively small sample size, only the model with the single mandate indicator is estimated. Reported results represent the marginal effects multiplied by 1000, which can be interpreted as the effect on the rate (per 1000 women in the sample).

Table 1.11: Effect of contraception insurance coverage mandates on fertility outcomes, NLSY

	Abortions	Births	Pregnancies	Pregnancy losses
Mandate	2.601	-1.575	1.351	-1.819
	(2.82)	(5.36)	(6.78)	(3.13)
Mandate	5.002	-17.73**	-9.182	-3.168
	(4.27)	(8.39)	(10.4)	(5.16)
Insurance Status	-3.881	22.58***	38.74***	-2.396
	(2.83)	(5.86)	(7.15)	(3.49)
Mandate * Insurance Status	-1.140	12.56	19.70**	5.721
	(3.93)	(7.77)	(9.28)	(4.62)

All specifications include state and year fixed effects as well as controls for age, education, race marital status, number of children, urban residence, income-to-poverty ration, mother's education, and employment status. Estimates are marginal effects multiplied by 1000. Standard errors are in parentheses. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$.

The results indicate that mandates had no significant effect on any of the fertility outcomes for the NLSY sample in the model with a single mandate indicator. In the model that includes an interaction term between the mandate indicator and insurance status, it appears that pregnancies increased for women who report having insurance. Interpretation of this result is difficult in the context of mandates since the NLSY does not distinguish between different insurance types. In the model that includes the insurance interactions, mandates decreased birth rates, but the difference between individuals with and without insurance is not significant.

1.4.3 Policy Extension: Affordable Care Act

The Affordable Care Act (ACA) is the health care reform legislation which was passed in 2010. A major part of the reform was making preventive care more accessible and affordable. More specifically under the ACA, beginning August 1, 2012, women's preventive health care services are covered with no cost sharing — this includes prescription contraceptive drugs and devices.[16] This legislation effectively extends the existing contraception insurance mandates to the twenty states that do not currently have mandates, and also extends coverage to all employer-based insurance plans (including employers that self-insure).

The estimates from the flexible time specification are used to make back-of-the-envelope calculations of a lower bound effect of the Affordable Care Act in the twenty states that do not currently have mandates. These estimates are a lower bound for several reasons. The first reason is that the current mandates still allow for cost-sharing, while the ACA will remove cost-sharing from plans.[17] Secondly, employers that fully insure are impacted by the current mandates, whereas both employers that fully insure and employers that self-insure will be impacted by the ACA. As with the current mandates, the ACA has limited religious exemptions, but the language is similar to current exemptions, such that exemptions should not have an increased impact under the new legislation.

The aggregate estimates from the flexible time specification are used to make the calculations, but to keep the estimates conservative, only the initial year effect is used (rather than the full seven year effect). The mandates decrease abortions (births) by approximately 6.3% (3%), which implies the ACA will result in approximately 108,000 fewer abortions and 37,000 fewer births (in a single year) in the twenty states that do not currently have contraception insurance mandates.

[16] Sterilization procedures and patient education and counseling are also included under the new coverage laws; the law does not include coverage of abortifacient drugs.

[17] In an effort to give insurers the flexibility to control costs, certain forms of cost-sharing will be allowed, specifically if a generic brand is available for certain contraceptives, the insurance company can charge cost-sharing for the branded drug.

1.5 Conclusion

This paper examines the effect of insurance coverage mandates for contraception on fertility outcomes. Using aggregate state-level data from 1993-2007 and a flexible mandate-effect model, I find the mandates decrease abortion and birth rates. These effects remain stable over the long run, which extends seven years post-mandate. The effect on abortions is larger than the effect on births: in the year of mandate introduction, abortions decrease by 6.3% and births decrease by 3%, but the initial effect is delayed since births are not an immediate outcome. These estimates may be somewhat understated due to spillover effects caused by insurance companies that determine policies at the national level (recall that 58% of insurance companies in Sonfield et al. (2004) reported determining rates at the national level). Coverage mandates have been successful at decreasing fertility outcomes across all demographics: individual level estimates do not indicate that differential effects exist by demographic characteristics other than age.

The effects found in this study are similar to other studies which examine the impact of various public policies on fertility outcomes. For example, Mellor (1998) finds expanding Medicaid access to family planning services leads to a 7.2% decrease in the probability of giving birth, and Kearney and Levine (2009) find income-based Medicaid expansions decrease non-teen births by 2%. Levine et al. (1996) finds that adding Medicaid funding restrictions on abortion leads to about a 3% reduction in the abortion rate but does not impact birth rates once state trends are added. This result is of particular interest from a policy standpoint since abortion funding restrictions have been used as a primary policy tool for decreasing abortions. The decrease in the abortion rate approximately doubles when women are given better access to contraception as opposed to restricting abortions, which suggests providing better contraception access may be a more effective policy tool.

While controls for percentage of individuals with employer-based insurance are included in the models, more detailed insurance data would allow for better identification of the groups affected by mandates. Additionally, data linking contraception use or expenditures with detailed insurance status or coverage information would provide a means of determining what specific channels are driving the results, specifically whether individuals who had no coverage are obtaining coverage, or if individuals are gaining coverage of additional contraception types. Determining how mandates affect the mix of contraceptives being used by women would give insight into whether the policy is successful in encouraging women to use more effective contraception methods (and therefore having a better chance of avoiding an unintended pregnancy). If mandates do not induce women to use the most effective contraception methods, it might be necessary to pair mandates with a second policy tool (such as increased information about contraception effectiveness) to further decrease unintended pregnancies. Finally, in providing more access to contraception, these mandates have the potential to go beyond impacting fertility outcomes. Future research could examine how increased access to contraception through insurance mandates impacts education, marriage, and labor market outcomes, as well as maternal behavior during pregnancy (such as obtaining prenatal care).

Chapter 2

Effectiveness of state-level vaccination mandates: Evidence from the varicella vaccine

When the Centers for Disease Control and Prevention (CDC) released its list of the "Ten Great Public Health Achievements" of the 20th century, vaccination was at the very top of the list (CDC, 1999).[1] Vaccines have been instrumental in eradicating deadly diseases like smallpox and polio and dramatically decreasing the incidence of diseases like measles and mumps. Once the safety and efficacy of a vaccine has been established, the biggest challenge facing public-health authorities is ensuring that at-risk populations become immunized. The public-health community attempts to achieve "herd immunity," a level of immunization within the population that effectively eliminates the risk of an outbreak of the disease. Unfortunately, achieving widespread vaccination can be difficult since immunization involves costs for individuals such as potential side effects, possibly direct monetary costs, etc. and is subject to a classic free-rider problem since greater immunity arises from others being immunized. Moreover, in the absence of a vaccination registry, monitoring is extremely difficult.

Historically, the United States has ensured adequate levels of childhood immunization through the use of immunization mandates.[2] To deal with the monitoring

[1]Substantial portions of this chapter are published in an article coauthored by Jason Abrevaya and Karen Mulligan: "Effectiveness of state-level vaccination mandates: evidence from the varicella vaccine." *Journal of Health Economics.* 30(2011), 966-976.

[2]Other countries have used different approaches for childhood immunization: in Canada, vacci-

problem, immunization records have been required for school entry, and more recently, daycare entry. While vaccine regulation is overseen by federal authorities in the United States, the authority to *mandate* vaccinations rests with the individual states. The earliest school-entry immunization laws arose in response to outbreaks of smallpox in the 19th century, with Massachusetts, New York, Connecticut, and Pennsylvania the first states to require smallpox vaccinations for school entry (Hodge, Jr. and Gostin, 2001). Subsequent to the mandates for smallpox vaccinations, state-level school-entry mandates were instituted for several other immunizations, including those for measles, mumps, pertussis, and polio.

Currently, the Food and Drug Administration (FDA) is the agency within the United States responsible for evaluating the safety and efficacy of new vaccines. Subject to FDA approval, the CDC, as well as other groups, including the American Academy of Pediatrics (AAP), provides recommendations pertaining to the use of a new vaccine.[3] These recommendations include the appropriate subpopulations for immunization, immunization schedules, dosage, and cost effectiveness. Following the approval of a new vaccine, individual states may institute a mandate for school and/or daycare entry or leave the immunization decisions to individuals. In the case of the varicella vaccine, which was recommended for children by both the CDC and AAP in 1995, the response by individual states varied. Six states instituted a school- and/or daycare-entry mandate prior to 2000, and most other states followed suit within a few years; as of the end of 2009, only Idaho and Wyoming remained without a varicella-vaccine mandate.

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nations are not mandated but are provided as part of universal health care coverage; in Mexico, there are national immunization weeks held several times throughout the year (National Network for Immunization Information (2010)).

[3]The National Network for Immunization Information (2010) provides additional details on the vaccination approval/recommendation process and the advisory groups involved.

Even in the presence of an immunization mandate, children may be "exempted" from the mandate for medical reasons, religious reasons, or philosophical reasons.[4] Every state allows exemptions for medical conditions that make the vaccine unsafe for the child. According to the National Conference of State Legislatures (2010), every state but Mississippi and West Virginia currently allows religious exemptions and 20 states currently allow philosophical exemptions.[5] In addition to exemptions, poor enforcement of the mandates can limit their effectiveness. Enforcement often takes place locally at daycare centers and schools and relies on accurate record-keeping and truthful immunization records.

Despite the importance of state-level immunization mandates within the United States, there has been extremely limited research focused on the effectiveness of these mandates. A practical obstacle to this research has been the availability of national data on immunizations. While such data are now available, specifically, the National Immunization Survey (NIS) used in this paper, these data are too recent to analyze the effectiveness of most state-level mandates since these mandates pre-dated the immunization data. An exception is the varicella vaccine, whose introduction in 1995 coincides with the earliest NIS samples. As mentioned above, states reacted differentially to the introduction of the varicella vaccine with mandates rolled out at different times in different states. The variation in state-level mandate timing allows us to use longitudinal data in order to estimate the causal effect of a mandate.

While it may not be surprising to find that states with mandates also have high immunization rates, others have pointed to the inherent difficulty of separating correlation from causation. According to Hodge, Jr. and Gostin (2001), for instance,

[4]Malone and Hinman (2003) discuss the legal issues surrounding immunization exemptions.

[5]A religious exemption allows a parent to claim an exemption if immunization contradicts one's religious beliefs. Philosophical exemptions vary from state to state, but these exemptions are not restricted to religious beliefs.

"...[w]hether these desired health effects are the direct result of school vaccination requirements is more difficult to ascertain." We are aware of only one other study, Davis and Gaglia (2005), that has formally looked at the association between state mandates and immunization levels. Using cross-sectional NIS data from 2002, Davis and Gaglia (2005) establish a significant correlation between existence of a state mandate and varicella vaccination rates among children aged 19-35 months, controlling for child and parental characteristics. Davis and Gaglia (2005) do find that earlier mandates lead to higher vaccination rates, but they are unable to investigate the dynamics of the mandate effects with a single year of immunization data.

In contrast, using an approach proposed by Wolfers (2006) for longitudinal data, we are able to estimate mandate effects in the year of mandate introduction as well as subsequent years, while controlling for unobserved state characteristics. Through the use of a model with flexible time effects, we are able to disentangle aggregate immunization trends from mandate effects. Importantly, we can also formally check whether mandates have an "effect" prior to their introduction; if we see such an effect, it would indicate that the mandate introduction itself is related to existing trends in vaccination rates.[6]

The outline of the paper is as follows. Section 2.1 provides background information on the varicella vaccine and details on the available immunization and mandate data. Descriptive statistics for the NIS sample, along with vaccinated and unvaccinated subsamples, are provided. Section 2.2 presents the main results of the paper. Using both state-level and individual-level data from the NIS, we find significant causal effects of mandates upon vaccination rates among preschool children aged

[6]Alternatively, it could be that vaccinations begin rising in anticipation of the mandate introduction. As a preview of the results, the state-level findings in Section 2.2 suggest no such pre-mandate effects, so neither explanation seems to be a particularly important factor for varicella vaccination rates.

19-35 months. These effects appear in the year of mandate adoption, peak two years after adoption, and exhibit a minimal difference from the aggregate trend about six years after adoption. For a mandate enacted in 2000, the model and estimates imply that 22% of the increase in state-level immunization rates was caused by the mandate introduction. To examine whether mandates have differential effects depending on socioeconomic status, we also conduct the analysis separately for subgroups based upon race, education, and income. We find no evidence that varicella mandates contributed to a change in the health-status gap between individuals of different socioeconomic backgrounds. The results provide some evidence of differential effects in the year of mandate introduction, consistent with the idea that individuals with higher socioeconomic status are quicker to obtain and act upon information about mandates. Finally, using the estimated causal impacts of state mandates in conjunction with previous cost-benefit analyses of the varicella vaccine, we provide back-of-the-envelope calculations that suggest that the state mandates make sense from an economic standpoint. The mandate benefit is estimated to be \$5.70 per child. Section 2.3 concludes.

2.1 Background and data sources

In 1995, the United States introduced a universal childhood varicella vaccination program, with recommendations that all children between the ages of 12 months and 12 years receive one dose of the varicella shot.[7] Prior to the availability of the vaccine, approximately 4 million cases (15-16 per 1000 population) of varicella occurred annually nationwide (Wharton, 1996). Although varicella is considered by many to be a relatively mild childhood disease, approximately 270 per 100,000 cases

[7]There remains significant debate internationally over the need for universal childhood varicella vaccination. While several countries have adopted vaccination programs, there are many European countries that have delayed implementation of such programs (Bonanni et al., 2009).

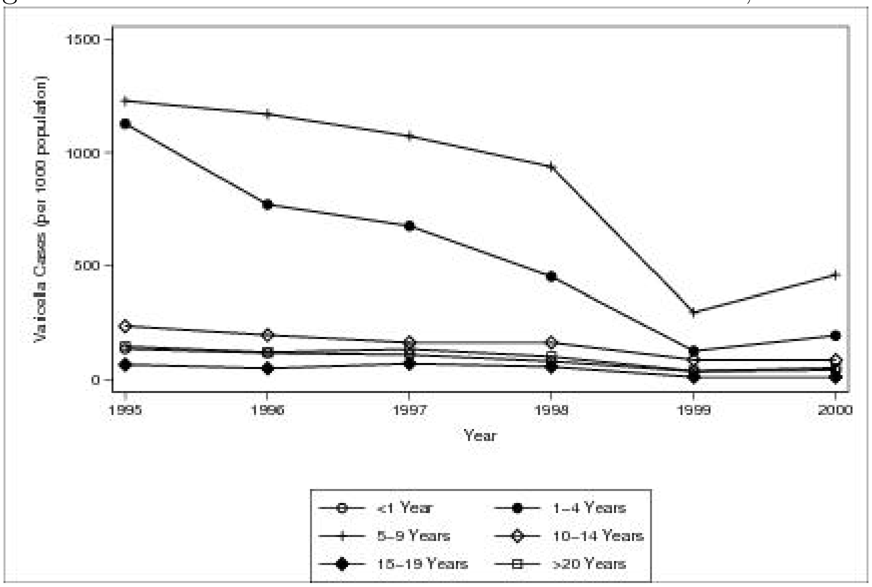
required hospitalization between 1988 and 1995 (Galil et al., 2002), and approximately 2.6 per 100,000 cases resulted in death between 1990 and 1994 (Meyer et al., 2000). The District of Columbia enacted the first varicella immunization mandate in 1997. By 2000, 19 states had either daycare- or school-entry mandates. As of the end of 2009, Idaho and Wyoming were the only remaining states without a daycare- or school-entry mandate.

Evidence exists of decreased varicella incidence since the introduction of the vaccine. Varicella is not a nationally reportable disease, which poses challenges to determining true disease incidence on a national level. However, in 1995 with the introduction of the vaccine, the CDC instituted active surveillance of varicella in three cities to better track varicella cases. Seward et al. (2008) report a decline in varicella cases between 1995 and 1996 and another decline in cases in 1999. Figure 2.1 presents the number of cases by age reported in one of the active surveillance sites between 1995 and 2000.[8] While varicella incidence declines for all age groups, the decline is largest for children between the ages of one to nine, who were specifically targeted by vaccine recommendations. Although uncommon, death is the most serious potential outcome of contracting varicella. In addition to decreasing the incidence of varicella, another goal of the vaccine is to reduce the severity of the disease if contracted. Nguyen, Jumaan, and Seward (2005) report that death rates due to varicella have declined since the universal vaccination program began, from 124 deaths in 1990 to 26 deaths in 2001, with the largest decline seen for adults.

Information regarding state varicella mandates was obtained from the Immunization Action Coalition. The complete list of mandate introduction dates, for both daycare-entry and school-entry mandates, is provided in Table 2.1. A daycare-entry

[8]Data taken from Seward et al. (2008). Incidence rates are for Antelope Valley, CA, one of the designated surveillance sites for varicella following the introduction of the vaccine.

Figure 2.1: Incidence of varicella from a surveillance site, 1995-2000



(school-entry) mandate requires that children be immunized prior to entry into a daycare (school) facility. While both types of mandates are often enacted at the same time by states, there are several instances where (i) states do not enact these mandates simultaneously (e.g., Missouri had a daycare mandate in 2001 and a school mandate in 2005) or (ii) states have enacted only one type of mandate (e.g., Nevada has a school mandate but no daycare mandate).

For data on immunizations, we use state and individual-level data from the 1996-2007 National Immunization Survey (NIS). The NIS is an annual survey conducted by the CDC. An unfortunate limitation of the NIS data is that immunization data are gathered only for children aged 19 to 35 months. Using the individual-level immunization survey data from the NIS, the National Center for Health Statistics constructs state-level varicella vaccination rates for children aged 19 to 35 months. The sample covers all 50 states and the District of Columbia.

Table 2.1: Varicella vaccination mandates, by state

	Year in Effect			Year in Effect	
State	Daycare	Elementary	State	Daycare	Elementary
Alabama	2000	2001	Montana	2006	
Alaska	2001		Nebraska	2004	2004
Arizona	2005	2005	Nevada		2003
Arkansas	2000	2000	New Hampshire	2003	2003
California	2001	2001	New Jersey	2004	2004
Colorado	2000	2000	New Mexico	2000	2002
Connecticut	2000	2000	New York	2001	2003
Delaware	2002	2003	North Carolina	2002	2006
Dist. of Columbia	1997	1997	North Dakota	2004	2004
Florida	2001	2001	Ohio		2006
Georgia	2000	2000	Oklahoma	1998	1998
Hawaii	2002	2002	Oregon	2000	2000
Idaho			Pennsylvania	1997	2002
Illinois	2002	2002	Rhode Island	1999	1999
Indiana	2003	2004	South Carolina	2000	2001
Iowa	2004	2004	South Dakota		2000
Kansas	2009	2004	Tennessee	1999	2002
Kentucky	2001	2001	Texas	2000	2000
Louisiana	2003	2003	Utah	2008	2002
Maine	2002	2003	Vermont		2008
Maryland	2000	2001	Virginia	1997	2002
Massachusetts	1998	1999	West Virginia	2000	
Michigan	2000	2002	Washington	2006	2006
Minnesota	2004	2004	Wisconsin	2001	2001
Mississippi	2002	2002	Wyoming		
Missouri	2001	2005			

Source: Immunization Action Coalition. Note: A blank cell indicates no mandate as of 2009.

Trends in immunization rates between 1996 and 2007 are shown in Figure 2.2. National rates are plotted along with the average immunization rates for states that enacted a school-entry mandate prior to 2001, the average for states that enacted a school-entry mandate after 2003, and the average for states that had not enacted a mandate prior to 2007.[9] The average vaccination rates for states with mandates prior to 2001 are higher than the national rate in 1999 through 2002. Although the average vaccination rate for states that enacted mandates after 2003 trailed the national rate since the introduction of the vaccine, the vaccination rate is roughly equivalent to the national rate in 2007 and also the average rate for states with mandates prior to 2001. In contrast, in 2007, the average vaccination rates for states that had not enacted a mandate by 2007 are about 10% lower than the national rate. Table A1 in the Appendix provides additional state-level vaccination rates in selected years.

Descriptive statistics for the NIS individual-level data are reported in Table 2.2, also broken down into subsamples based upon vaccination status. Of the full sample of 324,553 children, 64.8% have received the varicella vaccine, and for the subsample from 1999-2007, 4.8% have contracted varicella with a higher 13.2% among unvaccinated children.[10] Hispanic children have higher vaccination rates (69.1%) than whites (64.9%) and blacks (63.4%). Vaccination rates increase with mother's age: 65.6% for mothers younger than 20, 68.3% for mothers between 20 and 29, and 71.3% for mothers 30 and older. As expected, the vaccination rate is also higher for more educated mothers. The vaccination rate for children whose mothers have a college education is 69.3%, roughly 6-8 percentage points higher than the other education categories. All income levels below $50,000 have vaccination rates of around 60%, while the highest income bracket has a vaccination rate of 70.6%. At the time

[9] The reported averages are simple averages over the states, not population-weighted averages.

[10] The question for whether the child had contracted varicella became available in the NIS beginning in 1999.

Figure 2.2: Varicella vaccination rates in the United States, 1996-2007

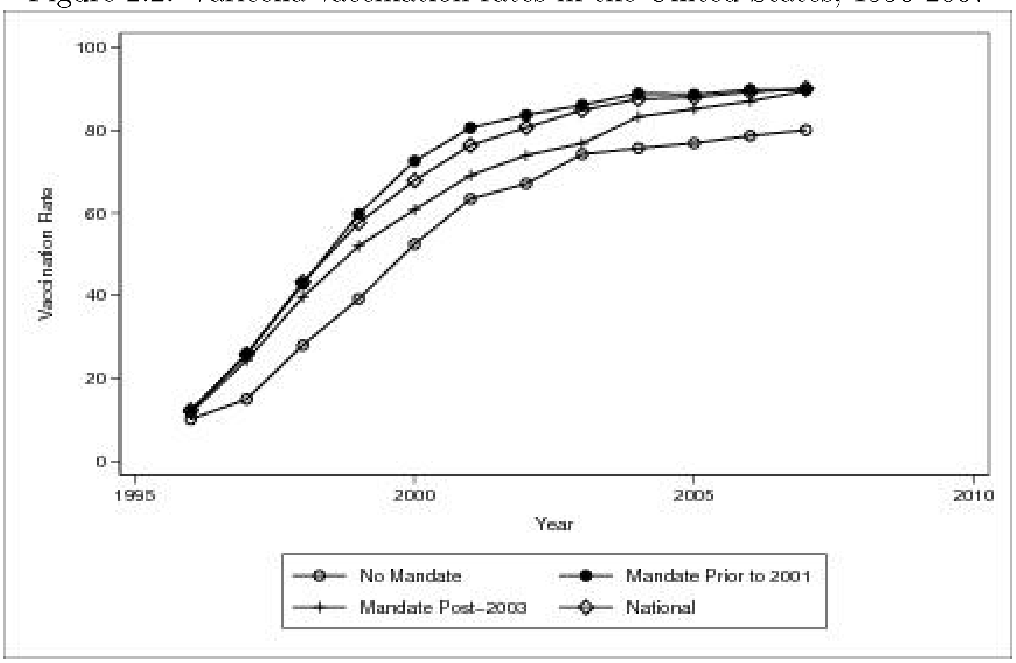


Table 2.2: Descriptive statistics, National Immunization Survey (1996-2007)

		Full sample (mean)	Vaccinated subsample (mean)	Unvaccinated subsample (mean)	Fraction of category vaccinated
Child	Vaccinated	0.648	1	0	1
	Had varicella*	0.048	0.026	0.132	
	Female	0.488	0.488	0.489	0.648
	Male	0.512	0.512	0.511	0.649
	Black	0.150	0.147	0.156	0.634
	Hispanic	0.179	0.190	0.157	0.691
	Other non-white race	0.073	0.077	0.066	0.682
	White	0.598	0.586	0.620	0.635
	Age: < 24 months	0.397	0.298	0.294	0.651
	Age: 24-29 months	0.353	0.356	0.346	0.655
	Age: 30-36 months	0.351	0.346	0.359	0.640
Mother	Age: < 20	0.023	0.023	0.023	0.656
	Age: 20-29	0.382	0.402	0.344	0.683
	Age: 30+	0.595	0.574	0.633	0.713
	Less than high school	0.113	0.110	0.121	0.626
	High school graduate	0.279	0.262	0.311	0.609
	Some college	0.213	0.207	0.224	0.630
	College graduate	0.394	0.421	0.344	0.693
	Income: <$10K	0.109	0.102	0.121	0.608
	Income: $10-20K	0.141	0.135	0.153	0.619
	Income: $20-30K	0.141	0.130	0.160	0.599
	Income: $30-50K	0.219	0.208	0.239	0.617
	Income: >$50K	0.390	0.424	0.326	0.706
Mandates	Daycare	0.496	0.605	0.295	0.791
	Elementary	0.442	0.543	0.256	0.796
	No Mandate	0.481	0.369	0.687	0.498
Sample Size		324,553	210,461	114,092	

* "Had varicella" item available only in years 1999-2007.

of their interview, 49.6% (44.2%) of children live in a state with a daycare-entry (school-entry) mandate in effect; 48.1% live in a state with no mandate in effect. Note that the no-mandate observations tend to be earlier within the sample, which partly explains the much lower vaccination rate of 49.8%.

2.2 Models and results

2.2.1 State-level regression model

In order to capture the full dynamic effect of school-entry mandates on vaccination rates, this paper follows the estimation strategy of Wolfers (2006).[11] Specifically, the following model is specified for state-level vaccination rates:

$$V_{st} = \sum_{k \in \mathcal{K}} \beta_k M_{st}^k + \theta_s + \theta_t + \epsilon_{st}, \tag{2.1}$$

where s indexes state, t indexes time, V_{st} is the vaccination rate, M_{st}^k is a dummy equal to one in the k^{th} period following the enactment of the mandate ($k = 0$ implies the mandate is enacted that period), θ_t are year indicator variables, and θ_s are state indicator variables. The state indicator variables θ_s allow for time-invariant unobservable state characteristics to affect vaccination rates, whereas the year indicator variables θ_t allow for a flexible specification of the aggregate trend in vaccination rates. β_k is interpreted as the impact of a mandate in state s at time t on the vaccination rate k periods following the mandate relative to states without mandates at time t. The set $\mathcal{K}$ indicates the number of leads and lags included in the model. For the results presented below, the set $\mathcal{K} = \{-2, -1, 0, 1, 2, 3, 4, 5, 6, 7\}$ requiring ten indicator variables is used.[12]

As pointed out by Wolfers (2006), the specification in (2.1) is preferred to a specification with a single mandate indicator since the results from the latter specification can confound pre-existing trends with the response of the vaccination rates to the

[11] Wolfers (2006) considered the effect of state-level unilateral divorce laws on divorce rates. See also Friedberg (1998). Divorce laws in his context are analogous to immunization mandates here, and divorce rates in his context are analogous to immunization rates here.

[12] Different choices for $\mathcal{K}$ yield very similar results, available upon request from the authors. The pre-mandate (negative k) indicator variables turn out to be insignificant regardless of the specification of $\mathcal{K}$, whereas post-mandate (positive k) indicator variables die out in significance past the fourth year.

vaccination mandates. Additionally, the dynamic effects of the policy are likely not captured by treating the policy as a structural break. To further assess the possible endogeneity of mandate introduction, we examined the relationship between introduction date and several state characteristics. Specifically, one might think that that states with low initial vaccination rates, high population density, a more educated population, or higher average income might implement mandates sooner compared with other states. In two regressions, one using measures from the 1990 Census and one using measures from the 2000 Census, we found that only high-school-diploma percentage was statistically significant in predicting earlier mandates. Since this variable should be well-captured by state fixed effects and also by individual mother data, we are not particularly concerned by this source of endogeneity.[13] Of course, we can not rule out the possibility of bias arising from state-level shocks in a given year, for example a widely publicized series of chickenpox deaths.

2.2.2 Individual-level regression model

The NIS individual-level data does not constitute a traditional panel dataset since each individual is interviewed only once. Nevertheless, we can still adopt the approach from the previous section in modeling individual-level vaccination since we can deal with unobserved state-level characteristics by including state indicator variables. Augmenting the specification in (2.1) with individual control variables X_i and using a linear-probability specification for the binary vaccination variable V_i, we specify the following model:[14]

$$\Pr(V_i = 1 | X_i, s(i), t(i)) = \sum_{k \in \mathcal{K}} \gamma_k M^k_{s(i)} + X_i \alpha + \delta_{t(i)} + \delta_{s(i)}, \tag{2.2}$$

[13]Detailed results are available from the authors.

[14]Probit estimation of the same model yields extremely similar results to the linear probability model. The latter has been used for greater ease of interpretation for marginal effects.

where i indexes individuals, $s(i)$ is individual i's state, and $t(i)$ is the year in which individual i was sampled. Like the state-level model, the specification in (2.2) allows for unobserved state heterogeneity, a flexible aggregate trend in vaccination rates, and a flexible dynamic specification for the state-level mandate effects. The vaccination indicator V_i is equal to one if individual i has ever received a varicella vaccination. The wording of the question regarding varicella vaccination in the survey is such that the data only records whether an individual has *ever* received a varicella vaccine. This might raise concerns that a portion of the individuals were vaccinated prior to mandate enactment and, therefore, the decision to vaccinate could not have been a consequence of the mandate. This limitation in the question wording should not impact the results substantially given the young ages in the sample (19-35 months) coupled with the fact that varicella vaccinations are not administered before a child is one year old.

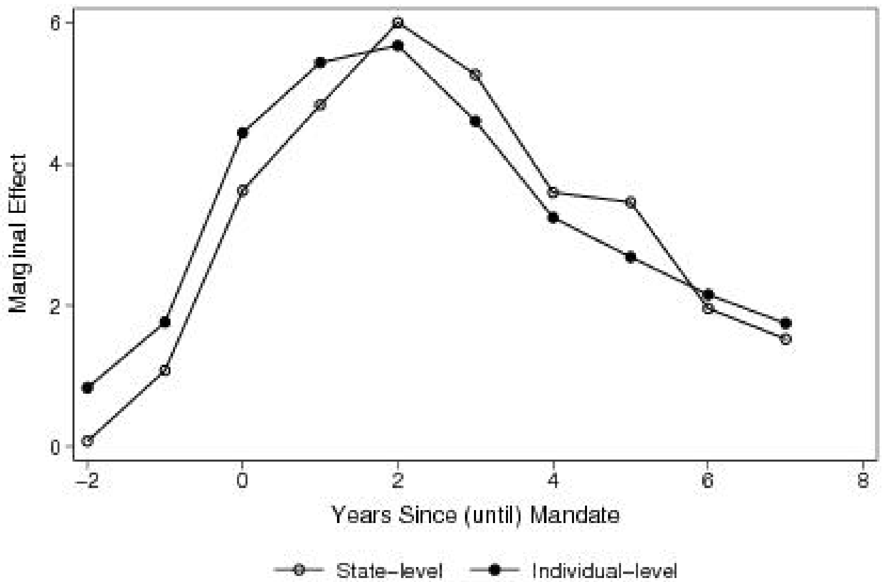
2.2.3 Results

2.2.3.1 Regression analysis

Given the high correlation between daycare-entry and school-entry mandate enactment, we define the mandate indicator variables as equal to one if *either* of the two possible mandate types are in place.[15] The main estimation results are presented in Table 2.3. For all regressions, standard errors have been clustered at the state level. The first two columns present unweighted and population-weighted state-level results, and the last two columns present the individual-level results with a full set of control variables and only state/year controls. Note that all estimates are reported in percentage-point terms; for example, looking at the first column, the effect in the

[15] Specifications which defined the mandate indicator variables (i) using only school-entry mandates, (ii) using only daycare-entry mandates, and (iii) jointly using both school entry and daycare entry mandates were also estimated. Results are available upon request.

Figure 2.3: Effect of mandates on vaccination rates



year of mandate introduction is a 3.87 percentage-point increase in the vaccination rate.[16] To help visualize the effects, the population-weighted state-level estimates and the full-control individual-level estimates are also plotted in Figure 2.3.

For the state-level regressions, there is no evidence of statistically significant pre-mandate effects, and the mandate effects appear in the year of the mandate introduction. For the individual-level regression, the one-year pre-mandate effect is marginally significant at the 10% level. Given that most states announce vaccination requirements at least one year in advance, this effect might indicate that individuals vaccinate one year prior to the mandate in anticipation of the upcoming school year. Even with the one-year pre-mandate effect, the mandate effect is significantly larger

[16]For the individual-level results, the effects are interpreted as percentage-point effects on an individual's vaccination probability.

Table 2.3: Effect of mandates on vaccination rates, regression results

	State-level regressions		Individual-level regressions	
	Unweighted	Population Weighted	Full Controls	State, Year Controls
2 years	-0.06	0.07	0.83	0.74
pre-mandate	(0.90)	(1.77)	(0.92)	(0.92)
1 year	0.51	1.08	1.76*	1.68*
pre-mandate	(1.04)	(1.57)	(0.97)	(0.97)
Year of mandate	3.87***	3.63***	4.45***	4.31***
introduction	(1.14)	(1.07)	(1.15)	(1.13)
1 year	5.35***	4.85***	5.45***	5.34***
post-mandate	(1.29)	(1.23)	(1.26)	(1.23)
2 year	6.77***	6.01***	5.69***	5.55***
post-mandate	(1.39)	(1.09)	(1.33)	(1.3)
3 year	5.75***	5.28***	4.62***	4.53**
post-mandate	(1.42)	(1.06)	(1.40)	(1.36)
4 year	5.37***	3.60***	3.25**	3.14**
post-mandate	(1.56)	(1.06)	(1.54)	(1.51)
5 year	4.52**	3.46***	2.69	2.54
post-mandate	(1.58)	(0.97)	(1.68)	(1.65)
6 year	2.96*	1.96**	2.16	2.04
post-mandate	(1.55)	(0.84)	(1.7)	(1.66)
7 year	2.86	1.52	1.75	1.68
post-mandate	(1.77)	(1.20)	(1.68)	(1.63)

Notes: Each column reports results from a separate regression using data from the National Immunization Survey. Each estimate corresponds to a single indicator variable, defined relative to the year of mandate introduction. Estimates are reported in percentage points. The individual-level results are marginal effects from linear probability model regressions. All regressions include year and state indicator variables. The individual-level regressions also include the control variables listed in Table A3. Standard errors are clustered at the state level. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$.

in the year of the mandate introduction (p-value < 0.01). The mandate effects remain statistically significant for six years post-mandate for state-level regressions and four years post-mandate for individual-level regressions. The overall pattern evident in Figure 2.3 is an inverted U-shape, with the magnitude of the effects peaking 1-2 years after mandate introduction and weakening six to seven years after mandate introduction.

For the discussion of the magnitude of the effects, we focus upon the population-weighted state-level model (second column of Table 2.3). For the year of the mandate introduction, the causal effect is given by the change from the 1-year pre-mandate estimate ($\widehat{\beta}_{-1} = 1.08$) to the year-of-introduction estimate ($\widehat{\beta}_0 = 3.63$), which is an effect of a 2.55 percentage-point increase in the vaccination rate in the initial mandate year. Note that the post-mandate effects provided in the table are *not* cumulative; each estimate represents the difference from the overall trend in that given year. So, the 1-year post-mandate estimate of 4.85 percentage points (above trend) represents an additional 1.22 percentage-point increase relative to the initial-year effect. The 2-year-post-mandate effect, the peak, represents an additional 1.16 percentage-point increase in vaccination rates.

For the full-control specification of the individual-level model, the estimates associated with the non-mandate control variables, denoted X_i in equation (2.2), are reported in the Appendix in Table A2. As expected from the descriptive statistics in Section 2.2, vaccination is more likely among Hispanics and among children of more educated and wealthier mothers. Turning to the mandate effects in Table 2.3, the pattern of significance mirrors that of the state-level estimates. The estimates are significant at the 5% level in the year of mandate introduction and in each of the four years after mandate introduction. Although the magnitudes for the 5-year and beyond estimates are similar to those found in the state-level regressions, statistical significance disappears due to slightly higher standard errors for the individual-level regressions.

2.2.3.2 Effects of a hypothetical mandate

Since varicella immunization rates experienced a rapid increase across the United States during this time period even in states *without* mandates, it is important to determine how much of the increase in mandate states can be attributable to the mandates themselves. Given that the immunization-rate increases were steeper soon after the vaccine introduction between 1995 and 2000 relative to the post-2000 period, we consider the effects of hypothetical mandates at two different times. The results are summarized in Table 2.4, for a hypothetical mandate introduced in 1998 and a hypothetical mandate introduced in 2000.[17] The "State-level results" are based upon the population-weighted state-level results in Table 2.3, whereas the "Individual-level results" are based upon the individual-level results with full controls in Table 2.3. The estimated changes are reported using the year prior to mandate as the baseline, with the percentage of the overall increase attributable to the mandate also reported. To provide a complete view of both the short-run and long-run importance of mandates, the estimated effects are calculated for time horizons extending to seven years after mandate introduction.

The state-level estimates indicate that for a hypothetical 2000 mandate, 22.0% of the initial-year increase in vaccination rate is attributable to the mandate. The analogous estimate for a hypothetical 1998 mandate is 12.9%. The cumulative effects of the mandate for the first three years following the mandate are 15.0% for the 2000 introduction and 8.1% for the 1998 introduction. Overall, the results indicate

[17] Given that our mandate indicators were defined as either type of mandate, we make no distinction whether the hypothetical mandate is daycare-entry or school-entry.

Table 2.4: Effect of state mandates relative to time trends

	State-level Results			Individual-level Results		
	Change due to overall time trend	Change due to mandate	Percentage attributable to mandate	Change due to overall time trend	Change due to mandate	Percentage attributable to mandate
Hypothetical 1998 mandate:						
Change from 1997 to 1998	17.20	2.55	12.9%	15.13	2.69	15.1%
Change from 1997 to 1999	31.33	3.77	10.7%	26.78	3.68	12.1%
Change from 1997 to 2000	40.35	4.93	10.9%	33.79	3.93	10.4%
Change from 1997 to 2001	47.89	4.20	8.1%	39.99	2.85	6.7%
Change from 1997 to 2002	51.13	2.52	4.7%	41.75	1.49	3.4%
Change from 1997 to 2003	55.18	2.39	4.1%	43.86	0.93	2.1%
Change from 1997 to 2004	58.27	0.88	1.5%	45.50	0.39	0.9%
Change from 1997 to 2005	59.15	0.45	0.8%	44.46	-0.01	-0.0%
Hypothetical 2000 mandate:						
Change from 1999 to 2000	9.02	2.55	22.0%	7.01	2.69	27.7%
Change from 1999 to 2001	16.56	3.77	18.5%	13.21	3.68	21.8%
Change from 1999 to 2002	19.80	4.93	20.0%	14.97	3.93	20.8%
Change from 1999 to 2003	23.85	4.20	15.0%	17.08	2.85	14.3%
Change from 1999 to 2004	26.94	2.52	8.6%	18.72	1.49	7.4%
Chnage from 1999 to 2005	27.82	2.39	7.9%	17.68	0.93	5.0%
Change from 1999 to 2006	29.54	0.88	2.9%	7.63	0.39	4.9%
Change from 1999 to 2007	30.94	0.45	1.4%	5.69	-0.01	-0.3%

Notes: Results are based upon the population weighted state-level regressions and individual regressions with the full set of controls. The time-trend change is computed as the difference between adjacent year-indicator-variable coefficient estimates. The mandate change is computed as the difference between the appropriate mandate indicator variable estimates.

that the impact of the mandate is a *short-run* phenomenon. The importance of the mandate effect relative to the aggregate time trend, measured by the percentages in Table 2.4, is cut by more than a half by the fourth year after the mandate and disappears completely approximately six to seven years after the mandate.

The individual-level estimates in Table 2.4 are quantitatively similar to the state-level estimates, although the implied short-run mandate effects are slightly larger. For instance, the 2000 mandate is estimated to be responsible for 27.7% of the vaccination increase in the year of introduction relative to the state-level estimate of 22.0%.

The estimates imply that mandate effectiveness is a short-run phenomenon that lasts only a few years. Empirically, the aggregate time trends are substantially more important over a horizon of six years or more. These aggregate time trends reflect nationwide trends, specifically a greater acceptance of the safety and effectiveness of the varicella vaccine and also incorporation of the vaccine into standard pediatric care. These latter effects, even in the absence of mandates, appear to account for nearly all of the increased vaccination rates at a longer time horizon.

2.2.3.3 Implications for health equity

To supplement our model estimates based upon the full individual sample, we consider estimates based upon various subsamples in order to determine whether varicella vaccination mandates have contributed to health-quality disparities. The descriptive statistics of Section 2.2 indicate that certain socioeconomic groups (Hispanic, high income, high education) have higher vaccination rates. An important question is whether or not vaccination mandates have had a differential impact on different socioeconomic groups. Whether mandates would widen or narrow the health-status gap is *ex ante* unclear. If fully enforced, mandates could narrow the health-status gap by forcing individuals to obtain vaccinations when they would not have previously. Mandated vaccines are recognized as the medical standard of care, which makes them eligible for private insurance coverage, and many clinics administer the vaccine at low cost or free. Additionally, low income children have access to the Vaccines for Children (VFC) program, which provides vaccines at no cost to children who might not otherwise be vaccinated due to inability to pay. On the other hand, the mandates may widen the health-status gap if certain groups are more affected by them. Such a differential effect could arise if a group is more likely to utilize day care and be subject to the day-care mandate. Also, if mandates are not well enforced, certain groups may be more likely to ignore the mandate.

To look at the possibility of differential effects due to socioeconomic or demographic status, we consider three different splits of the sample: (i) white versus non-white, (ii) low-income versus high-income households (using a $50,000 cutoff for household annual income), and (iii) non-college educated mothers versus college-educated mothers. The subsample estimates in Table 2.5 are presented as hypothetical mandate estimates, similar to the full-sample results in Table 2.4. The results do not indicate substantial differences between the effects for the various subsamples, especially after the year of mandate introduction. There are some differences that arise in the year of the mandate introduction, with slightly higher vaccination take-up rates for the white, more educated, and higher income subsamples. For instance, the college-educated mother subsample has a 3.09 percentage-point increase in vaccination in the year of mandate introduction as compared to a 2.44 percentage-point increase for the non-college-educated mother subsample; for the 1998 hypothetical mandate, the mandate accounts for 18.2% of the vaccination increase experienced by the college-educated mother subsample as compared to 13.3% for the non-college-educated mother subsample.

The results provide some evidence of differential effects in the year of mandate introduction, which is consistent with the idea that individuals with higher socioeconomic status are quicker to obtain information about mandates and also to act upon this information. Overall, however, the results do not indicate that the vaccination mandates led to a significant widening or narrowing of the health-status gap in the years following mandate introduction.

2.2.3.4 Estimated benefit of the mandates

While the results above provide strong evidence that vaccination mandates were indeed effective at raising vaccination rates, an economic justification for vaccine mandates requires that the associated costs are outweighed by the associated benefits. To provide a back-of-the-envelope analysis of the benefits associated with the vaccination mandates, we combine the estimated causal effects from above with previous cost-benefit estimates for the varicella vaccine. Our vaccine cost-benefit estimates are based upon Zhou et al. (2008), as it is the most recent and comprehensive study available.[18]

We consider the economic benefit associated with a mandate for a single nationwide birth cohort, assumed to be 4.1 million individuals as in Zhou et al. (2008). For the mandate's causal effect, we use the year-of-mandate estimate of 2.7 percentage points for the individual-level regression with full controls.[19] This mandate differential of 2.7 percentage points is assumed to persist throughout the lifetime. The following additional assumptions are made:

[18]Results based upon the earlier Zhou et al. (2005) are quantitatively similar, but Zhou et al. (2008) has the advantage of breaking down costs and benefits into disease-related and vaccine-related categories.

[19]From Table 2.3, this estimate is obtained by subtracting the 1.76 1-year-pre-mandate estimate from the 4.45 year-of-mandate estimate.

Table 2.5: Effect of state mandates — subsample results

		Non-white			Non-college educated mothers			Low income	
	Change due to overall time trend	Change due to mandate	Percentage attributable to mandate	Change due to overall time trend	Change due to mandate	Percentage attributable to mandate	Change due to overall time trend	Change due to mandate	Percentage attributable to mandate
Hypothetical 1998 mandate:									
Change from 1997 to 1998	17.12	2.39	12.3%	15.85	2.44	13.3%	15.90	2.66	14.3%
Change from 1997 to 1999	28.23	3.68	11.5%	27.40	4.10	13.0%	27.99	4.09	12.7%
Change from 1997 to 2000	34.80	3.56	9.3%	34.95	4.62	11.7%	35.25	4.65	11.7%
Hypothetical 2000 mandate:									
Change from 1999 to 2000	6.57	2.39	26.7%	7.55	2.44	24.4%	7.27	2.66	26.8%
Change from 1999 to 2001	12.23	3.68	23.1%	14.14	4.10	22.5%	14.03	4.09	22.6%
Change from 1999 to 2002	12.92	3.56	21.6%	15.64	4.62	22.8%	14.99	4.65	23.7%

		White			College-educated mothers			High-income	
	Change due to overall time trend	Change due to mandate	Percentage attributable to mandate	Change due to overall time trend	Change due to mandate	Percentage attributable to mandate	Change due to overall time trend	Change due to mandate	Percentage attributable to mandate
Hypothetical 1998 mandate:									
Change from 1997 to 1998	13.99	2.91	17.2%	13.84	3.09	18.2%	13.18	2.80	17.5%
Change from 1997 to 1999	25.88	3.73	12.6%	25.58	3.11	10.8%	23.75	3.17	11.8%
Change from 1997 to 2000	33.13	4.41	11.7%	31.52	2.98	8.7%	30.03	3.18	9.6%
Hypothetical 2000 mandate:									
Change from 1999 to 2000	7.25	2.91	28.6%	5.94	3.09	34.2%	6.28	2.80	30.8%
Change from 1999 to 2001	13.85	3.73	21.2%	11.38	3.11	21.4%	11.43	3.17	21.7%
Change from 1999 to 2002	16.36	4.41	21.2%	13.46	2.98	18.2%	14.39	3.18	18.1%

- The pre-mandate vaccination rate is 88%, the national vaccination rate in 2005.
- 15.5% of the vaccinated population contracts varicella (the median rate in the twenty studies reviewed by Marin et al. (2008)).
- 99.62% of the unvaccinated population contracts varicella (based upon Zhou et al. (2008) and consistent with Wharton (1996)).
- The varicella contraction rates are the same with and without the mandate. This assumption will lead to a conservative benefit estimate as it ignores potential herd-immunity effects with increased vaccination rates.
- The cost per varicella case is $439.98 (Zhou et al., 2008). This amount includes direct costs, such as hospitalization, drug claims, and outbreak management costs, and indirect costs, such as value of life lost and work-days missed.
- The cost per vaccination is $159.14 (Zhou et al., 2008). This amount includes the actual cost of the vaccine, travel time, and possible medical complications.

Table 2.6 presents a summary of the estimated benefits associated with the varicella mandate for a single birth cohort. For a nationwide mandate, we estimate a net decline of 93,121 varicella cases within the cohort. While the vaccine-related costs increase by over $17.62 million due to the increase in vaccinations, these costs are more than offset by the decline in disease-related costs of $40.97 million. The net benefit associated with the mandate for this cohort is estimated to be $23.35 million or $5.70 per child.

While there are no data available on the costs of state-level mandate implementation, the mandate-benefit calculation provides an upper bound on costs that would make such a mandate economically worthwhile. If one applies the per-child

Table 2.6: Benefits of a varicella mandate for a single birth cohort

	Pre-Mandate	Post-Mandate	Change
Cohort size	4,100,000	4,100,000	
Vaccination rate	88.0%	90.7%	+2.7%
Total vaccinated	3,608,000	3,718,700	+110,770
Total varicella cases	1,049,370	956,250	−93,121
Cases in vaccinated population	559,240	576,399	+17,159
Cases in unvaccinated population	490,130	379,851	−110,279
Cost of varicella cases	$461,701,989	$420,730,681	−$40,971,307
Cost of vaccinations	$574,177,120	$591,793,918	+$17,616,798
Decrease in costs due to mandate			$23,354,509
Decrease in costs due to mandate (per child)			$5.70

The mandate effect of 2.7 percentage points is based upon the individual-level results in Table 2. Varicella contraction probabilities of 99.62% and 15.5% for unvaccinated and vaccinated individuals, respectively, are assumed. The cost-per-case and cost-per-vaccine values of $439.98 and $159.14 are based upon Zhou et al. (2008).

benefit of $5.70, the benefit for California, with roughly 550,000 births in 2005, is roughly $3.1 million whereas the benefit for Maine, with roughly 14,000 births in 2005, is roughly $80,000. Recall that these estimates are for a *single* birth cohort. Given that an administrative infrastructure already exists in every state for other vaccination mandates, the only costs associated with the varicella-vaccine mandate are those required to change the school-entry forms and to inform the medical community and public about the new requirement.[20] These costs are short-term costs, whereas the benefits accruing to the mandate can apply to successive birth cohorts. If the same benefit applied to five cohorts, for instance, the benefit to California would be roughly $15.5 million and the benefit to Maine roughly $400,000. It seems likely that the marginal cost of mandate implementation is quite a bit less than these estimates. Moreover, the benefit estimates should be viewed as conservative since they ignore possible herd-immunity effects and are based only upon data from 19-35 month-old children.

[20]Costs due to increased vaccinations are already part of the mandate benefit calculation above.

2.3 Conclusion

This study is the first to formally estimate the causal effects of state-level vaccination mandates upon immunization rates within the United States. Using longitudinal immunization data and the differential timing of state-mandate introduction, we establish that state-level mandates were indeed successful at increasing varicella immunization rates, above and beyond the aggregate upward trends in immunization rates observed throughout the country after introduction of the varicella vaccine in 1995. The mandate effects are, however, somewhat short-lived with the peak effectiveness occurring roughly two years post-mandate; after about six years, the causal impact of mandates dies out.

It is worthwhile to compare our causal mandate-effect estimates with the results in Davis and Gaglia (2005). For cross-sectional NIS data in 2002, Davis and Gaglia (2005, Table 4) report roughly a 7-9 percentage-point higher immunization probability for children in mandate states than children in non-mandate states. This "raw" difference can be compared to the individual-level mandate effects reported in Table 2.4, in which we find that the short-run causal effects of the mandates are on the order of 2-4 percentage points. The mandates themselves, therefore, account for a little under half of the observed difference in 2002 immunization rates.

There are a few limitations to the current study. First, the results are specific to the varicella vaccine. Although we would expect the pattern of the mandate effects to be similar for other childhood vaccines, the magnitude of mandate effects for a specific vaccine will depend on several factors, including parents' perceptions of the relative costs and benefits of the vaccine for their child. Whereas many parents viewed varicella as a relatively mild affliction and may have been hesitant to vaccinate their child without a mandate, more serious diseases like the recent H1N1 influenza virus might not require mandates. Second, although we have identified significant effects from state mandates, we do not know the channels by which this effect is transmitted. Mandates could be effective by raising public awareness or doctors' awareness even in the absence of the compulsory nature. Insurance companies might react to mandates by offering coverage for newly mandated vaccines. In addition to examining the specific channels that influence mandate effectiveness, future research could take a more detailed look at the extent to which local enforcement practices and exemption policies affect mandate effectiveness. Finally, a more convincing cost-benefit analysis of vaccine mandate effectiveness will require more detailed information on the costs associated with mandate implementation. State-level data on such costs would complement the mandate-benefit estimates that we have provided.

Chapter 3

Access to Emergency contraception and its impact on fertility and sexual behavior

Half of all pregnancies in the United States are unintended — in 2001, 3.1 unintended pregnancies occurred (Finer, 2006). Although Emergency Contraception (EC) is not as effective as regular birth control methods, the availability of EC in the United States provides women with the opportunity to prevent unindented pregnancies in the absence of regular birth control or if regular birth control fails. Despite its availability through prescription, changing EC to non-prescription status has been a contentious issue in the US. The debate regarding over the counter status and the FDA's reluctance to approve over the counter access was the catalyst for the Center for Reproductive Rights filing a suit in 2005 against the FDA claiming that the agency's failure to approve Plan B for over the counter access impermissibly denied women access to EC. Indeed, the judge's opinion in the case, stating that the FDA had "acted in bad faith and in response to political pressure" highlights how politically charged the issue has been.[1]

To date, the impact of access to EC has primarily been studied through randomized control trials (RCT). Raymond et. al. (2007) provide a review of all studies related to EC, with particular emphasis on studies that examine the impact of increased access. Across 23 separate studies, 14 of which were RCT, there is no evi-

[1] Much of the political debate stems from the misconception that EC causes an abortion, such that EC gets swept up with the abortion debate.

dence of differences between intervention and control groups in pregnancy or abortion rates, however, there is consistently evidence of higher EC use among intervention groups. Girma and Paton (2006, 2011) examine the impact of the introduction of local pharmacy schemes in which EC is provided over-the-counter (OTC) to teenagers by pharmacies in England. Using both matching techniques (2006) and difference in difference (2011), they find no causal link between free OTC access to EC and teen conception rates. However, they do find evidence that introduction of free EC schemes is positively related to sexually transmitted infection (STI) diagnoses among teenagers.

This study takes advantage of variation in EC legislation across states over time to determine the effect of over the counter access to EC in the United States on state-level abortion rates, birth rates, and risky sexual behavior, which is measured by sexually transmitted disease (STD) rates. Additionally, the paper examines whether there are differential effects across various age and race groups — effects for teenagers are of particular interest since currently individuals must be 17 or older to obtain EC without a prescription in the majority of the US. In addition to aggregate data, individual-level analysis is performed with the NLSY, which has the benefit of being able to link sexual behaviors to other individual characteristics such as income, marital status and education. The expected effect of less restrictive access to EC on pregnancies is ambiguous. Over the counter access to EC can be thought of as an additional form of insurance against contraceptive failure or non-use. As a result, individuals might engage in increased levels of risky sexual behavior. The decision whether to actually use EC following risky sex may be different than the individual expected to take when deciding to engage in risky sex. Therefore, it is possible that easier access to EC might actually increase pregnancies.

This paper is the first to study the effect of increasing EC access for the

United States on a population level.[2] Indeed, in their review of existing EC studies, Raymond et. al. (2007) point out that to date no study has showed that increased access reduces unintended pregnancy or abortion rates on a population level. The results are of particular interest from a policy standpoint since the importance of over the counter access to EC in the US, particularly for minors, is still part of the political debate.

The aggregate results indicate that over the counter access to EC decreases births by 5.4% relative to baseline and increases STDs by 12.5% relative to baseline. The higher STD rates indicate that individuals are engaging in more risky sexual behavior. The impact of over the counter access to EC is larger for adults compared with teenagers: births (STDs) decrease (increase) by 4.6% (17.2%) for adults compared with 1.5% (2.2%) for teenagers. The dynamic specification is consistent with EC legislation having effects beyond the year of introduction: the effects on both birth rates and STD rates remain significant four years following the over the counter switch. Individual-level results using the NLSY also find an increase in various measures of risky sexual behaviors as a result of over the counter EC legislation.

Section 3.2 provides a background on the institutions regarding EC, section 3.3 presents the data, section 3.4 presents the estimation strategy, section 3.5 presents the results, and section 3.6 concludes.

3.1 Institutional Background

The first product specifically marketed as an emergency contraceptive in the U.S. (Preven) received approval in the fall of 1998; the second EC product, Plan B,

[2] As I neared the completion of this chapter, it was brought to my attention that Zuppann (2011) has a similar working paper. A comparison of our results can be found in the conclusion section.

became available in 1999. Initially, EC could only be obtained with a prescription. EC has been shown to be most effective up to 72 hours following uprocteded sex, but more recent evidence indicates some degree of effectiveness up to 5 days after unprotected sex. The need for a prescription (which requires scheduling and visiting a physician in addition to going to a pharmacy) can cause delay in access to EC following unprotected sex, and therefore generates the potential for decreased effectiveness of EC. In 2003, numerous medical groups including the American Medical Association and the American College of Obstetricians and Gynecologists, indicated their support for switching Plan B to OTC status to the FDA. By this time, non-prescription access to EC was already available in many countries, including the United Kingdom, Portugal, and Finland.

In December 2003, an FDA advisory committee voted to switch Plan B from prescription to OTC status, but the FDA rejected the switch in 2004.[3] In August 2005, the FDA announced Plan B was safe for OTC use by women 17 and older, but they also announced they would delay a final decision indefinitely.[4] The FDA approved the switch of Plan B to OTC status for individuals aged eighteen and older in August 2006; Plan B would be kept behind the pharmacy counter but would be available without a prescription. The age restriction was decreased to seventeen in 2009.

While access to EC for minors is still by prescription only, minors still face fewer barriers to access because they can legally have someone older than 17 purchase EC for them. Prior to the FDA switching Plan B to OTC status in 2006, 9 states

[3]The vote tally was 23-4 in favor of the switch.

[4]No medical reasons necessitate EC to be prescription only products for individuals of any age. The FDA cited the following issues for their delay: whether Plan B could be both prescription-only and OTC depending on age, whether prescription and OTC versions of the same drug could be marketed in the same package, and whether the age restriction could be enforced.

Table 3.1: EC Legislation, by state

State	Year in Effect
Washington	1998
California	2002
Alaska	2003
Hawaii	2003
New Mexico	2003
Maine	2004
Massachusetts	2005
New Hampshire	2006
Vermont	2006

Source: National Conference of State Legislatures, 2011.

passed initiatives which allowed individuals of any age to obtain EC directly from a pharmacist without a prescription. In 1998, Washington was the first state to pass such legislation, and eight other states subsequently passed legislation.[5] Table 3.1 presents the states with over the counter EC legislation as well as the year the law took effect.

3.2 Data

Data on state-level EC legislation were obtained from the National Conference of State Legislatures (NCSL). Aggregate data represent state-level data for the years 1993-2007. Currently 2007 is the most recent year of data available for abortions and STDs. The three dependent variables considered are the birth rate, the abortion rate, and the STD rate, which is a measure of risky sexual behavior. Means for the

[5]State-level legislation supersedes the FDA ruling. As a result, minors in those states can still obtain EC over the counter.

aggregate outcomes are given in Table 3.2.

Birth rates are collected from the Center for Disease Control (CDC) vital statistics website, and can be separated by various age and race categories. The birth rate is given as the number of births per 1000 women aged 15-44.[6] The abortion rate represents the number of abortions performed in a state per 1000 women aged 15-44 who reside in that state. These data are collected annually by the CDC, however some states do not report abortion statistics to the government.[7] As with the birth data, abortion data can be separated by various age and race categories. Population estimates from the Census Bureau are used to calculate rates for subgroups, since these data are reported in totals rather than rates. Data for select STDs (chlamydia, gonorrhea, and syphilis) are collected by the CDC annually. The three rates are combined to generate a composite STD rate (per 1000 women), which serves as a measure of risky sexual behavior. STD rates can also be further broken down into various age and race categories.

A placebo test is conducted using data from 1993-1997 (all years in the sample prior to the first otc legislation). The "pre-otc" period includes 1993-1995, and the "post-otc" period includes 1996-1997; results are shown in Table 3.3.[8]

3.2.1 Individual-level data: NLSY 1997

In order to further examine the impact of over the counter EC access on sexual behavior, individual-level analysis is performed using the 1997 National Longitudinal

[6]For regressions involving age or race subgroups, the rate is calculated as the number of births per 1000 women within that subgroup.

[7]Specifically, starting in 1998, California and New Hampshire do not report abortion statistics. Alaska, Louisiana, Oklahoma, and West Virginia do not report in all years, but they report in the majority of the years.

[8]The STD data is not available until 1996. A placebo test is conducted using 1996-1997 data, as well as 1996-1998 data. The results from the 1996-1998 data are shown in the table.

Table 3.2: Abortion, Birth, and STD Rates (per 1000 women aged 15-44) by EC Legislation, 1993-2007 (CDC data)

	Full Sample	Pre-FDA States			FDA States		
		Pre-OTC	Post-OTC	Difference	Pre-OTC	Post-OTC	Difference
Abortion Rate	16.75	25.05	17.45	-11.48	16.29	15.74	-0.96***
	(1.54)	(5.94)	(1.20)	(6.91)	(0.53)	(1.16)	(0.28)
Birth Rate	14.05	14.27	13.33	-0.789	14.04	14.20	0.04
	(0.33)	(1.07)	(0.37)	(0.456)	(0.11)	(0.26)	(0.10)
STD Rate	6.76	3.95	5.71	2.01***	6.75	8.06	1.35***
	(0.29)	(0.44)	(0.75)	(0.355)	(0.11)	(0.24)	(0.11)

Means are population weighted with standard deviations in parenthesis. Pre-FDA states includes the nine states that passed OTC EC legislation prior to the FDA ruling in 2006. FDA states includes the 42 states that have not passed legislation but were affected by the FDA ruling. STD data is available beginning in 1996. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$.

Table 3.3: Placebo Test: Abortion, Birth, and STD Rates (per 1000 women aged 15-44) by EC Legislation, 1993-1997 (CDC data)

	Pre-FDA States			FDA States		
	Pre-OTC	Post-OTC	Difference	Pre-OTC	Post-OTC	Difference
Abortion Rate	32.1	31.4	-0.70	17.39	17.74	-0.35
	(5.67)	(5.39)	(3.33)	(1.68)	(1.89)	(1.89)
Birth Rate	16.1	15.6	0.50	14.3	14.16	0.14
	(0.97)	(0.87)	(1.36)	(0.3)	(0.32)	(0.79)
STD Rate	4.11	4.59	-0.48	5.57	6.36	-0.79
	(0.31)	(0.49)	(0.89)	(0.16)	(0.35)	(0.71)

Means are population weighted with standard deviations in parenthesis. Pre-FDA states includes the nine states that passed OTC EC legislation prior to the FDA ruling in 2006. FDA states includes the 42 states that have not passed legislation but were affected by the FDA ruling. STD data is available beginning in 1996. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$.

Table 3.4: Sexual Behavior Summary Statistics, NLSY 1997

Full Sample, 1997-2009:					
	Mean	Std. Dev.	Min	Max	N
Had sex ever	0.645	0.479	0	1	38,567
Had sex in past 12 mos.	0.728	0.445	0	1	31,890
No. of sexual encounters in past 12 mos.	128.7	208	0	999	25,431
No. of times used condom in past 12 mos.	33	98.5	0	999	24,627
Risky sex	0.003	0.054	0	1	28,151
Individuals in states with otc EC access:					
	Mean	Std. Dev.	Min	Max	N
Had sex ever	0.837	0.37	0	1	15,630
Had sex in past 12 mos.	0.855	0.352	0	1	15,139
No. of sexual encounters in past 12 mos.	146.6	213.6	0	999	10,426
No. of times used condom in past 12 mos.	30.5	96.9	0	999	10,275
Risky sex	0.003	0.052	0	1	13,870

Statistics are calculated using sample weights. Individuals in states with otc EC access includes states that passed legislation prior to the FDA ruling and state that did not. Risky sex is equal to one if the individual has had sex with either an IV-drug user or someone with a stranger in the past 12 months. Sample sizes vary by outcome due to variations in nonresponse.

Survey of Youth (NLSY). The NLSY interviewed 4385 women aged 12 to 18 in 1997 and has reinterviewed them annually through 2009. Five outcomes related to sexual behavior are examined: whether the individual has ever had sex ever, whether the individual has had sex in the past twelve months, the number of sexual encounters in the past twelve months, the number of times the individual used a condom in the past twelve months, and whether someone has had sex with either a stranger or an IV-drug user in the past twelve months (denoted "risky sex"). Table 3.4 gives summary statistics for the full NLSY sample as well as for the subsample of individuals in states with over the counter EC access.

3.3 Estimation Strategy

Since only nine states passed over the counter EC legislation prior to the FDA ruling, the preferred dynamic specification (presented below) relies on nine states to identify the estimates. In order to take advantage of more data for identification (this includes both the nine states with legislation and the remaining states that were affected by the FDA ruling), a simple framework is examined first:

$$Y_{st} = \beta OTC_{st} + X_{st}\gamma + \theta_s + \theta_t + \epsilon_{st} \tag{3.1}$$

where s indexes state, t indexes time, Y_{st} represents the abortion rate, birth rate, or STD rate, $Mandate_{st}$ is an indicator equal to one if there is over the counter EC access in state s at time t, and X_{st} is a control for median state income.

In order to capture the full dynamic effect of changes in over the counter access to EC, this paper follows the estimation strategy of Wolfers (2006). Specifically, the following model is specified for state-level abortion rates, birth rates, and STD rates:

$$Y_{st} = \sum_{k \in \mathcal{K}} \beta_k OTC_{st}^k + X_{st}\gamma + \theta_s + \theta_t + \epsilon_{st}, \tag{3.2}$$

where s indexes state, t indexes time, Y_{st} represents the abortion rate, birth rate, or STD rate, OTC_{st}^k is a dummy equal to one in the k^{th} period following the switch from prescription-only to OTC status for EC ($k = 0$ implies the switch took place that period), X_{st} is a control for median state income, θ_t are year indicator variables, and θ_s are state indicator variables. The state indicator variables (θ_s) allow for (time-invariant) unobservable state characteristics to affect the outcome variable of interest, whereas the year indicator variables (θ_t) allow for a flexible specification of the aggregate trend in the outcome variable of interest. β_k is interpreted as the impact of a switch to OTC status in state s at time t on the abortion/birth/STD rate

k periods following the switch (relative to states without OTC access at time t). The set $\mathcal{K}$ indicates the number of leads and lags included in the model. For the results presented below, the set $\mathcal{K} = \{-4, -3, -2, -1, 0, 1, 2, 3, 4\}$ (requiring nine indicator variables) is used.[9]

As pointed out by Wolfers (2006), the specification in (3.2) is preferred to a specification with a single prescription to OTC switch indicator since the results from the latter specification can confound pre-existing trends with the response of abortion rates to the OTC switch. Additionally, the dynamic effects of the policy are likely not captured by treating the policy as a structural break. To further assess the possible endogeneity of switching to OTC status, I examine the relationship between the presence of OTC EC legislation and several state characteristics. For example, one might think that that states with high abortion rates or high birth rates prior to the availability of EC or a more educated population might make EC available OTC sooner compared with other states. Results are presented in Table 3.5.

In order to rule out the dynamic model picking up pre-existing trends in fertility outcomes and risky sexual behavior, controls for the trends in the abortion, birth, and STD rate are included in one specification, and a second specification includes average levels of fertility outcomes and risky sexual behavior. The 'other birth control legislation' variable refers to whether or not a state has mandated contraception insurance coverage, which could be an indication of the importance a state places on contraceptive legislation. None of the state-level characteristics are significant predictors of the presence of over the counter EC legislation, indicating that pre-existing trends in fertility outcomes and risky sexual behavior can be ruled out as potential

[9]Different choices for $\mathcal{K}$ yield very similar results, which are available upon request from the author. However, due to the limited number of observations, estimates of lag terms beyond K=4 are unreliable.

Table 3.5: Endogeneity Check: EC Legislation Presence

Dependent variable = 1 if state has OTC EC legislation prior to FDA ruling				
Variable	Estimate	Std. Error	Estimate	Std. Error
Pct. with high school diploma	0.014	0.016	0.01	0.017
Pct. with college degree	0.002	0.018	-0.001	0.018
Pct. vote Bush in 2000	-0.004	0.008	-0.013	0.008
Other birth control legislation	0.122	0.105	0.104	0.071
Trend in abortion rate	0.054	0.089		
Trend in birth rate	-0.869	0.534		
Trend in STD rate	-0.028	0.034		
Average lagged abortion rate			-0.008	0.008
Average lagged birth rate			-0.005	0.031
Average lagged STD rate			-0.022	0.019

The variables are averages for the 2000s. Results are similar using data from the 1990s. The first regression examines whether trends in fertility outcomes predict the presence of legislation, and the second regression examines levels of fertility outcomes.

sources of endogeneity. However, it should be noted that it is not possible to rule out state-level shocks in a given year as a potential source of bias.

3.3.1 Individual-level models

For the NLSY dataset, models are estimated that are analogous to the model in (3.1) augmented with individual controls:

$$Y_i = \beta OTC_{s(i),t(i)} + X_i\gamma + \delta_{t(i)} + \delta_{s(i)} + \epsilon_{s(i),t(i)} \tag{3.3}$$

where i indexes individuals, Y_i is the sexual behavior outcome of interest, X_i is a vector of individual controls, $s(i)$ is individual i's state, and $t(i)$ is the year in which individual i was sampled.[10] Due to the small sample size of the NLSY in many of the state-year cells, the dynamic specification is not estimated. Additionally, this

[10]For binary outcome variables, both OLS and probit estimation was conducted. Since results are similar for both estimation techniques, LPM estimates are presented.

analysis does not exploit the panel nature of the data, but rather pools all available observations.

3.4 Results

Two specifications are estimated for the simple framework in (3.1): one which defines the over the counter switch indicator as equal to one if a state has legislation, and one which defines the over the counter switch indicator as equal to one if a state has legislation *or* if the FDA ruling is in place. Estimation for both specifications is performed on the following subgroups: teenagers, adults, individuals in their twenties, individuals in their thirties, whites, and blacks. The state-level estimation results for the single indicator model, as well as baseline outcomes and implied percentage changes, are presented in Tables 3.6 and 3.7.

Over the counter access to EC increases abortions 0.49% in the specification with states that have otc legislation and 1.19% for the specification with all states, however these effects are not significant. (OTC access to EC does not significantly affect abortion rates for any of the subgroups.) OTC access to EC has a relatively large effect on births: births decrease by 5.39% in the specification that only includes states with legislation, and births decrease by 4.2% in the specification with all states. The effects are strongest for adults, particularly for women in their twenties. The STD rate increases due to over the counter EC access, indicating that individuals are exhibiting more risky sexual behavior. These effects are much larger for adults (11.2%) compared with teenagers (2.6%). OTC access to EC does not significantly affect any of the outcomes for teenagers, however they have the same sign as for adults.

The results for the flexible time specifications are presented in Tables 3.8-3.11.

Table 3.6: Effect of over-the-counter EC legislation on state-level outcomes, Pre-FDA States

	Aggregate	Teenager	Adult	Aged 20-29	Aged 30-39	White	Black
Abortion Rate	0.097	0.236	0.045	0.231	-0.234	-0.305	-0.922
	(0.693)	(0.234)	(0.429)	(1.186)	(0.307)	(0.411)	(1.232)
Baseline	19.75	5.30	11.77	30.74	10.15	8.25	24.51
Implied pct.	0.49	4.45	0.38	0.75	2.31	3.70	3.76
Birth Rate	-0.768	-0.194	-2.306***	-7.705***	0.576	-2.790***	-3.544***
	(0.153)	(0.243)	(0.526)	(1.256)	(0.678)	(0.502)	(1.037)
Baseline	14.24	13.03	49.98	111.33	59.19	48.99	57.26
Implied Pct.	5.39	1.49	4.61	6.92	0.97	5.70	6.19
STD Rate	0.678***	0.607	0.578***	1.650***	0.602***	0.193***	1.916*
	(0.171)	(0.810)	(0.144)	(0.483)	(0.141)	(0.086)	(0.983)
Baseline	5.43	27.11	3.37	14.29	2.29	1.37	16.11
Implied Pct.	12.49	2.24	17.15	11.55	26.29	14.09	11.89

Pre-FDA States results refers to the specification that defines an OTC indicator equal to one if the state has passed otc EC legislation. Estimates are reported in rates per 1000. The regressions are population-weighted and robust standard errors are clustered at the state level. State and year fixed effects are included, as well as controls for median state income. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$

Table 3.7: Effect of over-the-counter EC legislation on state-level outcomes, All States

	Aggregate	Teenager	Adult	Aged 20-29	Aged 30-39	White	Black
Abortion Rate	0.235	0.199	0.044	-0.011	0.031	-0.521	-1.229
	(0.479)	(0.177)	(0.304)	(0.803)	(0.304)	(0.352)	(1.025)
Baseline	19.75	5.30	11.77	30.74	10.15	8.25	24.51
Implied Pct.	1.19	3.75	0.37	0.03	0.31	6.32	5.01
Birth Rate	-0.598***	-0.250	-1.889***	-5.976***	-0.073	-2.401***	-2.451***
	(0.109)	(0.187)	(0.357)	(0.812)	(0.430)	(0.358)	(0.502)
Baseline	14.24	13.03	49.98	111.33	59.19	48.99	57.26
Implied Pct.	4.20	1.92	3.78	5.37	0.12	4.90	4.28
STD Rate	0.467***	0.702	0.378***	1.315***	0.427***	0.125***	0.968**
	(0.137)	(0.593)	(0.111)	(0.454)	(0.110)	(0.042)	(0.471)
Baseline	5.43	27.11	3.37	14.29	2.29	1.37	16.11
Implied Pct.	8.60	2.59	11.22	9.20	18.65	9.12	6.01

The All states results refers to the specification that defines an OTC indicator equal to one if a state has either passed otc EC legislation or if it is affected by the 2006 FDA ruling. Estimates are reported in rates per 1000. The regressions are population-weighted and robust standard errors are clustered at the state level. State and year fixed effects are included as well as controls for median state income. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$

Table 3.8: Effect of OTC EC legislation on abortion rates, by age group

	Full Sample		Aged 15-19		Aged 20-29		Aged 30-39	
	Estimate	Implied Pct.	Estimate	Implied Pct.	Estimate	Implied Pct.	Estimate	Implied Pct.
Year of OTC switch	-0.892***	4.52	-0.128	2.42	-1.344***	4.37	-0.130	1.28
	(0.234)		(0.110)		(0.480)		(0.232)	
1 year post-otc-switch	-0.31	1.57	0.088	1.66	-0.427	1.39	-0.024	0.24
	(0.353)		(0.165)		(0.719)		(0.274)	
2 year post-otc-switch	0.202	1.02	0.296*	5.58	0.378	1.23	0.359**	3.54
	(0.391)		(0.171)		(0.629)		(0.174)	
3 year post-otc-switch	1.112*	5.63	0.431***	8.13	1.056	3.44	0.368	3.63
	(0.581)		(0.146)		(1.251)		(0.369)	
4 year post-otc-switch	-0.138	0.70	0.264	4.98	-0.985	3.20	-0.285	2.81
	(0.680)		(0.204)		(1.241)		(0.366)	
R^2	0.97		0.94		0.95		0.97	
N	722		676		681		681	

Estimates are from a flexible time specification. Each column reports results from a separate regression on different subsamples. Each estimate corresponds to the marginal effect in the k^{th} year following the mandate calculated as the difference from the effect in the year prior to EC legislation. Estimates are reported in rates per 1000. All regressions are population weighted by subgroup and include state and year indicator variables and controls for median income. Standard errors are clustered at the state level. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$.

Table 3.9: Effect of OTC EC legislation on birth rates, by age group

	Full Sample		Aged 15-19		Aged 20-29		Aged 30-39	
	Estimate	Implied Pct.	Estimate	Implied Pct.	Estimate	Implied Pct.	Estimate	Implied Pct.
Year of otc switch	-0.194***	1.33	-0.149	1.14	-2.476***	2.22	0.503	0.85
	(0.077)		(0.112)		(0.485)		(0.352)	
1 year post-otc-switch	-0.536***	3.76	-0.336	2.58	-5.708***	5.13	-0.528	0.89
	(0.190)		(0.254)		(1.322)		(0.549)	
2 year post-otc-switch	-0.980***	6.88	-0.414	3.18	-9.482***	8.52	0.142	0.24
	(0.261)		(0.474)		(2.330)		(1.076)	
3 year post-otc-switch	-0.972***	6.83	-0.405	3.11	-10.505***	9.44	0.876	1.48
	(0.315)		(0.473)		(2.363)		(0.709)	
4 year post-otc-switch	-1.413***	9.92	-0.276	2.12	-16.107***	14.47	0.891	1.51
	(0.435)		(0.564)		(2.586)		(0.892)	
R^2	0.95		0.98		0.94		0.98	
N	765		765		765		765	

Estimates are from a flexible time specification. Each column reports results from a separate regression on different subsamples. Each estimate corresponds to the marginal effect in the k^{th} year following the mandate calculated as the difference from the effect in the year prior to EC legislation. Estimates are reported in rates per 1000. All regressions are population weighted by subgroup and include state and year indicator variables and controls for median income. Standard errors are clustered at the state level. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$.

Table 3.10: Effect of OTC EC legislation on STD rates, by age group

	Full Sample		Aged 15-19		Aged 20-29		Aged 30-39	
	Estimate	Implied Pct.	Estimate	Implied Pct.	Estimate	Implied Pct.	Estimate	Implied Pct.
Year of otc switch	0.334**	6.15	0.331	1.22	0.853*	5.97	0.260***	11.33
	(0.137)		(0.562)		(0.506)		(0.084)	
1 year post-otc-switch	0.348**	6.41	0.843	3.11	1.341**	9.38	0.174**	7.59
	(0.170)		(0.613)		(0.608)		(0.078)	
2 year post-otc-switch	0.356*	6.56	0.739	2.73	1.414**	9.90	0.217**	9.46
	(0.184)		(0.878)		(0.696)		(0.096)	
3 year post-otc-switch	0.700***	12.89	1.749*	6.45	2.399**	16.79	0.375***	16.35
	(0.256)		(1.039)		(1.030)		(0.122)	
4 year post-otc-switch	0.673***	12.39	0.915	3.38	1.642***	11.49	0.415**	18.09
	(0.180)		(0.781)		(0.607)		(0.156)	
R^2	0.91		0.91		0.91		0.89	
N	612		612		612		612	

Estimates are from a flexible time specification. Each column reports results from a separate regression on different subsamples. Each estimate corresponds to the marginal effect in the k^{th} year following the mandate calculated as the difference from the effect in the year prior to EC legislation. Estimates are reported in rates per 1000. All regressions are population weighted by subgroup and include state and year indicator variables and controls for median income. Standard errors are clustered at the state level. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$.

Table 3.11: Effects of OTC EC legislation, by race

Whites:	Abortion Rate		Birth Rate		STD Rate	
	Estimate	Implied Pct.	Estimate	Implied Pct.	Estimate	Implied Pct.
Year of otc switch	-0.246	2.98	-0.843***	1.72	0.094**	6.86
	(0.170)		(0.280)		(0.044)	
1 year post-otc-switch	0.061	0.74	-2.084***	4.25	0.174*	12.70
	(0.310)		(0.605)		(0.087)	
2 year post-otc-switch	0.211	2.56	-3.841***	7.84	0.198*	14.45
	(0.393)		(1.006)		(0.115)	
3 year post-otc-switch	0.928	11.25	-3.423***	6.99	0.407***	29.71
	(0.813)		(1.058)		(0.117)	
4 year post-otc-switch	0.791*	9.59	-5.345***	10.91	0.396***	28.91
	(0.405)		(1.241)		(0.144)	

Blacks:	Abortion Rate		Birth Rate		STD Rate	
	Estimate	Implied Pct.	Estimate	Implied Pct.	Estimate	Implied Pct.
Year of otc switch	-0.162	0.66	-0.795	1.39	0.318	1.97
	(0.708)		(0.627)		(0.681)	
1 year post-otc-switch	-0.357	1.46	-2.13**	3.72	0.793	4.92
	(0.837)		(0.950)		(0.770)	
2 year post-otc-switch	1.110	4.53	-4.964***	8.67	1.087	6.75
	(1.061)		(1.141)		(0.839)	
3 year post-otc-switch	3.783**	15.43	-5.972***	10.43	3.535***	21.94
	(1.450)		(1.13)		(0.888)	
4 year post-otc-switch	3.918**	15.99	-6.419**	11.21	2.272	14.10
	(1.564)		(2.950)		(1.611)	

Estimates are from a flexible time specification. Each column reports results from a separate regression on different subsamples. Each estimate corresponds to the marginal effect in the k^{th} year following the mandate calculated as the difference from the effect in the year prior to EC legislation. Estimates are reported in rates per 1000. All regressions are population weighted by subgroup and include state and year indicator variables and controls for median income. Standard errors are clustered at the state level. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$.

The results from the flexible time specifications indicate that the over the counter EC legislation has an effect beyond the year of introduction. To better visualize the effects for adults compared to teenagers, the estimates are graphed in Figure 3.1. The effect of EC legislation on the birth rate and STD rate is larger for adults compared with teenagers.

For a discussion of the magnitude of the effects, I focus on the full sample birth rate regression (the first column in Table 3.4. For the year of the over the counter switch the causal effect is given by the year of otc switch estimate $\widehat{\beta}_0 = -0.194$, which is an effect of 0.194 fewer births per 1000 women in the year of the initial switch to over the counter EC access. The post-switch effects are not cumulative; therefore, the 1-year post otc-switch estimate of -0.536 represents an additional 0.342 decrease in the birth rate relative to the intial year effect.

To determine whether OTC access to EC had differential effects on various subgroups, estimation was conducted using various age and race subsamples. The initial effect on abortions is largest for women in their twenties in the year of legislation (abortions decreased 4.37% compared with 2.4% for teenagers). The effect on abortions is also changing over time — by the third year following legislation, OTC access has caused an increase in abortions. The effect of OTC access on births is quite large, particularly for women in their twenties, and is increasing over time: in the fourth year following legislation, births have decreased 14.5% for women in their twenties. The effect of OTC access to EC is large for all age subgroups, but is still larger for adults compared with teenagers. As with births, the effect on STDs is increasing over time. The increasing effects for births and STDs might indicate individuals are becoming better informed regarding EC access.

Figure 3.1: Dynamic effect of otc EC legislation on aggregate outcomes

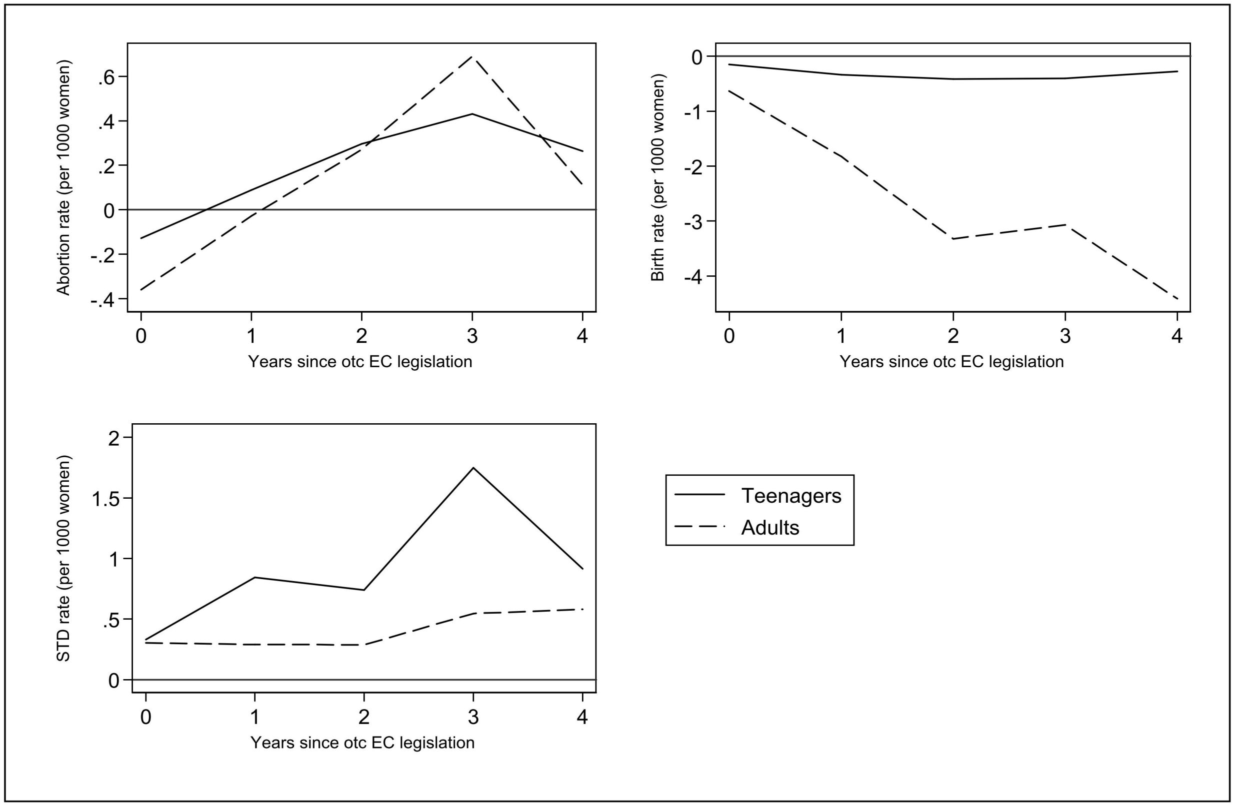


Table 3.12: Effect of OTC EC legislation on sexual behaviors

	Pre-FDA States	All States
Probability of having sex ever	0.02	0.015
(N=32597)	(0.012)	(0.016)
Probability of having sex in the past 12 months	0.034**	0.046**
(N=26316)	(0.013)	(0.022)
Number of sexual encounters in past 12 months	13.798	12.729**
(N=21576)	(8.67)	(5.26)
Number of times used a condom in past 12 months	-5.439*	-8.732***
(N=20856)	(3.257)	(3.180)
Probability of having risky sex	0.603***	0.001
(N=23376)	(0.029)	(0.002)

Each row shows estimates from effect of otc EC legislation on various sexual behavior outcomes. Robust standard errors are clustered at the state level. Significance — *: $P < 0.10$, **: $P < 0.05$, ***: $P < 0.01$.

3.4.1 NLSY 1997 Results: Sexual Behaviors

Table 3.12 presents results from the sexual behaviors regressions using NLSY data. All regressions include controls for age, education, marital status, race, whether an individual lives in an urban area, mother's education, and state and year fixed effects.[11] The number of sexual encounters is truncated at 999 in the NLSY sample; estimates which limit the number of encounters in the past year to 365 are slightly lower in magnitude, but not significantly different from the estimates shown.

As with the aggregate estimation, two specifications were estimated. The first defines an OTC indicator equal to one if a state passed over the counter EC legislation; the second defines an OTC indicator equal to one if a state passed over the counter EC legislation *or* was affected by the FDA ruling. The effect of over the counter EC access has the expected sign for all sexual behavior outcomes: individuals are more likely to have sex (ever or in the past 12 months), have more sexual encounters,

[11] The coefficients on the controls are not of direct importance and so are not reported.

use condoms less often, and are more likely to have "risky sex" (defined as having sex with either a stranger or an IV-drug user). These results are consistent with the aggregate findings of more risky sexual behavior resulting from over the counter EC access.

3.5 Conclusion

This study is the first to formally estimate the causal effects of switching access for EC from prescription-only to over-the-counter within the United States. Using longitudinal abortion, birth, and STD data and the differential timing of state-legislation introduction, I find significant and relatively large effects of changing EC access from prescription-only to over-the-counter. These effects are different for teenagers and adults, especially with respect to decreases in birth rates. The difference in these effects might stem from education and information available regarding contraceptives. The American College of Obstetricians and Gynecologists recommend that women begin Pap test screening at age 21, be screened every 2 years through age 30, so that older women who are more likely to receive a regular checkup might be better informed regarding both regular contraceptives and EC compared with teenagers.

Despite slight differences in the actual legislation under consideration, it is worthwhile to compare the estimates with the results in Girma and Paton (2006, 2011).[12] In their 2006 paper, they do not find a significant effect of free access to EC on teenage pregnancy rates. I estimated models (results not presented) which used a constructed pregnancy rate (following Levine et. al. (1996)), and find no significant

[12]Girma and Paton examine the impact of free over-the-counter access of EC for teenagers in the UK, whereas access in the US was switched from prescription-only to over-the-counter but was not free. In fact, many individuals would have had cheaper access under the prescription-only scheme since EC would be covered under insurance plans.

impact of over the counter EC access on teenage pregnancy rates. In their 2011 paper, they find no effect on conception rates but a positive effect on STI rates, indicating riskier behavior among teenagers — these results are in line with the findings in this paper. A very similar working paper by Zuppann (2011) examines the effect of over the counter EC legislation on sexual and relationship outcomes. The findings in this paper are consistent with his findings: over the counter access to EC decreases birth rates and increases STD rates at the aggregate level, and increases risky sexual behavior at the individual level (using the NLSY dataset).

This paper has several shortcomings that result directly from data availability. Since the FDA ruling was in the middle of 2006, having data for 2008 and beyond would provide a more clear cut "after" period. Additionally, although we can imply that risky sexual behaviors are increasing as a result of easier EC access given the increases in STD rates, it would be optimal to have population-level data on condom use or multiple sexual partners to determine which channels are driving these results. Additionally, it would be of interest to determine whether these behaviors are driven primarily by men or women (or both). Future research could examine contraception use (both regular and EC), and would allow us to gain further insight into whether EC is being used as a substitute for regular contraception, or whether it is used more frequently when available over the counter.

Appendix

Table A1: Varicella vaccination rates (children aged 19-35 months), by state in selected years

State	1996	2000	2005	2007	State	1996	2000	2005	2007
Alabama	7.7	75.7	93.5	92.0	Montana	11.4	54.3	75.5	78.5
Alaska		47.3	81.2	80.5	Nebraska	13.4	63.5	89.9	93.8
Arizona	10.9	65.3	83.6	86.0	Nevada		61.4	84.4	83.3
Arkansas	8.5	77.6	85.9	89.2	New Hampshire	11.6	66.0	82.9	95.2
California	16.1	76.0	89.5	93.2	New Jersey	19.4	67.8	83.8	92.5
Colorado	12.1	60.5	87.2	88.9	New Mexico	12.7	68.0	86.3	88.8
Connecticut	19.0	76.3	91.0	94.2	New York	12.4	70.6	87.6	88.4
Delaware	6.8	69.2	89.5	92.1	North Carolina	9.9	76.4	91.3	93.3
D. of Columbia	16.9	84.5	90.6	94.0	North Dakota	7.4	58.8	87.2	91.5
Florida	15.0	60.9	91.1	90.2	Ohio	12.6	60.2	86.3	89.1
Georgia	10.4	75.1	91.9	91.6	Oklahoma	11.9	72.4	85.8	89.7
Hawaii	11.3	77.5	89.4	95.5	Oregon	13.2	76.7	76.2	84.2
Idaho		38.0	77.4	75.5	Pennsylvania	24.9	74.4	89.2	91.9
Illinois	6.9	47.9	86.3	88.7	Rhode Island	12.9	81.6	96.2	92.1
Indiana	11.2	57.9	82.8	88.3	South Carolina	12.8	70.3	87.4	91.5
Iowa	5.8	50.9	83.4	88.2	South Dakota		39.7	85.7	85.3
Kansas	10.0	57.8	81.5	88.7	Tennessee	8.4	69.9	89.8	92.3
Kentucky	7.1	63.0	83.3	87.9	Texas	8.8	73.6	88.9	90.0
Louisiana	4.7	65.1	89.0	91.5	Utah		52.7	81.2	86.6
Maine		55.0	84.2	85.5	Vermont	12.0	57.3	68.5	77.6
Maryland	16.7	82.5	90.7	96.8	Virginia	18.7	77.6	89.9	87.8
Massachusetts	6.5	79.5	95.4	87.4	Washington	6.4	48.7	76.6	84.0
Michigan	8.3	69.6	93.4	89.5	West Virginia	6.9	59.9	81.1	89.2
Minnesota	21.5	61.4	86.7	89.1	Wisconsin	10.4	56.7	87.0	86.7
Mississippi		53.0	88.4	88.4	Wyoming		57.6	77.2	78.5
Missouri	10.7	59.9	87.9	89.4					

Source: National Immunization Survey. Note: A blank cell indicates lack of state-level data availability.

Table A2: Individual-level vaccination regressions

	Variable	Marginal effect	Standard error
Child	Female	-0.08	(0.16)
	Hispanic	2.48***	(0.49)
	Black	-0.50	(0.48)
	Other non-white race	0.48	(0.45)
	Age: < 24 months	-0.87***	(0.21)
	Age: 24-30 months	1.38***	(0.16)
Mother	Age: < 20	-0.16	(0.93)
	Age: 20-29	0.27	(0.25)
	High school graduate	1.66***	(0.29)
	Some college	2.86***	(0.37)
	College graduate	5.01***	(0.42)
	Income: $10-20K	0.66**	(0.27)
	Income: $20-30K	-0.52	(0.37)
	Income: $30-50K	0.44	(0.34)
	Income: >$50K	3.43***	(0.43)

Notes: The omitted categories are white race, child's age > 30 months, mother's age $\geq$ 30, education less than high school, and income less than $10K. Estimates have been scaled by 100 to be intepretable as percentage points. Significance — *: $P < 0.10$, **: $P < 0.05$., ***:$P < 0.01$.

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