INSURANCE COVERAGE MANDATES FOR PREVENTIVE CARE: THE MARKET FOR CONTRACEPTIVES

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A DISSERTATION

in

Health Care Management and Economics

For the Graduate Group in Managerial Science and Applied Economics

Presented to the Faculties of the University of Pennsylvania

in

Partial Fulfillment of the Requirements for the

Degree of Doctor of Philosophy

2015

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Dedicated to my mother.

When I was ten, I had to write a report for my 5th grade teacher on a topic of my choosing. I asked her for advice and she said, "How about birth control?" So really, this is all her fault.

ACKNOWLEDGEMENTS

This project would not have been possible without help and support from many people. First and foremost, a huge amount of thanks goes to my dissertation committee: Dan Polsky, Mark Pauly, Scott Harrington, Guy David and Kevin Volpp. Each of them has been an incredible source of help and mentorship over the last four years. As my committee chair, Mark Pauly was the source of many thought-provoking questions and comments. He challenged me to think clearly about the economic questions I was tackling in my work, and he always did it with kindness and good humor. Scott Harrington taught me everything I know about econometrics, and was always willing to take the time to sit down with me and discuss my model specifications in detail. He read every word of this dissertation, and my work is vastly improved as a result. Guy David helped me think through my research questions, and was always available to me when I had a problem or needed encouragement. And Kevin Volpp gave me my very first research assistant job, where I learned to use STATA for the first time, and has served as a mentor and role model to me as I worked to find my own identity as a physician scientist.

The greatest thanks are due to Dan Polsky, my advisor. For four years, he has been the person I turned to with my questions, my worries, and my research ideas. When, as a first-year graduate student, I confessed to him that I wasn't sure I'd ever feel like a "real economist," he reassured me that I would, and then explained what a fixed effect regression model was. When I excitedly told him that I wanted to research the impacts of insurance coverage mandates on contraceptive use, he smiled at my enthusiasm and told me to get to work. And this past year he has examined my methods and results, provided me with guidance and focus, and become a co-author and colleague, all while juggling a busy schedule of his own. I could never have succeeded in this program without him, and I hope that we continue to collaborate for many years to come.

Many people at Penn have provided me with logistical and personal support over the years. In the HCMG department, Joanne Levy believed in me from day one of my application to the program, and provided me with huge amounts of support, both personal and financial. In the MSTP program, I cannot say enough good things about Maggie Krall, who is an invaluable source of support and good advice.

Here at Wharton, I feel lucky to have gained a wonderful group of colleagues who have also become my best friends. I am especially grateful to Preethi Rao, Ellie Prager, Shulamite Chiu, Ari Friedman, Kaitlin Daniels, Alix Barasch, and many others too numerous to list. From first-year problem sets to Halloween costume parties, from coding advice to weekends on the Jersey Shore, they have made my years here at Wharton a pleasure and a gift.

My other huge source of support has been my family. My parents suggested the idea of an MD/PhD program to me in the first place, and they continually inspire me, both personally and professionally. My brother Eli has always helped me keep (or regain) my perspective about the important things in life. And last but not least, I owe Corey Dukes, my partner and about-to-be-husband, a huge debt of thanks for his love, dedication, and sacrifice. I look forward to our next adventure together.

2015

Nora Verlaine Becker

ABSTRACT

INSURANCE COVERAGE MANDATES FOR PREVENTIVE CARE: THE MARKET FOR CONTRACEPTIVES

Nora Verlaine Becker

Daniel Polsky

Laws that mandate contraceptive coverage by private health insurance plans are common at the state level, and the Affordable Care Act (ACA) also recently mandated coverage at the national level. Little empirical work has examined the potential impact of these laws on women's contraceptive utilization. I perform both 1) a short-term analysis of the impact of the ACA's mandate using available data, and 2) an examination of 29 state-level contraception coverage mandates passed between 1999 and 2010 that could shed light upon the long-term utilization impacts of the national mandate. For these analyses, I use two datasets: the first a 50-state survey with an extensive set of individual-level covariates, and the second a proprietary claims dataset with detailed information on contraceptive utilization and out-of-pocket spending. I find suggestive evidence that the state mandates resulted in increased insurance coverage of some methods of contraceptives, but find no resulting changes in overall utilization or the type of method chosen. I find that the ACA mandate has caused large decreases in out-of-pocket spending on contraceptives, but I detect only very small changes in utilization in response, implying that demand for contraceptives among privately insured women is fairly price-insensitive. My results suggest that mandating insurance coverage of contraceptives is unlikely to result in immediate or large changes in patterns of contraceptive use in the U.S.

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> "As part of the health care reform law that I signed last year, all insurance plans are required to cover preventive care at no cost. That means free check-ups, free mammograms, immunizations and other basic services. We fought for this because it saves lives and it saves money — for families, for businesses, for government, for everybody."
>
> – President Barack Obama, February 12th, 2012

In policy discussions, prevention is often cited as a panacea for the problems in the U.S. health care system; better health for less money. With this rationale, the Affordable Care Act (ACA) included a mandate that private health insurance cover all preventive services with no consumer cost-sharing. Empirical research, however, suggests that this claim is overly optimistic. Preventive care is not all the same, and the cost and health impacts of of this mandate are likely to vary widely by service. One particular type of preventive service included in the ACA's mandate, prescription contraception, has drawn political and legal attention. But little empirical analysis has been directed at the potential impacts of mandating contraceptive coverage on consumer demand for contraceptives and and product choice.

Contraceptives are among the most widely used medical services in the U.S.; 99% of sexually active women have used at least one type of contraceptive in their lifetime (Jones et al., 2013). Furthermore, use of prescription contraceptives has been shown to result in net savings in medical costs (Trussell et al., 2009; Foster et al., 2009). As a consumer product, it has important effects on families and the economy; its use has been found to increase labor force participation, wages and family incomes (Bailey et al., 2012; Bailey, 2013; Goldin and Katz, 2002; Ananat and Hungerman, 2007).

Economic theory and empirical evidence suggest that decreasing the out-of-pocket (OOP) costs of contraception to consumers will result in increased utilization (Pauly, 1968; Manning et al., 1987). Furthermore, differences in relative price changes may induce a change in the distribution of methods chosen. However, mandating private health coverage of contraceptives is not guaranteed to increase contraceptive use or reduce costs. The impact of an insurance mandate varies by two important factors: the elasticity of demand and the pre-existing levels of insurance coverage for the product. If demand for a product is inelastic enough, it's possible that an insurance mandate for a very cost-effective service may still ultimately increase insurer spending, potentially raising the cost of insurance (Pauly and Held, 1990). Very few studies have estimated the price responsiveness of consumers to the out-of-pocket price of contraceptives in the U.S., and therefore the impact of mandating coverage of contraceptives cannot be predicted from prior research alone.

This study will examine state and national laws that mandate inclusion of contraception coverage in private health insurance plans. Specifically, I will examine two types of contraceptive coverage mandates: twenty-nine state-level mandates passed between 1998 and 2011, and the ACA's national mandate. These results contribute valuable empirical evidence to the larger question of the effects of mandating insurance coverage of preventive services on utilization.

1.1. The market for contraceptives in the U.S.

Contraceptive methods vary widely in their product characteristics. Some are covered by insurance and require a physician visit and prescription, while others are available over-the-counter. The mechanism of action varies, as does the frequency with which a method must be used to be effective. Methods also vary in their out-of-pocket price. Figure 1 briefly describes each method by its method of administration and effectiveness. Contraceptive methods tend to become more expensive, and require less frequent use, as they become more effective.

Methods can be grouped into different categories. The first are non-prescription methods; these include condoms, spermicide, the sponge and calendar-based methods. Because these methods are free or available over-the-counter (OTC), they are unaffected by laws that change insurance coverage of contraceptives. Among prescription contraceptives, there are two infrequently used methods–the diaphragm and the cervical cap–that must be used at the time of intercourse to be effective. Both of these methods require a one-time fitting by a gynecologist and are therefore classified as prescription-only. The next category are the shorter-term methods that must be used daily or weekly such as oral contraceptive pills (OCPs), the vaginal ring, and the cutaneous patch. These methods require a prescription, are subject to a co-pay, and consumers typically purchase one, two or three months supply at a time. Finally, there are two types of methods–intrauterine devices (IUDs) and subdermal implants–that are known together as long-acting reversible contraceptives (LARC) because they provide very effective pregnancy prevention for years. There are currently three IUD products on the market: two hormonal IUDs that last for three and five years, and a copper IUD that lasts 10 years. There is one implant on the market that lasts 3 years. All of these products require an in-office procedure to be inserted and removed; depending on the insurance plan, both the device itself and the procedure may be subject to a deductible or coinsurance.

The final contraceptive products are emergency contraception (EC) and surgical sterilization. Emergency contraception is a one or two pill formulation of hormones designed to prevent pregnancy immediately following intercourse. Emergency contraception was initially prescription-only, and after a lengthy and controversial regulatory process, one brand (Plan B) was made available over-the-counter (OTC) in August 2006 for individuals aged 18 and older. These age and brand restrictions have been relaxed piecemeal through subsequent regulatory changes. Currently, several one-pill generic brands are available OTC without age restriction, while two-pill generics are available “behind the counter,” i.e., without a prescription but not on the shelf, for consumers aged 17 and older. There is one EC brand, ella, that uses a different compound than all other products on the market, that is still available prescription-only (Trussell et al., 2014). Finally, sterilization, either male or female, is a surgical and permanent method of contraception. Female sterilization (tubal ligation) is covered by the mandates that I study, but male sterilization (vasectomy) is not.

A mention must also be made of medical abortifacients, that is, medications designed to induce abortion. These are not prescription contraceptives and are not included in any of the mandates that I examine.

A recent study of women aged 15 to 44 found that 62.2% were using some method of contraception, and 48.1% were using a prescription contraceptive method. Sterilization (male or female) was the most common choice (22.7%), followed by oral contraceptive pills (17.1%), and the IUD (3.5%), with all other prescription methods combining to make up 4.5% of women (Jones et al., 2012).

1.2. Prior literature

This project contributes to three major areas of the literature: insurance coverage mandates, the impacts of cost-sharing on demand for medical services, and the demand for preventive care in general and contraception in particular.

1.2.1. Insurance coverage mandates

Insurance coverage mandates are a frequently used policy tool. As a government intervention, they are more efficient but less equitable than direct public provision of a service (Summers, 1989). Empirical research has shown that the costs of insurance coverage mandates are imperfectly passed to consumers in the form of lower wages (Gruber and Krueger, 1990; Gruber, 1994a). However, state-level mandates in particular may not always bind if most insurance plans already offer coverage for the mandated service (Gruber, 1994b).

State-level insurance coverage mandates for preventive services in particular have been growing in popularity. A 2006 study found that 20% of the 1,471 active state-level coverage mandates were specifically for preventive services (Laugesen et al., 2006). Studies of these mandates have found mixed results. Mandates for mammography coverage have been found to significantly increase mammography rates (Bitler and Carpenter, 2011), and mandates for coverage of cervical cancer screening have been found to increase rates of pap smear testing (Bitler and Carpenter, 2012). But mandates for mental health treatment have been found to have no effect on suicide rates (Klick and Markowitz, 2006), and mandates for coverage of diabetes preventive care found impacts on use of some but not all diabetes prevention tools (Li et al., 2010). A study examining the impact of the dependent coverage provision of the ACA found that young adults newly eligible for coverage under their parents' plans recieved more regular dental and health check-ups and increased blood pressure management, but saw no significant change in rates of influenza vaccination or pap smear testing (Han et al., 2014). All of the above studies used a difference-in-difference estimation strategy to isolate causal impacts of the policy they studied.

Results from these studies are not generalizable to contraception coverage mandates, because their results depend on pre-existing coverage levels and demand elasticity for the service. In addition, contraception is unique among preventive services for a few reasons. First, there is more variety of product characteristics and OOP costs than is typically seen for preventive services, with options ranging from short-term solutions that cost a few dollars to permanent surgeries that can cost thousands of dollars. Second, the population that consumes contraception is young women of reproductive age, a group that on average consumes few other health services. Finally, contraceptive use carries normative implications that other preventive services lack, and remains a flashpoint in the cultural discussion of the sexual revolution. For all these reasons, it is important to study contraceptive coverage mandates and empirically estimate their direct effect on consumers.

Rationale for an insurance coverage mandate for contraceptives

There are several potential rationales for or against an insurance coverage mandate for contraceptives, either at the state or federal level. In his classic paper examining the economics of mandated benefits, Summers (1989) gives three reasons why it may be appropriate to mandate employee benefits: paternalism, positive externalities, and mitigating adverse selection. All three are worth discussing in the case of contraceptives.

1. Paternalism: If there is reason to believe that consumers are not making rational choices with regards to the costs and benefits of a service, it may be welfare-increasing to change the cost to affect their behavior. There is evidence that this may be the case for contraceptive use in the U.S. Studies have shown that women who under-rate their risk of pregnancy are less likely to use emergency

contraception following unprotected sex (Moreau et al., 2005; Sørensen et al., 2000). It's also likely that some women, especially young women or adolescents, may not be able to accurately esimate the social and financial costs of bearing a child. A recent working paper by Kearney and Levine (2014) finds that media exposure to the realities of teen childbearing results in drops in teen birth rates, suggesting that many teens may lack the ability to fully imagine the realities of parenthood on their own. Lastly, current estimates suggest that about half of pregnancies in the U.S. are unplanned, and of those, 43% end in abortion (Finer and Zolna, 2011). One rationale for a contraceptive coverage mandate is therefore that it may increase use of contraceptives among women who are unable to accurately assess the cost and probability of pregnancy.

2. Positive externalities: The presence of positive externalities from use of a service is another rationale to mandate insurance coverage. When a service produces a positive externality, it means that benefits accrue to individuals who do not bear the cost of the service. There is some evidence this may be the case for contraceptive use; results of studies of the legalization of the pill in the 1960s and 1970s suggest that contraceptive use may produce economic benefits not just to contraceptive users but to their families and children as well. See Section 1.2.3 for a detailed discussion of this body of literature.

Selection: Unlike the first two examples, the possibility of selection on contraceptive coverage does not provide a rationale for mandating insurance coverage of contraception. In the case of health insurance, if some employers offer the benefit and others do not, sicker employees may be more likely to choose employers who offer the benefit. However, it is not at all obvious that selection on coverage of contraception alone occurs among employees. And even if it did, it could potentially be an example of positive selection rather than adverse selection; women who use contraception are less likely to have children, and therefore less likely to take time off and use other costly medical services. I have not seen an in-depth discussion of this idea anywhere in the literature, but regardless, mitigating adverse selection does not seem to be a good rationale for a contraceptive coverage mandate. Furthermore, if selection exists at the employer level, it makes it less likely that an insurance coverage mandate will have an effect on rates of contraceptive use.

1.2.2. The impact of cost-sharing on demand for medical services

The classic RAND health insurance experiment demonstrated that patients are price sensitive to the out-of-pocket cost of medical services (Manning et al., 1987), and the results seen in that study have been corroborated by more recent work (Gruber, 2006). Subsequent work focusing on demand for prescription drugs finds similarly that higher cost-sharing generally leads to less consumption, although not always a switch to generic drug options (Gibson et al., 2005; Eaddy et al., 2012; Gaynor et al., 2006).

The largest recent expansion of prescription drug coverage, the Medicare Part D program, has been studied extensively by researchers. Ketcham and Simon (2008) find Medicare Part D decreased OOP costs and modestly increased utilization, implying an elasticity of -0.22, while Duggan and Morton (2010) also find that utilization increased, although the price-responsiveness varied by drug and by the status of prior prescription drug coverage. Decreased OOP costs have also been shown to reduce cost-related medication nonadherence (Madden et al., 2008). These studies, however, exclusively examine an elderly population with many more chronic health conditions than the population of younger women in private health insurance. I've found far fewer studies examining the impact of decreases in OOP cost on prescription drug utilization among this population.

A newer strand of the health economics literature has begun to study the impact of varying cost-sharing at the patient level by medical service. This is known as "value-based insurance design," or VBID. Traditional health insurance includes one level of cost-sharing that applies to all consumers. However, traditional insurance is predicated upon the economic assumption that, when facing cost-sharing, patients will only consume medical care for which the marginal benefits equal or exceed the marginal costs. This assumes the consumer has perfect knowledge of the costs and benefits of all potential medical care. If this assumption is relaxed, it becomes optimal to vary cost-sharing levels to induce patients to use the services that will provide the most marginal benefit to them. In practice, this typically takes two forms: 1) reducing co-payments for all patients for clinical services deemed to be "high-value", or 2) implementing a more individualized approach where individual copayments are based on patient characteristics (Chernew et al., 2007). An insurance coverage mandate for contraceptive care could be considered an example of the first type of VBID. There is no question that contraception can be considered a "high-value" medical service from a budgetary perspective; it's use has been shown to produce cost savings of $1.3 to $7 per dollar spent, depending on the method (Foster et al., 2009). These are cost-savings on the same order as that of childhood vaccinations.

VBID programs are increasingly popular; a 2010 study found that while only 20% of large employers had a VBID program, 81% were interested or very interested in implementing one during the next five years (Choudhry et al., 2010b). Because these programs are relatively new, studies assessing their impact on health care use are a small but growing field of the health policy literature. In general, early studies of VBID programs have been mixed. Some have found that decreasing co-pays for high-value drugs or services have resulted in increases in use of the targeted service, however, most studies have found only small or moderate effect sizes (Chernew et al., 2008; Choudhry et al., 2010a; Cranor et al., 2003).

1.2.3. Demand for preventive care and contraception

Theory of demand for preventive care

Demand for prevention was first modeled by Ehrlich and Becker (1972). They find that insurance lowers the amount of preventive care demanded as long as the price of insurance does not contract on the amount of preventive care consumed. This inefficiency is referred to as *ex ante* moral hazard in subsequent literature. They also find that risk aversion is neither necessary nor sufficient to determine an optimal value of preventive care that an individual will demand. This is somewhat counter-intuitive, but it has been shown in subsequent literature as well. An individual's demand for preventive care is dependent on the functional form of their utility and cannot be assumed to be increasing in risk aversion (Dionne and Eeckhoudt, 1985; Eeckhoudt and Gollier, 2005; Jullien et al., 1999).

Ellis and Manning (2007) solve for the socially optimal cost-sharing levels for prevention and treatment. They find that some coverage of preventive care is optimal as long as the premium price does not reflect the amount of prevention consumed. This is because the consumer does not take the impact of their use of preventive care on premium price into account when calculating their demand for preventive care, and therefore underconsumes prevention relative to the social optimum.

At the market level, Pauly and Held (1990) show that providing insurance of a preventive service that is cost-effective (in the sense that its use results in lower total expected medical spending) can still result in a net increase in insurer medical expenses. They show that covering preventive services for which demand is elastic will be more likely to reduce insurer spending, because providing coverage will induce a greater increase in utilization, offsetting the costs of paying for the costs of prevention. However, if demand for a preventive service is inelastic enough, providing coverage will only increase utilization by a small amount and total costs to the insurer will rise because they achieve fewer cost savings but are still paying for the preventive service for people who were already consuming it before coverage was provided. Frakt (2014) calls this effect "crowding out" of private consumption of the preventive service.

Studies of demand for contraceptives

Most estimates of the demand elasticity for contraception are from studies in the developing world. These studies have typically found demand for contraceptives to be relatively inelastic, depending on the method studied (Lewis, 1986). Of the studies that have estimated price effects of overall use of contraceptives, estimates range from 0 to -0.15 (Matheny, 2004). However, many of these studies take a cross-sectional approach and rely on self-reported price estimates by relatively uninformed consumers. Further, they often ignore the possibility of substitution to other brands or methods, or to lower-cost providers, in response to price changes (Janowitz and Bratt, 1996).

Studies of responses to price changes in the U.S. are uncommon. Recently, an in-progress working paper examined the price impacts of the Deficit Reduction Act of 2005, which inadvertantly increased the price of oral contraceptive pills at college health centers more than three-fold. They find that this price change reduced OCP use among college women by 1.5 percentage points (3-4%). The decline was two to three times as large for college women with large amounts of credit card debt or without health insurance. They use a back-of-the-envelope calculation to estimate the demand elasticity of college women for OCPs between -0.09 and -0.04, a very inelastic estimate (Collins and Hershbein, 2013).

In a recent prospective cohort study, the Contraceptive CHOICE Project, 9000 women in a metropolitan area were educated about reversible contraception and offered their choice of method at no cost; methods were presented in the order of most to least effective. The women were followed for two to three years. Among study participants, 75% chose a LARC method, and the subsequent rates of unintended pregnancy, births and abortions among these women were significantly lower than those seen nationally (Secura et al., 2014; McNicholas et al., 2014). Although these findings are suggestive, it is hard to disentangle the effects of the zero OOP price from the accompanying changes in provider behavior and contraceptive counseling to isolate a pure price effect on demand.

There is also a body of literature that studies the impact of changes in legal access to contraception. These could be seen as changes in the non-monetary cost of consuming contraception. These studies typically use geographic variation in the timing of laws or policy changes to generate causal estimates of the impact of these policies on outcomes of interest. For instance, several papers have studied the impact of geographic variation in the legalization of the contraceptive pill on subsequent fertility rates, family sizes, women's wages, and outcomes for the first generation of children born to women with access to the birth control pill. These studies find that increased legal access to the pill for married women in the 1960s explains 40% of the drop in the U.S. marital fertility rate between 1955 and 1965 and 30% of the convergence of the gender gap in wages in the 1990s (Bailey, 2010; Bailey et al., 2012). Furthermore, economic gains from access to contraception are perpetuated in further generations;

decades after individuals' access to contraception increases, their children have higher college completion, labor force participation, wages and family income (Bailey, 2013). Similarly, other studies have used variation in the legal diffusion of the birth control pill among unmarried women and found that access to birth control decreases fertility and subsequent entry into poverty, and increases age at first marriage and subsequent entry into professional school (Goldin and Katz, 2002; Ananat and Hungerman, 2007; Browne and LaLumia, 2014).

All of these studies exploit the variation in legal access to the pill for married and unmarried women in the 1960s and 1970s. In contrast, very few studies have examined the margin of increased coverage of contraception in health insurance. A study by Kearney and Levine (2009) leveraged variation in expansion of Medicaid coverage of family planning services in the mid-1990s to examine the effect of access to contraception on fertility rates of women on Medicaid. They find that income-based subsidies of contraception lowered the fertility rate of teens by 4% and of non-teens by 2% (Kearney and Levine, 2009).

To the best of my knowledge, only three other studies have looked at the state-level contraception mandates that I plan to examine. The first, by Magnusson et. al. (2012), uses data from the 2006-2008 wave of the National Survey of Family Growth. They examine cross-sectional variation in birth control utilization in states with and without mandates, and find that privately-insured women are more likely to use birth control consistently in states with contraception coverage mandates (Magnusson et al., 2012). These findings, while suggestive, do not allow for causal inference because they cannot show that states with mandates did also not have higher contraceptive use prior to the mandates.

The second and third studies examine the state-level mandates using a cross-sectional difference-in-difference approach. Both studies use data from the Behavioral Risk Factor Surveillance Survey (BRFSS) to examine a subset of states for which data on contraceptive use is available. However, both of these analyses are done using survey data, and are unable to exclude women in insurance plans not subject to state insurance regulations.

Atkins and Bradford (2014) use women in two states (Delaware and Iowa) that implemented mandates as their treatment group and three states without mandates (Kentucky, Nebraska and South Dakota) as their control group. Their regression specification includes year and state fixed effects. They find that women living in states following mandate implementation are 5% more likely to report use of OCPs relative to women living in states without mandates. They find no change in use of other prescription contraceptive methods (Atkins and Bradford, 2014).

The final study, an in-progress working paper, also uses data from the 1998 through 2011 BRFSS waves to examine contraceptive utilization, this time using all states for which BRFSS data is available. The states that included questions about family planning in the BRFSS survey varied from year to year, so although their data contains women in all 50 states, there are only nine mandates for which they have at least one year of pre- and post-mandate data (DE HI, IO, IL, MA, ME, NC, NH, and NM) (Dills and Cotet-Grecu, 2014). In addition, the mandates they list in their appendix appear to be slightly inaccurate; there is a mandate mistakenly assigned to Alabama, and no mandate listed for Arizona, Colorado, Michigan and Montana. They find that contraceptive mandates are associated with a 19 percentage point rise in OCP use among 18-19 year olds (an 86% increase), and a 5 percentage point rise in OCP use among 20-24 year olds (16% increase). The 19pp rise in OCP use among 18-19 year olds suggests a demand elasticity much larger than previous literature has suggested.

They also find a significant increase in sexual activity among 18-19 year olds and a significant decrease in sterilization among 20-34 year olds. They then move from individual-level BRFSS data to aggregate state-year-level data to examine abortion rates, fertility rates, prenatal care rates, and rates of delivery complications. They find decreased fertility, increased prenatal care, and decreased delivery complications among Hispanic women, but not other racial/ethnic groups. They find no effects on abortion rates for any groups.

My project also fits into the growing body of research that seeks to examine and quantify the impact of the ACA. However, few studies focus solely on the contraception coverage mandate itself. One study has examined the change in OOP prices for women following the ACA mandate among a longitudinal survey of 892 women. They found that the percent of privately insured women paying zero for their OCPs had increased from 15% to 67% between fall of 2012 and spring of 2014, and the average monthly price had decreased from $14 to $6. Increases of similar magnitude were seen among users of injectable contraception, IUDs and the vaginal ring (Sonfield et al., 2014). A report by IMS Health on prescription drug use in 2013 found that prescriptions of hormonal contraceptives had increased by 4.6% between 2012 and 2013, with the fraction of patients with zero cost-sharing rising from 20% to 50%. The report doesn't specify exactly which types of contraceptives are included in their data, but they estimate that the ACA mandate reduced OOP spending on contraceptives in the U.S. by $483 million in 2013 (IMSHealth, 2014). To my knowledge, no study has yet examined the impact of the ACA mandate on contraceptive use in a causal empirical framework.

My proposed analysis will add to the existing literature in several important ways. First, it will examine a newer dimension of increased access to contraception: that of improved coverage of contraception by health insurance. This margin of access has been little-studied in the empirical literature. My analysis of the state-level mandates differs from prior studies of these mandates for several reasons: I examine all of the state-level mandates rather than a subset, I look at whether different types of mandates have differential impacts, and in my claims data analysis of the state mandates I can identify women in plans affected by the mandate better than in previous studies. Second, to my knowledge this will be the first study to date to examine the ACA's mandate in a causal framework. My results, while necessarily short-term, will add empirical evidence to a hotly debated policy issue lacking in rigorous empirical analysis. Lastly, this project will add to the larger body of literature studying the effects of insurance coverage mandates for preventive services.

1.3. Theoretical motivation

1.3.1. Expected utility framework: One period, one method

I use a model of expected utility to conceptualize contraceptive use as a preventive service, in this case a service to reduce the probability of unwanted pregnancy. A woman has utility purely over consumption, $U(x)$, and utility is increasing in x. There is only one potential birth control method with price P, which she can choose to use or not. If she uses it, her probability of pregnancy is lowered from ϕ to $\phi - \triangle\phi$, with $0 \leq \phi \leq 1$. If she becomes pregnant, she incurs a cost of B. Her income is M. She will choose to use the birth control method if her expected utility from use is greater than her expected utility from no use, in other words, if the following inequality holds:

$$(1 - \phi + \triangle\phi)U(M - P) + (\phi - \triangle\phi)U(M - P - B) >$$
$$(1 - \phi)U(M) + \phi U(M - B)$$

A small decrease in P will increase the left-hand side of the equation while leaving the right-hand side unchanged, making a woman more likely to choose to purchase contraception. We can rearrange the above equation to read:

$$\triangle\phi[U(M - P) - U(M - P - B)] >$$
$$(1 - \phi)[U(M) - U(M - P)] + \phi[U(M - B) - U(M - P - B)]$$

On the left are the expected utility gains from using the birth control method and therefore lowering the risk of incurring the cost of pregnancy. On the right are the expected utility losses from incurring the cost of the birth control method, regardless of pregnancy outcome. The result is intuitive; a woman will choose to utilize a birth control method if the expected gain from reducing her risk of pregnancy outweighs the certain loss from paying for the method. The determining factors for which choice a woman makers are her baseline probability of pregnancy (ϕ), the magnitude of the change in that probability from using the birth control method ($\triangle\phi$), and the size of B relative to P.

Let us assume now that among a population with N women seeking to avoid pregnancy, each individual woman has an individual cost of unwanted pregnancy, B_i. B_i is distributed according to some probability distribution, $f(B)$. There are important reasons why B_i is likely to vary across women. Direct medical costs related to pregnancy will vary by type and generosity of insurance coverage. B_i also could incorporate income losses from time spent on unpaid parental leave or the cost of childcare.

For now, let's assume that the marginal utility of consumption is constant, i.e., utility is linear in consumption according to the formula $U(x) = ax + b$. It's then easy to show that the user will choose to purchase birth control if $B_i > \frac{P}{\triangle\phi}$. The share of women who chose to use birth control in the population is equal to:

$$Prob(B_i > \frac{P}{\triangle\phi}) = 1 - F(\frac{P}{\triangle\phi})$$

Total population demand, D, is therefore equal to $D = N \times [1 - F(\frac{P}{\triangle\phi})]$. As B_i increases, it is more likely to cross this threshold value of $\frac{P}{\triangle\phi}$, so demand for contraception is increasing in B_i ($\frac{dD}{dB_i} > 0$). If P decreases, as it would if the out-of-pocket price of contraceptives falls, the threshold any given B_i must exceed is lowered, so $\frac{dD}{dP} > 0$ as well.

However, if we assume that women are risk averse and that their utility over consumption is concave, $\frac{dD}{dP}$ is still positive but we can no longer sign $\frac{dD}{dB_i}$. This can be seen by rearranging the original decision rule to put all costs that include B_i on one side of the equation as shown below. A woman chooses to use contraception if:

$$\triangle\phi[U(M-P) - U(M-P-B_i)] - \phi[U(M-B_i) - U(M-B_i-P)] >$$
$$(1-\phi)[U(M) - U(M-P)]$$

Looking at this equation, $U(M-P) - U(M-P-B_i)$ is increasing as B_i increases, but $U(M-B_i) - U(M-B_i-P)$ is also increasing as B_i increases. We therefore cannot say whether demand for contraception among risk-averse women is increasing or decreasing in B_i without making more assumptions about the functional form of utility.

Although this result seems counter-intuitive, it is consistent with the rest of the theoretical literature examining demand for preventive services that I discussed in the prior section. In general, the theoretical literature on prevention has found that demand for prevention cannot be assumed to be increasing in risk aversion, and that assuming concavity of utility with respect to consumption is neither necessary nor sufficient for demonstrating an optimal level of preventive services that a consumer will demand (Ehrlich and Becker, 1972; Dionne and Eeckhoudt, 1985; Jullien et al., 1999).

In summary, economic theory unambiguously predicts that a drop in the out-of-pocket price for contraception will cause the amount of contraception demanded to increase. However, theoretical predictions of whether demand for prevention increases in the cost of the unwanted outcome are ambiguous. If we assume that women are risk neutral, demand for prevention is increasing in the cost of an unwanted pregnancy. But if we assume that consumers are risk averse, we cannot make such a prediction and must instead examine this question empirically.

1.3.2. Expected utility framework: Two periods, two methods, and non-monetary costs

I now consider this model in a world where there are two available birth control methods and two periods. A woman has a risk of pregnancy ϕ in each period, and discount rate β, where $0 < \beta < 1$. If she gets pregnant, she incurs cost B_i. In period one, she chooses either no method, Method 1 with price P_1, or Method 2 with price P_2. Method 1 is analogous to an oral contraceptive; it lowers the risk of pregnancy from ϕ to $\phi - \triangle\phi$, but must be purchased in each period. Method 2 is analogous to an IUD or sterilization; it lowers the risk of pregnancy to zero, and only needs to be purchased once. However, if a woman chooses Method 2, she also incurs a one-time non-monetary cost of C_i. This could be considered a cost in time and discomfort from receiving the IUD or a surgical procedure. If there are network effects on contraceptive choice, C_i could also represent emotional discomfort from choosing a method that is less commonly used and with which the woman is less familiar. C_i may be lower for better educated women or women who know other women who have successfully used the IUD.

A woman will choose the method that maximizes the sum of expected utility in both periods:

$$\begin{gathered}
No\,method: \ (1-\phi)U(M) + \phi U(M-B_i) \\
+\beta(1-\phi)U(M) + \beta\phi U(M-B_i) \\
Method\,1: \ (1-\phi+\triangle\phi)U(M-P_1) + (\phi-\triangle\phi)U(M-P_1-B_i) \\
+\beta(1-\phi+\triangle\phi)U(M-P_1) + \beta(\phi-\triangle\phi)U(M-P_1-B_i) \\
Method\,2: \ U(M-P_2) - C_i + \beta U(M)
\end{gathered}$$

If we again assume that utility is linear in consumption in the form $U(x) = ax + b$, these expressions simpify and the woman will choose whichever is largest (less negative) of the following three expressions:

$$\begin{gathered}
No\,method: \ -\phi B_i(1+\beta) \\
Method\,1: \ -\phi B_i(1+\beta) + [\triangle\phi B_i - P_1](1+\beta) \\
Method\,2: \ -P_2 - \tfrac{1}{a}C_i
\end{gathered}$$

Comparing the first and second lines, we can see that the same choice between no method and Method 1 is embedded in this decision: if Method 2 is not an option, the woman will choose Method 1 if $B > \frac{P}{\triangle\phi}$. However, both options must now also be compared with P_2, C_i and the utility gain that a woman gets from having an expected cost of pregnancy equal to zero. For the expression for expected utility from Method 2, note that a, the multplier on income in the utility function, scales the relative weight that a woman will place on the income loss from Method 2 and the non-monetary utility cost C_i. As a increases, $-P_2 - \frac{1}{a}C_i \to -P_2$.

We can again consider what might induce a woman to change her choice of method. As with the simpler model above, an increase in B_i or a drop in P_1 make it less likely a woman will choose no method. With the addition of a second period in the model, the discount factor also matters. As $\beta \to 1$, that is, as the woman values future costs closer to present costs, the options of no method and Method 1 become more negative relative to Method 2, suggesting that forward-thinking women will be more likely to choose Method 2. Obviously, a drop in P_2 or C_i will make Method 2 more attractive. The impact of a drop in both prices simultaneously is more difficult to predict because it depends on the relative magnitudes of all the other parameters in the model.

1.3.3. Theoretical predictions

From the above model, I can therefore draw the following predictions:

1. A price decrease for a contraceptive method will increase the probability that a given woman chooses that method.

2. When prices for two methods decrease simultaneously, it reduces the probability that a women will choose no method. It will also likely result in substitution from one method to the other, but the direction of that substitution must be determined empirically.

1.3.4. Other theoretical alternatives

The above model considers contraception purely as a preventive service. However, there are alternative models that could be considered. Other models that have examined contraceptive choice have considered the possibility that contraceptive choice impacts utility via sexual activity or relationship status (Collins and Hershbein, 2013). Another important issue is that contraceptive choice is actually a dynamic, rather than a static, process. An alternative modelling strategy would be to consider contraception as a form of future investment and use a human capital model such as that of Grossman (1972). However, because my empirical strategy does not involve modeling contraceptive choice dynamically, I believe that is beyond the scope of this project.

Another possiblity is that women are employing some type of hyperbolic discounting to their contraceptive product choice. LARC methods are cheaper over 5 or more years of use, but their costs are concentrated up-front rather than spread over smaller monthly or tri-monthly expenditures. Add to that the fact that LARC methods require a small–but uncomfortable–procedure to be inserted/implanted, and these higher up-front costs may cause women who overweight the present relative to the future to choose products like condoms or the pill instead. A recent study of fertility and discounting that used inconsistent saving attitudes as a proxy for time inconsistency did find empirical evidence that women who hyperbolically discount have different fertility patterns than women who do not (Wrede, 2011).

In addition, social networks have a potentially large impact on contraceptive choice, and I could choose to model this more explicitly than simply adding a non-monetary cost for Method 2 in my model above. An older study examining differences in contraceptive method choice by country highlighted the fact that most countries have one or two dominant methods at most, and that the dominant method varies widely by country; in 1991, the two most commonly used methods in Belgium were the pill (46.4%) and female sterilization (11.4%) while in South Korea they were female sterilization (38.5%), male sterilization (12%) and condoms (10.2%), with only 3% of user choosing the pill (Potter, 1999). It seems implausible that these differences could be fully explained simply by a model of heterogenous preferences. A number of studies in the demography literature have attempted to theoretically or empirically model contraceptive choice as influenced by social networks or social factors, and have generally found that social networks are important in contraceptive choice (Kohler, 1997; Kincaid, 2000; Montgomery and Casterline, 1996; Valente et al., 1997). While interesting, any empirical analysis of network effects would require some way to identify networks of contraceptive users, something that isn't possible with the data currently available to me.

One area where social networks may have a particularly large impact in the U.S. is in IUD choice. In the 1970s, the then-most popular brand of IUD, the Dalkon Shield, was found to be associated with increased risk of infection and subsequent infertility. These finding were the subject of congressional hearings and considerable press attention in the U.S. In the subsequent decade, all other IUD manufacturers withdrew from the U.S. market, citing fears of lawsuits. IUD models currently on the market have been extensively studied and found to be safe, but a generation of women and providers in the U.S. still have a negative impression of the method. In contrast, the Dalkon Shield product withdrawal had much less impact in Europe, where it was less commonly used and where a copper IUD model remained available during the 1970s and 1980s. Overall, rates of IUD use in Europe average 10-15% of contraceptive users, compared with 2% in the U.S in 2002 (Sonfield, 2007). However, there's some evidence that this trend is reversing in the U.S., as new and safer models of IUDs are again growing in popularity, from 0.8% of women using contraception in 1995 to 5.6% in 2006-2010 (Jones et al., 2012).

Women may also simply choose their contraceptive products "irrationally" for other reasons. There is certainly some suggestive evidence that this is true; see Section 1.2.1 for a detailed discussion of the literature that suggests that some women may be unable to correctly estimate their risk of pregnancy or the potential costs of an unplanned pregnancy. If this is the case, I could consider a model that incorporates principles from the behavioral economics literature such as prospect theory.

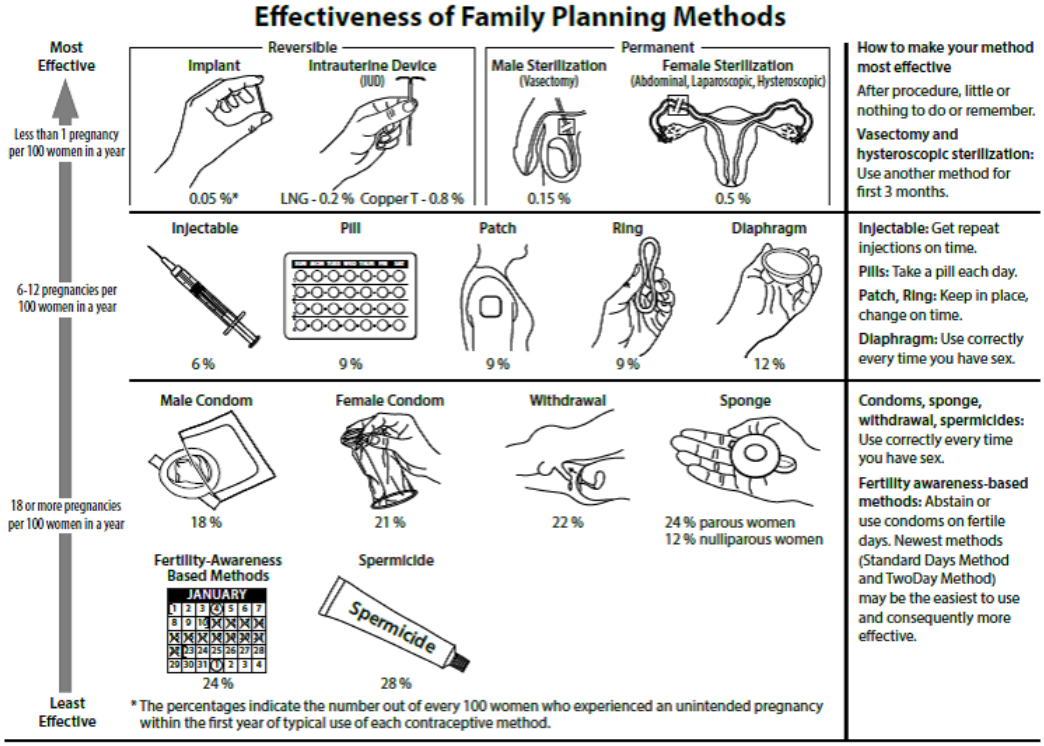
Lastly, this model also currently ignores the potential positive externalities of contraceptive use that have been found in the empirical literature. If access to contraceptives truly results in positive economic externalities, then there is potentially a normative analysis that could be done to estimate welfare gains from mandating coverage. However, since my empirical analysis does not estimate these gains, for now I have chosen not to incorporate this into my model.

1.4. Outline

The remainder of this dissertation proceeds as follows. In Chapter 2, I estimate the impact of state-level insurance coverage mandates using a nationally representative survey, the National Survey on Family Growth. In Chapter 3, I analyze the same state-level mandates using an administrative claims dataset. This second dataset allows me to both better identify the women affected by the state mandates, and to search for evidence that the mandates causally impacted insurance coverage of contraceptives. Chapter 4 contains an analysis of the impact of the Affordable Care Act's mandate on OOP costs and utilization of contraceptives. In Chapter 5, I summarize my empirical results, and discuss the policy implications, limitations, and future research directions of the project.

1.5. Tables and Figures

Figure 1: Contraceptive methods by mechanism and effectiveness



Source: Centers for Disease Control and Prevention (CDC, 2014)

CHAPTER 2 : The impact of state-level contraception coverage mandates in survey data

2.1. Introduction

In this chapter, I examine the impact of state-level contraception coverage mandates on OOP costs and contraceptive utilization using data from a nationally representative survey, the National Survey on Family Growth (NSFG). I find no evidence that the mandates impacted overall contraceptive utilization or the distribution of methods chosen.

2.2. State-level mandates

Between 1999 and 2010, 29 states passed laws that mandated coverage of prescription contraceptives. One mandate—in Texas—was effectively repealed several years after passage, leaving 28 mandates currently in effect. These mandates require private health insurance plans to include coverage for prescription contraceptives at "equivalent cost-sharing levels" to other covered prescription drugs. All employers that self-insure are exempt from these mandates under the Employee Retirement Income Security Act (ERISA). Once law, these mandates take effect for an individual when their plan renews for a new plan-year, most often in January of the year following the mandate's effective date.

Most of the state-level mandates require insurers to cover all FDA-approved forms of contraception, although many allow insurers to use formularies to limit coverage to certain brands. Emergency contraception is explicitly excluded by two of the states (Wolters Kluwer, 2013). However, for all other mandates emergency contraception is included only to the extent that it requires a prescription, and most brands are now available OTC and therefore not included in the mandates.

These mandates vary across several dimensions. The first is whether they specifically mandate coverage of related outpatient contraceptive services in addition to prescription drug and device coverage, i.e., whether a plan must cover both the cost of an IUD and the cost of the insertion procedure. Mandates also vary by which categories of religious employers can exempt themselves from the mandate. The Alan Guttmacher institute (AGI), a nonprofit reproductive health policy research and advocacy organization, has classified these mandate into four exemption categories: none, limited, broad and expansive. Table 1 lists which types of employers fall under these categorizations (AGI, 2014).

Table 2 summarizes each mandate by effective date, whether it included outpatient coverage, and exemption category. The effective date is when the mandate took legal effect, not when the law that included the mandate was passed. The mandates included in Table 2 have been reconciled from three sources: the National Conference of State Legislatures website, personal communications with the staff of the Alan Guttmacher Institute,[1] and a Wolters Kluwer Law & Business White paper (NCSL, 2012; Wolters Kluwer, 2013; AGI, 2014).

There is suggestive evidence that these mandates were binding for some insurance plans. The Kaiser Family Foundation's Employee Health Benefits Annual Survey asked about coverage of contraceptives for several years during the early 2000s. Figure 2 shows the overall rates of coverage for OCPs, all reversible methods, and sterilization, broken out by workers in smaller firms (under 200 employees) and larger firms (over 200 employees) from 2001 to 2004. For comparison, both figures include

[1] The effective dates for each mandate were obtained from AGI by request.

the percentage of workers with prescription drug benefits and prenatal care benefits. Coverate rates for reversible methods and sterilization are presented as dots instead of lines to reflect that those numbers were not reported in 2002 or 2004. For both larger and smaller firms in 2001, prescription drugs and prenatal care were almost universally covered during this period, with little change seen in rates of coverage over time. In contrast, coverage of OCPs, reversible methods, and sterilization were all significantly lower, with 54% of workers at smaller firms and 69% of workers larger firms offered coverage of OCPs offered by their plans. Coverage rates for all leading reversible methods was only 31% for smaller firms and 45% for larger firms in 2001.

This coverage landscape was changing rapidly; by 2004, coverage of OCPs had risen to 87% and 89% for smaller and larger firms, respectively. Similar rises were seen for coverage of reversible methods and for sterilization (Levitt et al., 2000, 2001; Claxton, 2002; Claxton et al., 2003). This is the precise period in which the majority of the contraceptive coverage mandates were being implemented.

Similarly, a cross-sectional AGI survey of private health insurance plans conducted in both 1993 and 2002 found that private insurance coverage of birth control rose by 40 to 60 percentage points in this time period, depending on the birth control method. In their 2002 survey, they also found single-state plans in states without mandates were significantly less likely to cover the five leading birth control methods than plans in states with mandates (Sonfield et al., 2004).

This evidence only demonstrates that the increase in state-level mandates co-occured with an increase in the national rates of insurance coverage of contraceptives; to date, I have not seen any study use an appropriate analytic approach to argue that it was the passage of the mandates that directly caused an increase in insurance coverage of contraceptives during this time period. It's possible that the rise of insurance coverage of contraceptives coincided with but was not caused by the increasing use of state contraceptive coverage mandates. They could both have resulted from increased consumer demand or political activism surrounding the issue of women's rights in the workplace.

An unanswered question in the literature is why the insurance coverage of contraceptives was so much lower than that of other prescription drugs in the early 2000s. I have not found a good explanation for why this was, but one possibility is demand among consumers or selection into employers. If some employers employed primarily men, older women, or women uninterested in contraception, they would be less likely to offer insurance coverage for those services. If this is the case, it would make it less likely that insurance coverage mandates would have an effect on utilization, because the women in employers who did not offer coverage prior to the mandate would have lower demand in general for contraception.

2.3. Data: The National Survey of Family Growth

The National Survey of Family Growth (NSFG) is a nationally representative survey of women ages 14 to 45 that asks detailed questions concerning all aspects of a woman's sexual activity and history, in addition to gathering other demographic information concerning income, employment, and family circumstances. I combine three waves of this survey (1995, 2002, and 2006 – 2010) to create a pooled cross-sectional dataset that spans the time period during which the contraception coverage mandates were passed. While my analysis includes data from all 50 states, the survey only has data both pre- and post-mandate for 25 of the 29 mandates. The terms of my data agreement do not allow me to disclose which mandates are missing from my analysis.

I restrict my primary analysis sample only to women who report that they are covered by a private health insurance plan. My sample size is 19,249 women, 5,592 of whom were living in states with active contraceptive coverage mandates when they were interviewed. For my primary analysis, I do not exclude women who were pregnant, not sexually active or physically unable to become pregnant, because those choices may be endogenous to the OOP cost of prescription contraceptives and therefore the policy I am studying. I do perform some sensitivity analyses using a sample further restricted to women seeking to avoid pregnancy who are physically able to become pregnant.

I explore different model specifications to see if my results are sensitive to my selection of covariates. I create groups of variables–the ones I considered the most important *a priori*–and add them to the models sequentially to test for sensitivity to different categories of covariates. Table 3 lists the covariate groups I use for my analysis, along with the condition indices (CIs) of the groups that I chose. The CIs vary from 16 to 50, suggesting that some of these covariates are definitely collinear. While high CI values for groups of covariates can be a concern, they can be safely ignored if the variance inflation factor on the coefficient of interest remains low (Allison, 2012). The first two categories of covariates are either public or restricted variables available from the NSFG survey. Contextual-variables at the county level from the NSFG have the years the data was gathered for each wave listed in parentheses. The third category of covariates, state-level covariates, are time-varying covariates from a dataset of state-level laws that may impact a woman's access to contraceptives. These include variables such as Medicaid coverage of family planning, laws that impact access to abortion, generosity of state-level welfare benefits, etc. This dataset was compiled by Melissa Kearney and Philip Levine for their working paper examining trends over time in teen birth rates, and the sources of each of these covariates can be found in their appendix (Kearney and Levine, 2012).[2]

Because the products available on the market changed during the time-frame of the study, I collapse prescription birth control use into the following categories: short-term hormonal (the pill, the patch or the ring), IUD, implant, injection, sterilization, diaphragm or cervical cap, and emergency contraception. Women in the survey were given the option to report using multiple methods of birth control at the same time, so a women can be classified as using more than one prescription method. I define LARC or "long-term" prescription contraceptive use as use of an IUD or an implant. Women who reported only using non-prescription methods or no method were classified as non-users of prescription contraception.

Accessing certain restricted variables requires performing all analyses in the National Center for Health Statistics (NCHS) Research Data Center (RDC) in Hyattsville, MD. Funding for these analyses was made possible by a grant from the Wharton Risk Center Ackoff Doctoral Student Fellowship.

2.4. Methods and analytic strategy

I use a difference-in-difference analysis to isolate the marginal effect of passing a coverage mandate upon contraceptive utilization. My two primary outcomes of interest are use of any prescription contraceptive (the extensive margin of use) and use of any long-acting prescription contraceptive (the intensive margin of use).

[2]This dataset was obtained upon request from Philip Levine, Professor of Economics at Wellesley College.

My general model is as follows:

$$P(Y_{its} = 1) = f(\beta[1 = \text{Mandate}]_{ts} + \gamma_t + \theta_s + \mathbf{X_{its}} + \mathbf{Z_{ts}})$$

Here, i indexes individuals, s indexes states and t indexes years. The independent variable is equal to one if the woman is interviewed in a year following mandate implementation in state s. This analysis includes state and year fixed effects. The coefficient of interest is β, which reflects the relative change in birth control utilization after mandate implementation in states that implemented mandates compared with states that did not implement mandates. I use linear probability models or fixed-effects logit models for binary outcomes, and cluster robust standard errors at the state level.

Vector $\mathbf{X_{its}}$ includes personal demographics, childhood experiences, prior sexual history and number of partners, prior contraceptive experience, economic circumstances, and number of pregnancies. Geographic-level covariates included in $\mathbf{Z_{ts}}$ include county-level restricted variables from the NSFG and time-varying state level variables concerning laws that may impact a woman's choice of contraceptives. See Table 3 for a detailed list of covariates.

I also examine differences between mandates with different exclusion policies. Here I use AGI's categories of mandates' exemption policies: none, limited, broad, or expansive (Table 1). My empirical model to test for differences by mandate type is:

$$\begin{gathered}P(Y_{its} = 1) = f(\beta_1[1 = \text{Mandate, none}]_{ts} + \beta_2[1 = \text{Mandate, limited}]_{ts} \\ +\beta_3[1 = \text{Mandate, broad}]_{ts} + \beta_4[1 = \text{Mandate, expansive}]_{ts} \\ +\gamma_t + \theta_s + \mathbf{X_{its}} + \mathbf{Z_{ts}})\end{gathered}$$

Here, β_1 is the impact of a mandate with no exemptions allowed, while β_2 through β_4 reflect the impact of mandates with the three possible exemption policies.

Mandates also vary by whether they include coverage of related outpatient services. I examine the difference between these two types of mandates using a model very similar to the above analysis of differing exemption policies:

$$P(Y_{its} = 1) = f(\beta_1[1 = \text{Mandate, no outpt covg}]_{ts} + \beta_2[1 = \text{Mandate, outpt covg}]_{ts}$$
$$+\gamma_t + \theta_s + \mathbf{X_{its}} + \mathbf{Z_{ts}})$$

Here, β_1 is the impact of a mandate that doesn't include coverage of outpatient services, and β_2 is the impact of a mandate that does mandate coverage of outpatient services.

My identification strategy is sensitive to the typical assumption that drives identification for difference-in-difference models; that is, the possibility that states that enacted mandates would have had different trends in the absence of the mandates than states without mandates. Probably the most concerning source of endogeneity in this case is legislative endogeneity, that is, that states enacting these laws also tend to enact other laws that may affect contraceptive choice. I control for this by including a comprehensive set of state-level controls for various legislative policies that could potentially impact contraceptive choice, as described in the previous section.

I considered including parametric state-specific linear time trends to the model, but for the majority of mandates I only have one or two periods prior to the mandate implementation. In this situation, the trend that effect size is differenced from is almost entirely in the post-period, conflating policy effects and trends (Angrist and

Pischke, 2009). I therefore do not include parametric state-specific time trends in these analyses.

2.5. Results

Table 4 shows weighted descriptive statistics for my NSFG analytic sample by mandate status. There are differences between the two populations. In general, women living in states with mandates are more likely to be Hispanic (14.6% vs. 7.35%), live in non-rural areas (88.5% vs. 78.9%), be higher income (43.8% vs. 33.8% make more than $60,000/year) and be non-religious (19% vs. 12.8%). Table 5 shows the overall weighted average use of contraceptives methods by state mandate status. Cross-sectionally, women in states with mandates are more likely to use prescription birth control than women in states without mandates (41.2% vs. 40.5%), although this difference is not statistically significant. Use of LARC (aka “long-term”) contraceptive methods is twice as common in states with mandates as in states without, and this difference is statistically significant.

Figure 3 shows the percentage of women using prescription contraceptives by years pre- and post-mandate, in the states that implemented mandates. There are some clear trends over time, as use of sterilization appears to fall while LARC use and short-term hormonal use increase, but there are no obvious changes in level or slope following mandate implementation. Figure 4 shows rates of contraceptive use broken out by the year of mandate implementation. States with earlier mandates do not appear to show increases in contraceptive use following mandate implementation relative to states with mandates that were implemented later.

Table 6 shows the results for the baseline model, with and without different sets of covariates, for the outcome “any prescription birth control use.” The coefficient of interest is approximately 0.02 for all specifications, representing a 2% increase in the probability of prescription BC use, but no specification achieves statistical significance. Table 7 shows the same specifications, but this time the outcome is use of any LARC method. The coefficient of interest is non-significant and very close to zero for all specifications.

Table 8 shows regressions with the mandates broken out by category of refusal provision. The reference category is states with no mandates. Although none of the coefficients achieve statistical significance, there is a suggestive trend that the magnitudes of the coefficients of interest decrease as the refusal provisions grow more inclusive, i.e., as more businesses are allowed to exempt themselves from the mandates. For instance, the coefficient on states with mandates that allowed no exemptions is 0.04, decreasing to 0.02 for intermediate refusal categories, and decreases further to -0.01 for mandates with expansive refusal provisions. Table 9 displays results from the mandates broken out by whether the mandates specifically include coverage of related outpatient contraceptive services. The reference category is states with no mandates. The coefficients of interest for these models are non-significant.

Collinearity is a concern for models with many different covariates, and can inflate standard errors. I used the Stata command *estat vif* following my regressions to examine models for collinearity that may be impacting my coefficient of interest. Although there was significant collinearity between some of the regressors, the variance inflation factor (VIF) on the coefficient of interest for the fully-adjusted model (outcome = "any prescription contraceptive use") (Column 4 of Table 6) was still only 3.24, less than the rule-of-thumb value of 10. The VIF on the basic model with the outcome any prescription contraceptive use (Column 1 of Table 6) was approximately 1.5. I'm therefore not too concerned that collinearity is inflating my standard errors for these models.

2.5.1. Robustness checks

I also conduct the following robustness checks for this analysis:

- I examine the models broken out by age and income category, and find no significant results for any subgroups of women.

- I examine the models using a smaller analysis sample of non-pregnant women both physically capable of becoming pregnant and seeking to avoid pregnancy. This population, while potentially endogenous, is also the most likely to demand contraceptives and so we might expect a stronger demand response in this sub-population. Results for these analyses were also statistically insignificant.

- Models run using logit specifications were similarly insignificant.

- I also tried unweighted or differently weighted versions of models above. While the coefficient magnitudes were very similar, at times the coefficients of interest on unweighted models achieved statistical significance. The results I report here are the regressions done according to the instructions provided in the NSFG documentation.

2.6. Discussion

My results suggest that the state-level contraception coverage mandates did not have an impact on contraceptive use. There are several potential reasons why this may be the case. It's possible that these mandates simply didn't bind because the majority of plans already offered coverage of contraceptives, although the available national data I discuss in Section 2.2 suggests that there was a significant subset of employers in my data who did not offer coverage of contraceptives to their employees at the beginning of my study period.

However, it is possible that the impact of the mandates themselves on insurance coverage was small or nonexistent. The changes in insurance coverage of contraceptives could have taken place concurrently with the implementation of mandates if both were driven by consumer activism and increased policy discussions surrounding contraceptive coverage. Or the the change in coverage seen in surveys could be driven primarily by increases in coverage among employers who self-insure their plans, driven by consumer demand rather than regulatory requirement.

Another explanation for my results is that demand for contraceptives may be very inelastic in privately insured populations. This would be consistent with the estimates of demand elasticity for OCPs reported by Collins and Hershbein (2013) and from studies of demand elasticity in the developing world. In the next chapter, I attempt to distinguish between these possibilities using an administrative claims dataset.

2.6.1. Limitations

The primary limitation of the NSFG survey data is that it has a small number of periods (1995, 2002, and 2006 - 2010). This makes it difficult to test or control for differential pre-trends in the treatment vs. control states. Additionally, although it is possible to identify women in private health insurance, it's not possible to identify women in fully-insured vs. self-insured plans. I address some of these challenges with the analysis described in the next chapter.

It's also possible that I'm limited in my power to detect an effect by the sample size of the data. To do a *post hoc* power analysis, I must calculate a standardized "effect size" measure called Cohen's f^2. This effect size is equal to a ratio of R^2 values for the regression with and without the coefficient of interest. If R^2_+ is the R^2 for my baseline model, and R^2_- is the regression for my baseline model including all fixed effects but excluding the contraception coverage mandate dummy, then:

$$f^2 = \frac{R^2_+ - R^2_-}{1 - R^2_+}$$

I plan to return to the RDC to perform this power calculation. But I can guess at a range for now. For my baseline model, $R^2_+ = 0.0139$. If I assume that $R^2_- = 0.0138$ (an effect size of $f^2 = 0.001$), then my baseline analysis including only state and year fixed effects has a power of 99.2%. If I assume that $R^2_- = 0.01385$ (an effect size of $f^2 = 0.0005$), then my power falls to 87%. If I assume that $R^2_- = 0.01381$ (an effect size of $f^2 = 0.0001$), then my power falls to 28%. All of these calculations were done with the free software G*Power 3.1.

The fact that I'm not able to separate out women in health plans offered by self-insured employers will also limit my power. Since self-insured employers are exempted from state mandates, women in these plans can live in states with mandates but be insured by exempted plans. This is a significant proportion of my sample; according to the Kaiser Employer Health Benefits Annual Survey, the percent of people in employer-sponsored health insurance offered by self-insured employers ranged from 44-59% between 1996 and 2010 (Claxton et al., 2003, 2010). If I recalculate the power analysis I've described above for a sample that is 50% smaller, for effect sizes equal to 0.001, 0.0005, and 0.0001, I get 92%, 70% and 25% power respectively. So halving my sample size does reduce my power, but the effect size is the larger determinant of my statistical power.

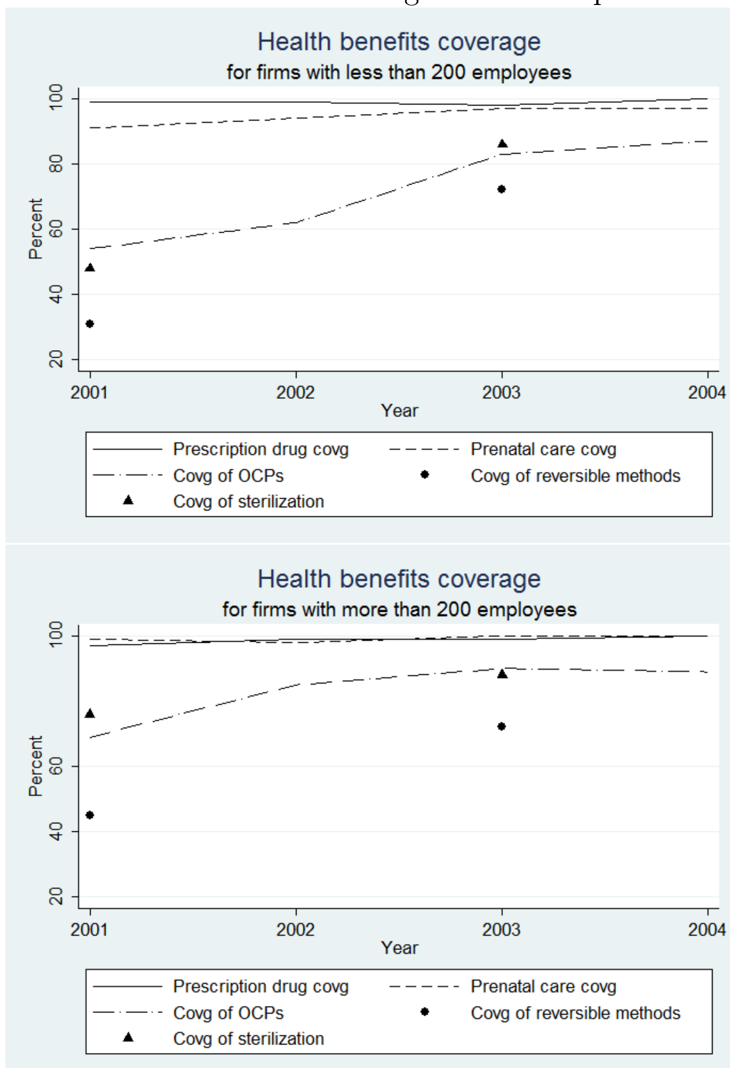
2.6.2. Future work

There are several additional analyses I plan to do on my next visit to the RDC. The first is to perform the post-hoc power analysis I describe in Section 2.6.1. The second is to attempt to test whether the mandates caused changes in insurance coverage of contraceptives. The NSFG has one variable concerning health insurance coverage of contraception; women who report that they obtained a prescription for birth control in the last 12 months are asked whether they self-paid for the method or whether their insurance paid for it, or both. I can therefore use a difference-in-difference strategy to test whether women receiving birth control prescriptions report that their insurance paid for that prescription–either in whole or in part–at higher rates in states with mandates. There are some potential issues with this analysis. The first is that any power limitations I have in my baseline analysis will be exacerbated in this one, because the question was only asked of a subset of women. Secondly, I'm concerned about systematic mis-reporting for this question. In particular, given the low levels of understanding of how health insurance works in the general population, I'm concerned that women purchasing their birth control at a pharmacy may report that they self-paid, when in fact they paid a co-pay. If this is the case, this would potentially bias me towards finding a null result for this question.

Lastly, I can also explore alternate weighting schemes for my analyses. In all of my results thus far, I use the weights provided by the NSFG as instructed in the NSFG documentation. However, I could consider an alternate weighting strategy, or consider collapsing to the state-level to perform an analysis that weights each state equally.

2.7. Tables & Figures

Figure 2: Private insurance coverage of contraceptives over time



Source: Kaiser Family Foundation and Health Education Research & Trust Employee Health Benefits Annual Survey Reports, 2001 to 2004.

Table 1: AGI's exemption categories for state-level mandates

Type of employer allowed an exemption	None	Limited	Broad	Expansive
Chuches and church associations		X	X	X
Religiously affiliated schools and charities			X	X
Hospitals				X

Table 2: Contraception coverage mandates by state

<table>
<tr><th>State</th><th>Effective Date</th><th>Exemption category</th><th>Outpatient coverage</th><th>Pre & post NSFG data?</th><th>Pre & post OI data?</th></tr>
<tr><td>Arizona</td><td>12/31/02</td><td>expansive</td><td>yes</td><td rowspan="29">Yes for 25 of 29 mandates</td><td>yes</td></tr>
<tr><td>Arkansas</td><td>7/12/05</td><td>broad</td><td>no</td><td>yes</td></tr>
<tr><td>California</td><td>1/1/00</td><td>limited</td><td>no</td><td></td></tr>
<tr><td>Colorado</td><td>1/1/11</td><td>none</td><td>no</td><td>yes</td></tr>
<tr><td>Connecticut</td><td>10/1/99</td><td>expansive</td><td>no</td><td></td></tr>
<tr><td>Delaware</td><td>6/7/00</td><td>expansive</td><td>yes</td><td></td></tr>
<tr><td>Georgia</td><td>7/1/99</td><td>none</td><td>no</td><td></td></tr>
<tr><td>Hawaii</td><td>1/1/00</td><td>expansive</td><td>yes</td><td></td></tr>
<tr><td>Illinois</td><td>1/1/04</td><td>expansive</td><td>yes</td><td>yes</td></tr>
<tr><td>Iowa</td><td>7/1/00</td><td>none</td><td>yes</td><td></td></tr>
<tr><td>Maine</td><td>3/1/00</td><td>broad</td><td>yes</td><td></td></tr>
<tr><td>Maryland</td><td>10/1/98</td><td>expansive</td><td>yes</td><td></td></tr>
<tr><td>Massachusetts</td><td>1/1/03</td><td>broad</td><td>yes</td><td>yes</td></tr>
<tr><td>Michigan</td><td>8/21/06</td><td>broad</td><td>yes</td><td>yes</td></tr>
<tr><td>Missouri</td><td>1/1/02</td><td>expansive</td><td>no</td><td>yes</td></tr>
<tr><td>Montana</td><td>3/28/06</td><td>none</td><td>yes</td><td>yes</td></tr>
<tr><td>Nevada</td><td>10/1/99</td><td>none</td><td>no</td><td></td></tr>
<tr><td>New Hampshire</td><td>1/1/00</td><td>none</td><td>yes</td><td></td></tr>
<tr><td>New Jersey</td><td>7/1/06</td><td>broad</td><td>no</td><td>yes</td></tr>
<tr><td>New Mexico</td><td>7/1/01</td><td>expansive</td><td>no</td><td></td></tr>
<tr><td>New York</td><td>1/1/03</td><td>limited</td><td>no</td><td>yes</td></tr>
<tr><td>North Carolina</td><td>1/1/00</td><td>broad</td><td>yes</td><td></td></tr>
<tr><td>Oregon</td><td>1/1/08</td><td>limited</td><td>yes</td><td>yes</td></tr>
<tr><td>Rhode Island</td><td>1/1/01</td><td>broad</td><td>no</td><td></td></tr>
<tr><td>Texas</td><td>1/1/2002 - 1/1/2004</td><td>none</td><td>yes</td><td>yes</td></tr>
<tr><td>Vermont</td><td>10/1/99</td><td>none</td><td>yes</td><td></td></tr>
<tr><td>Washington</td><td>1/1/02</td><td>none</td><td>yes</td><td>yes</td></tr>
<tr><td>West Virginia</td><td>8/2/05</td><td>expansive</td><td>yes</td><td>yes</td></tr>
<tr><td>Wisconsin</td><td>1/1/10</td><td>none</td><td>yes</td><td>yes</td></tr>
</table>

Table 3: NSFG analysis covariate groups

NSFG Basic Covariates	
Age Number of pregnancies Marital status (categories) Race/ethnicity (categories) Highest level of education (categories) Income bins (categories) Any abortion provider in the county (92/00/05) (Yes = 1) County family planning providers/10000 women age 15-44 (94/00/06)	Condition index = 19.73
NSFG Additional Covariates	
Metro status (categories) County avg. abortions/1000 women age 15-44 (92/00/05) Religion (categories) Number of children in household Employment status (categories) Number of marriages Has ever cohabitated (Yes = 1) Number of unwanted pregnancies Number of 'too soon' pregnancies Number of abortions Number of miscarriages Number of additional expected children	Condition index ≈ 50
State-level covariates	
Welfare: TANF waiver (Yes = 1) Welfare: Family cap (Yes = 1) Medicaid abortion funding restriction (Yes = 1) Parental consent/notification for abortion for minor (Yes = 1) Mandatory wait period for abortion (Yes = 1) Medicaid family planning waiver, income-based (Yes = 1) Medicaid family planning waiver, income-based, excludes teens (Yes = 1) Medicaid family-planning waiver, duration-based (Yes = 1)	Condition index = 16.88

Table 4: Descriptive statistics for NSFG analysis sample, by mandate status

	States without mandates		States with mandates	
N (unweighted)	13657		5592	
Age	30.61	(0.14)	30.58	(0.20)
Cty Fam Plan Providers per 10,000	1.11	(0.04)	1.29	(0.07)
Pregnancies	1.63	(0.03)	1.53	(0.04)
Unwanted Pregnancies	0.17	(0.01)	0.2	(0.01)
Too Soon' Pregnancies	0.45	(0.01)	0.42	(0.02)
Abortions	0.19	(0.01)	0.2	(0.01)
Miscarriages	0.26	(0.01)	0.24	(0.01)
Exp Addl Pregnancies	1.03	(0.03)	1.05	(0.03)
Marital status				
Married	53.60%	(0.76%)	50.30%	(1.15%)
Cohabitating	6.06%	(0.31%)	7.03%	(0.50%)
Widowed	0.31%	(0.06%)	0.33%	(0.12%)
Divorced	5.67%	(0.28%)	5.08%	(0.41%)
Separated	1.93%	(0.13%)	1.81%	(0.24%)
Never married	32.40%	(0.73%)	35.50%	(1.01%)
Race/ethnicity				
Hispanic	7.35%	(0.37%)	14.60%	(1.02%)
Non-hispanic white	77.40%	(0.62%)	66.70%	(1.50%)
Non-hispanic black	10.90%	(0.49%)	11.00%	(0.87%)
Non-hispanic other	4.37%	(0.34%)	7.75%	(0.63%)
Income				
Under $10,000	4.12%	(0.32%)	3.88%	(0.37%)
$10,000 to $19,999	9.40%	(0.37%)	8.36%	(0.49%)
$20,000 to $24,999	5.78%	(0.26%)	4.64%	(0.43%)
$25,000 to $29,999	6.83%	(0.24%)	5.70%	(0.38%)
$30,000 to $39,999	15.00%	(0.41%)	13.40%	(0.67%)
$40,000 to $49,999	13.20%	(0.44%)	8.99%	(0.45%)
$50,000 to $59,999	11.90%	(0.36%)	11.20%	(0.59%)
$60,000 or more	33.80%	(0.72%)	43.80%	(1.18%)

Standard errors in parentheses. Means are given for continuous variables and percentages for categorical variables. All means, proportions and standard errors are calculated using NSFG sample weights, with the exception of total number of observations.

Table 5: Rates of Contraceptive Method Use by State Mandate Status

	Prescription BC use	Non-prescription or no use	Short-term hormonal	Sterilization	LARC use	IUD	Implant	Injectable
No mandate	0.405	0.163	0.208	0.157	0.0153	0.0119	0.00342	0.0174
	(0.00562)	(0.00390)	(0.00517)	(0.00488)	(0.00164)	(0.00151)	(0.000570)	(0.00144)
Mandate	0.412	0.159	0.231	0.138	0.0243	0.0218	0.00248	0.0170
	(0.0103)	(0.00663)	(0.00829)	(0.00766)	(0.00261)	(0.00231)	(0.000786)	(0.00227)

Standard errors in parentheses. N = 30,769. Means and standard errors are calculated using NSFG survey weights.

Figure 3: Contraception use pre- and post-mandate

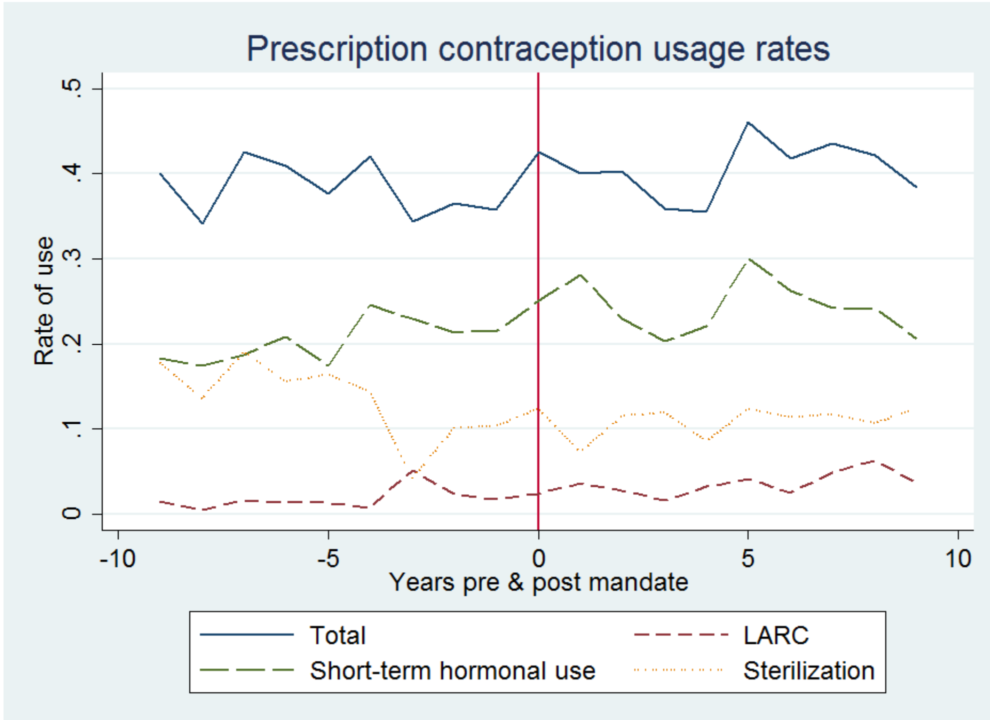


Figure 4: Contraceptive use by year of mandate implementation

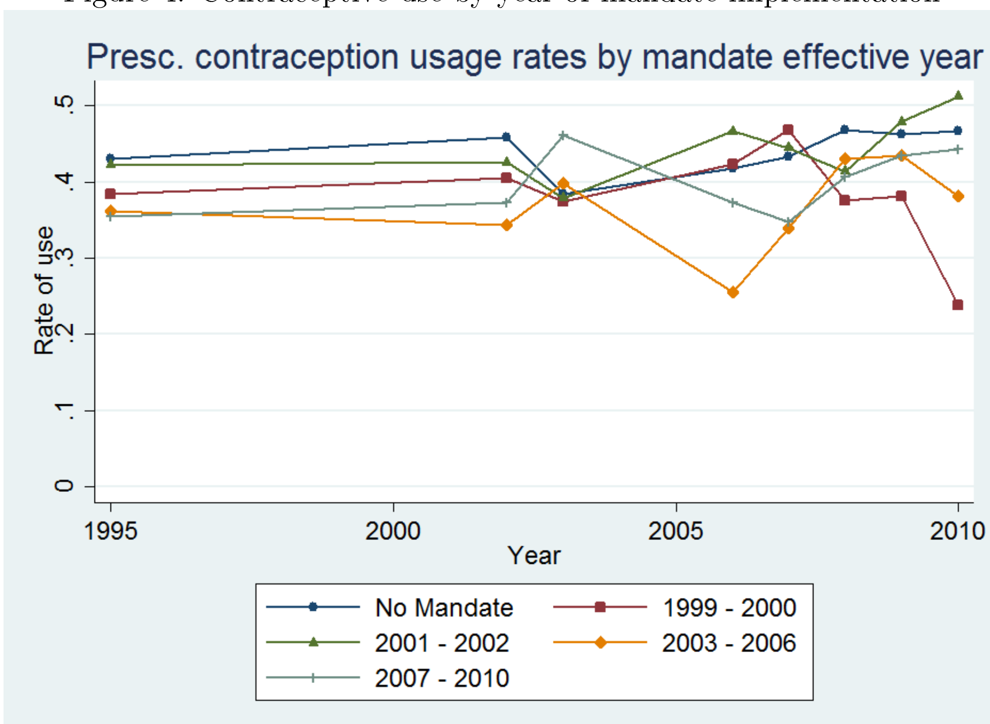


Table 6: LPM: Effect of Coverage Mandate on Any Prescription BC Use

	(1)	(2)	(3)	(4)
	Presc. BC use	Presc. BC use	Presc. BC use	Presc. BC use
Contraceptive Coverage Mandate	0.0213	0.0218	0.0217	0.0244
	(0.0165)	(0.0163)	(0.0159)	(0.0197)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
NSFG Basic Covariates	No	Yes	Yes	Yes
NSFG Addl Covariates	No	No	Yes	Yes
State-level covariates	No	No	No	Yes
Observations	30769	30769	30724	30724
R-squared	0.014	0.062	0.091	0.091

Standard errors in parentheses. All regressions are linear probability models weighted with NSFG survey weights. See Appendix for lists of covariates in each category. * $p<0.05$ ** $p<0.01$ *** $p<0.001$

Table 7: LPM: Effect of Coverage Mandate on Any LARC Use

	(1)	(2)	(3)	(4)
	Long-term BC use	Long-term BC use	Long-term BC use	Long-term BC use
Contraceptive Coverage Mandate	-0.00411	-0.00351	-0.00419	-0.00432
	(0.00382)	(0.00380)	(0.00385)	(0.00447)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
NSFG Basic Covariates	No	Yes	Yes	Yes
NSFG Addl Covariates	No	No	Yes	Yes
State-level covariates	No	No	No	Yes
Observations	30769	30769	30724	30724
R-squared	0.016	0.025	0.033	0.034

Standard errors in parentheses. All regressions are linear probability models weighted with NSFG survey weights. See Appendix for lists of covariates in each category. * $p<0.05$ ** $p<0.01$ *** $p<0.001$

Table 8: LPM: Effect of Coverage Mandate on Any Prescription BC Use by Mandate Refusal Provision

	(1)	(2)	(3)	(4)
	Prescription BC use	Prescription BC use	Prescription BC use	Prescription BC use
Mandate no refusal	0.0426	0.0474	0.0425	0.0538
	(0.0359)	(0.0344)	(0.0326)	(0.0336)
Mandate limited refusal	0.0212	0.0314	0.0297	-0.00404
	(0.0221)	(0.0216)	(0.0215)	(0.0343)
Mandate broad refusal	0.0207	0.0161	0.0190	0.0540
	(0.0252)	(0.0246)	(0.0245)	(0.0301)
Mandate expansive refusal	-0.0136	-0.0305	-0.0231	-0.0328
	(0.0270)	(0.0267)	(0.0267)	(0.0330)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
NSFG Basic Covariates	No	Yes	Yes	Yes
NSFG Addl Covariates	No	No	Yes	Yes
State-level covariates	No	No	No	Yes
Observations	30769	30769	30724	30724
R-squared	0.014	0.062	0.091	0.092

Reference category is states with no mandate. Standard errors in parentheses. All regressions are linear probability models weighted with NSFG survey weights. See Appendix for lists of covariates in each category. * $p<0.05$ ** $p<0.01$ *** $p<0.001$

Table 9: LPM: Effect of Coverage Mandate on Any Prescription BC Use by Mandate Outpatient Coverage Provision

	(1) Prescription BC use	(2) Prescription BC use	(3) Prescription BC use	(4) Prescription BC use
Mandate no outpatient covg	0.0187	0.0260	0.0252	0.00597
	(0.0200)	(0.0198)	(0.0194)	(0.0247)
Mandate outpatient covg	0.0232	0.0190	0.0194	0.0357
	(0.0214)	(0.0208)	(0.0204)	(0.0237)
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
NSFG Basic Covariates	No	Yes	Yes	Yes
NSFG Addl Covariates	No	No	Yes	Yes
State-level covariates	No	No	No	Yes
Observations	30769	30769	30724	30724
R-squared	0.014	0.062	0.091	0.092

Reference category is states with no mandate. Standard errors in parentheses. All regressions are linear probability models weighted with NSFG survey weights. See Appendix for lists of covariates in each category. * p<0.05 ** p<0.01 *** p<0.001

CHAPTER 3 : The impact of state-level contraception coverage mandates in administrative claims data

3.1. Introduction

In this chapter, I analyze the impact of state-level contraceptive coverage mandates using a large administrative claims dataset, the Clinformatics™ Data Mart from OptumInsight. While I find no evidence that these mandates impacted utilization, I do find some evidence that the mandates had an effect on coverage of contraceptives. For all analyses in this chapter I examine the same state-level mandates that I study in Chapter 2. Please refer to Section 2.2 for a detailed description of the mandates and the larger context of insurance coverage of contraceptives during this period.

I perform two analyses: an analysis of utilization of contraceptives and an analysis of changes in the number of methods claimed within an employer group.

3.2. Data: Claims database from OptumInsight

The OptumInsight (OI) database contains longitudinal medical and prescription claims information from May 2000 to June 2013. Although this dataset contains little personal information about individuals, it also has information not available from typical survey data: an employer group identifier, information on whether that employer fully insures or self-insures, the exact type of birth control method delivered and the out-of-pocket cost of the method to the patient. Using this data, I can quantify both the impact of these mandates on contraception utilization and OOP costs.

In the absence of a contraceptive claim, it is impossible for me to know whether a woman without a contraceptive claim was truly not using birth control or whether she was paying out-of-pocket or getting free or reduced cost birth control from another provider. However, I can still quantify shifts into prescription contraceptive use covered by a woman's primary health plan. As with my survey analysis above, I can also study shifts from one method of birth control to another. I will use a difference-in-difference specification to estimate the impact of a state level mandate on the probability of a woman claiming a covered contraceptive. An advantage of the OI dataset over the NSFG survey is that OI allows me to exclude women in plans offered by self-insured employers who are not subject to state mandates.

The OI claims files are very large; one quarter of medical claims typically has 20 to 25 million observations. I therefore use a 5% random sample of all individuals and limit my analytic dataset to all women between the ages of 13 to 45 who are enrolled in private health insurance in plans where the employer does not self-insure. My utilization and OOP spending is calculated at the person-month level, as this is both the smallest unit of coverage seen in the data and a natural unit of time to consider contraceptive use.

I group prescription contraceptive methods into eight categories (Table 10), based on their delivery method and the location where they are provided to patients. For each woman in the data, I link her pharmacy and medical claims to identify whether she had a claim for a contraceptive within each category in each month. If she did have a claim for a prescription contraceptive, I calculate her monthly OOP expenses within each contraceptive category. I also calculate total monthly medical and pharmaceutical OOP spending for every woman.

Utilization and OOP spending are calculated using pharmacy claims for methods delivered in a pharmacy, such as oral contraceptives, the contraceptive patch and ring, and diaphragms and cervical caps. To identify contraceptive claims in the pharmaceutical claims data, I use a variable provided with the dataset that identifies prescriptions that fall into different contraceptive categories. Utilization and OOP spending are calculated from medical claims for methods provided in a physician office, including the IUD, the implant and the contraceptive injection. Table 11 lists the procedural and diagnostic codes that I use to identify the IUD, implant, contraceptive injection and sterilization in the medical claims data.

The number of mandates that I can examine is limited by the fact that some mandates were implemented prior to the start of the dataset. The fourth column of Table 2 lists the states in which I have data available in the periods both pre- and post-mandate. Of the 29 mandates that have been passed in the U.S., I have pre- and post-mandate data for 15 of them in this analysis.

3.2.1. Estimating contraceptive utilization

For each woman, I create a binary variable equal to one if she claims a certain method in a given month. I then estimate use of each method by combining information from the data with typical use patterns.

For oral contraceptives, the patch, and the ring, contraceptive claims and contraceptive use are likely to be very similar, as most women receive a month's supply at once. However, about a third of women in the data receive multiple months' supply at once. The pharmaceutical claims data contains a "days supply" variable that estimates the number of days covered by a dispensed medication. I created the following decision rule to estimate duration of use: I assign only a month's use if the estimated days supply was less than or equal to 31, two months of use if the days supply was between 31 and 61, and three months use if the supply was greater than 61.

Longer-term methods, such as IUDs, implants or the contraceptive injection, work for longer periods of time and this must be taken into account to estimate contraceptive use. For the injection, a single shot provides three months of contraceptive effect. Therefore, if a woman had a claim in month one, I assume she is using that method for that month as well as the subsequent two months. For IUDs, there are diagnostic and procedure codes that can be used to identify if and when the IUD was removed. If a woman had no removal claim during her period in the data, I estimate her use to last until she exits the data or five years are up, whichever comes first. If a woman has no IUD insertion in the data but does have an IUD removal in a certain month, I back-date her use of the IUD until the beginning of her enrollment period. There is no diagnostic or procedural code that allow me to distinguish between surveillance, removal or removal and re-insertion of an implant, so I therefore conservatively assume an average of 1.5 years of use based on prior literature following Implanon users (Funk et al., 2005). I also estimate use of female sterilization using medical claims files, and if I observe a claim for sterilization I assume that method to be "in use" for all subsequent periods an individual appears in the data.

Use of diaphragms, cervical caps, and emergency contraception are difficult to estimate, since these methods must be used immediately before or after intercourse. There is no way to estimate from the data which women may use these methods infrequently vs. reliably, even if they have filled a prescription for them. I therefore err on the side of underestimation and only assign women use of those methods for the month in which they filled their prescription.

I use the term "claim rate" to refer to the percentage of women who had a claim for a given method in a given month. I use the term "usage rate" to refer to the percentage of women I estimate to be actively using a certain method in a given month, based on the criteria I've outlined here.

3.2.2. Estimating OOP spending on contraceptives

For OOP spending calculated from pharmaceutical claims, I use the total OOP spending reported in the data for that prescription medication. I also calculate a total monthly pharmaceutical OOP value for every month a woman appears in the data. When I calculate mean monthly OOP costs for methods delivered via the pharmacy, I simply average the OOP costs reported for that method each month. These values are an average monthly cost per-prescription, not per-month at the individual level, because women often fill prescriptions for several months at one time.

For the medical claims, there are often multiple claims for a single visit, and the data contains an "encounter" variable that contains all claims relating to a single encounter or office visit. I estimate OOP spending on contraceptives by aggregating all patient cost-sharing for the encounter where the method or device itself was delivered. This is because there are often OOP costs associated with the procedure that will be coded separately from the procedure or device itself, such as painkillers, pregnancy tests, etc., and I wish to include all associated OOP costs. However, this means that it's possible that if a woman had several procedures during her visit, this method may capture OOP expenditures for non-related procedures, such as childbirth or abdominal surgery. In particular, I'm worried about sterilization, IUD or implant insertions that occur immediately post-partum and may therefore be rolled into the costs of childbirth. For sterilization in particular, one of the procedural codes for sterilization specifically indicates that it was an "add-on" procedure performed during a C-section. I therefore do not include OOP costs for C-section-related sterilizations in my OOP cost analyses.

I do not include cost-sharing for physician appointments to discuss contraceptive management or obtain prescriptions for contraceptive medications or devices that are then obtained at a pharmacy. I also do not include cost-sharing for IUD or implant removal, because many women get one IUD removed and another reinserted at the same appointment, making it difficult to disentangle these costs. All costs are presented in inflation-adjusted 2010 dollars.

3.2.3. Group-level identifiers

The OptumInsight data lack both a plan-level identifier and information on when an individual switches or renews their plan, so I cannot examine plan-level contraceptive utilization or estimate when someone's particular plan added contraceptive coverage or became subject to a mandate. However, the data does contain a employer group-level identifier, and I use it to the best of my ability. This group identifier is a coarse employer identifier. In most cases, one group ID represents one employer, however, when several small employers have collectively contracted with the insurer, they will all have the same group ID. Similarly, a very large employer could potentially have several group IDs contained within it if different parts of the company contract with the insurer separately.

It's also unclear how to identify people who have individual market coverage in the data. About 10% of the employer group IDs in the data are associated with only one individual, but it is unclear whether these are truly all individuals who purchased their plans on the individual market. When I discussed this variable with the data provider, they informed me that their data (unavailable to me) indicate that only about 3% of group IDs in the data are truly individual plans; the remaining 7% were coded as small group plans, but still assigned to single individuals. It's not clear to me why this is the case, and so in general I view the group-level identifier as an imperfect employer identifier.

3.2.4. Issues with data quality in the Optum data

My initial time series of contraceptive utilization showed very strange trends in the first year of the data, from the second quarter of 2000 to the second quarter of 2001. Overall contraceptive use during this time period appeared to drop rapidly, and then increase rapidly, and then level off by the third quarter of 2001, after which it stayed relatively stable. I was concerned this variation might be due to issues with the quality of the data rather than true changes in utilization. I calculated the rates of use of several other common prescription drugs during this same time period for my sample, and saw the same trends in these prescriptions. I then went back and calculated the rate of any prescriptions of any type among this population over this time period, and found that overall rates of any claims over time also displayed the same strange pattern. Figure 5 shows the rates of contraceptive claims compared with rates of any pharmaceutical claims in the data. It's clear from this figure that this early drop in rates of claims is not specific to contraceptives but instead reflects overall prescription claim rates during these quarters in the dataset.

I therefore conclude that there are data quality issues with the prescription claims for the first four quarters of the data. I have brought this to the attention of the data provider but have not received an explanation for why the patterns of prescription claims in those quarters are so radically different than in subsequent years. Because of this, I drop the first year of data and conduct all of my analyses beginning in the third quarter of 2001. It's unfortunate that this issue arose, because this early period was also the most common period in which mandates were being implemented. Dropping these quarters of the data meant that I lost the pre-mandate periods for several mandates enacted in 2001.

3.3. Analysis of contraceptive utilization

Using a 5% sample of individuals, I analyze the data for commercially-insured women ages 13 to 45 who have one or more months of insurance coverage between 2001m7 and 2011m12, and who are not in plans offered by employers who self-insure. Some women are enrolled in multiple plans at one time. I keep the majority of these women in the dataset, but I do drop small numbers of women who are enrolled simultaneously in multiple plans that vary by state, fully-insured vs. self-insured status, year of birth, consumer-driven health plan status, or private vs. public insurance.

One of the mandates, in Texas, was removed after two years. For all analyses, I drop observations from Texas after the mandate was removed.

In principle, this dataset also allows for person-level fixed effects. However, implementing an individual-level fixed effects model is computationally infeasible without taking a very small sample of the data and potentially sacrificing generalizibility of the results. I therefore do not use person-level fixed effects for the following models. However, in future work I hope to explore the possibility of implementing a first-differenced model at the individual level.

3.3.1. "Stacked" difference-in-difference analysis

Due to the long time period over which data is available, I create a "stacked" dataset with of cohorts of mandate implementation. This specification is very similar to a standard difference-in-difference specification when the treatment begins at different times for different units, but it has a more explicit comparison group limited to a set window of time around each mandate (Gormley and Matsa, 2011; Gormley, 2015). Each mandate gets its own cohort, unless multiple mandates went into effect on the same day, in which case I group those mandates together. For each cohort, the month of mandate implementation becomes the "zero" month, and I limit the data to four years pre- and post-mandate. The states where the mandates were implemented at the same time are the "treatment" group for each cohort, and if another state implemented a mandate during this window of time in a non-"zero" month, I drop the post-mandate observations for that state. The "control" group for each cohort is therefore all the observations within states whose mandate status did not change during this window, and all of the pre-mandate observations for states who implemented mandates in a non-"zero" month during this time window. These cohorts are then "stacked" to create a final analytic dataset.

This analysis has the advantage of examining a more standardized window of time around each mandate. It also allows much more direct visualization of the data, with a clear "treatment" and "control" group for each cohort whose trends can be examined pre- and post-mandate implementation. One of the consequences of structuring the data in this fashion is that individual observations are duplicated multiple times throughout the dataset, however, this does not bias the results as long as the standard errors are clustered at the level of the treatment (in this case, the cohort-state level).

My model is specified as follows:

$$Y_{cims} = f(\gamma_{cm} + \theta_{cs} + \beta[1 = \text{Mandate}]_{cms} + \mathbf{X_{ims}})$$

Here, i indexes individuals and s indexes states, m indexes months, and c indexes mandate cohort. All specifications therefore include time-cohort and state-cohort fixed effects. For some regressions, I also add in a state-cohort-specific linear trend, $t \times \theta_{cs}$. The coefficient of interest is β, which is the marginal impact of a mandate in the treatment states relative to the control states. Individual covariates included in $\mathbf{X_{its}}$ include age, race, and the median education level of that individual's census tract. Of these, only age is time-varying; the other two are only available at one point in time in the data.

My key dependent variable is the probability of contraceptive use by method and among different age groups. I examine both extensive and intensive outcomes, that is, both the probability of any use and the probability of use of a certain type of method conditional on use of any prescription contraceptive. For intensive outcomes, I group methods to be either short-term (the pill, patch, ring, injection or diaphragm/cervical cap), long-term (the IUD and implant) or permanent (sterilization).

As a sensitivity check, I also estimate a dynamic version of the above specification, replacing the single coefficient of interest with a coefficient for treatment states interacted with a set of pre and post-mandate dummy variables. The year of mandate implementation (year zero) is excluded as the comparison group.

$$P(Y_{ims} = 1) = f(\gamma_{cm} + \theta_{cs} + \textstyle\sum_{t=-4}^{-1} \beta_t \times \lambda_t \times [1 = \text{Treatment}]_{cs}$$
$$+ \textstyle\sum_{t=1}^{3} \beta_t \times \lambda_t \times [1 = \text{Treatment}]_{cs} + \mathbf{X_{ims}})$$

This flexible specification has two advantages. First, I can check for pre-trends in states with and without mandates as a identification test. Secondly, I can test whether there are any longer-term impacts of the mandates that occur in the years after implementation. As with the baseline model above, I test this model on both the extensive (overall rate of use) and intensive (types of methods chosen) margins. For the intensive margin of use, I divide the methods into three categories: sterilization, LARC methods (the IUD and implant) and short-term methods (everything else) and run them on the subset of women actively using contraception in the dataset.

3.3.2. Results from utilization analysis

Figure 6 shows the overall rates of claims and estimated rate of use for any prescription contraceptives in the data over time. Figure 7 shows use over time, grouped by the approximate year of mandate implementation. The mandates are grouped into five categories: 1999 to 2001, 2002 to 2003, 2004 to 2006, 2007 to 2011, and no mandate. There are no immediately obvious differences in utilization between states that implemented mandates earlier vs. later.

Because different mandates went into effect for different states in different years, I found that the simplest way to visualize the data was to disaggregate my stacked dataset and create a figure for utilization in each mandate cohort. That way, for each time period I can examine the rate of use in the treatment states compared with the control states. There are eleven mandate cohorts, and their usage rates over time are shown in Figure 8. Examining these figures, there seem to be very little change in the treatment states relative to control states following mandate implementation. There are a few interesting outliers–in particular, there is a large drop in contraceptive use in Michigan following the mandate implementation.

Table 12 shows the results of the regression analysis for the outcome Prob(Any Use) for all women and for subgroups of women by age. There is no statistically significant change in overall rates of contraceptive utilization following mandate implementation. Table 13 shows the same model specification for the probability of using a particular method conditional on using any prescription contraceptives, and similarly finds no statistically significant effect of the mandates on intensive utilization.

For my dynamic difference-in-difference models of utilization, I present the results as figures with coefficient estimates and 95% confidence intervals plotted. For all of these models, the year of mandate implementation is the comparison group for the coefficient estimates. Figure 9 presents the results of the dynamic model for the extensive margin of use. None of the coefficients are statistically significantly different from zero. Figures 10, 11, and 12 present the results for conditional use of short-term methods, LARC methods, and sterilization respectively. Similar to the extensive margin, almost none of the coefficients are statistically different from zero. There are some suggestive potential trends. For instance, the use of short-term methods appears to increase in years two and three following the mandate. In contrast, there is potentially a negative pre-trend in the use of LARC methods in states with mandates relative to those without. These findings are potentially worth further investigation, but it is difficult to draw definite conclusions from them given their lack of statistical significance.

Overall, my results in this analysis suggest that there were no significant changes in overall rates of contraceptive use or the distribution of methods chosen by contraceptive users.

3.3.3. Robustness checks

I perform several sensitivity analyses and robustness checks for these results:

- Add additional time-varying state-level covariates: I have a dataset of time-varying state-level covariates that may be correlated with contraceptive use at the state level that I use in my analysis of the NSFG survey data in Chapter 2. It does not include 2011, the final year of the OI dataset. I have run my baseline analyses excluding 2011 and adding in these state-level covariates, and results were qualitatively unchanged, except that in some model specifications the coefficient of interest became negative and significant, in the opposite direction than would be expected from the policy change.

- Examine extensive and intensive use by age group: Younger women, particularly teens, tend to use contraceptives at lower rates and be lower income than older women. I examined the intensive and extensive margins of use by age group, but found no differential effects of the mandate when comparing across ages.

- Discrete time survival analysis: Because women may most actively choose their birth control when they first enroll in a new plan, I conducted a discrete time survival analysis, with the outcome being the probability of first use of a contraceptive following entry into the dataset. I experimented with models on both the extensive and intensive margins of use, and found no significant effects on utilization in any of the models.

- Analysis of OOP costs: I conducted a difference-in-difference analysis with OOP costs of different methods as the dependent variable. In general results were non-significant and sensitive to model specification.

3.4. Analysis of changes in the within-group number of methods claimed

The group-level identifier is coarser than a plan-level identifier, and it can contain multiple small employers and/or multiple plans. Because of this, coverage of different contraceptive methods could vary within-group. However, if even one plan adds coverage of a new method in response to a mandate, there could conceivably still be an increase in the number of methods claimed within that group. I therefore estimate the number of methods claimed within groups in the 12 months preceding and following a mandate to see if I can detect a differential change in groups in treatment states relative to control states.

Examining within-group patterns required drawing a different sample of the Optum data. My initial sample was a 5% sample of individuals, which meant that many individuals were drawn in the sample without the other people in their group. I therefore drew a 5% sample of groups and used it to create a new sampled dataset.

3.4.1. Estimating contraceptive utilization

Contraceptive use and OOP price were estimated in the same manner as described above, with one exception. Drawing a simple sample of group IDs meant that women who switched groups but remained in the dataset would disappear. This potentially posed a problem in estimating contraceptive use, since I use the longitudinal nature of the data to estimate use within a woman over time, and I didn't want to lose information unnecessarily. For instance, if a woman received a sterilization procedure when she was in one group and then switched to another group, keeping one group and not the other would introduce measurement error in the data. To address this issue, I kept all periods in which a woman was in the dataset if she appeared at all in a sampled group. I then used all available periods to estimate utilization. However, when I perform group-level analyses, I drop the observations for which a woman did not appear in a sampled group.

3.4.2. Estimating group-level patterns of coverage

My primary outcome is the number of methods claimed in a group in a given month. As with my other analyses above, I conduct a stacked difference-in-difference analysis, creating mandate "cohorts" that are then stacked into one dataset. However, many of the groups contain very small numbers of women, and given that even the most commonly-used methods are often not claimed at the monthly level, I concluded that the yearly level was a more appropriate unit of analysis for trying to impute group-level coverage of contraceptive methods.

For each mandate cohort, I keep only observations in the twelve months immediately preceding and immediately following the implementation of a mandate. I drop groups that self-insure, and I also drop groups that partially self-insure, that is, some of their plans were self-insured and some were not. Many groups also cross state boundaries; because this analysis is conducted at the group level rather than the state level, this was only an issue if the group overlapped both a treatment and a control state in a given cohort. Because these are all groups that do not self-insure, I assumed that if a group crossed state boundaries, they would have to be compliant with the state-level insurance coverage mandates in a treatment state. I therefore treat groups that cross between treatment and control states as essentially two separate groups, one treatment and one control, and analyze them separately in the data.

For each group, I then count the number of contraceptive methods for which a claim appears in both the 12 months preceding and the 12 months following a mandate implementation. I also examine whether a claim for a certain type of method appears in the group's claims in the pre-period, post-period, or both.

3.4.3. First-difference analysis

Ideally, I would perform a difference-in-difference analysis using group-level fixed effects, using a model specification as follows:

$$Y_{gtc} = \beta[1 = \text{Mandate}]_{gtc} + \lambda[1 = \text{Post}]_{tc} + \theta_{gc} + \epsilon_{gtc}$$

Here, g indexes group, c indexes cohort, and t indexes time, which in this analysis has been collapsed to two periods, the 12 months pre and post-mandate. Y_{gt} is the number of methods claimed by women within that group in each time period.

However, there are too many groups in my data to include group-level fixed effects in the model. Instead I perform a first difference analysis, subtracting the values of each regressor in the first period from their values in the second period. Only groups with claims in both periods remain in the data, and the cohort-time and cohort-group fixed effects drop out, leaving the model as follows:

$$(Y_{gc,t=1} - Y_{gc,t=0}) = \beta[(1 = \text{Mandate})_{t=1} - (1 = \text{Mandate})_{t=0}]_{gc} + (\epsilon_{gc,t=1} - \epsilon_{tc,t=0})$$

The outcome is now the difference in number of claimed methods within group between the pre- and post-periods, and $(1 = \text{Mandate})_{t=1} - (1 = \text{Mandate})_{t=0}$ only takes a value equal to one for groups in treatment states. Standard errors are robust to heteroskedasticity. Because I have reason to believe that the variance in the number of methods claimed per group increases as group size decreases, in some models I weight by the square root of the number of women in the group.

I also estimate the above model for each type of method. In that case, the dependent variable, $Y_{gc,t=1} - Y_{gc,t=0}$, can take a value of 0 if there was no claim for a certain method in either period, 1 if there was a claim in the post-period but not the pre-period, and -1 if there was a claim in the pre-period but not the post-period.

3.4.4. Regression results

On average, the non-self-insured groups in the dataset are very small. About 50% have less than 5 women, another 25% have 6 to 10 women, another 15% have 10 to 25, and the remaining 10% have 25 or more women. Given these small numbers of women per group, it's unsurprising that the median number of methods claimed in a year is one. Figure 13 shows the mean number of methods claimed, by month, for groups in treatment states vs. control states, broken out by mandate cohort. The time series are rather mixed; some seem to suggest that the mean number of methods claimed per group rose in treatment groups in comparison to control groups following mandate implementation, while others seem to suggest that it fell.

However, the figures present pooled averages over time, while my regression analysis considers only the within-group change in number of methods claimed per month. Table 14 shows the results of my regression analysis. I perform the analysis three ways, first using unweighted OLS with a simple treatment dummy, then OLS weighted by group size, and then unweighted OLS adding an interaction with the number of women in the group. In Column 1, we see that the coefficient of interest is statistically significant but has a very small magnitude of an additional 0.04 methods per group. In Column 2, I present the same model, weighted by the square root of the number of women in the group. This coefficient is statistically significant and has a large magnitude (0.313) relative to the mean number of methods claimed within a group over this period. When I leave the regression unweighted and instead add an interaction for the size of the group, it's clear that this effect is being driven almost completely by groups with more than 25 women. The coefficients on groups with zero to 5 women and 10 to 25 women are non-significant, and the coefficient on group with 6 to 10 women is statistically significant but small in magnitude (0.07). However, the coefficient for groups with more than 25 women is 0.42, which means that larger groups that did not self-insure saw an average increase of 0.42 methods claimed in the year following mandate implementation, compared with groups of the same size in control states. Because I can only know if a certain group's plans covered a given method if a women in that group claims that method, I don't interpret these results as indicating that the smallest groups did not see a change in insurance coverage while the largest groups did. My ability to detect an effect of a mandate increases with group size, and therefore the variance of the number of methods within a group increases as group size decreases. I therefore present the remaining analyses using OLS weighted by group size only.

Tables 15 and 16 present the within-group change in the probability of a given method being claimed in groups in treatment states vs. control states. Table 15 presents the results for the short-term methods in the data, and we see that almost the entire magnitude of the effect can be explained by increased rates of claims for the patch and the injection. In contrast, the rates of claims for the implant actually fall slightly, and the remaining methods are non-significant. These results suggest that the mandates did cause more methods to be covered by insurance plans, specifically the injection and the patch.

3.4.5. Robustness checks:

I perform several robustness checks for this analysis:

- Dropping cohorts: I re-ran these models while sequentially dropping one cohort at a time. The results were very similar in both magnitutes and statistical significance for all subsets, suggesting that one cohort in particular is not driving these results.

- Analysis of within-group rate of claims: Rather than using a differenced binary variable as my outcome, I used the within-group change in rate of claims by method as my outcome. Results for the patch were similar to those reported in Tables 15, but the results for the injection and the implant became statistically insignificant.

3.5. Discussion

I find no statistically significant effects of the mandates on contraceptive utilization, either at the extensive (overall rates of use) or intensive (the type of method chosen) margins. I conduct a fairly extensive analysis of utilization, using multiple model specifications and examining differences in usage by age group, finding almost uniformly non-significant results.

When I turn to testing whether the state mandates had any impact on insurance coverage of contraceptives, I find evidence that they did. There is an increase in the number of contraceptive methods claimed within a group following mandate implementation, specifically, in the rates of within-group claiming of the patch and the injection. But either the change in coverage was not frequent enough, or demand was inelastic enough, that overall rates of utilization and the distribution of methods chosen were unchanged.

An unanswered question from thse analyses is why I am finding increased rates of within-group claims for some methods for my group-level analysis, but no increased rate of use for those same methods in my individual-level utilization analyses. This could be explained by the fact that in general my estimates from my utilization analysis are imprecisely defined. In general, use of all non-OCP methods is infrequent in the data, and so it is correspondingly difficult for me to detect an effect on utilization of those methods. In contract, my group-level analyses are designed to be sensitive to any new additional method being claimed within a group, because the within-group sum is increased by one whenever a new method is claimed, regardless of the method.

In addition, in my group-level analysis I examine only changes in claims, not in use.

In Section 3.2.1 I describe how I used the patterns of claims by method to estimate use of different methods in my data. It's possible that this estimation strategy added additional measurement error to my utilization analysis, making it more difficult to detect any true effects. I could consider re-doing my utilization analysis as an analysis of rates of claims instead. Another possible explanation for my results is that for my group-level analysis, my window of time pre- and post-mandate is restricted to a year, while my window for the utilization analysis is four years.

3.5.1. Limitations

This analysis has some important limitations. The lack of plan-level information and a plan-level identifier limit my ability to impute plan-level coverage of contraceptives. In addition, the dataset has very limited individual-level covariates that I can use in my analysis.

My OOP cost estimates almost certainly underestimate the total cost of obtaining contraception for women in my sample. Imprecise diagnostic codes and variation in coding practices across providers makes it difficult to differentiate appointments that included discussions of contraceptive use from appointments made for the sole purpose of obtaining a new contraceptive prescription. I therefore exclude these from encounters from my estimates. Similarly, I do not currently include the cost of removals in my estimates for IUDs or implants.

Another limitation of this analysis is that my dataset, while very large, only comes from one insurer. My results therefore may not be generalizable to the nation as a whole. Given this insurer's large market share, however, I believe my results are still relevant because they reflect the experience of a large subset of the group health insurance market in the U.S.

3.5.2. Future directions

There are several additional explorations I hope to implement with this analysis. The first is to use women in plans offered by employers who self-insure as an additional control group in a triple-difference analysis. Secondly, I plan to investigate the feasibility of exploring individual-level analyses, perhaps in a first-difference framework as I did with my group-level analyses here.

Future research on these state mandates will need better data on how insurance coverage of prescription contraceptives actually changed in response to a mandate. An ideal dataset for studying these mandates would contain some plan-level information about the OOP cost of prescription contraceptives for all women, not just the women who actually purchased. In other words, it would contain counterfactual OOP prices faced by women who chose not to consume contraceptives. Although my results suggest that demand for contraceptives appears to be unresponsive to price, I could quantify a demand elasticity for contraceptives much more exactly with better data on the actual change in OOP cost faced by women.

3.6. Tables & Figures

Table 10: Contraceptive method categories in OI dataset

Category	Delivery method	Delivery location	Patient costs included in OOP total
Oral contraceptive	Oral	Pharmacy	Cost of medication
Emergency contraceptive	Oral	Pharmacy	Cost of medication
Patch	Cutaneous	Pharmacy	Cost of device(s)
Ring	Intravaginal	Pharmacy	Cost of device(s)
Diaphragm or cervical cap	Barrier	Pharmacy	Cost of device(s)
IUD	Intrauterine device	Physician office	Cost of device & insertion procedure
Implant	Subcutaneous device	Physician office	Cost of device & insertion procedure
Injection	Injection	Physician office	Cost of medication & injection procedure
Female sterilization	Surgical	Office or hospital	Cost of procedure

Table 11: Diagnosis & procedure codes used to identify contraception provided in a physician office in OI dataset

Type of Method	ICD-9 Diagnosis	ICD-9 Procedure	CPT-4	HCSC
IUD insertion	V25.11, V25.13	697	58300	J7300, J7302
IUD removal	V25.12	9771	58301	
Implant insertion	V25.5			J7307
Injection				J1050, J1051, J1055
Female steriliza-tion	V252		58565, 58600, 58605, 58611, 58615, 58670, 58671	A4264

Figure 5: Rates of claims and estimated use including data from 2000m5 to 2001m6

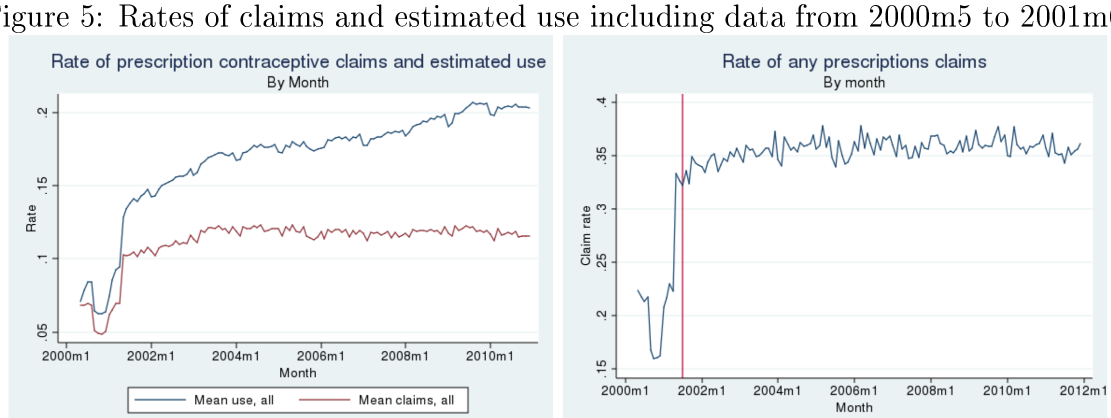


Figure 6: Extensive and intensive margins of contraceptive use, by month among fully-insured women

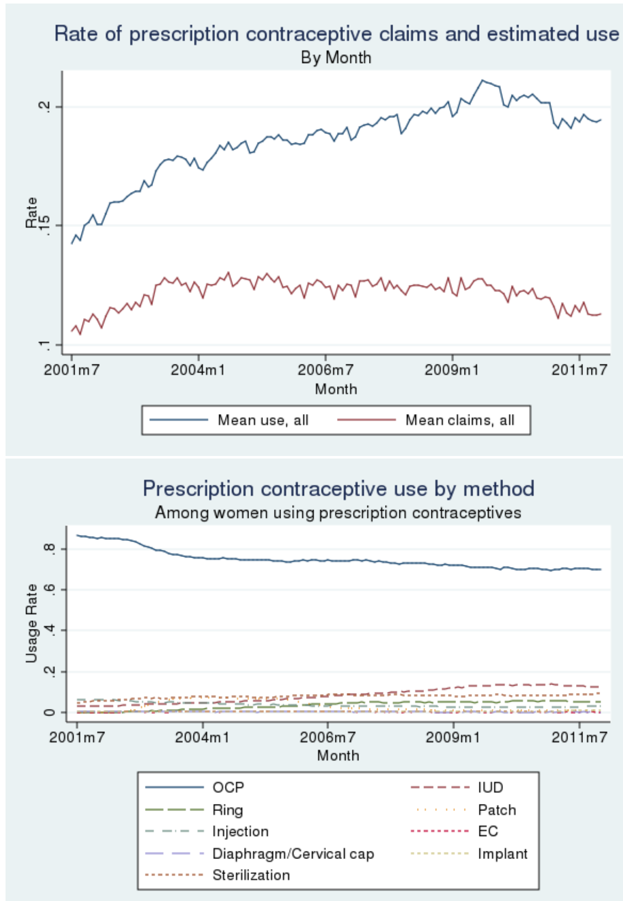


Figure 7: Mean use by over time by year of mandate implementation

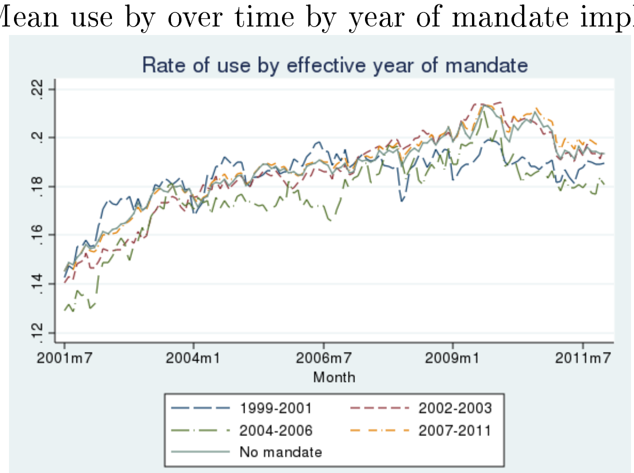


Figure 8: Rate of use of any method by months pre- and post-mandate, by individual mandate cohort

Table 12: Stacked Diff-in-diff: Effect of state mandate on any contraceptive use by women in fully-insured plans

	(1) All	(2) All	(3) 13-20	(4) 13-20	(5) 21-30	(6) 21-30	(7) 31-45	(8) 31-45
State Mandate	0.000790	-0.00407	-0.000725	0.000887	0.000468	-0.00263	-0.000794	-0.00544
	(0.00577)	(0.00338)	(0.00294)	(0.00322)	(0.00972)	(0.00738)	(0.00680)	(0.00483)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-State linear trends	No	Yes	No	Yes	No	Yes	No	Yes
Observations	46120230	46120230	9689951	9689951	12379106	12379106	24051173	24051173
R^2	0.011	0.011	0.081	0.081	0.022	0.023	0.026	0.027

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Outcome in all regressions is the probability of any prescription contraceptive use. Standard errors are cluster-robust at the cohort-state level.

Table 13: Stacked Diff-in-diff: Effect of state mandate on type of contraceptive methods chosen by users in fully-insured plans

	(1) Short-term use	(2) Short-term use	(3) LARC use	(4) LARC use	(5) Sterilization	(6) Sterilization
State Mandate	0.00606	0.000303	-0.0127	-0.00189	0.00437	-0.0000126
	(0.00714)	(0.00495)	(0.00665)	(0.00529)	(0.00338)	(0.00276)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Cohort-State linear trends	No	Yes	No	Yes	No	Yes
Observations	8649586	8649586	8649586	8649586	8649586	8649586
R^2	0.091	0.092	0.042	0.044	0.069	0.069

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Outcome in all regressions is the probability of type of method conditional on any prescription contraceptive use. Standard errors are cluster-robust at the cohort-state level.

Figure 9: Dynamic model: Any use

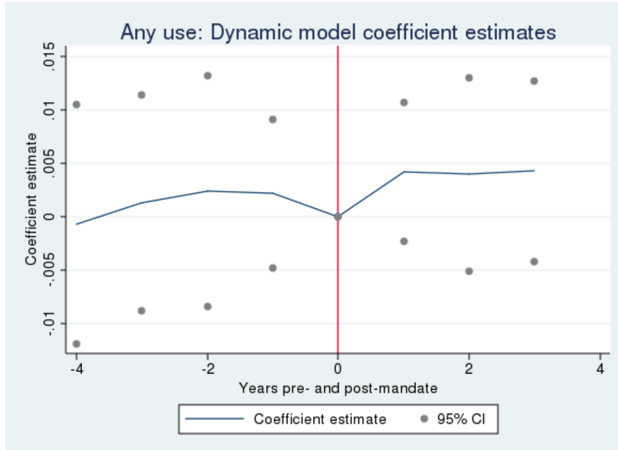


Figure 10: Dynamic model: Conditional use of short-term methods

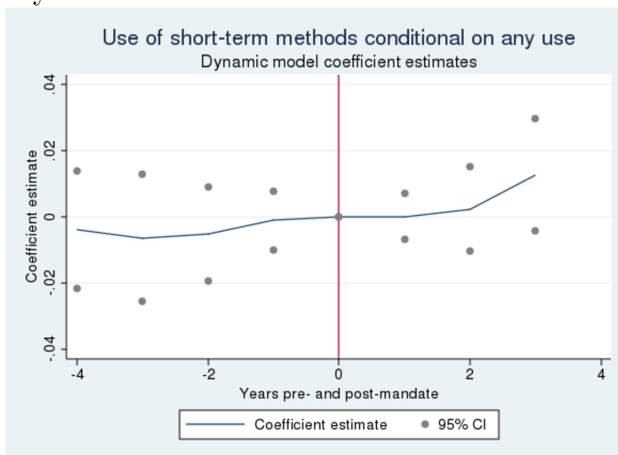


Figure 11: Dynamic model: Conditional use of LARC methods

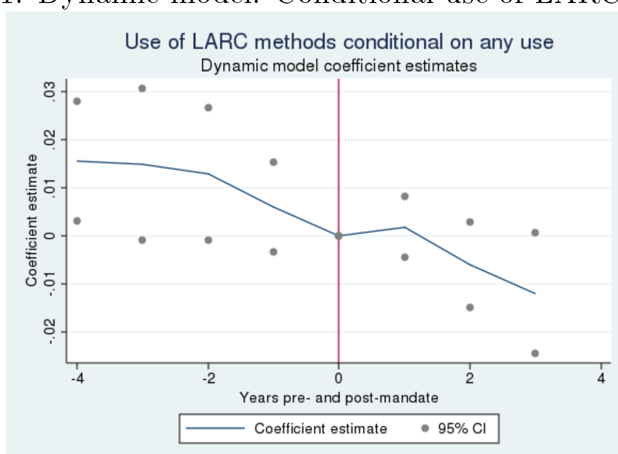


Figure 12: Dynamic model: Conditional use of sterilization

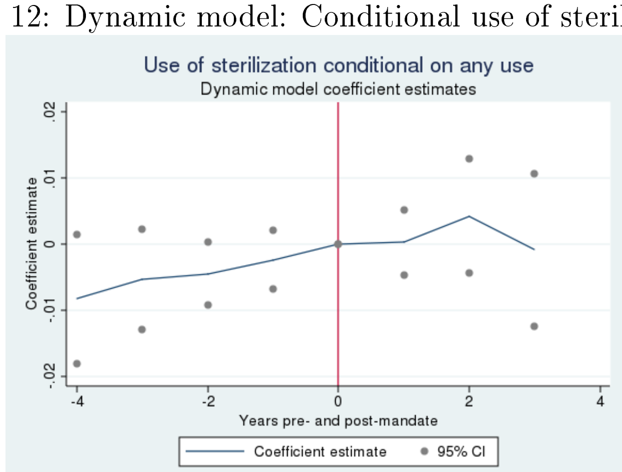


Figure 13: Mean number of methods claimed by group, by individual mandate cohort

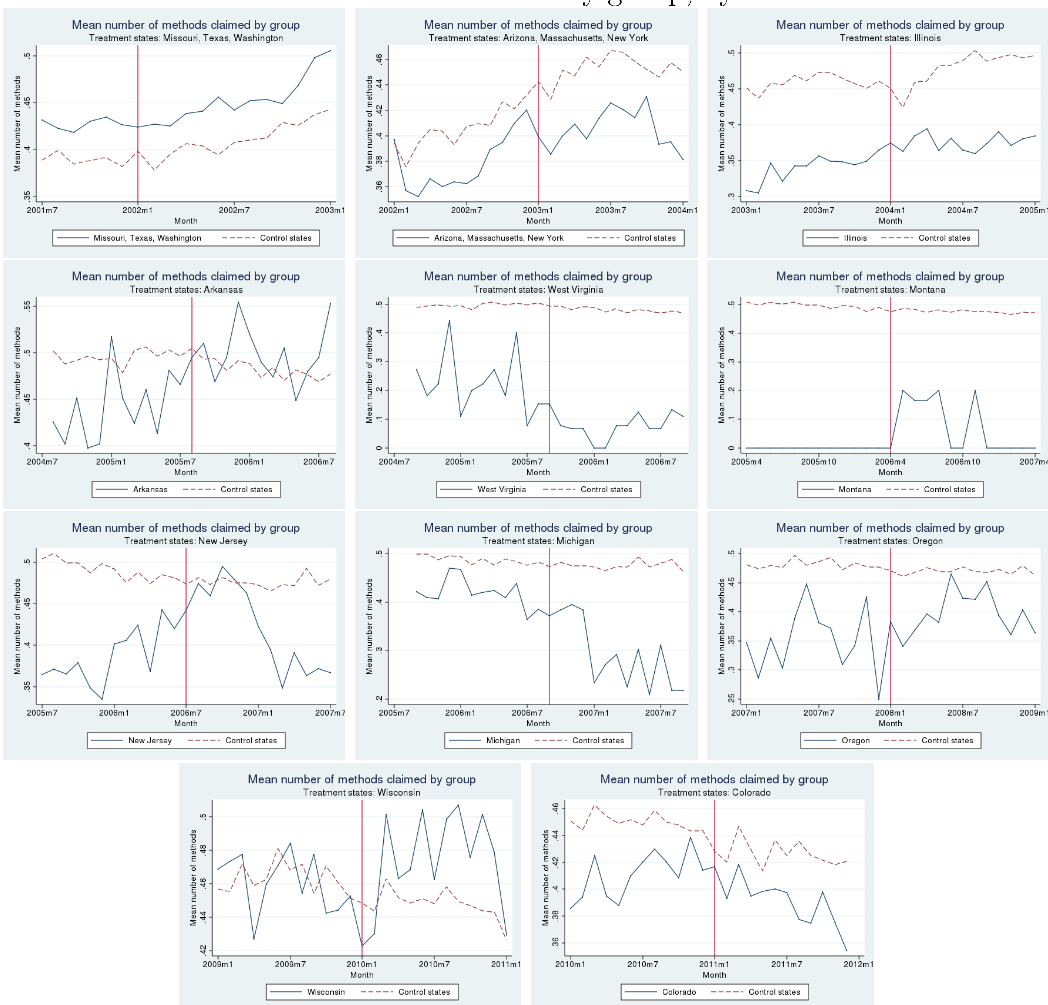


Table 14: Effect of state mandates on number of methods claimed by group

	(1) Unweighted OLS	(2) Weighted by group size	(3) Unweighted OLS
1(Mandate, post) - 1(Mandate, pre)	0.0400**	0.313**	-0.00590
	(0.0136)	(0.119)	(0.0124)
Mand x 6 to 10			0.0739*
			(0.0363)
Mand x 10 to 25			-0.0341
			(0.0445)
Mand x 25 or more			0.420***
			(0.0811)
Constant	0.0297***	0.188***	0.0297***
	(0.00321)	(0.0269)	(0.00321)
Observations	62248	62248	62248
R^2	0.000	0.002	0.001

Robust standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Outcome in all regressions is the within-group change in number of methods claimed from the 12 months pre-mandate to the 12 months post-mandate.

Table 15: Effect of state mandates on type of short-term method claimed within group

	(1)	(2)	(3)	(4)
	OCP	Ring	Patch	Injection
1(Mandate, post) - 1(Mandate, pre)	0.00684	-0.00560	0.203***	0.0427*
	(0.00595)	(0.0258)	(0.0407)	(0.0212)
Constant	0.00226*	0.0629***	0.00399	0.00219
	(0.00112)	(0.00624)	(0.00796)	(0.00522)
Observations	62248	62248	62248	62248
R^2	0.000	0.000	0.009	0.000

Standard errors in parentheses. All models weighted by group size. Standard errors are robust.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Outcome in all regressions is the change in type of method claimed from the 12 months pre-mandate to the 12 months post-mandate.

Table 16: Effect of state mandates on type of long-term methods and sterilization claimed within group

	(1) IUD	(2) Implant	(3) Sterilization
1(Mandate, post) - 1(Mandate, pre)	0.00454	-0.0104*	0.0500
	(0.0429)	(0.00479)	(0.0445)
Constant	0.0545***	0.00789*	0.00554
	(0.00855)	(0.00336)	(0.00672)
Observations	62248	62248	62248
R^2	0.000	0.000	0.000

Standard errors in parentheses. All models weighted by group size. Standard errors are robust.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Outcome in all regressions is the change in type of method claimed from the 12 months pre-mandate to the 12 months post-mandate.

CHAPTER 4 : The impact of the Affordable Care Act mandate

4.1. Introduction

Beginning in August 2012, the Affordable Care Act began requiring private health insurance plans to cover prescription contraceptives with no consumer cost-sharing. In this chapter, I use the OptumInsight data to examine the impact of the Affordable Care Act (ACA) mandate on the OOP price of contraceptives and post-mandate contraceptive utilization. I find strong evidence that the OOP price has fallen substantially following the implementation of the mandate, but only very small changes in extensive or intensive utilization in response to this decrease in OOP price.

4.1.1. The Affordable Care Act mandate

The ACA mandate goes further than the state-level mandates I've examined in prior chapters. It requires that "preventive services"–a category of services which includes both prescription contraceptives and their related services–be covered with zero consumer cost-sharing. This mandate went into effect on August 1st, 2012, and requires that insurance plans come into compliance at the beginning of the subsequent plan-year, which for many women was January 1st, 2013. This mandate includes all FDA-approved contraceptive methods, including female sterilization and prescription emergency contraception, but excludes abortifacients and over-the-counter emergency contraception (Kraemer, 2014).

This mandate applies nationally, with exceptions allowed for grandfathered plans and religious employers. Grandfathered plans are plans that have not substantially changed their cost-sharing requirements since March 2010. These plans are gradually being phased out of the employer-sponsored health insurance (ESHI) marketplace but still currently enroll a significant subset of employees. The 2013 Kaiser Employee Health Benefits Survey found that nationally 36% of covered workers were enrolled in a grandfathered health plan, down from 48% in 2012 (Claxton et al., 2013). Grandfathered plans in the individual marketplace could not enroll new members after March 2010, but grandfathered plans in the employer-based market can still enroll new employees as long as the firm has maintained consecutive enrollment in the plan since March 2010.

Religious employers–as defined by the Department of Health and Human Services (HHS)–are exempt from the ACA mandate. HHS has also issued a special accomodation allowing other non-profit religious organizations, such as hospitals or universities, to shift the responsibility of requiring contraceptive coverage to the insurer, rather than the employer (HHS, 2013). In June 2014 the Supreme Court ruled that "closely held" corporations whose shareholders consider themselves religious are also not subject to the mandate. Although a final decision has not yet been made, the current administration is expected to extend the same accomodation to these employers that it extended to non-profit religious organizations. However, the constitutionality of this accomodation has also been challenged and is being actively litigated (Annas et al., 2014).

The law requires that insurance companies cover "all FDA-approved" methods of contraception, but insurance companies are not required to cover every birth control brand in the market. Instead, insurance companies are allowed to employ "reasonable medical management" or RMM, to contain costs. Two recent reports have found that insurers are interpreting this RMM allowance in varied ways, and some are interpreting it so broadly that they are potentially in violation of the law (Sobel et al., 2015; Benyo et al., 2015). For example, some companies were not covering the patch or the ring, arguing that these methods were equivalent to OCPs because they use the same hormones. One company surveyed covered the hormonal IUD but not the copper IUD. In response to these reports, on May 11th, 2015, the Department of Health and Human Services issued a new Frequently Asked Questions memo concerning which methods are required to be covered by the law. These guidelines specify that insurers must cover at least one brand in each of the 18 FDA-approved contraceptive method categories with no cost-sharing. Within each category, insurers can use cost-sharing to direct consumers to lower-cost methods within the category. The guidelines also state specifically that insurers must exempt individuals from cost-sharing for methods which their physicians deem medically necessary, even if it is a method normally covered with cost-sharing (HHS, 2015).

4.2. Data: Claims database from OptumInsight

For this analysis, I use a 10% sample of the OptumInsight data from the first quarter of 2008 through the second quarter of 2013. OOP costs and utilization of contraceptives are estimated the same way they were for the state mandates analysis; see Section 3.2 for a detailed description of these methods.

Monthly OOP costs show significant seasonal variation, so for some figures I adjust for this variation by regressing pre-August 2012 OOP costs on a set of monthly dummies (January, February, etc.), and then plot the residual variation in OOP costs. All OOP costs are presented in inflation-adjusted 2013 dollars. I only do this for some descriptive figures; in all regression analyses I simply include a full set of month-year dummies (Jan-2008, Feb-2009, etc.) to adjust for seasonality.

As a descriptive exercise, I also estimate the share of total OOP costs spent on contraceptives, and then perform a back-of-the-envelope calculation to estimate the mean and median implied savings-per-woman from the ACA mandate. To estimate the share of OOP costs spent on prescription contraceptives, I focus on users of OCPs and new IUD insertions, the two most commonly used reversible prescription contraceptive methods in the U.S. To minimize selection bias, I limit this portion of my analysis to women who were continuously enrolled in insurance from January 2012 to June 2013, and then compare spending patterns among OCP users and women who received IUD insertions in the "pre-period" (January to June 2012) vs. the "post-period" (January to June 2013). I define OCP users as women who were continually enrolled between January 2012 and January 2013 and had at least one claim for an OCP in both the pre- and post-periods, and I include spending in both periods for OCP users. I define IUD users as women who had an IUD inserted in either the pre- or post-period, and I include spending for IUD users only in the period in which the woman received her IUD.

For each woman, I sum their OOP spending on either OCPs or their IUD insertion, and divide that value by their total OOP spending during that period. Using these percentages and the mean and median total OOP spending values for these users, I then estimate the mean and median implied savings on OCPs and IUD insertions per woman attributable to the ACA mandate. Implied savings are calculated by multiplying the mean (median) total spending by the mean (median) percentage of spending spent on that method for each period, then subtracting the 2013 estimate from the 2012 estimate. This calculation therefore takes into account the possibility that total average OOP spending might have changed during this time period. For OCP users, this value is then multiplied by two to estimate total yearly spending.

Very rarely, women are mistakenly coded for both an IUD and implant insertion in the same encounter, resulting in double-counting costs. I drop these values before averaging the shares. I also drop any negative OOP expenses reported in the data. All costs are presented in inflation-adjusted 2013 dollars.

4.3. Descriptive statistics and figures

My analytic dataset is a 10% sample of individuals from the OI database, limited to women in private health insurance between 2008m1 and 2013m3 between the ages of 13 and 45. The total sample size is 17,645,135 observations, with 790,894 individual women. The mean and median lengths of enrollment in the data are 22.3 months and 17 months, respectively.

Figures 14, 15, and 16 present descriptive time series of the claim and usage rates for all prescription birth control methods, short-term prescription birth control methods, and LARC methods. Each figure has a vertical red line to represent August 2012, when the mandate went into effect. There are no striking changes in utilization, either in short-term or long-term methods, in either the claim rates or the estimated usage rates. Note the sharp rises in estimated usage rates for total and short-term methods during the first three months of 2008; these are an artifact of the the way I've estimated usage, since many women on a three-month prescription schedule will not show up as using a method until they refill their prescriptions in month two or three of their cycle. There are also yearly patterns where utilization dips in the first month of each year. This is also an artifact of my method of estimating use, since the first month of each year is when the greatest number of women enter and exit the dataset. Women who exit are likely replaced by women who, having obtained a new health insurance plan, take a month or two to renew their prescription. This dip does not reflect a true drop in utilization since many women on short-term methods are likely to have a month or two of their method in reserve.

I also explore the intensive margin of contraceptive use; that is, the distribution of methods chosen by contraceptive users. I present use first by all methods (Figure 17) and then by all methods excluding OCPs and IUDs (Figure 18). There are some interesting trends over time in the distribution of methods chosen. IUD use has grown steadily since 2008 relative to all other methods, from about 10% of users to slightly les than 20% of users. OCP use, meanwhile, has declined from about more than 80% to about 70%. Together, OCPs and IUDs consistently make up about 90% of contraceptive use. Sterilization is the third most popular option, at about 8% of user by the end of the study period. The vaginal ring is the fourth most popular option at about 5-6% of users, followed by the tri-monthly injection at about 3%. Use of the implant is rare but has risen from near zero to about 1.5% of users. Use of the patch is rare and has declined slightly; use of diaphragms, cervical caps and emergency contraception is basically nonexistent relative to other methods. It's important to note that most brands of emergency contraception are available over-the-counter during this period, so my estimates of emergency contraceptive likely dramatically underestimate use. There are no obvious immediate shifts in the distribution of methods chosen following the implementation of the ACA mandate.

I also present the mean monthly OOP costs for all short-term (Figure 19) and long-term (Figure 20) methods. Several interesting trends are visible in these figures. The first is that all of the average monthly costs of the short-term methods are considerably less than the long-term methods. The second is that there is a lot of seasonal variation in the OOP cost. This variation is much more pronounced among methods that are administered in physician offices: IUDs, implants, and the injection. This is because costs for these methods count towards health insurance deductibles, and as the year progresses women are more likely to have spent up to their deductible or OOP limit and incur less cost-sharing for a given method. OOP costs for the short-term methods tend to increase a small amount in the first month of each year, which is possibly due to increased copays taking effect as plans renew.

The average costs for most methods fall sharply following August 2012. The only two methods whose costs appear mostly unchanged are the ring and the patch. This correlates with the anecdotal reports I'd heard from women's health providers and the recent studies reporting variation in insurer compliance with the mandate (Akers, 2014; Sobel et al., 2015; Benyo et al., 2015). Now that the administration has clarified which categories of contraceptive methods are required to have an option included in plans with no cost-sharing, I expect the OOP cost for the ring and patch will drop moving forward as insurers come into full compliance with the mandate.

I also present seasonally adjusted OOP costs for OCPs and IUDs, the two most commonly used methods (Figure 21). The average adjusted monthly OOP price of OCPs fell from \$33 to \$19 between June of 2012 and June of 2013; the average adjusted OOP price of IUDs fell from \$267 to \$120 in that same time period.

In order to assess the relative magnitude of these OOP cost changes for contraceptive users, Table 18 reports total mean and median OOP spending and the percentage of that spending spent on contraceptives for OCP users and women who receive IUD insertions. Because the mandate was implemented mid-year in 2012, I compare spending percentages in the first six months of 2012 with the first six months of 2013. For women who were enrolled in insurance continuously and had at least one claim for OCPs in both periods, the mean and median percentages of OOP spending spent on OCPs drop from 44.0% and 36.0% to 22.4% and 0%, respectively. For women who received an IUD during these same periods, the mean and median OOP spending percentages in the period they received their IUD drop from 30.3% and 13.2% to

11.3% and 0%, respectively.

I use these values to perform a back-of-the-envelope estimation of the per-woman savings on yearly OCP costs for OCP users and IUD insertions for women receiving IUDs. I estimate that the average OCP user is saving \$254.91 per year, and the median OCP user is saving \$204.65 per year. Mean and median savings on IUD insertions are estimated to be \$248.30 and \$107.95 per woman, respectively (Table 18).

4.4. Regression analyses

Unlike the state-level mandates, there is no geographic or temporal variation in the implementation of the ACA mandate. I therefore identify the impact of the ACA mandate by using variation in the magnitude of the change in the OOP price at the group level. Some groups show bigger average changes in OOP cost for some methods than others; if there is any response to the change in OOP price, there would be a larger change in use in groups where the OOP price dropped by a larger amount.

In order to examine within-group changes, I take a 10% sample of groups (rather than individuals). I limit my regression analyses to the period January 2012 to June 2013, as this is the period in which I quantify the within-group change in price. In order to quantify the within-group change in price of a given contraceptive method, it is also necessary to limit my analysis to groups in which the rate of claims of that method was non-zero in both periods for which I quantify the price change.

I use the following regression specification to test whether there are differential changes in use by group:

$$P(Y_{ijmg} = 1) = f(\lambda[1 = Post]_m + \theta \triangle OOP_{jg} + \beta[1 = Post] \times \triangle OOP_{jg} + \mathbf{X}_{img})$$

Here, i indexes individuals, j indexes the type of method, m indexes months, and g indexes groups. The outcome of interest is the probability of any contraceptive use. $\triangle OOP_{jg}$ is the within-group change in the OOP price of method j between the first six months of 2012 and the first six months of 2013. This variable can be thought of as the "dosage" effect of the mandate; some groups saw a drop in OOP price for a given method, while others saw no change or increased OOP price following the mandate.

I perform the analysis using both the mean and median changes in OOP price. The coefficient of interest is β, which tests the hypothesis that the change in use of contraceptives after the mandate varies by the changes in OOP price for a method. Because $\triangle OOP_{js}$ is negative, a negative estimate for β would suggest that use increased more in states with a larger drop in price.

I perform this analysis for the two most commonly claimed methods in the data, OCPs and IUDs. My analytic sample varies by method because use of OCPs and IUDs varies by group. For each method, I limit my analysis to groups where the rate of use of the method was non-zero for both of the periods where I calculate the mean and median change in OOP price. For the OCP analysis, my final sample is 3,279,542 observations, and 256,514 individual women enrolled in 4,051 groups. For the IUD analysis, my final sample is 2,250,458 observations, and 173,352 individual women enrolled in 245 groups.

I also use this model to analyze choice of method on the intensive margin; that is, testing for substitution to OCPs and IUDs within contraceptive users. I use the same model specification as above, except my dependent variable is the probability of use of OCPs or IUDs, conditional on using any prescriptive contraceptive.

As a sensitivity analysis, I also test a version of the above specification that includes a interrupted linear time trend interacted with the $\triangle OOP_{jg}$ variable:

$$
\begin{gathered}
P(Y_{ijmg}=1)=f(\mathbf{X}_{img}+\beta_1 Time_m+\beta_2[1=Post]_m+\beta_3\triangle OOP_{jg} \\
\beta_4[1=Post]_m\times\triangle OOP_{jg}+\beta_5 Time_m\times\triangle OOP_{jg}+\beta_6[Time\ since\ 2012m8]_m \\
\beta_7[Time\ since\ 2012m8]_m\times\triangle OOP_{jg}
\end{gathered}
$$

This could be considered a de-trended dose-response difference-in-difference analysis, or alternatively, a dose-response interrupted time series analysis. It allows for both a level and slope change in utilization following the ACA mandate, and allows that level and slope change to vary by the "dosage" effect of the mandate, $\triangle OOP_{jg}$.

4.4.1. Regression results

Figure 22 present scatterplots of the state-level change from 2012 to 2013 in mean OOP prices of OCPs (IUDs) by the change in state-level use of OCPs (IUDs). There is large variation at the employer-group level in the magnitude of the change in OOP price for both methods. These figures do suggest a relationship between the magnitude of the change in OOP price and the change in utilization of that method; although it is noisy, there does appear to be a slightly negative relationship between the change in mean OOP price and the change in rate of use for both methods. Figures plotting the change in median price by the change in rate of use were qualitatively similar (results not shown).

To visualize the change in utilization by the "dosage" effect of the ACA mandate,

Figure 23 presents time series of claim rates of OCPs and IUDs. For both figures, the solid blue line represents the average employer-group rate of utilization for groups whose mean OOP price dropped in 2013 relative to 2012. The dotted red line represents the average claim rates for a given method among employer-groups whose average OOP price stayed the same or decreased in this same period. For both figures, claim rates are presented as averages, weighted by the number of enrollees in that group in that month. Both figures suggest that employers that saw OOP price decreases for OCPs or IUDs experienced increased claim rates for that method. The figures also do not suggest big differences in pre-trends between the groups that were more or less affected by the ACA mandate.

Tables 19 and 20 present the results of regressions testing whether usage rates of OCPs and IUDs vary by the magnitude of the average change in price by group. The coefficients of interest are the interaction between the change in OOP price and the post-mandate dummy. For the analysis of OCP utilization, the coefficients of interest are negative and significant, regardless of whether the identifying variation is the change in mean or change in median OOP price. The coefficient on "Post x Change in Mean OOP price" is -0.000433. This can be interpreted as the expected change in utilization when the mean OOP price for OCPs increases by \$1. If we scale this coefficient by the overall drop in mean OOP price for OCPs over this period, \$12.37, we would expect the ACA mandate to result in an increase of $0.000433 \times \$12.37 = 0.0054$ in the probability of using OCPs. The rate of use of OCPs overall is 0.1386, so this price change has only a small impact on overall OCP use. We can use this change to produce a back-of-the-envelope arc elasticity estimate. If we assume that the OOP price dropped by 100%, this estimate comes out to be: $-\frac{0.0054}{0.1386}/1 = -0.039$. This is a very inelastic estimate for demand for OCPs, but it is consistent with the only other demand elasticity estimate from the U.S. market in the literature (-0.04 to -0.09) (Collins and Hershbein, 2013).

Similarly in the IUD analysis, the coefficient on the "Post x Mean IUD change" model is negative and significant. However, when I use the change in the median instead of the change in the mean, the estimate becomes non-significant. If we use the coefficient on "Post x Mean IUD change" to estimate an arc elasticity for IUDs, we also get a very inelastic estimate. The mean OOP price of IUDs during this period dropped from \$262.38 to \$84.3, so if we scale the coefficient by this price change, we get a $0.00000714 \times \$178.08 = 0.0013$ change in the rate of IUD use. The baseline rate of use of IUDs is 0.0342, and if we assume a 100% drop in OOP price, this produces an elasticity estimate of $-\frac{0.0013}{0.0342}/1 = -0.038$.

Tables 21 and 22 present the results of models that include the interrupted linear trend. There are two coefficients of interest to note here: the level x dosage interaction Post x $\triangle OOP_{jg}$, and the slope x dosage interaction $[Time since 2012m8]_m \times \triangle OOP_{jg}$. These are given for the mean and median OOP change for OCPs in Table 21, and for IUDs in Table 22. I find results for these models that are robust for the mean changes in OOP price, but not the median change in OOP price. The coefficient on the level change is not statistically significant for any model, but the coefficient on the slope change for the mean change in OOP price of OCPs is -0.0000322 and statistically significant at the 10% level ($p = 0.052$). The coefficient for the slope change on the mean change in OOP price of IUDs is -0.00000181 and statistically significant at the 5% level.

It's also important to note that this analysis has some potential endogeneity bias that may bias me towards the null. Women willing to pay a higher OOP price pre-mandate are likely to differ from women willing to pay a lower OOP price in a different state or group. If the consumers in states with a larger change in the magnitude of the OOP price for a method have more inelastic demand for the product, this would bias me towards a null finding in this analysis.

Overall, the results from this analysis suggest that consumers are very unresponsive to OOP price of OCPs and IUDs. Although the models demonstrate a statistically significant price response, the magnitude of the effect size is small.

4.4.2. Robustness checks:

I've conducted several robustness checks for this analysis.

- In addition to examining the extensive margin of OCP and IUD use, I also ran the above models only among contraceptive users to see if there were large shifts in the type of method chosen by user. Results from these models were non-significant.

- Triple differences: I also tried using the above models and adding a triple difference by age and by non-white status. The triple-difference by age was non-significant. The triple-difference by race, using a dummy variable for non-white race/ethnicity status, found a positive and statistically significant coefficient of interest on the triple interaction term. Because $\triangle OOP_{jg}$ is negative, this suggests that, compared with whites, non-whites increased their use of IUD less post-mandate than did whites.

- I also tested this analysis at the more aggregated state level, using the state-level change in the mean and median OOP price as my identifying variation. Results from these state-level models found non-significant impacts of the mandate on contraceptive use.

4.5. Sensitivity analysis: Enrollment in grandfathered plans

Another source of plausibly exogenous variation in mandate impact is whether or not a women is enrolled in a grandfathered plan that is not yet subject to the mandate. With perfect data, I would be able to see when an individual woman in my data switched or renewed plans and therefore make inferences about when her plan switched from grandfathered to non-grandfathered status. Unfortunately, the data contains no information regarding when a woman may have switched or renewed plans during an uninterrupted enrollment period, and very little information about plan characteristics.

However, because the data contains state of residence, I can leverage geographic variation in the percent of employees enrolled in grandfathered plans. From the staff of the Kaiser Employee Health Benefits Survey, I obtained the grandfathered plan enrollment percentages by census division from 2011 to 2013. Due to concerns regarding potential identification of employers, they were not able to release state-level estimates to me. Table 17 shows the enrollment percentages for the nine census regions over time.

I use the following baseline specification to test whether there are larger changes in utilization in divisions where fewer people were enrolled in grandfathered plans:

$$P(Y_{its} = 1) = f(\gamma_m + \theta_s + \beta[1 = Post\,ACA\,mandate] \times Enroll_{dt} + \mathbf{X}_{its})$$

This analysis includes data from January 2011 to June 2013. Here, i indexes individuals, s indexes states, and d indexes census divisions, while m indexes months and t indexes years. Individual covariates included in $\mathbf{X_{its}}$ include age, race, and the median education level of that individual's census tract. As before, of these individual covariates, only age is time-varying; the other two are only available at one point in time in the data. For some specifications of the model, I also add in division-specific time trends ($\mu_d \times t$) or state-specific time trends ($\theta_s \times t$).

$Enroll_{dt}$ is the census division-level rate of enrollment in grandfathered health plans in year t, and takes a value between 0 and 1. The coefficient of interest is β, which reflects any differential pattern of contraceptive use over time in areas where the mandate applied to more people than in areas where it applied to fewer people. Because an increase in $Enroll_{dt}$ means that the ACA affected fewer people in that area, a negative value for β would suggest that the mandate increased contraceptive use.

My identifying assumption in this analysis is that in the absence of the ACA mandate, trends in contraceptive usage in census divisions with higher grandfathered plan enrollment would have been equivalent to trends in contraceptive use in divisions with lower grandfathered plan enrollment. Given that trends in contraceptive use could potentially vary by geography, I also conduct a test of my identifying assumption using a dynamic model specification that interacts the enrollment in each census division in a given year with a series of month dummies, as follows:

$$P(Y_{its} = 1) = f(\gamma_m + \theta_s + \sum_{m=0}^{M} \beta_m \times \gamma_m \times Enroll_{dt} + \mathbf{X_{ims}})$$

As before, i indexes individuals, s indexes states, and d indexes census divisions, while m indexes months and t indexes years. Individual covariates included in $\mathbf{X_{its}}$ include age, race, and the median education level of that individual's census tract. My coefficients of interest are now the β_m values, which reflect any differential patterns of use in high vs. low enrollment census divisions in a given month.

Importantly, this model does not assume any change in slope or level of contraceptive use for any particular month. A finding that β_m values prior to the mandate are statistically equivalent to zero while β_m values following the ACA mandate are negative and statistically significant would support both my identifying assumption and an interpretation that the mandate had a positive impact on contraception use. In contrast, statistically significant values for β_m in the pre-mandate period, either positive or negative, would suggest a violation of my identifying assumption and potential bias of the coefficient β from the baseline model specification.

4.5.1. Regression results

Regression results from this analysis are presented in Table 23. The baseline model with state and time fixed effects finds a negative and significant association between being in a division with lower grandfathered plan enrollment and the post-mandate period. However, this finding is not robust to either division or state-specific linear trends. For a negative association between grandfathered plan enrollment and prescription birth control to be seen in the baseline difference-in-difference model but disappear with the addition of division or state-specific linear trends suggests that the parallel trends assumption of the model may be violated. I therefore turn to my dynamic model specification to examine whether there is a statistically significant association between census division grandfathered enrollment and contraceptive use prior to the implementation of the mandate.

Figure 25 presents the results from the dynamic model specification in as a figure, with each month's β_m coefficient plotted on the y-axis and months on the x-axis. The reference month is the month of mandate implementation, August 2012. The results suggest there is a clear pre-trend in the data that is unchanged after mandate implementation; during this time period, census divisions with lower levels of grandfathered plan enrollment have contraceptive use rates that are increasing faster than divisions with lower levels of grandfathered plan enrollment. This trend appears to have been present before the ACA mandate and appears unchanged following mandate implementation. My results in this analysis therefore support my finding that there seems to be very little change in utilization in response to the ACA mandate.

4.6. Discussion

I find evidence that OOP costs for birth control have dropped dramatically following the implementation of the ACA mandate. However, descriptive time series and regression analyses do not suggest any large shifts in utilization following these price changes, either in overall utilization rates or the type of product chosen. Using variation in the change in mean/median OOP price at the group level, I do find statistically significant impacts of the price change on utilization of OCPs and IUDs, but the magnitude of the effect is small even when statistically significant. I estimate an arc elasticity of demand of -0.039 for the pill and -0.038 for the IUD, both very inelastic estimates.

There are two potential reasons for this finding. The first is that for demand for contraception among women in private health insurance is inelastic. The second is that demand for contraceptives is more elastic than I estimate here, but adjusts slowly to price shifts, and my data is too short-term to see these responses.

4.6.1. Limitations

As I've discussed in earlier sections of this chapter, my current analysis potentially suffers from some selection that may bias me towards the null, because women in firms where the OOP price was higher prior to the ACA are have potentially more inelastic demand than women in firms with lower OOP price before the mandate. This limits my ability to definitively argue that the small demand responses seen in my results are due to inelastic demand for contraceptives. In addition, I only have eleven months of data following the implementation of the ACA mandate, and am therefore only able to detect very short-term effects of the mandate. When more data become available, further research will have to determine more definitively whether there are longer-run impacts on OOP spending and utilization.

4.6.2. Future work

It will be important to continue to examine the impacts of the ACA mandate in future work. There are several analyses I could consider implementing using this data. The first is to consider a discrete choice survival analysis with my outcome being time to first use of a contraceptive. I could also consider different samples of women who might be more actively choosing their contraceptive method; teenagers or post-partum mothers are two potential populations I could consider focusing on in more depth.

4.7. Tables and Figures

Table 17: Grandfathered plan enrollment by census division over time

Division	2011	2012	2013
New England	43.3%	32.1%	32.2%
Mid Atlantic	55.8%	39.7%	24.2%
East North Central	58.4%	37.5%	32.0%
West North Central	55.3%	49.3%	30.3%
South Atlantic	53.2%	56.4%	33.2%
East South Central	49.6%	57.3%	53.3%
West South Central	48.4%	53.1%	46.3%
Mountain	65.3%	42.8%	44.1%
Pacific	63.9%	57.1%	38.3%

Figure 14: Prescription contraceptive use by month

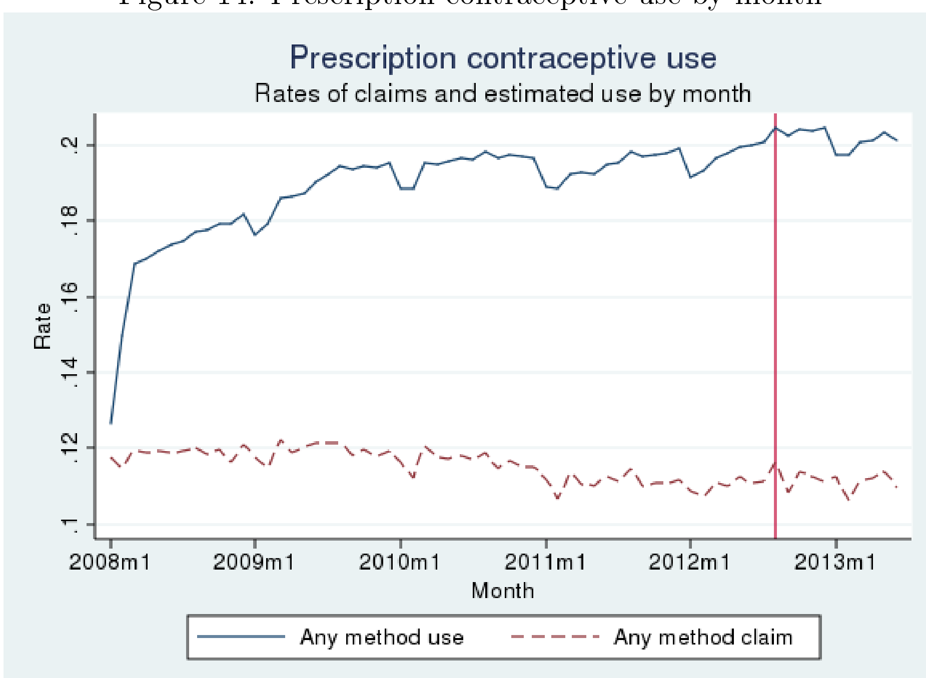


Figure 15: Short-term prescription contraceptive use by month

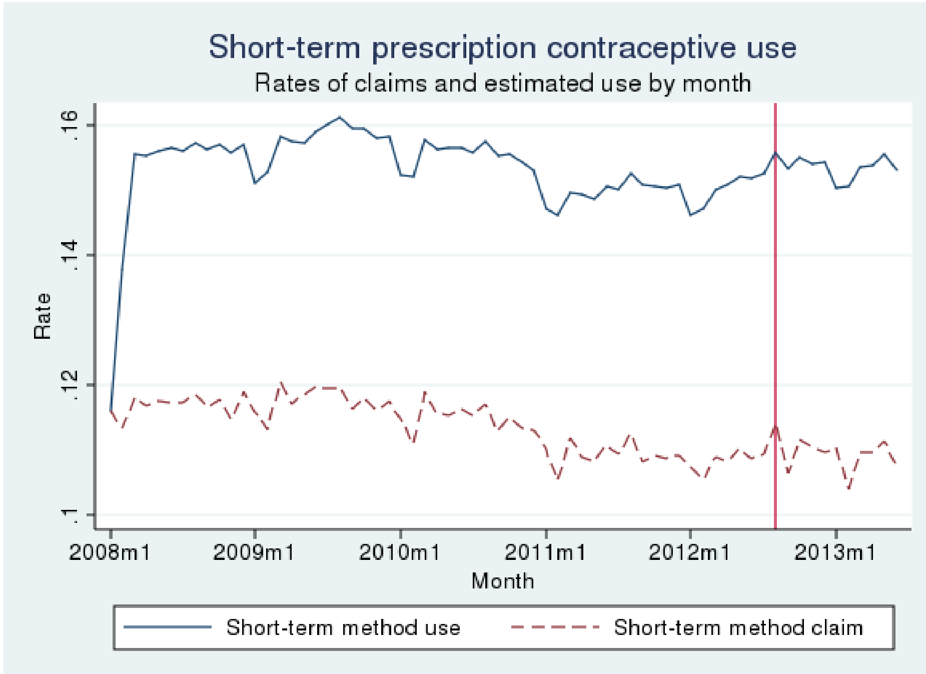


Figure 16: LARC use by month

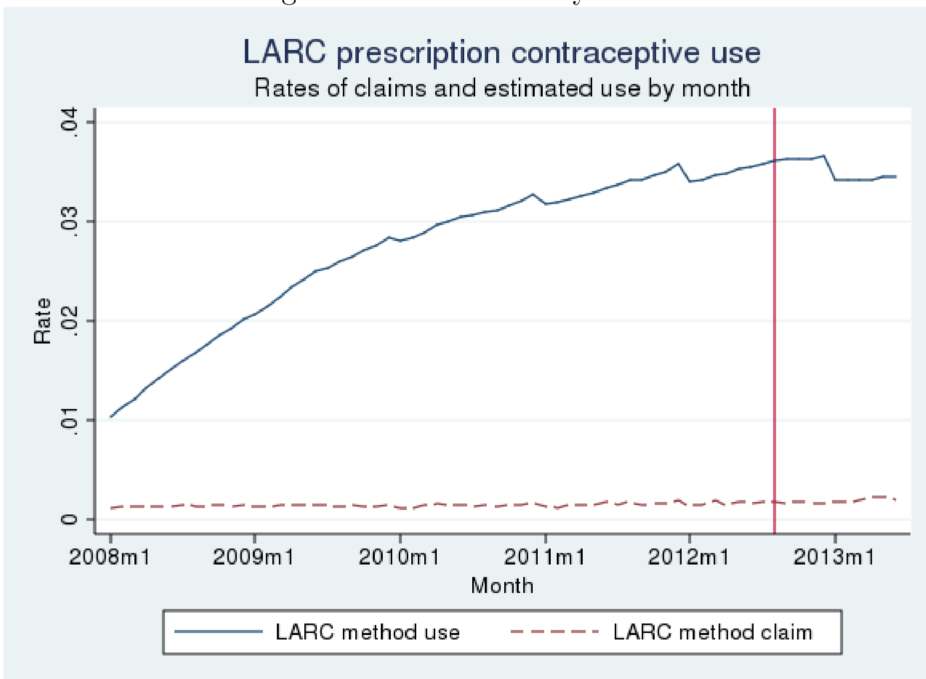


Figure 17: Conditional use by method

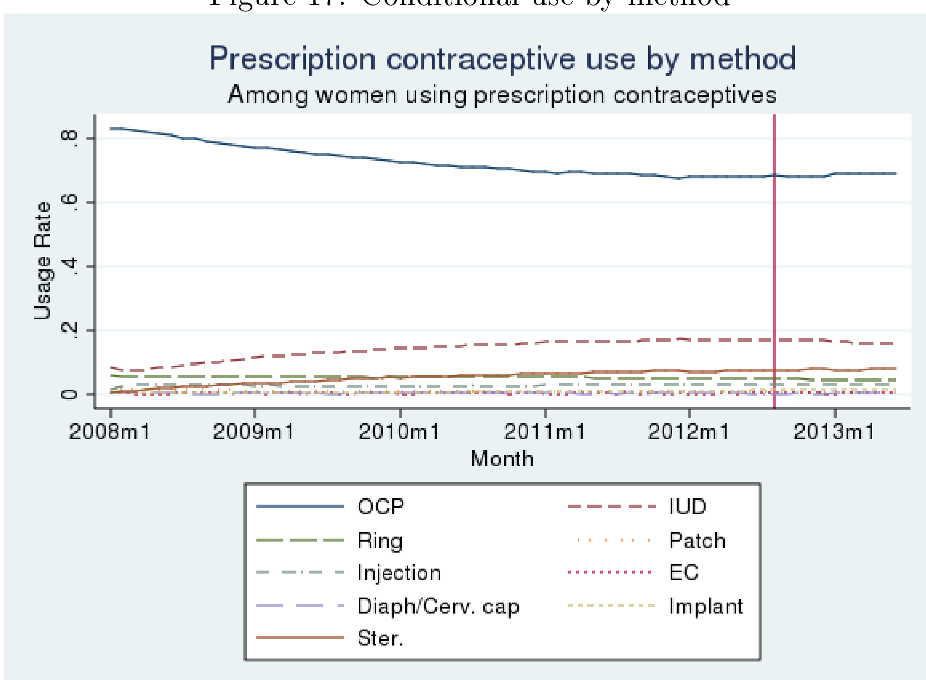


Figure 18: Conditional use of less frequently used methods by month

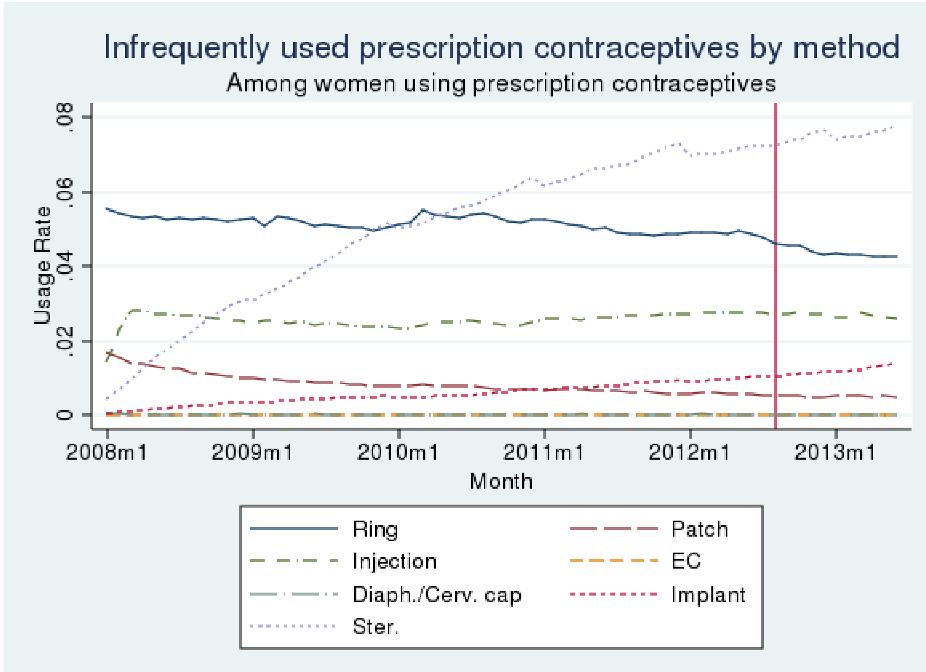


Figure 19: Unadjusted mean monthly OOP cost of short-term methods

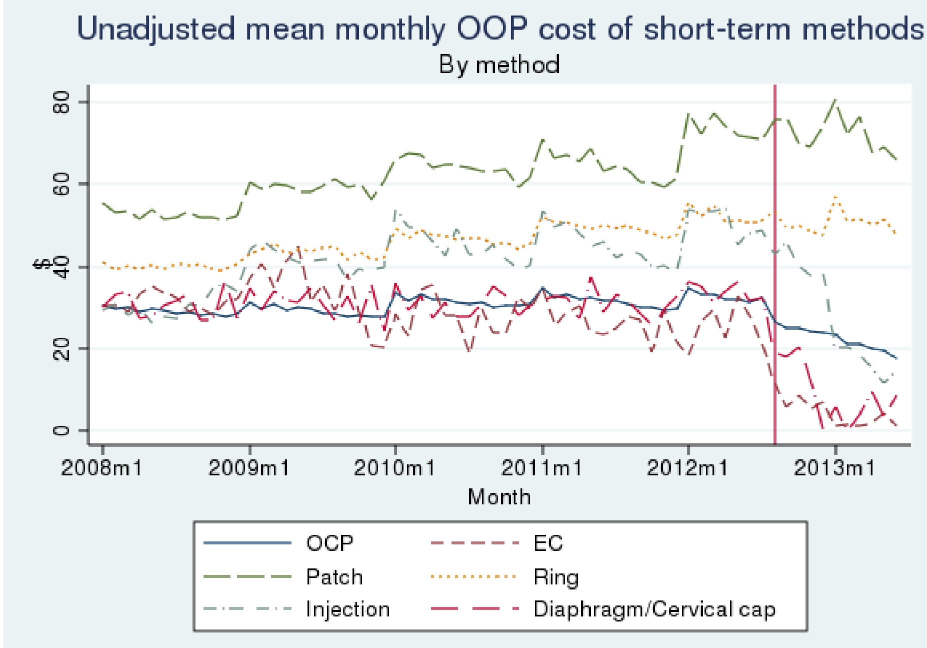


Figure 20: Unadjusted mean monthly cost of LARC methods

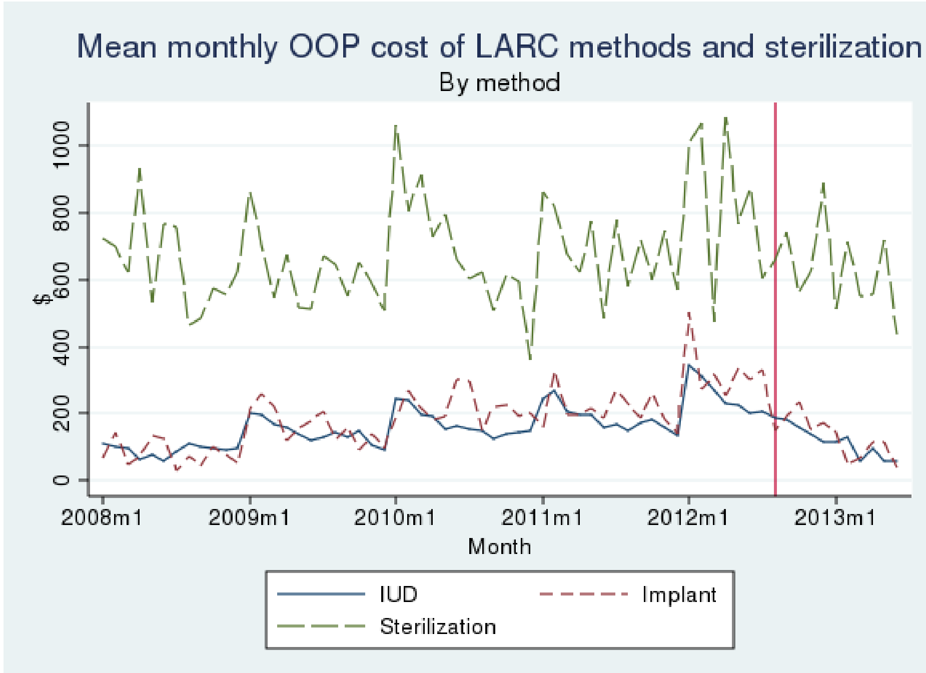


Figure 21: Adjusted mean monthly costs for OCPs and IUDs

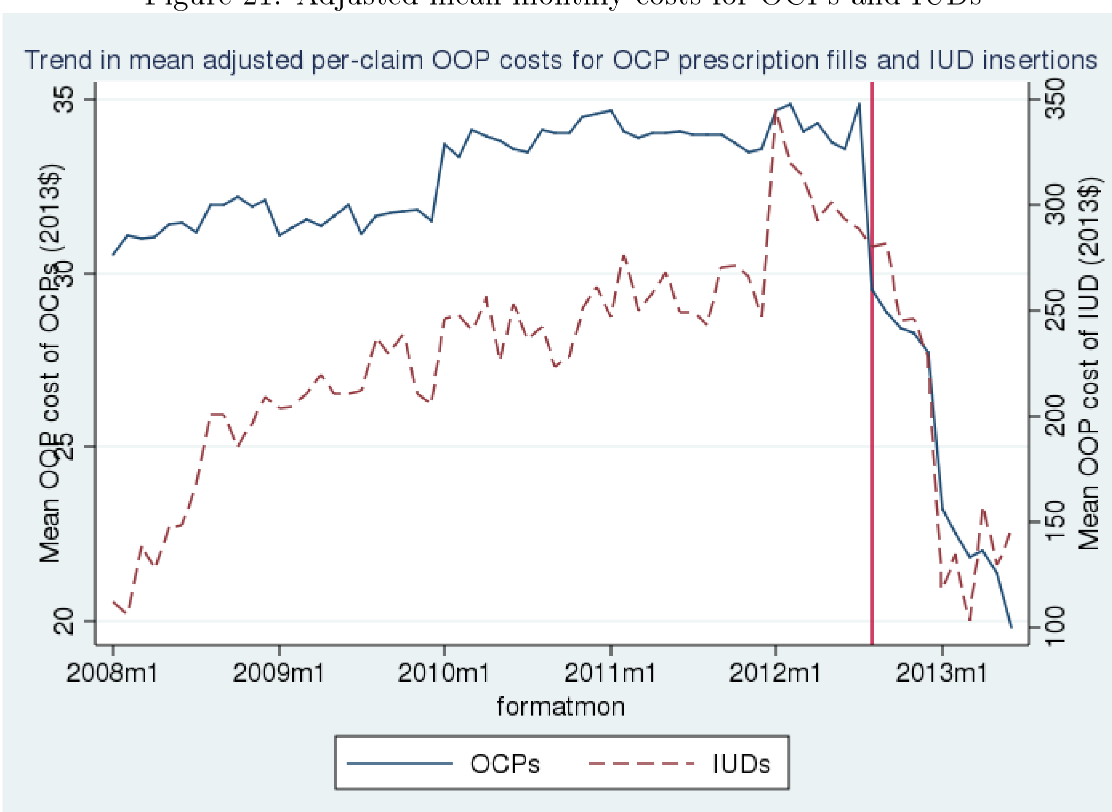


Table 18: Percentage of total OOP spending spent on prescription birth control by OCP users and women receiving IUDs

	January to June, 2012		January to June, 2013	
Panel A: OCP users	Mean	Median	Mean	Median
Total OOP spending	\$557.08	\$284.10	\$524.12	\$244.19
% of OOP spending spent on OCPs	44.00%	36.00%	22.40%	0.00%
	Mean		Median	
Implied savings per OCP user	\$254.91		\$204.65	
	January to June, 2012		January to June, 2013	
Panel B: IUD insertions	Mean	Median	Mean	Median
Total OOP spending	\$1,181.52	\$817.31	\$975.34	\$418.86
% of OOP spending spent on IUD insertion	30.30%	13.20%	11.30%	0.00%
	Mean		Median	
Implied savings per IUD insertion	\$248.30		\$107.95	

Table 19: Effect of ACA Mandate on OCP utilization: price change by group

	(1) OCP use	(2) OCP use
Post	-0.00161	-0.00106
	(0.00266)	(0.00298)
Mean OCP change	-0.000914^{+}	
	(0.000475)	
Post x Mean OCP change	-0.000433**	
	(0.000155)	
Median OCP change		-0.000684
		(0.000496)
Post x Median OCP change		-0.000318*
		(0.000159)
Individual covariates	Yes	Yes
Observations	3111220	3111220
R^2	0.013	0.013

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, * $p < 0.001$

All models weighted by group size. Individual covariates include age, race, and median census tract education level.

Figure 22: Change in mean OOP price vs change in usage rate by state, for OCPs and IUDs

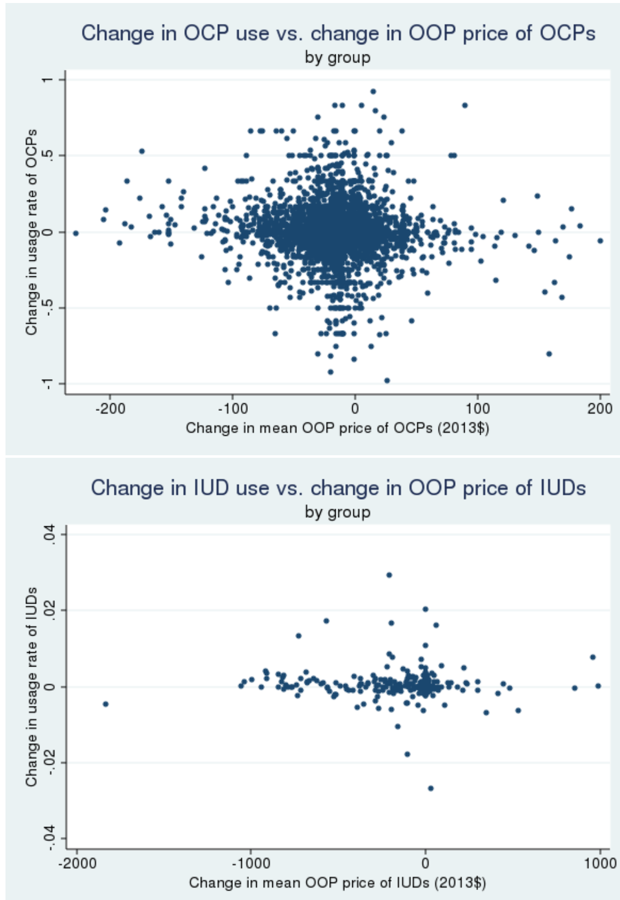


Figure 23: Claim rates of OCPs and IUDs by the group-level impact of the ACA mandate

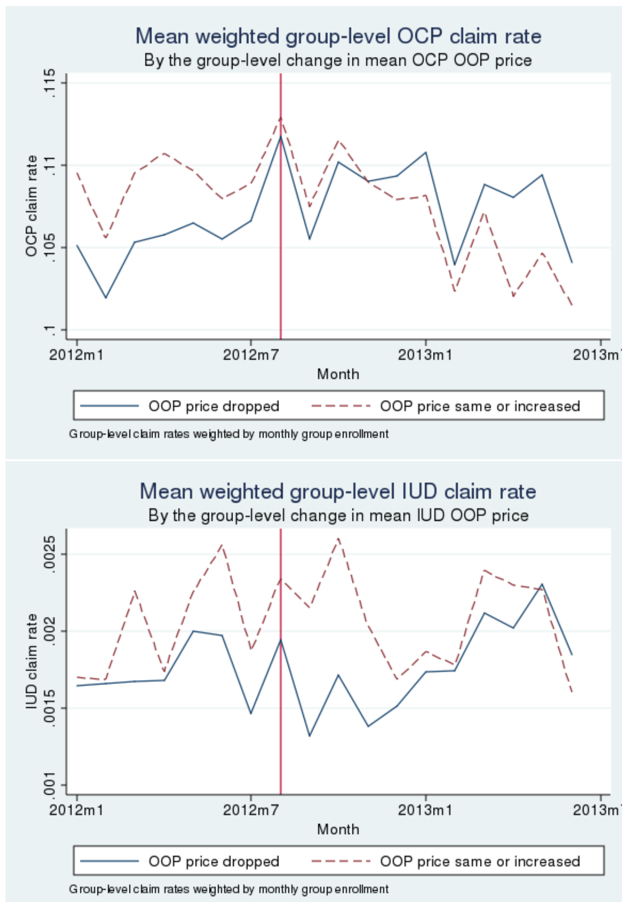


Table 20: Effect of ACA Mandate on IUD utilization: price change by group

	(1)	(2)
	IUD use	IUD use
Post	-0.000643*	-0.000753^{+}
	(0.000290)	(0.000402)
Mean IUD change	0.0000594*	
	(0.0000289)	
Post x Mean IUD change	-0.00000714*	
	(0.00000288)	
Median IUD change		-0.000000654
		(0.0000249)
Post x Median IUD change		-0.00000111
		(0.00000256)
Individual covariates	Yes	Yes
Observations	2136471	2136471
R^2	0.005	0.004

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, * $p < 0.001$

All models weighted by group size. Individual covariates include age, race, and median census tract education level.

Figure 24: Division-level usage rates vs. % of people enrolled in grandfathered plans

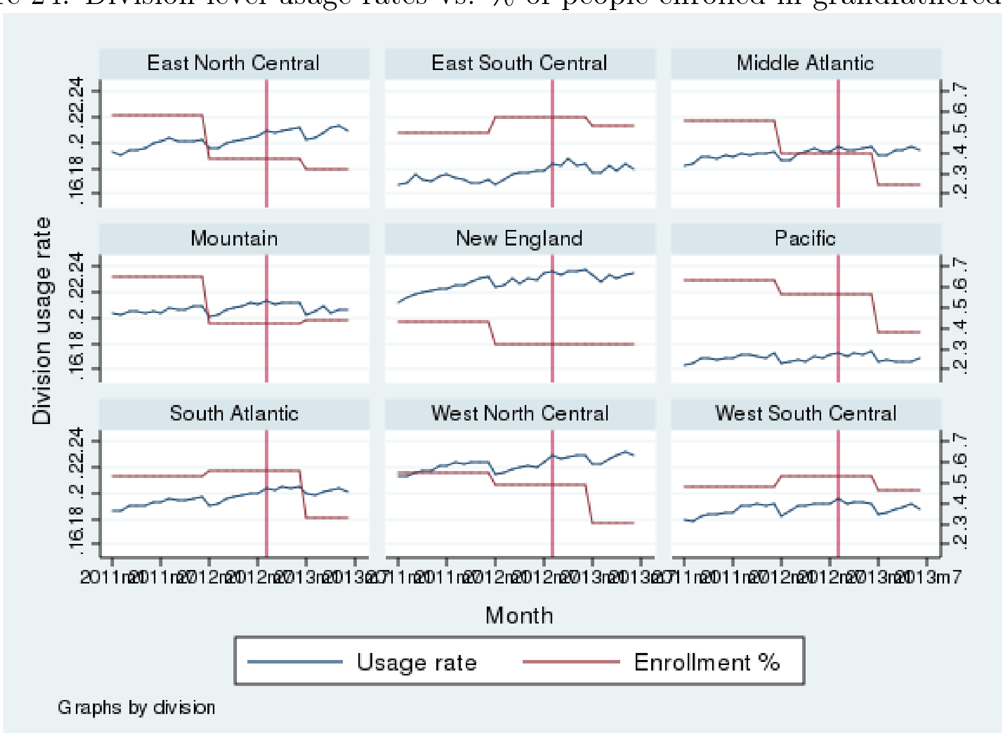


Table 21: Effect of ACA Mandate on OCP utilization: Price change by group with linear time trend

	(1) OCP use	(2) OCP use
Time	0.000132	0.000166
	(0.000218)	(0.000237)
Post	0.000509*	0.000495*
	(0.000221)	(0.000225)
Time since 2012m8	-0.000553*	-0.000509**
	(0.000146)	(0.000175)
Mean OCP change	-0.000815+	
	(0.000437)	
Time x Mean OCP change	-0.0000246+	
	(0.0000134)	
Post x Mean OCP change	-0.0000196	
	(0.0000785)	
Time since 2012m8 x Mean OCP change	-0.0000322+	
	(0.0000166)	
Median OCP change		-0.000614
		(0.000451)
Time x Median OCP change		-0.0000173
		(0.0000147)
Post x Median OCP change		-0.0000474
		(0.0000616)
Time since 2012m8 x Median OCP change		-0.0000190
		(0.0000145)
Individual covariates	Yes	Yes
Observations	3111220	3111220
R^2	0.013	0.013

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, * $p < 0.001$

All models weighted by group size. Individual covariates include age, race, and median census tract education level.

Table 22: Effect of ACA Mandate on IUD utilization: Price change by group with linear time trend

	(1)	(2)
	IUD use	IUD use
Time	0.000384*	0.000391*
	(0.0000228)	(0.0000259)
Post	0.00173+	0.00163+
	(0.000924)	(0.000959)
Time since 2012m8	-0.000973*	-0.000985*
	(0.000181)	(0.000202)
Mean IUD change	0.0000582*	
	(0.0000294)	
Time x Mean IUD change	0.000000314	
	(0.000000354)	
Post x Mean IUD change	0.000000903	
	(0.00000314)	
Time since 2012m8 x Mean IUD change	-0.00000181*	
	(0.000000729)	
Median IUD change		-0.00000244
		(0.0000256)
Time x Median IUD change		0.000000447
		(0.000000303)
Post x Median IUD change		-0.00000185
		(0.00000152)
Time since 2012m8 x Median IUD change		-0.000000548
		(0.000000351)
Individual covariates	Yes	Yes
Observations	2136471	2136471
R^2	0.005	0.004

Standard errors in parentheses

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, * $p < 0.001$

All models weighted by group size. Individual covariates include age, race, and median census tract education level.

Table 23: Relative Effect of ACA Mandate on census divisions with lower vs. higher grandfathered plan enrollment

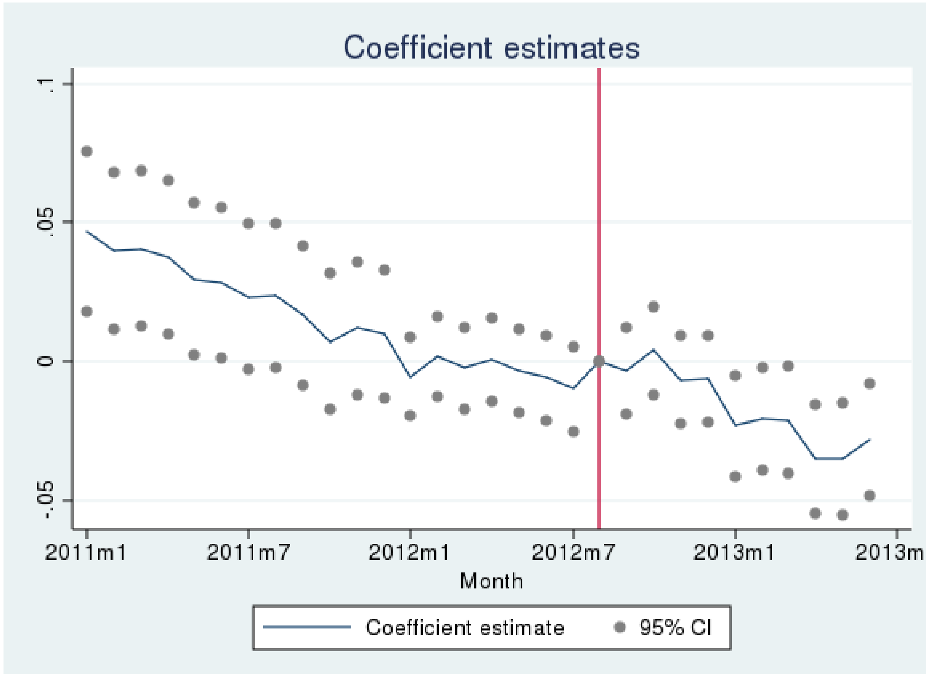
	(1) Prescription BC Use	(2) Prescription BC Use	(3) Prescription BC Use
Post-mandate x GF plan enrollment	-0.0134*	-0.00442	-0.00429
	(0.00594)	(0.00530)	(0.00530)
Individual covariates	Yes	Yes	Yes
Month-Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Division-specific linear trends	No	Yes	No
State-specific linear trends	No	No	Yes
Observations	7331950	7331950	7331950
R^2	0.013	0.013	0.013

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Individual covariates include age, race, and education.

Figure 25: Coefficient estimates of β_m values from dynamic regression model



CHAPTER 5 : Conclusion

This project is a comprehensive examination of the impact of contraception coverage mandates on contraceptive utilization. In this chapter I briefly summarize my results, and then discuss some implications and future directions for the project. Table 24 presents a summary of the results, limitations, and future directions for each analysis.

5.1. The impact of state-level contraceptive coverage mandates

Using both survey and administrative claims data, I find no evidence that the state-level contraceptive coverage mandates impacted contraceptive utilization. However, I do find some evidence that the state mandates did result in more methods being covered by employer groups. These impacts were explained mostly by increases in coverage of the patch. This could potentially explain why I saw little effect on the choice of longer-term methods in my analysis; if coverage of longer-term methods like the IUD was already included by most plans, I wouldn't expect to detect a shift towards those methods following the mandates. Regardless, my results suggest that women were unresponsive to the decreases in OOP cost for contraceptives following the mandates.

My results in these analyses contradict the findings from other work examining these state mandates. One study, Magnusson et al. (2012), was a purely cross-sectional analysis and did not employ a causal identification strategy. The other two studies, Atkins and Bradford (2014) and Dills and Cotet-Grecu (2014), use data from one survey, the Behavioral Risk Factor Surveillance Study, and only examine a subset of mandates. Neither are able to limit their analysis only to women in plans with employers who do not self-insure, an important limitation that I am able to address using my analysis of the OptumInsight dataset in Chapter 3.

Both of my analyses of the state mandates are subject to limitations. In my analysis of the NSFG, my power to detect an effect may be limited by my sample size. In my analysis using the OptumInsight data, the imperfect nature of the group identifier variable and the small size of most groups makes it difficult to impute group-level coverage of different contraceptive methods.

5.2. The impact of the ACA mandate

I find strong evidence that the ACA mandate has decreased OOP expenditures on prescription contraceptives. I see large decreases in mean and median OOP price for most contraceptive methods, with the median price of most methods falling to zero within several months of mandate implementation. In my primary analysis, I test for differential responses in utilization in employer groups with smaller or larger average changes in OOP price. I find that women in employer groups with larger drops in average OOP price of the pill or the IUD increase their utilization of these contraceptive methods, but the magnitude of the increases are small. Back-of-the-envelope arc elasticity of demand estimate for the pill and the IUD (-0.039 and -0.038, respectively) suggest that women in private health insurance are fairly price-insensitive in their demand for these products.

There are some limitations to this analysis. The short-term nature of my dataset makes it impossible for me to rule out longer-term impacts of the ACA mandate. In addition, there may be some selection bias in my identification strategy that could bias me towards finding no effect of the mandate. However, the results of my robustness checks using different identification strategy also support my findings that there are no large changes in utilization in response to the ACA mandate.

5.3. Policy implications, unanswered questions, and future research

My results suggest that women in private health insurance have inelastic demand for contraceptives. In this project, I modeled demand for contraceptives using neoclassical economic theory. However, my results suggest that there may be more important factors in women's choice of contraceptives than their OOP cost. One avenue for future research would be to reframe demand for contraceptives in the context of behavioral economic theory. Two tenets of prospect theory are 1) people are risk-averse over gains and risk-seeking over losses, and 2) people tend to overweight low probability events and underweight high-probability events (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992). I could consider this model in the context of purchasing prescription contraceptives; we would expect women to be risk averse when facing a low probability of a large loss such as an unplanned pregnancy. Additionally, because people overweight losses relative to gains, it's possible that the demand response to a copay decrease (a relative gain) may be smaller than the response to a copay increase (a relative loss). The combination of these factors could help to explain why I find that demand for contraceptives is so unresponsive to price. In this way, my results may be similar to studies of value-based insurance design programs that study the impact of co-pay decreases for high-value services on utilization. While some studies have found impacts on use, the effect sizes have generally been small or moderate, rather than large (Chernew et al., 2008; Choudhry et al., 2010a).

Another possible explanation for the lack of demand response is either varying demand or selection on contraceptive coverage at the employer level. If some employers primarily employ men, older women, or women uninterested in contraception for other reasons, these employers would have lower demand for contraception. Similarly, it's also possible that some women may choose their employer based on generosity of insurance coverage, although this second possibility seems less likely to me than the first. But both scenarios could explain both why insurance coverage of contraceptives was low relative to other drug coverage in the early 2000s and why I see very little, if any, demand responses to drops in the OOP price of contraceptives.

I see large OOP price decreases following the ACA mandate, but there are no large changes in utilization in response. Further research will investigate whether this apparent price unresponsiveness persists in the long-term. But in the absence of a change in utilization, the financial impact of the ACA mandate on wages is potentially similar to that of the mandated coverage of maternity benefits. If the wages of women of child-bearing age can be adjusted separately to account for the increased cost of insurance, then the incidence of the mandate will fall on them, much as the incidence of mandated maternity benefits has been found to be transferred almost completely to women's wages (Gruber, 1994a). If, on the other hand, wages of women cannot adjust separately from other employees, and instead wages decrease slightly for everyone, then the ACA mandate is a financial transfer to women of childbearing age from other employees. Opponents of the ACA mandate have argued that including contraceptives may increase the cost of insurance, while proponents have argued that insurance coverage of contraceptives would pay for itself in lowered medical costs for childbirth. My early results suggest that opponents of the ACA mandate are more likely to be correct. However, it's also not clear that any resulting rise in insurance premiums from the ACA mandate will be large enough to be economically significant.

There may also be non-OOP price-related barriers to accessing contraceptives. A recent prospective cohort study offered participants their choice of contraceptive at no cost, after counselling and education about all available methods. They found that with the triple barriers of cost, knowledge and access removed, 75% of participants chose a LARC method (McNicholas et al., 2014). However, there are important differences between this study and the likely impact of the ACA mandate. This study only enrolled women who were interested in starting a new method, and specifically counseled participants about the relative effectiveness of LARC methods vs. more short-term methods. In contrast, the ACA mandate lowers the OOP price of contraceptives for all women in private health plans, many of whom might be uninterested in changing their current contraceptive method. Furthermore, the ACA mandate does not directly change provider behavior or impact consumer knowledge about contraceptives, although some providers may take it upon themselves to educate their patients about the mandate. In some cases, women may not even be aware that their coverage has changed. A recent study of young adults' experiences shopping for health insurance on HealthCare.gov found that many were unaware that coverage for well-women visits and contraception were included as a preventive service with no cost-sharing (Wong et al., 2014).

My results highlight the challenges in implementing value-based insurance design. While a much-touted strategy for reducing health care costs, actual consumer responses to copayment or coinsurance decreases are likely to vary substantially. Recent research has found that for cardiac drugs, even dramatic decreases in copayments resulted in only modest changes in utilization (Choudhry et al., 2010a). When contraceptives were included in the ACA mandate, many women's health providers lauded their inclusion, arguing that the law would result in fewer unwanted pregnancies and abortions and reductions in health care costs. My research suggests that mandating coverage of contraceptives alone is unlikely to achieve these goals in the absence of further research into the factors affecting demand for contraceptives.

Table 24: Summary of results

	State mandates	State mandates	ACA mandate
Dataset	NSFG survey	OptumInsight claims	OptumInsight claims
Results	No statistically significant change in utilization	No statistically significant change in utilization or OOP costs; some evidence that mandates increased insurance coverage at the employer group level	Large drops in OOP price following mandate; little evidence of large changes in utilization. I estimate very inelastic demand elasticities for the pill (-0.039) and the IUD (-0.038).
Limitations	1) Cannot limit to women in non-self-insured plans 2) Potential power limitations	1) Data from only one insurer 2) Cannot impute plan-level insurance coverage	1) Short post-mandate period
Future directions	Return to RDC to complete power analysis	Search for better historical data on insurance coverage of contraceptives	Consider discrete choice analysis or analysis of subgroups more likely to be price-responsive; seek longer-term data as it becomes available

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