

Preliminary Draft: Please Do Not Cite

Emergency Contraception and Risky Sexual Behavior:

An Application of Synthetic Control Methods

Ruoding Tan

The Graduate Center, City University of NY

Ted Joyce

Baruch College, City University of NY and NBER

Silvie Colman

Mathematica Policy Research, Inc

Abstract

The public health consequence and economic impact of the over-the-counter (OTC) sell of Emergency Contraception (EC) have been the focus of a long-running health policy debate. An unsolved question is whether the improved access to EC would induce teenager and young unmarried women to change their sexual risk-taking behavior in a way that leads to an increase in STD. Although randomized field experiments have uncovered no effect of EC on risk-taking, critics have argued that the studies lack sufficient power to detect effects; On the other hand, population-based observational studies have lacked strong identification strategies. In this paper, we use a synthetic control methodology introduced by Abadie et al. (2010) to identify the potential casual effect of OTC access to EC on rates of STD among young women. We apply synthetic control analysis on the case study of Washington, the first state granted pharmacy access to EC 10 years prior to the nationwide approval. Our results suggest that access to EC had no effect on rates of STDs among teen and young women. We also find that easier access to EC does not affect teen abortion rates, but accounts for 6 percent of the decline in abortion among women 20-24 years of age. Our results are in direct contrast to recent manuscripts by Durrance (2010) and Oza (2010) who report large and significant increases in rates of STDs and declines in rates of abortion between 10 and 20 percent associated with OTC access to EC.

I. Introduction

In 2006, the Food and Drug Administration (FDA) approved over the counter (OTC) purchases of emergency contraception (EC) for women 18 years of age or older. The decision ended a decade long battle over making EC available for women without prescription requirement. Proponents of EC contend that the easier access to EC will prevent unintended pregnancy and abortion. Opponents, on the other hand, view the prescription requirement as a necessary restriction that curtails risky sexual activity. They note that any new contraceptive method could increase exposure to sexually transmitted disease (STD) by reducing condom use. As evidence supporting their concern, opponents, and even some more neutral observers, often point to a rise in STDs among adolescents in countries such as UK where EC had been made available without a prescription.

In the U.S., various concerns have also been raised regarding the alarming trend of STD prevalence, high rate of unintended pregnancy and abortion. The attention from social, public health and political perspectives is directed at primarily young unmarried women whose rates of STDs and unintended pregnancy are the highest. The STD prevalence among sexually active women 15-19 and 20-24 years old is close to 40 percent (Hampton 2008), making women in these age groups particularly vulnerable to reproductive health problem and unintended pregnancy. More than one unwanted pregnancy occurred for every 10 women 15-24 years of age (Finer and Henshaw 2006). Consequently, the burden of abortion falls heavily on these women. Over fifty percent of women obtaining abortions are teenagers and young adults 20-24 years old (Jones et. al 2010). The introduction of emergency contraception pill (EC), commonly known as morning-after-pill, or Plan B, can help to reduce the risk of unwanted pregnancy and the need for

abortions. As its name implied, EC has a narrow time frame of effectiveness. It must be used within 72 hours of unprotected sex and is most effective if taken within 24 hours. The OTC availability of EC is especially important for outreach to teens and minors who may have particular difficulty accessing physician services and obtaining a prescription within one to three days.

Prior to the FDA approval, there have been various policy efforts to promote wider and easier availability and access to EC during the past decade. Figure 1 displays a map of U.S. continental states with different color indicating the timeline of state legislative events allowing pharmacy access to EC. Before 1998, all states required a prescription for the use of EC regardless of a women's age. In 1998, the state of Washington became the first state to allow nonprescription sale of EC by launching the Emergency Contraception Collaborative Agreement Pilot Program. The program was the first policy initiative enabling pharmacists to directly dispense EC without a prescription to women of all ages. During the two-year implementation period, pharmacies from eighteen counties were involved in the program.¹ These counties, taken together, consist of more than 90% of the WA state population. Over 93% of the abortions were performed to women residing in these counties². Following Washington's lead, eight other states (AK, CA, HI, MA, ME, NH, NM, VT) took similar legislative effort to ensure the provision of EC's OTC access between 1998 and 2006 (Guttmacher Institute 2009) . Like Washington, some of these eight states allowed pharmacies to dispense EC without a prescription provided they had a collaborative practice agreement with local physicians. Other states agreed to follow a state protocol. . In August of 2006, the FDA approved the nation-wide nonprescription sale of EC to

¹ According to Durrance (2007), these counties are Benton, Clallam, Clark, Cowlitz, Island, King, Kitsap, Pierce, Skagit, Skamania, Snohomish, Spokane, Thurston, Wahkiakum, Walla Walla, Whatcom, Whitman, and Yakima.

² Authors' calculation based on Washington State Pregnancy and Induced Abortion Statistics, 1999 (www.doh.wa.gov/ehsphi/chs/chs-data/public/Abort_report_1999.pdf)

women age 18 or older. The access to EC for adolescents age 17 or younger, however, was still kept behind-the-counter. In April of 2009, the FDA approved the OTC availability of EC for 17-year-olds.

In this paper, we analyze whether pharmacy access to EC is associated with increased rates of gonorrhea and decreased rates of abortion among teens (ages 15-19) and young women (ages 20-24). The question can be linked to the more general question of whether legal or technological changes that reduced the risk of unintended birth lead to riskier sexual behavior. For example, there was concern that early legal access to the contraceptive Pill without parental consent would promote promiscuity among unmarried teens in the early 1970s; the legalization of abortion raised similar concerns (Klick and Straumen 2003; Akerlof et. al 1996; Bailey 2006; Gruber et. al). More recently, proponents of parental involvement laws for minors seeking an abortion have argued that such statutes reduce rates of unintended pregnancy. Several studies are consistent with these claims (Kane and Staiger 1996; Levine 2003). The initial studies of EC using randomized control trials reported no association with riskier sexual behavior. But these studies have been criticized for a lack of statistical power. Authors of two recent studies with much larger samples reported substantial increases in STDs and decreases in abortion rates associated with OTC access to EC in states that approved such availability prior to national FDA approval (Durrance 2010; Oza 2011). However, the large samples in these studies come at the expense of weaker designs.

From a research perspective, the lack of consensus on whether the effect of OTC access of EC is detrimental or beneficial is primarily driven by the problem related to research design and data availability. A major challenge is that only nine states allowed access to EC without a

prescription prior to the national approval. The small sample of early adopters has hampered identification strategies and limited statistical power. Moreover, many of these state laws were passed quite recently, leaving the researchers with limited length of sample period.

To address the aforementioned methodological limitations, we apply the synthetic control method recently introduced by Abadie et. al (2010) to study the impact of OTC access to EC on young unmarried women in Washington State. The synthetic control method is an appealing data-driven procedure for case studies with only one or very few treated units. The main idea is to create counterfactual for the treated unit by constructing a weighted combination of control units based on pre-intervention characteristics. The weights are chosen so as to minimize the mean squared prediction error between the outcome for the treated unit and the synthetic controls throughout the pre-intervention period. With the weights researchers are able to project the time path of the treated unit had there been no policy change or intervention. The method provides a systematic way to generate a credible counterfactual. It also provides as a straightforward means of statistical inference based on a type of permutation test.

The initiation of Washington Emergency Contraception pilot program provides a natural application of the synthetic control methodology. First, it was the first state to allow the OTC distribution of EC. Data on STDs and induced abortions are available back to 1981 from the Center for Disease Control (CDC) for a large number of states, allowing us to utilize the most post-implementation years and evaluating the long-term effect of OTC access to EC. In addition, Washington State law does not prohibit the provision of EC to minors. In fact, while the majority of teenagers in the U.S. are still required to obtain physician's prescription for the use of drug nowadays under FDA regulation, the innovative pilot project has enabled thousands of teens in

Washington to purchase EC directly from their pharmacists. Note that OTC access to EC may have a greater impact on teens relative to young women since the latter are better able to obtain prescriptions from private physicians for EC prior to or immediately after unprotected sex. The inclusion of teenage groups for OTC access in WA allows us to obtain separate estimate for teenagers and young adults respectively.

Following the statistical inference procedure in synthetic control analysis, we conduct a series of placebo studies to evaluate the causal inference of our estimates. We find no evidence suggesting that nonprescription sale of EC increases the rates STDs among teenage and young women. We also find that easier access to EC does not affect teen abortion rates, but accounts for 6 percent of the decline in abortion among women 20-24 years of age. Our results are in direct contrast to recent manuscripts by Durrance (2010) and Oza (2010) who report large and significant increases in rates of STDs and declines in rates of abortion between 10 and 20 percent associated with OTC access to EC.

II. Conceptual Issues

In an economic framework, the behavioral response of adolescents and young adults follows the prediction of rational decision model. Like the introduction of the contraceptive Pill or legalized abortion, fast and ready access to EC lower the monetary, time, search and psychological cost associated with obtaining the prescription for the pill. The policy change allows couples to engage in sex while lowering the risk of an unintended pregnancy, which in effect reduces the cost of unprotected intercourse and in turn increases the utility of sexual activity relative to abstinence. In other words, the easier pharmacy access raises EC's insurance value against unintended pregnancy.

The rational behavior model provides two testable hypotheses. First, as an example of moral hazard, individuals respond to the decreasing cost and increasing utility by taking fewer precautions to avert pregnancy and engaging in more risky sex behavior. As a well-insured and accessible backup plan, OTC access to EC may induce sexually active teenagers and young adults to substitute EC for condom use. Even assuming no change in contraceptive use, an increase in sexual activity would itself lead to an increased risk of STDs due to contraceptive failure or improper use. A competing theory of teen reproductive behavior argues that teens are spontaneous deciding to have sex. They give little consideration to the costs of unwanted pregnancy (Paton 2006). The alternative assumption does not support the moral hazard hypothesis and it would predict no change in the rate of STDs.

The second testable implication is that improved access to EC leads to a substitution away from abortion. After the act of unprotected sex, abortion and EC are the two options available for preventing an unwanted birth. The higher costs and controversy associated with abortion as a surgical procedure limit its wide accessibility to general populations. If EC and abortion are substitutes, we would expect to observe a decline in abortions at population level when EC is made available over-the-counter. However, the substitution hypothesis is built on the assumption of no change in sexual activity. If OTC access to EC causes more individuals to engage in risky sexual activity and leads to more unwanted pregnancy on the margin, the empirical implication based on aggregate level abortions rates would be unclear.

III. Review of Existing Evidence

Evidence of an association between increased access to EC and increased sexual activity remains inconclusive among Epidemiological studies, some of which use randomized designs report no

association between access to EC and changes in sexual behavior. Three experimental evaluations conducted in the U.S. analyzed the effect of advanced provision or pharmacy access to EC on the frequency of unprotected sex, condom use, use of other hormonal contraceptives, and number of sex partners. (Gold et al. 2004; Raine et al. 2005; Raymond et al. 2006). Two of the evaluations also analyzed changes in STDs (Raine et al. 2005; Raymond et al. 2006). These authors found that increased access to EC increased its use but there was no consistent link between increased EC access and a change in either pregnancy or unprotected sex. The lack of a change in pregnancies given increased use of EC is surprising but likely due to the studies being under-powered to detect changes in low frequency outcomes such as abortion or pregnancy. Sample sizes were relatively small and there was no control group crossover and sample attrition. In addition, the use of convenience samples makes it impossible to generalize the findings to the larger population of women. Gold et al. (2004), for example, evaluated the difference in the uptake of EC among a sample of adolescents attending an urban hospital-based clinic in Pittsburgh, Pennsylvania. While the authors also collected information on self-reported STDs, the available sample provided insufficient power to detect only relatively large (and most likely unrealistic) changes in the incidence of STDs resulting from an uptake of EC. Moreover, the high rate of attrition at the six-month follow-up (over 30 percent) raised the likelihood of bias in the estimates. Raine et al. (2005) recruited young women (ages 16-24) from four clinics in San Francisco and Daly City, California, and randomized them into two treatment groups (advanced provision and pharmacy access) and a control group (clinic access to EC). In addition to evaluating differences in self-reported sexual risk taking, the authors evaluated differences in the incidence of STDs and pregnancies as measured by the use of biomarkers. While the study was designed originally to have suitable power to examine differences in

pregnancy, shortly after the start of sample enrollment California implemented legislation allowing all women pharmacy access to EC. As a result, enrollment in the control group was ended before it reached the desired sample size and the control group sample was contaminated by subsequent access to EC services. In turn, like the Gold et al. study, this study had poor statistical power for examining the link between EC access and sexual risk taking and related outcomes.

Two recent studies have used state and federal changes in access to EC to evaluate its association with STDs and abortion (Durrance 2010, Oza 2010). Oza (2010), for instance, using a large-scale, national claims database found that OTC access to EC accounts for 37.2 percent of the decline in abortion and 17.8 percent of the increase in STD during the three years after FDA approval. The large estimated effects of easier access to EC, however, lack external validity due to several biases. First, the assumption that the nine early adopters serving as appropriate comparison groups for the rest of the U.S. may not be necessarily true. Unobserved factors associated with pharmacy access legislations that vary over time would easily confound the difference-in-difference model estimation. Another potential source of bias lies in the use of claim-based data. Women who seek reproductive health services at clinic or pay out-of-pocket are not covered by the data, a significant portion of these women are teenagers younger than 18 who are systematically different from women observed in the claim data. Moreover, it is hard to draw conclusions in terms of the dynamics of the long-term effect because the data is limited to three-year post-intervention period. Durrance(2010), another study using quasi-experimental design, focuses on Washington State only. Based on the fact that pharmacies across counties joined the pilot program at different time, Durrance(2007) identifies counties with at least one participating pharmacy as “treated” counties. The author finds large and significant effect using

county-level data of STD and abortion rates. The pharmacy access to EC is associated with a 23 percent increase in Chlamydia rates and a decrease in abortion rates of 12 percent and 14 percent among teens and young women. The difference-in-difference approach applied in the study, however, could suffer from several potential biases. First, the estimated effect could be contaminated by the border crossing—for example, a women living in a later participating county could easily drive to a neighboring county to purchase EC. The scenario of border crossing would bias the estimates toward finding no effect. Second, if the participation decisions made by pharmacists individually was driven by the local demand of EC, then the significant negative estimates reported in the study would simply implies negative correlation instead of a causal relationship.

IV. Data and Sample

Gonorrhea

The Centers for Disease Control and Prevention (CDC) maintains a surveillance system on sexually transmitted disease (STD) in all 50 states and D.C. As one of the most commonly reported STD in the US, gonorrhea rate is a good proxy for risky sex behavior. The disease has a very short incubation period, is easily cured, and, other than mother-to-infant infections, is only transmitted by sexual intercourse (Sen 2003). Moreover, gonorrhea is the only STD for which the CDC has consistent data by age-group and by race. The data collection process of gonorrhea is part of national public health notifiable disease reporting system. Each year, CDC receives the records of the incidences of gonorrhea collected and aggregated by state health departments across the country. In this paper, we use the annually released CDC data report about gonorrhea cases by state, 5-year age group and race from 1985 to 2006. The Washington pilot program was

launched in 1998. Thus, we have 13 years of pre-intervention data and more than 8 years in post-intervention period. Our sample period ends in 2006 because the OTC access to EC was nationally approved by FDA in 2006. The age-specific rates for teenage females 15-19 years of age and young female adult 20-24 years of age are calculated per 100,000 of the relevant population.

Recall that the potential candidates for synthetic control unit of Washington are states that meant to be able to reproduce the gonorrhea and abortion rates that would have been observed for Washington in the absence of non-prescription access to EC, we therefore exclude states that adopted some similar programs granting easier access to EC before the FDA approval during our sample period. There are 8 states (Alaska, California, Hawaii, Maine, Massachusetts, New Hampshire, New Mexico, Vermont) introduced similar programs during 1999-2006 post intervention period that are not included in the pool of control units (Trussel 2011). We also exclude D.C. because the gonorrhea rate in D.C. is 120 percent higher than the average gonorrhea rates of the other states. Finally, the group of control units consists of 41 remaining states.

Abortion

We use the CDC's annual abortion surveillance by age, state and year to estimate the effect of easier access to EC on abortion rates. Unlike the Guttmacher Institute's survey of abortion providers, the CDC surveillance does not include all 50 states and national estimates of abortion are approximately 15 percent less than totals reported by Guttmacher. The big advantage of the CDC data is that they are available annual and by age whereas the Guttmacher survey is conducted every 4 years after 1988 and is not stratified by age. We use abortion rates to teens

ages 15-19 and young adults ages 20-24. The abortion rate is defined as abortions per 1000 age-specific women.

To obtain a balanced state-level panel data, we only include states with no missing data throughout the sample period. We also exclude state that report zero abortions in some years since true zeros are extremely unlikely. Moreover, we discard states whose trend of reported number show unusual spikes or troughs. For example, the number of reported abortions in Nevada increased by 77% from 2000 to 2001. The unusual increase was most likely caused by data reporting problem. Finally, the remaining 25 states with complete and consistent data consist of the pool of control units for the following synthetic control analysis.

Population

The denominator of gonorrhea and abortion rates is the total number of female population in relevant age groups. We use population data by state, year, age, gender and race from the Surveillance Epidemiological and End Results (SEER) of the National Cancer Institute (NCI). The SEER population data are available from 1969-2008.

V. Empirical Methodology: Synthetic Control Method for Comparative Case Studies

A major challenge in estimating the effect of state policy or intervention in a single state is choosing an appropriate comparison group. Researchers often use neighboring states or all states with no equivalent policy (Card and Kruger 1993; Evans and Lien 2004; Colman and Joyce forthcoming). The choice of neighboring states can be subjective. If using all possible comparison states, the pre-policy level and trend of the outcome under study may differ substantially between the two groups. The latter can seriously bias the estimated impact of a

policy impact when using a difference-in-difference (DD) estimator. In addition, the DD estimator only controls for time-invariant sources of confounding, which may also be violated with a long pre-intervention period. Finally, estimation of the appropriate standard errors with only one or a few treatment states is an additional challenge as standard approaches to account for correlations within units are based on large-sample asymptotic theory (Donald and Lang 2007; Angrist and Pischke 2009).

The synthetic control methodology addresses each of these issues. The comparison unit is a weighted combination of units from a “donor pool” of possible controls. The weights are obtained by minimizing the distance between pre-intervention determinants of the outcomes in the treated and control units. The weights are then applied to outcomes in control units in post-intervention period to generate the synthetic outcome for the treated unit. The difference in each period between the actual and synthetic outcome in the treated unit is the estimated effect of the intervention. Another virtue of the synthetic control method is its transparency as plots of the pre-intervention difference between the actual and synthetic outcomes is a natural way to present the data. Lastly, the synthetic control method uses repeated placebo tests to estimate the distribution of effects for all control units. This distribution is used to assess whether the effect for the treated unit is large relative to that of the controls, a form of permutation test based on the exact distribution of the controls. This circumvents the problem of using large-sample tests for inference in the case-study context.

VI. Empirical Results

We present the results of synthetic control analysis for two outcome variables over two age groups: gonorrhea and abortion rates for teens 15-19 years of age and , young adult 20-24 years

old. Women 15 to 24 account for XX percent of all cases on gonorrhea in 1990 and XX percent of all abortions.

We use the following variables to predict the pre-intervention rates of gonorrhea and abortion: the percentage of population that is non-white, currently married living in rural area, poverty and college educated;. In addition, we include female labor force participation rate, the state unemployment rate, and real state income per capita. Finally we include alcohol control policies as well as state policies towards abortion. These are the total beer tax (state and federal), and indicator for whether the legal drinking age is above 20, whether the state has a Parental involvement law for abortions to minors, and restricted Medicaid payments for abortion. These variables are averaged over the ten-year pre-intervention period from 1987 to 1998 and augmented by adding three years of lagged value of outcome of interest (1997, 1990, 1985). Appendix A lists the predictors as well as their data sources. In all estimations, we minimize the mean squared prediction error of the pretreatment outcome variables of the synthetic Washington by using nested optimization procedure that searches among all sets of weights for the best fitting convex combination of the control units.

VI.1. Gonorrhea Rates

Figure 2(a) plots the trends in gonorrhea rates of teenage women 15-19 years of age in Washington and the average rates of the 41 control states. It is obvious that the 41 control states altogether may not serve as appropriate comparison groups for Washington to study the effect of easier access to EC on teen gonorrhea rates. During the pre-intervention period, the gonorrhea rates in Washington decrease smoothly while the level of gonorrhea rates in the 41 control states is substantially greater during the pre-intervention period. As discussed above, we construct

synthetic outcomes for Washington based on a combination of control states. With respect to gonorrhea rates, the synthetic Washington is a weighted average of 6 control states (weights in parentheses): Oregon (61%), Nevada (24.4%), Rhode Island (4.8%), West Virginia (4.3%), Michigan (3.8%) and Florida (1.7%). The rest of the states are assigned zero weight. Figure 2(b) displays the affinity between the teen gonorrhea rates for Washington and its synthetic counterpart prior to the 1998 program. Panel (a) of Table 2 in Appendix displays the mean of pre-intervention control variables of actual Washington, synthetic Washington, as well as the population weighted average of all the 41 states. The table highlights the fact that the characteristics of synthetic Washington is substantially closer to actual Washington relative to the average of 41 control states. The synthetic unit closely tracks the trajectory of teen gonorrhea rate and its predictor variables occurred in Washington prior to the pilot program. The root mean square prediction error (RMSPE)³ is 35.74, which corresponds to only 5% of the average teen gonorrhea rate in Washington prior to the program.

The estimated effect of easier access on teen risky sex behavior is the difference between teen gonorrhea rates in Washington and its synthetic version starting from 1998. Although there is noticeable divergence between Washington and its synthetic counterpart just after the initiation of the program (see Figure 3 (a)), the negative effect of easier access to EC on STD prevalence is counterintuitive and the significance of our estimates need to be evaluated. Based on the idea of statistical inference in synthetic control analysis, we perform a series of placebo tests by iteratively reassign the treatment to one of the 41 control states while shifting Washington to the pool of control states. Figure 3 (b) displays the distribution of estimated gaps from the 41 times iterative placebo runs. As the figure graphically shows, the magnitude of estimated negative

³ RMSPE is the square root of the mean squared discrepancies of the outcome variable between WA and its synthetic counterpart during the pre-intervention years.

effect of non-prescription sales of EC is not large relative to the placebo studies. Therefore, the counterintuitive negative effect is not statistically meaningful. There is no significant impact of easier access to on the prevalence of STD among teenager women 15-19 years of age.

For the gonorrhea rates among 20-24 year old women, the counterfactual that resembles the most of Washington with respect to the pre-intervention predictors is a weighted combination of another group of 6 states: Nevada (39.2%), Utah (30.4%), West Virginia (13.5%), Oregon (9.4%), New Jersey (2.7%) and Michigan (4.6%). Again, panel (b) of Table 2 in the Appendix presents the pre-intervention characteristics of actual Washington, synthetic Washington and the population weighted mean of all 41 control states. Panel (a) and (b) of Figure 4 plot the trend of young adult gonorrhea rates of WA versus 41 control states, and WA versus its synthetic counterpart respectively. An eyeballing of the figures reveals much more affinity between Washington and its new synthetic control unit during pre-intervention years. There is discernable divergence between the two lines after 1998. Nonetheless, the significance of seemingly large negative effect of easier access on young female gonorrhea rates (Figure 5(a)) was ruled out by the following statistical examination of 41 iterative placebo tests. As Figure 5(b) makes clear, under a random assignment of the treatment among all 41 control states, the magnitude of estimated effect is not significantly different from the results we obtained through placebo tests. Again, our results suggest that the quantity of estimated effect of easier access on young adults' gonorrhea rate is not large enough to be more than random. Taken together, the lack of any statistically significant relationship between the OTC access to EC and STD prevalence among adolescents and young adults provide little evidence of increasing risky sexual behavior among young women when they gain easier access to EC. The conclusion is in line with the finding of

no evidence of sexual behavioral changes with EC from the previous medical literature (Raymond, et al, 2003; Gold, et al, 2004; Jackson, et al, 2003).

VI.2. Abortion rates

Next, we turn to the examination of the substitutability between EC and abortion services. As discussed earlier, the number of candidate states that serve to reproduce the teen abortion rate in WA before 1998 is small due to data limitation. The control group consists of 25 states with consistent and complete reported abortion rates throughout the sample period.

We first focus on estimating the impact of nonprescription sale of EC on the use of abortion services among teenage women 15-19 years of age. The optimal weights are attributed to Rhode Island (65.8%), Montana (25.7%), Michigan (7.3%), New York (1.2%). Panel (a) and (b) of Figure 6 depicts the teen abortion rate for the actual WA, synthetic WA and the population weighted average of 25 control states. Figure 6 and 7 depicts the trend, the estimated effect and the results from placebo tests. Several points can be made from the graphs presented: First, There are big differences in terms of the trend and magnitude between WA and the 25 control states, suggesting that the 25 state may not provide an appropriate comparison group for WA. Second, the synthetic WA generally tracks the trajectory of teen abortion rate in actual WA with small RMSPE of 1.99 throughout the pre-intervention period. However, the two line start to diverge even before the implementation of the program. Recall that a synthetic unit only serve as meaningful counterfactual if the pre-intervention gaps on the outcome variables is small or close to zero. Finally, since the statistical examination rule out the possibility of any potential effect of OTC access to EC on teen abortion rates, we would not read too much into these findings.

Our finding for the effect of OTC access to EC on young adult abortion rates for women 20-24 years of age, however, suggest a partially significant impact of the WA program. The young adult abortion ratio in WA can be best reproduced by a weighted combination of 5 states: RI (78.3%), PA(8.3%), NE (8.2%), GA(4.6), NJ (0.6%). The application of synthetic control analysis (Figure 8b) dramatically shrinks the wide difference between WA and its comparison groups (Figure 8a) before 1998. Despite the reasonable good fit with RMSPE as low as 1.67 during pre-intervention period, the construction of synthetic unit produces a non-negligible post-intervention gap. Moreover, the estimated effect get more pronounced during the following 4 years and it starts to die out from 2002 (Figure 9a). The average estimated effect can be read as a 6% reduction of the mean pre-intervention average. As usual, we conduct a series of placebo studies and plot all the estimated effects in Figure 9b. The gap estimated for WA stands out (the dark line) because it is large relative to most of the gaps for the control states except Colorado and Minnesota.

Apart from the graphical examination, we follow the idea of Abadie et. al (2010) to use the ratio of post/pre-intervention RMSPE as a tool to evaluate the significance of our results. Recall that the post/pre-intervention RMSPE ratio is a measure of relative affinity between treated unit and its synthetic counterpart after and before the intervention. If the treatment effect estimated is not randomly created due to the lack of fit, we would expect to find relatively large post/pre-intervention ratios of RMSPE for WA as opposed to the other placebo runs. Figure 10 show that WA ranks the 2nd among the 25 states. If one were to assign the pilot program at random to all states in the data, the probability of obtaining a post/pre RMSPE as large as our results is 0.08. The probability is less than the level of 10% in conventional statistical tests, suggesting our estimated effect is partially significant.

VII. Conclusion

The public health consequence and economic impact of the over-the-counter sell of EC have been the focus of a long-running health policy debate. The arguments between advocates and opponents are directed at primarily teenage and young unmarried women whose rates of STDs, abortion and unintended pregnancy are the highest. An unsolved question is whether the improved access to EC, by significantly reducing the risk of unintended pregnancy, would induce teenager and young unmarried women to change their sexual risk-taking behavior in a way that leads to an increase in STD and a substitution away from abortions. The lack of conclusive empirical finding in the literature is primarily due to difficulty for identification. Applying synthetic control analysis on the case study of Washington, the first state granted pharmacy access to EC, we find little evidence to support the hypothesis that OTC provision of EC would increase STD rate among young unmarried women. We do not observe any change of abortion rates among teenage women 15-19 years of age due to the nonprescription sell of EC. For women 20-24 years old, our finding suggests a partial significant substitution effect of easier access to EC on the utilization of abortion services.

References

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. 2010. Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. *Journal of the American Statistical Association* 105: 493-505.

Chesson, H., Harrison, P., & Kassler, W. J. (2000). Sex under the influence: The effect of alcohol policy on sexually transmitted disease rates in the United States. *JL & Econ.*, 43, 215.

Chesson, H., Harrison, P., & Kassler, W. J. (2000). Sex under the influence: The effect of alcohol policy on sexually transmitted disease rates in the United States. *JL & Econ.*, 43, 215.

Durrance C. The Effects of Increased Access to the Morning-After Pill on STD and Abortion Rates. Manuscript. University of North Carolina; 2010.

Finer, L. B., & Henshaw, S. K. (2006). Disparities in rates of unintended pregnancy in the United States, 1994 and 2001. *Perspectives on Sexual and Reproductive Health*, 38(2), 90-96.

Gold, M. A., Wolford, J. E., Smith, K. A., & Parker, A. M. (2004). The effects of advance provision of emergency contraception on adolescent women's sexual and contraceptive behaviors. *Journal of Pediatric and Adolescent Gynecology*, 17(2), 87-96.

Grossman, M., Kaestner, R., & Markowitz, S. (2005). An investigation of the effects of alcohol policies on youth STDs. *Adv Health Econ Health Serv Res*, 16, 229-256.

Hampton, T. (2008). Researchers seek ways to stem STDs. *JAMA: The Journal of the American Medical Association*, 299(16), 1888.

Imbens, G., & Wooldridge, J. (2008). Recent developments in the econometrics of program evaluation: National Bureau of Economic Research Cambridge, Mass., USA.

Jackson, R. A., Schwarz, E. B., Freedman, L., & Darney, P. (2003). Advance supply of emergency contraception: effect on use and usual contraception-a randomized trial. *Obstetrics & Gynecology*, 102(1), 8.

Jones, R. K., & Kooistra, K. (2011). Abortion incidence and access to services in the United States, 2008. *Perspectives on Sexual and Reproductive Health*.

Oza AD. Plan B as insurance: the effect of over-the-counter emergency contraception on pregnancy termination and STIs. Manuscript. The Harris School of Public Policy Studies, University of Chicago, 2009.

Paton, D. (2006). Random behaviour or rational choice? Family planning, teenage pregnancy and sexually transmitted infections. *Sex Education*, 6(3), 281-308.

Raine, T. R., Harper, C. C., Rocca, C. H., Fischer, R., Padian, N., Klausner, J. D., et al. (2005). Direct access to emergency contraception through pharmacies and effect on unintended pregnancy and STIs. *JAMA: The Journal of the American Medical Association*, 293(1), 54.

Raymond, E. G., & Weaver, M. A. (2008). Effect of an emergency contraceptive pill intervention on pregnancy risk behavior. *Contraception*, 77(5), 333-336.

Sen, B. (2003). A preliminary investigation of the effects of restrictions on Medicaid funding for abortions on female STD rates. *Health economics*, 12(6), 453-464.

Trussell, J., & Raymond, E. G. (2010). Emergency contraception: a last chance to prevent unintended pregnancy. *Accessed at <http://ec.princeton.edu/questions/ec-review.pdf>*.

Figure 1: Timeline of OTC Access to EC

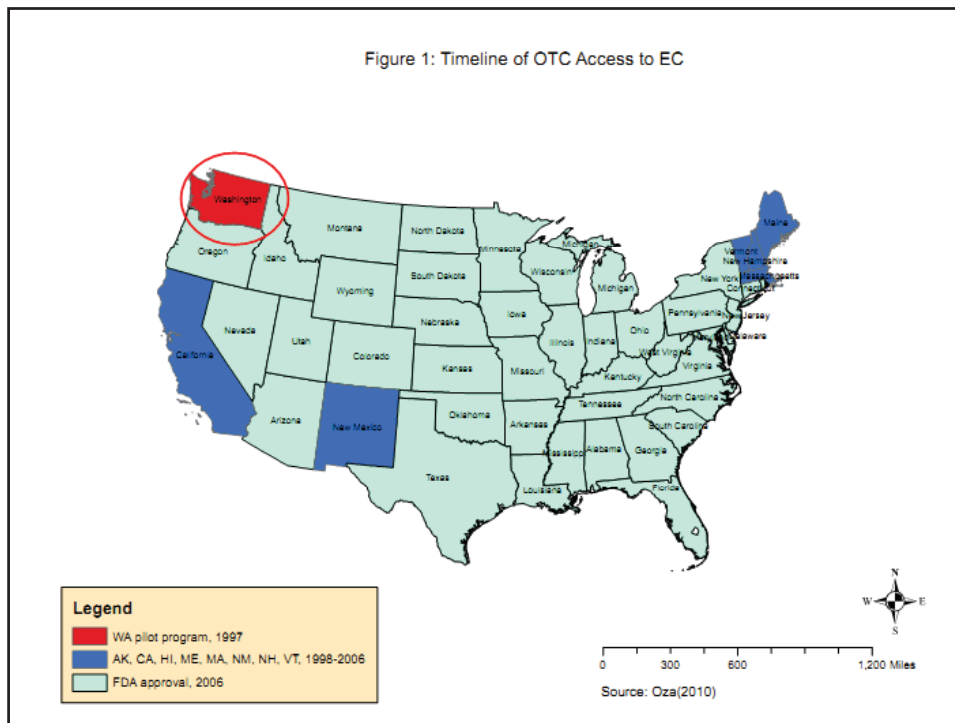
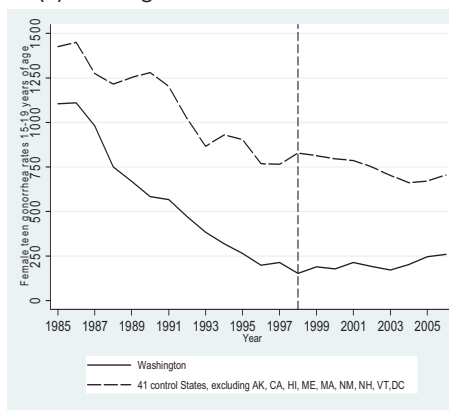


Figure 2: Trends in Gonorrhea Rates of Teen Women 15-19 Years of Age

(a) Washington vs. 41 control states



(b) Washington vs. Synthetic WA

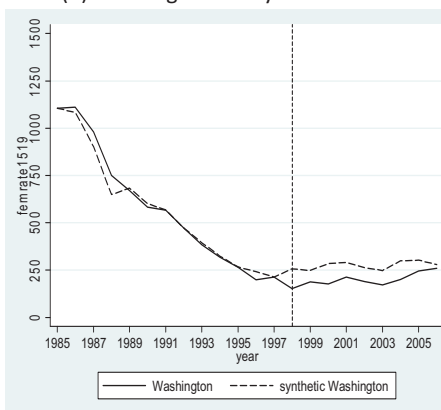


Figure 3: Estimation and Inference of Washington Pilot Program on Gonorrhea Rates of Teen Women 15-19 Years of Age

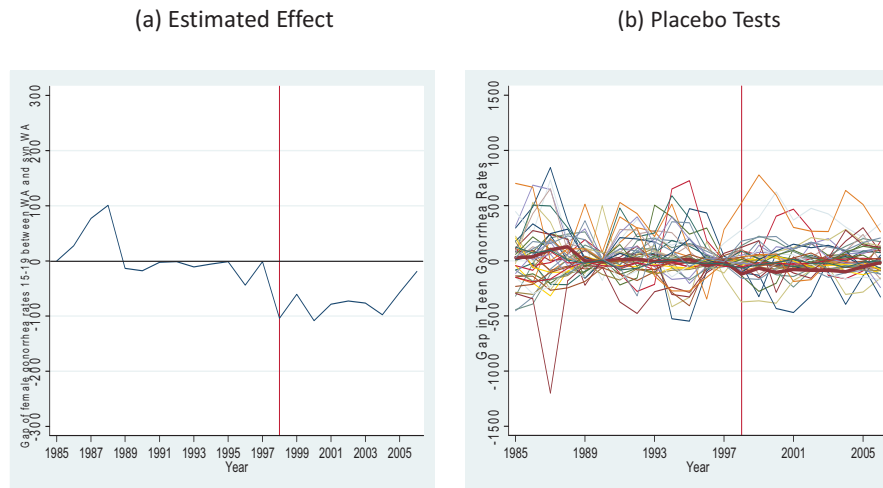


Figure 4: Trends in Gonorrhea Rates of Young Adult Women 20-24 Years of Age

(a) Washington vs. 41 control states

(b) Washington vs. Synthetic WA

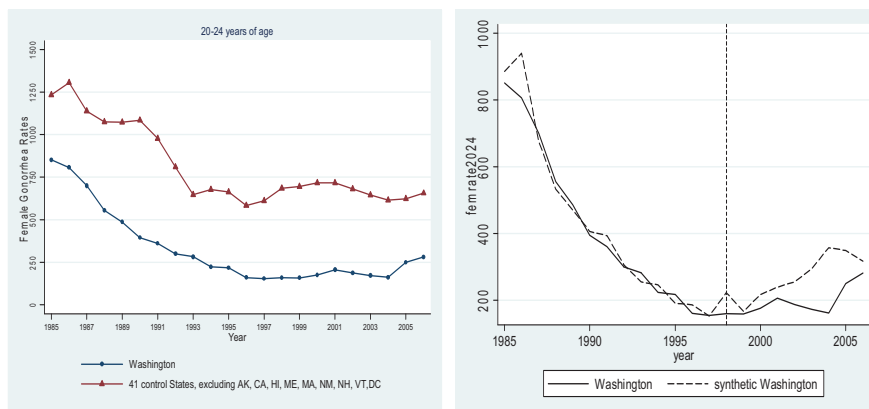


Figure 5: Estimation and Inference of Washington Pilot Program on Gonorrhea Rates of Young Adult Women 20-24 Years of Age

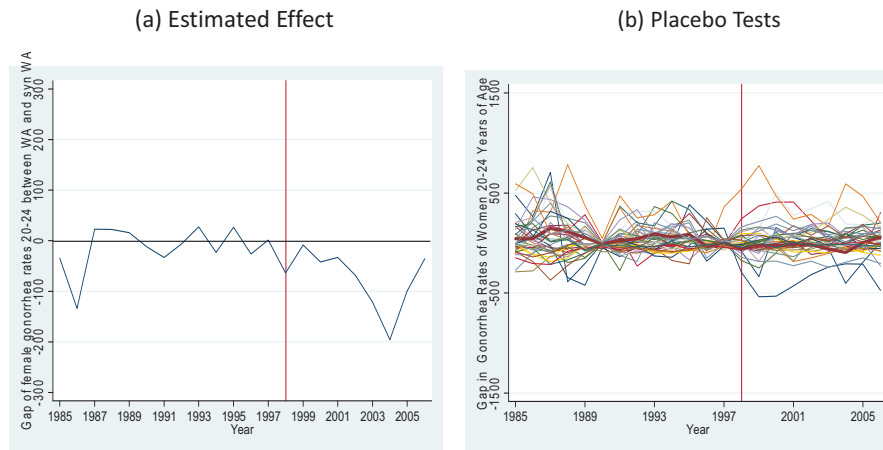


Figure 6: Trends in Abortion Rates of Teen Women 15-19 Years of Age

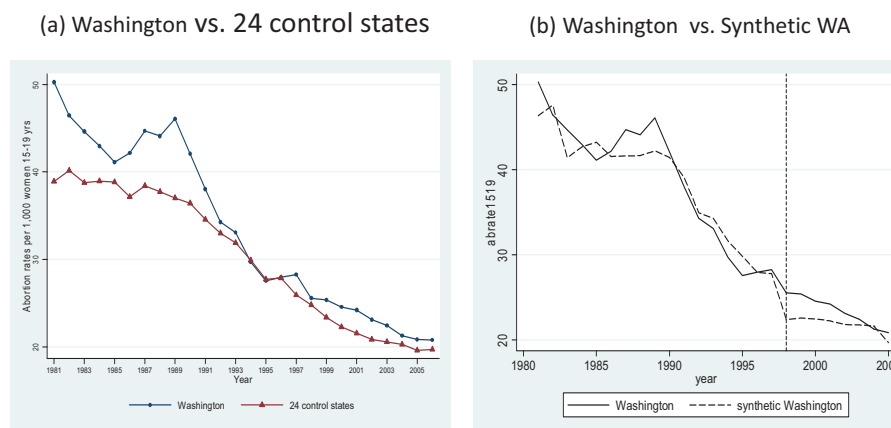


Figure 7: Estimation and Inference of Washington Pilot Program on Abortion Rates of Teen Women 15-19 Years of Age

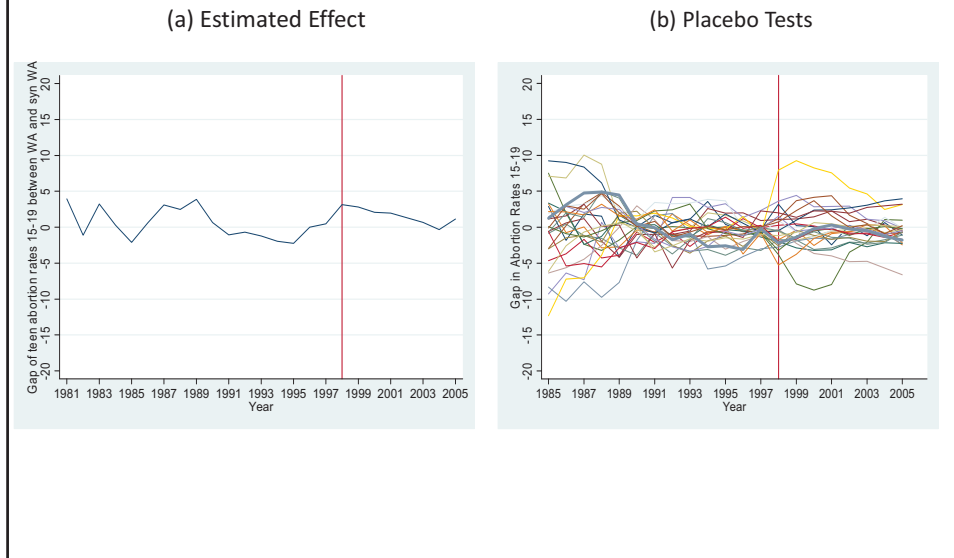


Figure 8: Trends in Abortion Rates of Young Adult Women 20-24 Years of Age

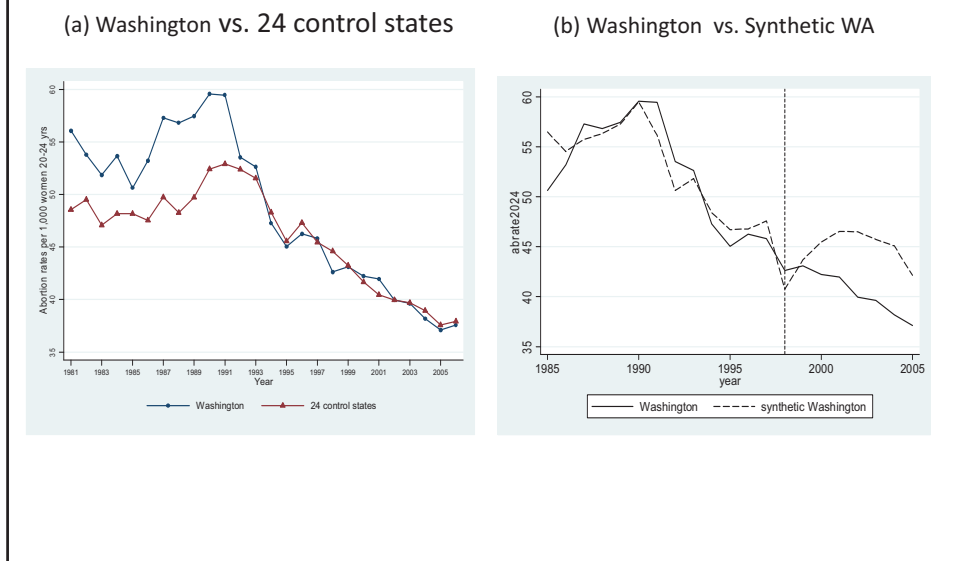


Figure 9: Estimation and Inference of Washington Pilot Program on Abortion Rates of Young Adult Women 20-24 Years of Age

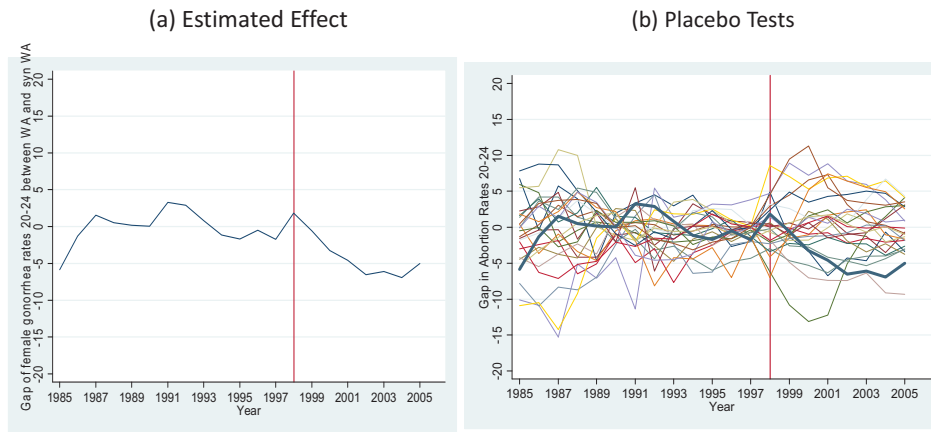
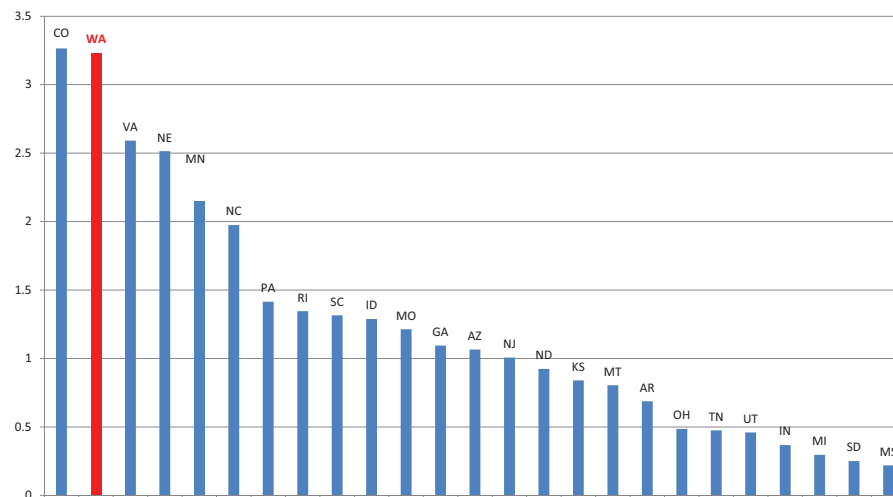


Figure 10: Post/pre-RMSPE Ratio
WA and 24 control States



Appendix

Mean Predictors of Outcome Variable

Panel (a) Gonorrhea Rates						
Control Variables	Ages 15-19			Ages 20-24		
	Washington		Non-OTC States weighted mean	Washington		Non-OTC States weighted mean
	Actual	Synthetic		Actual	Synthetic	
% of population in poverty	1.34	1.44	1.71	1.34	1.34	1.71
real per capita income	14.65	13.88	13.90	14.65	14.44	13.90
federal+state beer tax	0.55	0.50	0.65	0.55	0.54	0.65
legal drinking age 20 and older	1.00	0.99	0.79	1.00	0.91	0.79
female labor force participation	58.11	59.05	56.17	58.11	60.34	56.17
unemployment rate	7.54	7.13	6.67	7.54	5.95	6.67
% of rural population	23.36	23.71	27.27	23.36	23.09	27.27
% of nonwhite population	0.09	0.07	0.16	0.09	0.09	0.16
education attainment	23.25	19.76	19.17	23.25	19.07	19.17
% currently married	0.53	0.53	0.51	0.53	0.55	0.51
lagged outcome 1997	212.33	230.77	765.52	154.03	161.56	606.66
lagged outcome 1990	582.85	598.12	1266.26	394.62	409.49	1065.65
lagged outcome 1985	1105.39	1089.30	1420.36	851.68	852.08	1290.17
Panel (b) Abortion Rates						
	Ages 15-19			Ages 20-24		
	Washington		Non-OTC States weighted mean	Washington		Non-OTC States weighted mean
	Actual	Synthetic		Actual	Synthetic	
% of population in poverty	1.34	1.34	1.57	1.34	1.48	1.57
real per capita income	14.65	14.44	13.56	14.65	14.23	13.56
federal+state beer tax	0.55	0.54	0.66	0.55	0.55	0.66
legal drinking age 20 and older	1.00	0.91	0.81	1.00	0.96	0.81
female labor force participation	58.11	60.34	56.83	58.11	58.17	56.83
unemployment rate	7.54	5.95	6.53	7.54	6.14	6.53
% of rural population	23.36	23.09	31.04	23.36	17.02	31.04
% of nonwhite population	0.09	0.09	0.15	0.09	0.08	0.15
education attainment	23.25	19.07	18.68	23.25	20.27	18.68
% currently married	0.53	0.55	0.52	0.53	0.50	0.52
lagged outcome 1997	28.26	27.79	17.17	45.83	47.57	30.42
lagged outcome 1990	42.06	41.41	26.99	59.59	59.53	36.77
lagged outcome 1985	41.10	43.21	29.03	50.64	56.50	33.78