What Happens the Morning After? The Costs and Benefits of Expanding Access to Emergency Contraception*

Tal Gross† Jeanne Lafortune‡ Corinne Low§

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Abstract

Emergency contraception (EC) can prevent pregnancy after sex, but only if taken within 72 hours of intercourse. Over the past 15 years, access to EC has been expanded at both the state and federal level. This paper studies the impact of those policies. We find that expanded access to EC has had no statistically significant effect on birth or abortion rates. Expansions of access, however, have changed the venue in which the drug is obtained, shifting its provision from hospital emergency departments to pharmacies. We find evidence that this shift may have led to a decrease in reports of sexual assault.

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†Columbia University and NBER, tg2370@columbia.edu

‡Pontificia Universidad Católica de Chile, jlafortune@uc.cl

§Columbia University, csl2137@columbia.edu

1 Introduction

In the 1960s, the introduction of oral contraception had a profound impact on women's fertility, education, and labor market outcomes (Goldin and Katz, 2002; Bailey, 2006; Guldi, 2008; Bailey et al., 2012). Oral contraception, however, requires that women obtain a prescription and consume pills on a daily basis in order to prevent pregnancy. This paper studies the impact of increasing access to a different form of contraception, emergency contraception (EC), more commonly known as the "morning-after pill." EC, unlike oral contraception, is effective when taken within 72 hours following intercourse. While EC cannot be used on a daily basis, it offers women a chance to avert pregnancy after intercourse, when previously their only options would have been either abortion or carrying the pregnancy to term.

Access to EC has changed dramatically in the last 15 years. Early forms of EC were pioneered in the 1970s, but their existence was not widely known.[1] It was not until 1997 that the Food and Drug Administration (FDA) first approved a commercial EC product in the United States, Preven, available by prescription only. In 1999, "Plan B," the most widely known form of EC, was introduced, available only with a prescription. At the time of EC's introduction, researchers and policy-makers alike were optimistic about its potential to prevent unintended pregnancies and abortion (Trussell et al., 2004). A 2002 Guttmacher Institute report estimated that EC had been responsible for a"substantial proportion" of the decline in abortion rates over the last decade, estimating that EC had averted 51,000 abortions in 2000 alone (Jones et al., 2002). This technology was expected to be especially instrumental in preventing pregnancy from sexual assault; Trussell and Stewart (2000) estimate that provision of EC following assault could have prevented 22,000 of the 25,000 pregnancies resulting from reported assaults in 1998.

[1] Ellertson (1996) summarizes the early history of EC. Initially, EC was simply an off-label use of traditional oral contraceptives and intra-uterine devices.

To be effective, EC must be taken soon after intercourse. Because of this, various policies have been put in place since 1997 to increase access to EC. Between 1997 and 2007, nine states allowed pharmacists to directly dispense EC without a prescription and regardless of the patient's age.[2] We call such laws "pharmacy-access laws." Furthermore, 16 states, plus the District of Columbia, mandated that hospitals inform victims of sexual assault about EC.[3] We call such laws "ED-access laws." Finally, in 2006, the FDA allowed EC to be sold in pharmacies without a prescription to all women over the age of 18.[4]

This paper studies these expansions of access to EC. We study how such policies affected fertility and abortion rates. We also explore how the expansion of access to EC changed the venue in which women procure the medication, and the potential consequences of such a change.

Despite the convictions of many policy makers, the theoretical impact of EC on fertility and abortions is not obvious. We first present a simple model that explains the conditions under which easier access to EC will lower natality and abortions. The effect of EC is ambiguous, because easier access to EC, which mitigates a risk of sexual activity, may change women's behavior.

This paper then explores the impact that each of these policies has had on fertility-related outcomes. We first estimate the impact of state policies before the 2006 FDA policy change. We then estimate the impact of the FDA policy change by comparing outcomes in states that previously had EC-related legislation to those that did not. We find that pharmacy-access laws and ED-access laws had little effect on birth or abortion rates. The estimates, for instance, rule out decreases in overall fertility larger than 2

[2] Most of these states required that the pharmacist enter a collaborative practice agreement with a physician, the others simply established a protocol. In Appendix Table 4, we distinguish between the two types of laws as a robustness check and find similar effects for each type of laws.

[3] The majority of these laws mandate that the hospital itself provide the medication. Two states, South Carolina and Ohio, passed EC-related legislation for assault victims, but failed to enforce it. We ignore this legislation in our empirical specifications, following the classification of the Guttmacher Insitute.

[4] In 2009, this availability was extended to all women older than age 17. Our data do not allow us to study this, more recent, policy change.

percent.[5] We find similar results even amongst sub-populations that are less likely to use regular contraceptives. Nonetheless, we demonstrate that sales of EC rose dramatically during this time period. This suggests that women who purchased EC following the policy change may have faced a small risk of pregnancy beforehand or that a behavioral response counteracted the increase in contraceptive power.

Our results stand in stark contrast to research on other forms of contraception. Bailey (2010) finds that greater availability of the contraceptive pill decreased marital fertility, while Kearney and Levine (2009) demonstrate that the price of oral contraceptives affects the teenage birth rate.[6] Similarly, policies that have expanded access to abortion have had a significant impact on fertility and the composition of births (Ananat et al., 2009, 2007; Donohue and Levitt, 2001; Angrist and Evans, 1996; Levine et al., 1999; Gruber et al., 1999; Levine et al., 1996), while policies that mandate waiting periods for abortion may have decreased the abortion rate (Joyce and Kaestner, 2000).

Our results confirm those presented by Durrance (2013). While her work focuses on a single state, Washington, our results indicate that the absence of a significant impact on births is not particular to the first state to adopt these laws. These results contrast with those of two other studies (Oza, 2009; Zuppann, 2010) that focus on increased access to EC in the general US population. Oza (2009) studies the change in outcomes after the FDA policy change. She relies on a database of private insurance claims and finds that the FDA policy change decreased the number of abortions. Zuppann (2010) studies how pharmacy-access laws affected birth rates before the FDA policy change. He finds that the state laws led to large decreases in birth rates.[7]

Our results suggest that the findings from small-scale, randomized-controlled medical

[5] The 95-percent confidence intervals on our main estimates rule out decreases larger than 2 percent for *overall* fertility. The confidence intervals are wider for different age groups.

[6] In addition, there exists some evidence that oral contraceptives changed the *composition* of births (Ananat and Hungerman, 2011).

[7] To our knowledge, no other study has estimated the impact of ED-access laws so far.

trials of no effect of EC on fertility extend to the overall female population (Raymond et al., 2006; Raine et al., 2005). Raymond et al. (2007) review 23 studies of EC and conclude that randomized access to EC has not been shown to decrease unintended pregnancies. One study that does find effects of fewer unintended pregnancies among the group given a supply of EC to keep at home included only a small sample of subjects, selected because they had all previously used EC or had an abortion (Glasier and Baird, 1998). Glasier et al. (2004) find that the provision of emergency contraception also does not change abortion rates.[8]

Having found little evidence that easier access to EC has changed fertility-related outcomes, we then measure whether EC-related laws affect the venue in which women acquire the pill. To do so, we use a near census of ED visits for selected states. We find that the FDA ruling led to a large decrease in ED visits related to EC. This suggests that expansions of access to EC have affected the venue in which women acquire the medication and thereby lowered the total cost of distributing EC. We also find that, in the absence of pharmacy access laws, ED-access laws increase EC-related visits, indicating that guaranteed access to EC may play a role in determining whether women seek emergency medical care.

While cost-saving, the shift to over-the-counter provision of EC may have led to unintended consequences. Sexual assaults may be one reason women seek EC at the ED. Hospital staff, unlike pharmacists, provide other services beyond EC provision, and such services may not be utilized if EC is accessed over-the-counter. In particular, we find suggestive evidence that expansions of access to EC led to a decrease in the number of sexual

[8]A related question is whether access to EC may encourage risky behavior. Previous research has found little evidence for this. Raine et al. (2000) find that women given an at-home supply of emergency contraception shifted to less effective methods of contraception. This result, however, has not been found by other, similarly conducted studies (Jackson et al., 2003). Gold et al. (2004) find no effect of EC on the use of other contraceptives. Meanwhile, Belzer et al. (2005) suggest that teenagers who are given advanced provision of EC are more likely to have unprotected sex, but the methodology involved has been criticized (Trussell et al., 2006).

assaults reported to law enforcement. Such results must be interpreted cautiously; we rely on only one source of data on sexual assaults and find an impact only of pharmacy-access laws before the FDA ruling. Still, this finding is consistent with the fact that assault victims are likely to encounter less encouragement and opportunity to report the crime at a pharmacy than at an ED.

The paper proceeds as follows. Section 2 presents a theoretical framework that clarifies how access to EC ought to affect outcomes. Section 3 describes the data at our disposal and our empirical strategy. Section 4 then presents our empirical results; how access to EC affects births, abortions, ED visits, and reports of sexual assault. Section 5 concludes.

2 Theoretical Framework

This section explains how EC can affect fertility-related outcomes. EC is unlike traditional contraception in that it is intended for use after intercourse. Unlike abortion, however, EC must be taken before one knows whether intercourse has resulted in pregnancy. In this sense, EC lies between traditional contraception and abortion in a woman's decision tree. This section studies that decision tree with a simple model, inspired by the work of Levine and Staiger (2002) and Kane and Staiger (1996).

The model predicts how EC will affect the number of sexual encounters, pregnancies, abortions, and births. In general, the model suggests that the effect of EC on these outcomes is surprisingly ambiguous. The ambiguity stems from how EC can change exposure to pregnancy risk. Suppose, for instance, that women react to the introduction of EC by having more sexual encounters. That reaction alone increases the number of births, while the use of EC decreases the number of births. The net effect of EC on births and other outcomes is thus ambiguous. Below, we present this intuition more formally. Note that this is a similar result to that obtained by Myers (2012).

2.1 Structure of the Model

Suppose that women face a utility gain from intercourse, $S \in \left(0, \bar{S}\right)$, and a utility gain from having a child, $B \in \left[\underline{B}, \bar{B}\right]$. If $B > 0$, then a pregnancy is wanted, and if $B < 0$, then a pregnancy is unwanted. These variables are randomly distributed in the population based on a density function $g\left(s, b\right)$. Abortion is available once a woman is pregnant at a utility cost, A.[9]

Once pregnant, women compare the benefits of carrying the pregnancy to term to the cost of obtaining an abortion. They will choose abortion if $B < -A$. Thus, if a pregnancy occurs, a woman will receive a utility of $P \equiv \max\left\{B, -A\right\}$.

Suppose that, initially, the probability that a sexual encounter leads to pregnancy is q. Therefore, a woman will consent to sex if $S + q \cdot P > 0$. The share of women who have sex is:

$$\begin{aligned} \gamma\left(q\right) \equiv & \int_{0}^{\bar{B}} \int_{0}^{\bar{S}} g(S, B) dS dB \\ & + \int_{\underline{B}}^{-A} \int_{q \cdot A}^{\bar{S}} g(S, B) dS dB + \int_{-A}^{0} \int_{-q \cdot B}^{\bar{S}} g(S, B) dS dB. \end{aligned} \tag{1}$$

That is, the share of women who have sex is composed of those who want a baby (the first term in equation 1) and those who do not. The latter are composed of two groups: those who will pursue an abortion if pregnant (the second term in equation 1) and those who will not (the third term in equation 1). A woman can only become pregnant if she consents to sex, and thus the share of women who become pregnant is $\gamma(q) \cdot q$.

This framework can be re-interpreted to focus not on the decision to have sex, but on the decision regarding the *frequency* of sexual activity. One can consider the key parameters (S, B, and A) as reflecting not the overall population, but rather the distribution

[9]These costs and benefits reflect not only the financial cost of abortion or pregnancy, but also stigma, opportunity cost, and psychic costs.

faced by one woman each time she must decide whether or not to have sexual intercourse. The model would then lead to the number of sexual encounters a given woman has instead of the number of sexually active or inactive females in the population.

2.2 The Effect of Emergency Contraception

Suppose that EC is introduced, and that it lowers the probability of pregnancy from q to q' at a cost of c. The parameter q' reflects not just the effectiveness of the technology, but also the probability that a woman obtains EC and uses it correctly.

After sexual intercourse, a woman must compare the benefits of taking EC with its cost. She will prefer taking EC if $S + q \cdot P < S + q' \cdot P - c$.[10] Under these assumptions, EC will unambiguously raise the share of women who have sex, since some women with a previously negative total payoff to sex now face a positive payoff.[11]

The share of women who become pregnant, however, may rise or fall after the introduction of EC. Wanted pregnancies are unaffected by EC, because women for whom a baby is welfare-enhancing will not consume EC. Unwanted pregnancies, however, may increase or decrease. That ambiguity stems from two forces. On the one hand, the availability of EC leads more women to have sexual intercourse. On the other hand, pregnancy is now less likely to occur. The appendix demonstrates that unwanted pregnancies will decrease if the ability of EC to reduce pregnancies is large relative to the induced behavioral change.

The share of women who have an abortion may also rise or fall. There are two sources of uncertainty that cause individuals to use abortion (Levine and Staiger, 2002). First, some women decide to have sex based on a sufficiently high expected value of B, but are uncertain of the true value of B until a pregnancy occurs. Such women are unlikely to use EC to replace abortion, because they do not gain additional information about B from waiting until after intercourse. The second source of uncertainty is the stochastic nature

[10] We assume that EC is less costly than abortion, and thus $-A < \frac{c}{q'-q}$.

[11] The appendix presents a formal proof of both this prediction and the following predictions.

of pregnancy itself. Abortion is a cost that is only realized if pregnancy occurs, whereas traditional contraception and EC must be used before knowing whether pregnancy will occur. No additional information is gained by waiting until intercourse occurs, but EC may act as insurance against pregnancy. The net effect of EC on abortion is ambiguous, and for the same reason that the effect of EC on pregnancies is ambiguous. On the one hand, EC induces more women to choose sexual intercourse. On the other hand, the probability that these women need an abortion falls, because they consume EC. Finally, combining the ambiguous effect of EC on pregnancies and abortions yields the prediction that EC may raise or lower the number of births.

The ambiguity of EC on these outcomes depends on the magnitude of q' relative to q. If, for instance, EC were to reduce the probability of pregnancy from q to *zero*, then EC would unambiguously reduce the number of pregnancies, abortions, and births. EC, however, only reduces the probability of pregnancy by 75–95 percent (Trussell and Raymond, 2012). Over one year, a sexually active woman who uses EC as her only form of birth control faces a 20–40 percent risk of pregnancy. Consequently, we cannot rule out that the effect of EC on these outcomes is theoretically ambiguous.[12]

In this way, the model describes the introduction of EC and not expansions of access to EC. The model, however, leads to nearly identical predictions in either case. We clarify the difference between the introduction of EC and expansions of access in the theoretical appendix.

2.3 Emergency Contraception versus Traditional Contraception

This model does not explicitly capture the choice between traditional contraception and EC. Formally incorporating traditional contraception into the model would complicate the

[12]Note that the introduction of EC unambiguously increases the welfare of women in this framework.

derivations, but would not provide additional insights.[13] Instead, we discuss traditional contraception informally as follows.

Traditional contraception must be purchased before sexual intercourse. Women who are certain of the benefits of sex, S, will purchase traditional contraception rather than EC.[14] For such women, EC provides no additional benefit. There also exist, however, women for whom the benefits of sex, S, are uncertain. For such women, EC offers an advantage over traditional contraception. When the benefits of sex, S, are uncertain but expected to be low, women may not wish to purchase traditional contraception. If S is revealed to be very large, then such women can purchase EC after intercourse. Uncertainty in S, or rather, uncertainty over future sexual encounters, thus drives demand for EC.

A second reason women may choose EC involves stochastic shocks to q. For example, if a condom breaks during intercourse, then the probability of pregnancy is suddenly higher than it had been before. The woman may then consume EC, as a result. In this way, EC can be used once additional information about S and q is revealed.[15]

Without such uncertainty, very few economic agents would consume EC. First, EC is relatively expensive. Second, EC cannot be used frequently, and provides little additional benefit if a primary method of contraception is already used properly. In this sense, the availability of EC will only affect women who face greater uncertainty over future sexual encounters. Women who face such uncertainty are more likely to be young, poorer and unmarried.[16] For that reason, we stratify some of the empirical results below on age, race,

[13]Women would have to choose between traditional contraception and EC based on which one provided the highest protection at the lowest cost. If EC were relatively more expensive, no woman would use it and an expansion of access to EC would have no effect on outcomes. If, on the other hand, EC were relatively inexpensive, then some women would consume EC. Only in this case would EC affect outcomes.

[14]EC is comparable in cost to one month of oral contraceptives.

[15]Abortion is available once q has been realized and potentially, once additional information regarding B has been revealed.

[16]The National Campaign to Prevent Teen and Unplanned Pregnancy reports that 66 percent of pregnancies are unplanned for women less than 25 years old, versus 34 percent for women 25 and older; 60 percent of pregnancies are unplanned for women with income less than 200 percent of the poverty line, versus 38 percent for women at 200 percent or over; and 72 percent of pregnancies are unplanned for unmarried women, versus 26 percent for married women.

and marital status.

A final instance of high encounter-specific uncertainty is the case of sexual assault, where EC plays a materially different role than traditional oral contraceptives.

2.4 Victims of Sexual Assault

In the context of the model, victims of sexual assault are women for whom $S + q \cdot P$ is negative, and yet they are forced to have sex.[17] For such women, EC does not induce a behavioral response; the rate of sexual assaults is likely unaffected by the availability of EC. Furthermore, given the low probability of assault, women are unlikely to use traditional contraceptives specifically to insure against pregnancy resulting from assault. Assault victims may therefore face a high q, and correspondingly a greater gain to the introduction of EC. Thus, in contrast to the ambiguous results above, EC has an unambiguous effect on victims of sexual assault. For victims of sexual assault, the model predicts that the availability of EC reduces the number of births and abortions. Victims of assault, however, compose a small share of the total population. It is thus difficult to estimate the effect of EC on outcomes for that population alone. We are aware of no studies that assess the effect of contraception access on victims of sexual assault, and believe that this represents a fruitful avenue for future research.

We focus on testing empirically how the availability of EC affects the *reporting* of sexual assaults. When a sexual assault is reported to law enforcement, the victim provides a public good while bearing a private cost. She provides authorities with the identity of the perpetrator and thus lowers the probability that the perpetrator commits another assault.[18] As in the case of other public goods, reporting of assaults may be under-

[17]The model could be changed to incorporate the possibility of sexual assault algebraically. In that case, a share of women would become the victims of sexual assault. Those women would then all find it optimal to consume EC. Their existence would then strengthen the direct effect of EC on births and abortions, and weakens the average behavioral response.

[18]Although victims of sexual assault may be male or female, in this context we are concerned with female victims, since they have the potential to become pregnant and therefore be affected by access to

provided.[19] We present below suggestive evidence that access to EC in pharmacies reduces the share of assaults that are reported to law enforcement. This suggests that when the private benefit to reporting is diminished, fewer assaults are reported. In that sense, wider access to EC decreases the private costs borne by victims, but may also decrease the provision of a public good. We discuss this implication further below.

3 Data and Empirical Strategy

We measure the effect of EC with a simple, difference-in-difference framework. Specifically, we first evaluate the state laws that expanded access, and then test how states that passed such laws were differentially affected by the FDA policy change.

Table 1 presents the dates when EC-related laws were passed.[20] Between 1997 and 2006, 9 states passed pharmacy-access laws and 10 states passed ED-access laws. An additional 6 states, and the District of Columbia, passed ED-access laws after the FDA policy change made EC available at pharmacies.[21] The states that passed EC-related legislation may be systematically different from states that did not. Still, all of our empirical results control for fixed differences between the states. Moreover, we test for time-varying differences between states by including linear time trends in our regressions and by exploring event-study specifications.[22]

The FDA policy change was announced on August 24, 2006. However, unlike the state laws, the FDA policy required suppliers to produce a new, over-the-counter version of EC. Suppliers shipped that version in November of 2006, roughly three months after the

contraception.

[19] After an assault, the victim may also be tested for sexually-transmitted infections. This may involve an additional positive externality.

[20] For the results below, we adjust the relevant date that each law was passed based on the outcome. For visits to the ED, we use the actual date. For abortions, we add 60 days to the law's passage, to account for the average gestation at abortion. For births, we add 266 days.

[21] Note that both types of state laws did not restrict the age at which women could obtain EC, whereas the FDA ruling made EC available only to women older than 18.

[22] A table without state-specific time trends is included in the appendix.

FDA policy change. We thus consider the effective date of the FDA policy change to be November of 2006.

For the state laws, we estimate a regression of the form:

$$y_{st} = \beta \cdot I\left\{\text{EC State Law}_{st}\right\} + \gamma \cdot X_{st} + \alpha_s + \alpha_t + \alpha_s \cdot t + \varepsilon_{st},$$

where y_{st} is an outcome in state s at time t and $I\left\{\text{EC State Law}_{st}\right\}$ is equal to 1 when the state has such a law in place.[23] The regression allows each outcome to evolve along a separate linear time-trend and to differ permanently by state. We also include a variety of time-varying controls in each regression: the state unemployment rate, its poverty rate, welfare benefits for a family of four, the AFDC/TANF benefit level, and the availability of subsidized contraception through Medicare as compiled by Kearney and Levine (2009).[24] We adjust the standard errors to allow for auto-correlation between observations from the same state.[25] This framework requires one key assumption: that, in the absence of the policy changes, the path of the outcomes in each state would have differed only by a linear trend. We test the validity of that assumption below using more flexible event-study specifications.

To evaluate the 2006 FDA policy change, we estimate a similar regression in which states that had already passed a pharmacy-access law are the control group. We only use the latest period of the data (2004–2008) for that estimation since many of the control states changed their laws during the previous period.[26] Although the FDA policy was not

[23]For annual data, we code a law as having been implemented if the law (or its expected consequence) was in place for more than 183 days of the year. For monthly data, we require that the law or its consequences be in place for more than 14 days of the month.

[24]The information on welfare comes from the University of Kentucky Poverty Research Center database (2011) and from Bitler et al. (2006).

[25]For these regressions, we restrict our sample to the years before the FDA policy change, since we expect that states with such laws in place would be affected very differently by the FDA ruling. Specifically, for outcomes in which we expect an immediate change in behavior, we only look at years before (and including) 2005, while for births, where we expect the outcome to be delayed by a year, we only restrict our sample to years before (and including) 2006.

[26]In such regressions, we exclude any state that changed its policies such that it would have affected

a substitute for ED-access laws, it may have obviated such laws; thus, we also compare the impact of the FDA policy on states that previously had ED-access laws in place.

We have compiled outcomes from a variety of sources. We observe the number of births per month in each state from a census of births collected by the National Vital Statistics System.[27] For abortion rates, we rely on state-by-year estimates of the number of abortions calculated by the Centers for Disease Control and Prevention.[28]. We have also compiled data on sexual assaults reported to authorities via the Federal Bureau of Investigation (FBI) Uniform Crime Report.[29]

Finally, we have compiled a large database of ED visits by month and year based on data from the Healthcare Cost and Utilization Project (HCUP). Our sample includes a near census of ED visits from Arizona (2005–2008), California (2005–2008), Iowa (2004–2007), New Jersey (2005-2008), and Wisconsin (2004–2008).[30] Of these five states, New Jersey, Wisconsin, and California passed ED-access laws.[31] We construct aggregate counts of all ED visits by month for these states, and isolate ED visits in which the patient received EC or in which the patient was listed as a sexual assault victim.[32]

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the outcomes of interest in 2006.

[27] We stratify births by the age of the mother and set the number of births equal to 0.5 when the cell does not record any births. We have also stratified births based on the marital status and race of the mother. The results are extremely similar to the ones presented below.

[28] These abortion data rely on states themselves reporting the relevant statistics, unlike the survey data compiled by the Alan Guttmacher Institute. The latter, however, are not available on an annual basis by state. We have data on 48 states in our sample.

[29] The crime data exist at the state-year level. Some states make available monthly crime data, but too little such data exist to precisely estimate the regression above at the monthly level.

[30] The administrative data cover all hospitals regulated by the state. Thus, for instance, we do not observe ED visits at Veteran Administration hospitals. Such visits are likely a very small share of all visits related to EC.

[31] California also passed a pharmacy-access law. Both California laws were implemented before the HCUP sample period, which prevents us from measuring their impact.

[32] EC-related visits have International Classification of Diseases 9$^{\text{th}}$ Revision (ICD-9) code "V2503," and assault-related visits have ICD-9 code "V715."

4 Results

This section presents our empirical results. We first discuss the effect of access to EC on births and abortions, outcomes on which most of the public debate and previous literature has focused. As our model indicates, however, the effect of EC on such outcomes is theoretically ambiguous. We then describe how access to EC affects visits to hospitals and reports of sexual assault, outcomes for which the model suggests we are more likely to observe an impact.

4.1 Births and Abortions

Table 2 presents a series of difference-in-difference estimates that test for the effect of access to EC on monthly natality using as our dependent variable state-by-month log number of births. The regressions include state-specific linear time trends and time-varying control variables.[33] We focus on four different measures of natality: total births, total births for women under the age of 18, total births for women aged 18–30, and total births for women older than 30. The first panel restricts the sample to 1995–2006, before the FDA policy change. The second panel presents estimates based on 2004–2008, in which states with pharmacy-access laws compose the control group for the FDA policy change. The table presents regressions that include controls for both types of state laws simultaneously. We obtained similar results when evaluating each law separately.[34]

Panel A of Table 2 suggests little relationship between natality and ED-access laws; all point estimates are extremely small, although the confidence intervals only rule out decreases in overall natality of roughly 2 percent.[35] Additionally, Panel A presents no

[33]As described above, these time-varying control variables include the state unemployment rate, its poverty rate, welfare benefits for a family of four, the AFDC/TANF benefit level, and the availability of subsidized contraception through Medicare as compiled by Kearney and Levine (2009).

[34]In results available from the authors upon request, we examine the effect of each law separately.

[35]The regressions in Table 2 are demanding of the data; they include many controls. We find, however, qualitatively similar estimates when we exclude state-specific linear time trends and state-specific time-varying control variables.

evidence that pharmacy-access laws lowered births in each state. In contrast, the results suggest a 2.2 percent *increase* in births after states pass a pharmacy-access law (for women aged 18–30). That increase in births is surprisingly statistically significant. To test whether this increase is a true effect of the legislation, we run event-study regressions, which estimate the effect of pharmacy-access laws in each year before and after their passage. Figure 1 presents the results of that regression. The figure suggests that pharmacy-access laws did not have a discontinuous effect on natality. Natality in states that passed pharmacy-access laws was on an increasing (non-linear) trend before passage of the laws. This suggests that the results in panel A of Table 2 are misleading.

Furthermore, if the results of panel A were taken at face value, they would imply an unusual response to pharmacy-access laws. As described by our model, access to EC will increase the birth rate when the behavioral response to the drug is substantially larger than the birth-prevention effect of the drug. The point estimates seem unusually large given that less than 5 percent of women say that they have used EC in the last year. Those women who have used EC used it less than twice on average (Zuppann, 2010). Any increase in births would need to be driven by women changing their behavior based on availability of the drug, but becoming pregnant nonetheless.

Finally, the bottom panel of Table 2 suggests little effect of the FDA policy change on natality. For 18–30 year-olds, states with no pharmacy-access law experienced a statistically insignificant 1.8 percent decrease in natality after EC was available over-the-counter. Such estimates can reject a negative impact of the FDA policy change on births larger than 4 percent.

In all regressions, the outcome of interest is the logarithm of the number of births by month. We do not rely on annual birth rates as the outcome of interest, because precise estimates of population are only available with each decennial census, and less precise population estimates may introduce substantial measurement error. Appendix Table 1, however, provides estimates using annual log births (in the first four columns) and using weighted annual birth rates (in the last four). Our results are not materially different when using this alternative specification. We are unable to find a significant negative effect of EC-related legislation in either approach. The standard errors and magnitudes are fairly similar and allow us to rule out effects larger than 2–4 percent or about 2 births per 1,000 (from an average of 52). Appendix Table 3, however, shows that using the annual birth rate specification and dropping linear time trends, we replicate Zuppan's (2010) results and find a significant and negative impact of Pharmacy-Access Laws on fertility. However, in Panel B, we find that the introduction of FDA legislation led to a significant increase in fertility, something that may highlight the spurious nature of these results.

Potentially, we may not estimate a statistically significant effect here because the utilization and impact of EC is too low in the general population. We have conducted the analysis for many sub-populations (race, marital status, number of previous pregnancies, and so on) and have still found no negative impact. For instance, Table 3 presents estimates of our preferred specification solely for a number of sub-groups.[36] We focus progressively on a population that is least likely to be using traditional contraceptives, and thus at highest risk of unintended pregnancy without access to EC. Table 3 presents no evidence that natality fell for that population after the policies of interest.

Table 4 presents a similar set of estimates to Table 2, but with abortions by state and year as the outcome of interest. As a whole, the table suggests no negative effect of EC-related laws on abortion. No negative point estimates are statistically significant at conventional levels, but the confidence intervals are wide. For instance, the results only rule out a reduction in abortions among women aged 20–29 of more than 12 percent after ED-access laws. The estimated impact of pharmacy-access laws is positive for most age

[36]Information on educational attainment exists only for certain states and certain years, thus the number of observations in the last column of Table 3.

groups, but statistically insignificant. The results do not reject reductions of less than 7 percent.

In summary, neither Table 2 nor Table 4 present significant evidence that EC-related legislation affected births or abortions.[37] This confirms the results of medical studies, but care should be taken in interpreting these results.

First, one may wonder whether the legislation had little effect because it did not actually increase the consumption of EC. (This would be akin to a weak first stage in an instrumental variable setting.) We investigate this possibility with sales records from Barr Pharmaceuticals, the primary producer of EC. Unfortunately, the sales data are not available at the state-year level. Nevertheless, we directly observe the impact of the FDA policy change on national sales of EC. Figure 2 plots the total sales of Plan B from 2006 through 2009. The figure demonstrates the rapid decline of prescription sales for EC following the August, 2006 FDA policy change. The policy change also led to a rapid increase in over-the-counter sales, as Barr Pharmaceuticals released the newly packaged product for over-the-counter sale. From 2006 to 2007, Plan B sales more than doubled, increasing by $47 million, with unit sales going from approximately 16,000 sales per week to over 34,000 in 2007. Sales continued a steady climb, doubling again between 2007 and 2009. This large increase in Plan B sales is evidence of the direct effect of the FDA policy change. We are thus skeptical that the lack of a pattern in Table 2 and Table 4 is driven by lack of variation in sales of the pill. Furthermore Kavanaugh et al. (2011) argue that pharmacy access of EC is responsible for doubling the number of women who have ever used EC from 4 percent in 2002 to nearly 10 percent in 2008.

A second possibility is that we may simply be unable to detect significant effects of the

[37]We also explored whether birth and abortion rates of 18- versus 17-year-olds changed differently around the FDA ruling, given the differential treatment of ages under the policy. To do so, we used natality and abortion records from the state of Texas. We found no evidence of such a pattern. Moreover, we used the natality records to explore whether there was any change in women's attributes around the FDA ruling and were unable to obtain any significant results as well.

EC-related legislation because of the way EC operates. EC only prevents pregnancy from a single sexual encounter, so it eliminates a risk of pregnancy of only 3–5 percent, the risk of pregnancy from a single, unprotected sexual encounter. If, however, women use EC because they believe themselves to be at a greater risk of pregnancy, then women taking EC may face a 10 percent pregnancy risk, the approximate peak at pregnancy during the menstrual cycle (Wilcox et al., 2001).[38] In that case, if EC lowers this pregnancy risk by 75 percent, then women taking EC would experience a 7.5 percentage-point reduction in pregnancy risk.[39] If all new EC pills consumed in pharmacy access states after the policy change were used by women between 18 and 30, and if these women were previously at a 10 percent risk of pregnancy, and if EC caused no behavioral response, a change of as much as 3-4 percent in the level of births would be possible. Such a change is ruled out by our findings.

However, more likely than these extreme assumptions is that not all women who consumed EC were at an extremely high risk, or there was some behavioral response.

If women who take EC are actually at a decreased risk of pregnancy, then we would expect very small effects from expansion of access. For instance, women who take EC may do so principally because another method of contraception has failed. Some studies indicate that this is indeed the case (Trussell et al., 2004). If women who consume EC face a lower risk of pregnancy from a single sexual encounter than average, say 2 percent, then the pregnancies averted by additional EC access would be negligible relative to total births.

Similarly, if some women change their sexual behavior in response to the availability of EC, small and undetectable impacts are expected. The US population is much larger

[38]The risk of pregnancy from a single, unprotected sexual encounter reaches 29 percent on the day before ovulation, but due to ovulation occurring irregularly within the menstrual cycle, a woman is unlikely to pinpoint this peak risk.

[39]Clinical trials show EC to be up to 89 percent effective, but this effectiveness decreases with the time between intercourse and consumption of the pill.

than the number of EC pills consumed, thus it takes only a small fraction of all US women changing their behavior to offset the decrease in births driven directly by EC. For example, if 100,000 EC pills are consumed by women who increase their sexual activity as a result of the availability of EC, such a change would be sufficient to largely offset the effect of those who were already at risk and use the pill to reduce this risk.

Under either scenario, very large changes to births or abortions are unlikely, given that each additional pill prevents pregnancy from only a single sexual encounter. More broadly, unexpected sexual encounters may account for a small percentage of overall pregnancies. Roughly half of women seeking abortions had been using some form of contraception, and few report unexpected sex as a factor in their abortion (Jones et al., 2002). If individuals who use EC actually face a low risk of unintended pregnancy, and individuals most likely to experience unintended pregnancy are unlikely to seek EC, then the impact of expanded access will be greatly diminished. We conclude that policies offering over-the-counter access to EC avert a private cost in acquiring the pill through a physician, but do not avert the social cost of unintended pregnancy.

4.2 ED Visits

Despite the estimated null effect on birth and abortion rates above, state and federal legislation may have changed the way women acquire EC. To test for that possibility, Figure 3 presents monthly counts of EC-related visits to emergency departments. The vertical line indicates the date when the FDA allowed all women to obtain EC in pharmacies without a prescription.[40] The figure shows a clear decrease in EC-related visits after the FDA ruling. EC-related visits decrease from roughly 250 each month to roughly 150 each month. In contrast, the number of other visits seems to rise. Though a relatively small share of ED visits are related to EC, Figure 3 suggests that such visits became less common

[40]We restrict the sample to visits by women older than age 18. Only such women would have been affected by the legislative change. No drop is observed for EC-related visits by women younger than 18.

after women could obtain EC in pharmacies. Given that our five states capture about 20 percent of the population in the United States, this would suggest that the FDA policy change led to a decrease of about 500 visits per month to ED's.

In contrast to the pharmacy-access policies, we would expect ED-access laws to increase visits to the ED to obtain the pill, as its provision would be guaranteed. ED data do not exist that would allow us to estimate how each ED-access law affected the number of ED visits. Nevertheless, we have obtained records of ED visits for New Jersey, which passed such a law in April of 2005. Figure 4 presents ED visits in New Jersey before and after the state passed its ED-access law. The figure suggests that EC-related ED visits were decreasing before the law was passed and then increased dramatically immediately after the law was passed. In contrast, other ED visits experienced a secular increase before and after the law. The magnitude of the change suggests an increase of about 25 visits per month. Given that the population of New Jersey is about 3 percent of that of the United States, that increase is slightly larger than the decrease that was experienced after the FDA ruling. By contrast, no such pattern is observed in Wisconsin, which passed its ED-access law after the FDA policy change. This implies women did not increase their visits to the ED for EC in response to guaranteed access when a lower cost route, pharmacy access, is already available.

As a whole, these figures suggest that expansions of access to EC substantially changed the venue in which women procured the medication. Given the expense of ED visits (Bamezai et al., 2005), the figures suggest that such laws affect the total cost of distributing EC. These costs are both monetary as well as related to the time and stress associated with visiting the ED.[41]

[41] One can only speculate as to how pharmacy access has lowered the cost of distributing EC. We find that the FDA policy change eliminated 500 EC-related ED visits each month. The average cost of a non-trauma ED visit is roughly $300 (Bamezai et al., 2005), thus this change alone translates into annual savings of about $1.8 million. In addition, after the FDA policy change, women no longer had to visit a physician to procure a prescription for EC. These foregone visits to the doctor would then add to the total change in distribution costs. But any estimates of the social cost of doctor's visits, along with the

However, patients in an ED are given access to a wider array of staff and services than customers in a pharmacy. Potentially, that difference may lead to changes in outcomes. We test for such changes next.

4.3 Reports of Sexual Assault and EC

We next test whether expansions in access to EC affect reports of sexual assault. A priori, one might expect such an effect. In our model, victims of sexual assault are those for whom the impact of EC would be the largest. We thus expect EC-related legislation to affect reports of sexual assault through the following mechanisms. First, both ED-access laws and pharmacy-access laws may affect the venue in which women procure EC. The previous section explored this possibility. Second, the venue in which a woman procures EC may influence whether she chooses to report the crime. In either a pharmacy or an emergency department, it is the victim's decision as to whether a sexual assault is reported to the police. Hospital staff, however, may be more effective at encouraging women to report such crimes relative to the staff of pharmacies. In that case, when sexual assault victims procure EC in pharmacies they may be less likely to report the crime to the police.

This reasoning suggests that pharmacy-access laws will decrease reports of sexual assault, since more women will procure EC in pharmacies. It also suggests that ED-access laws will increase reporting of assaults. That said, the effect of ED-access laws on the reporting of assaults may be less pronounced. Many assault victims likely received EC in the ED before ED-access laws. Thus we would expect pharmacy-access laws to have the largest effect on reports of assault.

Table 5 presents the results of difference-in-difference regressions with reported sexual assaults as the outcome of interest. The second row of Panel A suggests that pharmacy-access laws did indeed reduce reported sexual assaults. In particular, states experienced

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associated time costs, are inherently speculative. The sum of $1.8 million each year is then a lower bound for the true change in distribution costs.

a significant 9 percent decrease in reported assaults after they passed pharmacy-access laws. This implies a decrease of 0.31 reported assaults per 10,000 people (compared to an average of 3.5 assaults per 10,000 people). While this effect is not large, it is consistent with the decreased number of visits to the ED (6,000 annually).

To check that this result is not spurious, Figure 5 presents the point estimates from an event-study specification that evaluates the effect of state ED-access laws on reports of sexual assault. None of the 95-percent-level confidence intervals in Figure 5 exclude zero. We find this unsurprising; an event-study specification is demanding, given that these data only exist at the state-year level. Still, we find the figure suggestive. The point estimates suggest that reported assaults dropped for all post-law periods and did so exactly the year that the pharmacy-access laws were passed. While that drop is not statistically significant at conventional levels, the point estimates do not form a linear trend, but rather a step function.

In addition, Table 5 suggests that ED-access laws increased the reporting of sexual assaults. Perhaps such laws increased the number of women choosing to go to the ED following a sexual assault. Those point estimates are only statistically significant at the 10–15 percent level. Nevertheless, the finding is consistent with the role that ED access has in guaranteeing care for prevention of pregnancy to assault victims. This further suggests that women who seek EC in hospitals are likely to report sexual assault.

Columns 3 and 4 of Table 5 report the results of a falsification check. We estimate similar regressions in which the number of aggravated assaults reported to the FBI is the dependent variable. Aggravated assaults are non-sexual in nature, and the reporting of such crimes should not be related to the availability of EC. Reassuringly, columns 3 and 4 suggest that EC-related legislation had no effect on aggravated assault.[42]

Panel B of Table 5 presents similar estimates for the FDA policy change. We find no

[42]Reports of robbery are similarly unaffected.

evidence of a statistically change in the report of sexual assaults after the national policy change. However, the results in Panel B are much noisier and none of the coefficients are statistically significant but they do not rule out effects of the size found in Panel A.

Finally, we examine the impact of the FDA policy change on the nature of ED visits. Specifically, we test whether the FDA policy change affected ED visits for sexual assaults in our HCUP sample. Figure 6 presents the number of such visits over time.[43] The number of sexual-assault-related visits fell dramatically around the time of the FDA ruling. In contrast, ED visits for other conditions remained on the same trend. Although visits for sexual assault became more common in the summer of 2007, the relative number of such visits remained below trend.[44] The effect of the FDA policy change is also clear, however, if we control for month-of-year fixed effects. The magnitude of this change is substantial; assault-related visits decreased by about 100 visits per month in our five-state sample.

Overall, this evidence is suggestive. It implies that pharmacy access to EC may have led to a decrease in reported sexual assaults. The welfare implications of this finding are unclear. Easier access to EC means lower transaction costs for victims of sexual assault. However, it may also limit the other services provided to sexual assault victims, and hinder the apprehension of perpetrators. More research is needed in this area, to confirm how access affects assault reporting, and what policy steps could be taken to mitigate the unintended consequences of increased access.

5 Conclusions

In summary, this paper studies the effects of access to EC. We first present a theoretical framework that suggests that the net effect of EC is ambiguous. On the one hand, there exists a direct effect—the consumption of EC prevents pregnancies. On the other hand,

[43] Only visits by women older than 18 are in the sample.

[44] Visits related to sexual assault are subject to a seasonal pattern, occurring more frequently in the summer than in the winter.

there exists an indirect effect; EC may induce a behavioral response which leads to more sexual encounters, and hence, more pregnancies. Finally, the likely impact of EC depends on when additional information on uncertain variables is revealed to the woman: information revealed near the time of intercourse (such as a broken condom) is related to EC use, while information that is gained long before or long after intercourse will make EC less useful relative to traditional contraception or abortion. Our model also suggests that the use of EC relative to traditional contraceptives and abortion will depend on the timing of information updates on the costs and benefits of unprotected sex and pregnancy.

Consistent with this model, we find no empirical evidence that expanded access to EC has decreased birth rates or abortions, even for at-risk populations. We caution that the associated confidence intervals are relatively wide, and that more research is needed to recover precise estimates. Still, we do not observe large changes in natality or abortion, as some opponents of EC have feared, nor do we find large decreases in unintended pregnancy, as some proponents had hoped. We find that wider access to EC increases utilization of EC, thus we do not believe that a lack of variation in the actual consumption of EC is driving our results. EC may mostly affect women for whom the chance of pregnancy is low, and thus it would be impossible to observe very large decreases in response to such policies.

These results clarify the dynamics of unintended pregnancies. The likely impact of EC depends on when additional information on uncertain variables is revealed to the woman: information revealed near the time of intercourse encourages EC use, while information that is gained long before or long after intercourse will make EC less useful relative to traditional contraception or abortion. If EC were to have a large effect on births, then one might conclude that, immediately after intercourse, women were anticipating unintended pregnancies, and turning to EC as a result. Our results, however, imply little effect of EC. This suggests that few unintended pregnancies are anticipated immediately after intercourse. Long term decisions may play a larger role in determining risk for unintended pregnancy, and the women facing the greatest risk of such pregnancies may not be the users of EC. Sexual assault victims represent an exception, given that they face a large unanticipated shock that EC can be used to mitigate.

Our results do suggest that expanded access to EC has changed the venue in which women obtain EC, encouraging women to visit EDs when access there is guaranteed, and then switch from EDs to pharmacies when the drug is available OTC. Visits to pharmacies are less expensive than visits to emergency departments. Thus, if nothing else, expansions in access to EC have lowered the total cost of distributing the drug.

This lower cost, however, appears to have brought a potential unintended consequence: access to EC in pharmacies may reduce the reporting of sexual assault. To mitigate this impact, new policies may be necessary to encourage crime reporting by sexual assault victims that visit pharmacies. Further evidence is needed on this, but such a possibility was not, to our knowledge, discussed in the debate over EC, and deserves greater attention.

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A Appendix

Before EC, the share of women who have an unwanted pregnancy is:

$$q \cdot \left(\int_{\underline{B}}^{-A} \int_{q \cdot A}^{\bar{S}} g(S,B) dS dB + \int_{-A}^{0} \int_{-q \cdot B}^{\bar{S}} g(S,B) dS dB \right).$$

The fraction of women who have an abortion before EC is introduced is:

$$q \cdot \int_{\underline{B}}^{-A} \int_{q \cdot A}^{\bar{S}} g(S,B) dS dB.$$

After EC is available, the model described in section 2 makes four predictions. First, it predicts that the share of women who have sex will unambiguously rise. The share of women who have sex after the introduction of EC is:

$$\int_{\underline{B}}^{-A} \int_{c - q' \cdot A}^{\bar{S}} g(S,B) dS dB + \int_{-A}^{\frac{c}{q'-q}} \int_{c - q' \cdot B}^{\bar{S}} g(S,B) dS dB$$
$$+ \int_{\frac{c}{q'-q}}^{0} \int_{-q \cdot B}^{\bar{S}} g(S,B) dS dB + \int_{0}^{\bar{B}} \int_{0}^{\bar{S}} g(S,B) dS dB.$$

This share is unambiguously larger than the share before EC, $\gamma(q)$.

Note also that as c increases, the share of women having sex decreases, because the derivative is given by:

$$- \int_{\underline{B}}^{-A} g(c - q' \cdot A, B) dB - \int_{-A}^{\frac{c}{q'-q}} g(c - q' \cdot B, B) dB$$
$$+ \frac{1}{q'-q} \left(\int_{-\frac{cq}{q'-q}}^{\bar{S}} g(S, \frac{c}{q'-q}) dS - \int_{-\frac{cq}{q'-q}}^{\bar{S}} g(S, \frac{c}{q'-q}) dS \right) =$$
$$- \int_{\underline{B}}^{-A} g(c - q' \cdot A, B) dB - \int_{-A}^{\frac{c}{q'-q}} g(c - q' \cdot B, B) dB < 0.$$

Second, the model predicts that the share of women who become pregnant after EC may rise or fall. The share of women who become pregnant once EC is available is:

$$q \cdot \left(\int_{\frac{c}{q'-q}}^{0} \int_{-q \cdot B}^{\bar{S}} g(S,B)dSdB + \int_{0}^{\bar{B}} \int_{0}^{\bar{S}} g(S,B)dSdB \right) +$$
$$q' \cdot \left(\int_{\underline{B}}^{-A} \int_{c+q' \cdot A}^{\bar{S}} g(S,B)dSdB + \int_{-A}^{\frac{c}{q'-q}} \int_{c-q' \cdot B}^{\bar{S}} g(S,B)dSdB \right).$$

For this number to be lower than the number of pregnancies before EC, EC must lower the number of unwanted pregnancies. (EC has no effect on the number of wanted pregnancies.) The number of unwanted pregnancies will fall only if:

$$\frac{q'}{q} < \frac{\int_{\underline{B}}^{-A} \int_{qA}^{\bar{S}} g(S,B)dSdB + \int_{-A}^{\frac{c}{q'-q}} \int_{-qB}^{\bar{S}} g(S,B)dSdB}{\int_{\underline{B}}^{-A} \int_{c+q'A}^{\bar{S}} g(S,B)dSdB + \int_{-A}^{\frac{c}{q'-q}} \int_{c-q'B}^{\bar{S}} g(S,B)dSdB} < 1,$$

that is, only if the effectiveness of EC surpasses the number of added sexual encounters it generates.

When the price of EC rises, the impact on pregnancies is given by the following expression:

$$-q' \cdot \left(\int_{\underline{B}}^{-A} g(c+q' \cdot A, B)dB + \int_{-A}^{\frac{c}{q'-q}} g(c-q' \cdot B, B)dB \right)$$
$$\frac{1}{q'-q} \cdot \left(-q \int_{-\frac{cq}{q'-q}}^{\bar{S}} g(S, \frac{c}{q'-q})dS + q' \int_{-\frac{cq}{q'-q}}^{\bar{S}} g(S, \frac{c}{q'-q})dS \right) =$$
$$-q' \cdot \left(\int_{\underline{B}}^{-A} g(c+q' \cdot A, B)dB + \int_{-A}^{\frac{c}{q'-q}} g(c-q' \cdot B, B)dB \right) + \int_{-\frac{cq}{q'-q}}^{\bar{S}} g(S, \frac{c}{q'-q})dS.$$

Third, the model predicts that the share of women who have an abortion may rise or fall. The share of women who have an abortion after EC is introduced is:

$$q' \cdot \int_{\underline{B}}^{-A} \int_{c+q' \cdot A}^{\bar{S}} g(S,B)dSdB.$$

This number may be larger or smaller than the number of abortions without EC. Abortion will only decrease if:

$$\frac{q'}{q} < \frac{\int_{\underline{B}}^{-A} \int_{qA}^{\bar{S}} g(S,B)dSdB}{\int_{\underline{B}}^{-A} \int_{c+q'A}^{\bar{S}} g(S,B)dSdB} < 1.$$

For an increase in the price, the comparative statics are much simpler and indicate that a lower price of EC will increase abortions since the derivative is given by:

$$-q' \cdot \int_{\underline{B}}^{-A} g(S, c + q' \cdot A)dS < 0.$$

Expansions of access to EC will unambiguously increase the number of abortions as long as abortions are more expensive than EC. On the one hand, the availability of EC induces more women to have sex. Some of these women are those who would want an abortion if EC fails. This mechanism thus raises the abortion rate. On the other hand, abortion will only now be needed when EC fails, and thus the availability of EC reduces the abortion rate. When the cost of EC (c) decreases, this second effect is not present. Based on the assumptions of the model, all women who were previously pursuing abortion were already consuming EC. Thus the only effect of a decrease in c is to increase the number of women who use EC. And, because EC is not foolproof, for some women, EC will fail and lead to more abortions. This result would not hold if, for some women, abortion is actually cheaper than EC, in which case, the effect of lowering c would be ambiguous again.

Finally, the model predicts that the number of births may rise or fall. Births will fall if:

$$\frac{q'}{q} < \frac{\int_{-A}^{\frac{c}{q'-q}} \int_{-qB}^{\bar{S}} g(S,B)dSdB}{\int_{-A}^{\frac{c}{q'-q}} \int_{c-q'B}^{\bar{S}} g(S,B)dSdB} < 1$$

An expansion of access will also have an uncertain impact since the derivative is given by:

$$-q' \cdot \int_{-A}^{\frac{c}{q'-q}} g(c - q' \cdot B, B) dB + \int_{-\frac{cq}{q'-q}}^{\bar{S}} g(S, \frac{c}{q'-q}) dS$$

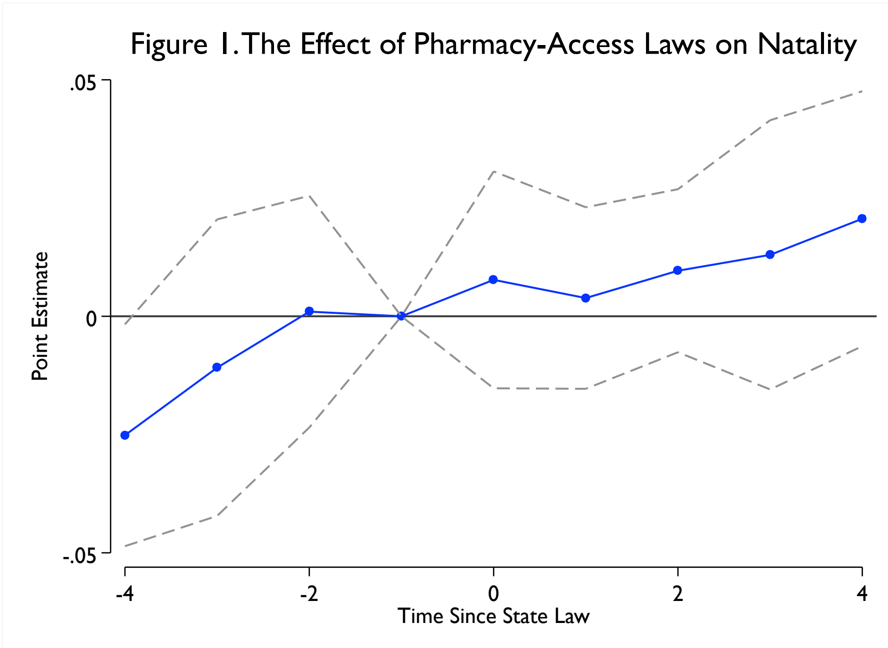
Figure 1. The Effect of Pharmacy-Access Laws on Natality
Point Estimate
.05
0
-.05
-4
-2
0
2
4
Time Since State Law

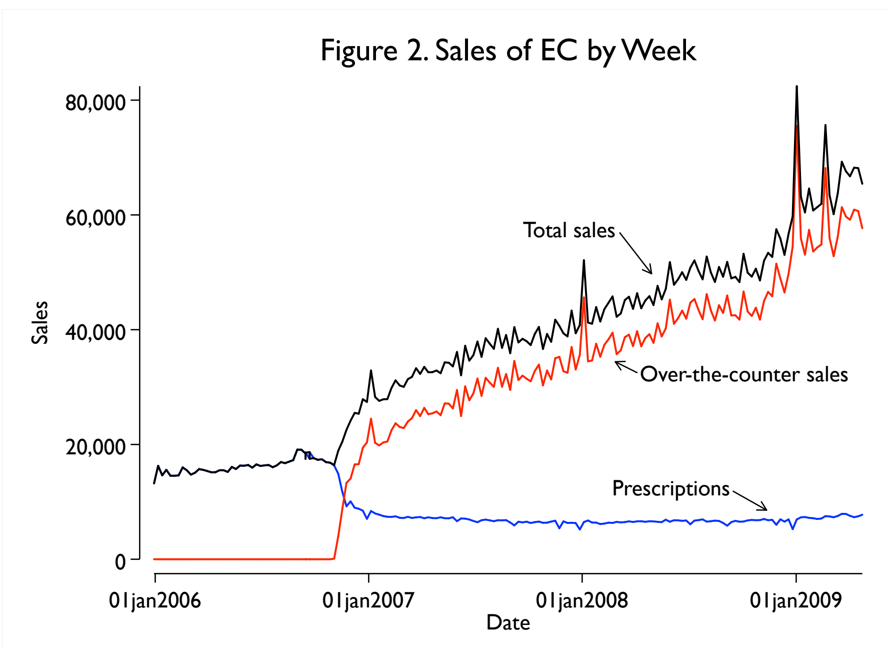
Figure 2. Sales of EC by Week
Sales
80,000
60,000
40,000
20,000
0
Total sales
Over-the-counter sales
Prescriptions
01jan2006
01jan2007
01jan2008
01jan2009
Date

Figure 3. ED Visits in Entire HCUP Sample

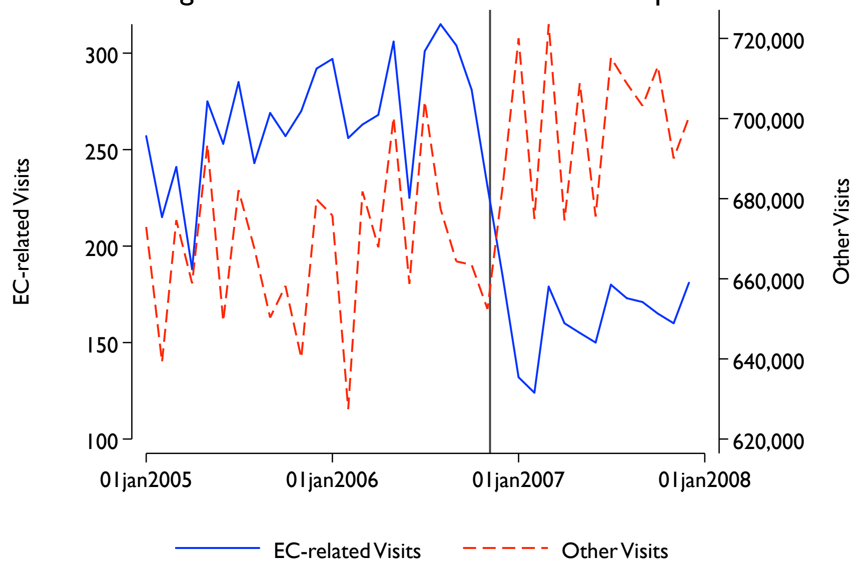

Figure 4. ED Visits in New Jersey

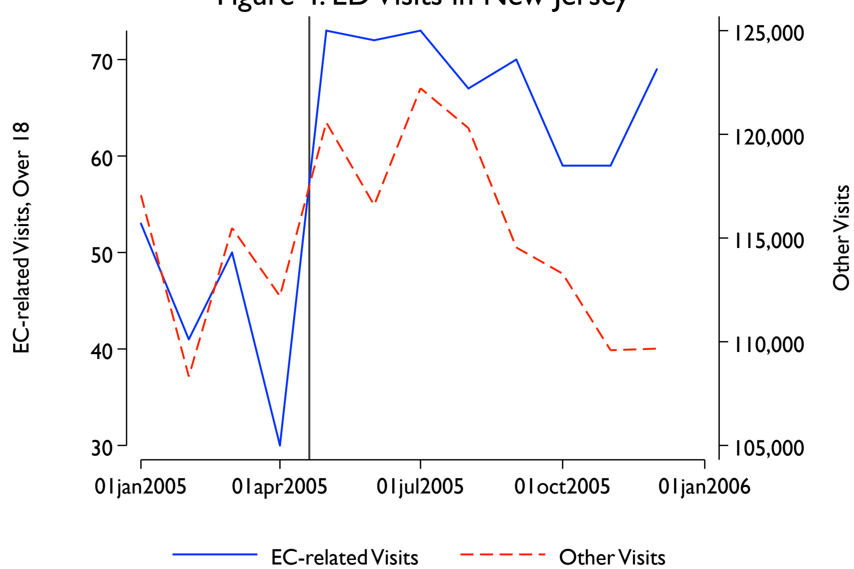

Figure 5. Effect of Pharmacy-Access Laws on Reports of Sexual Assaults

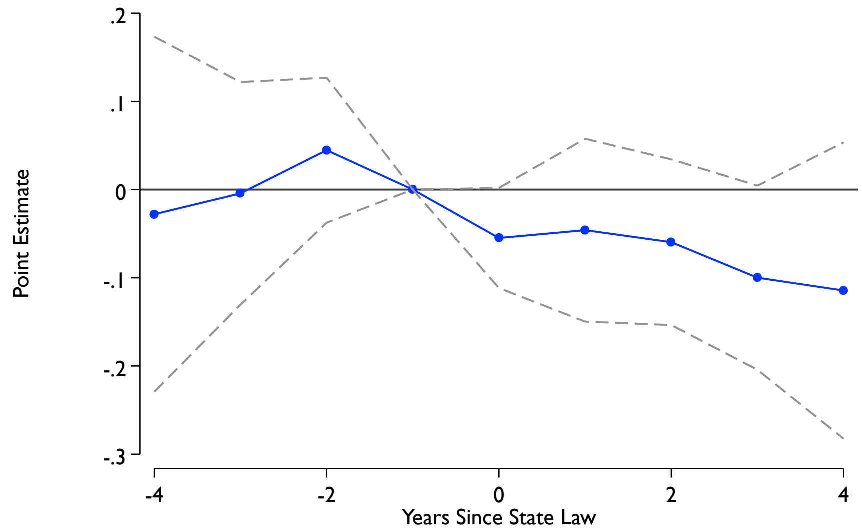

Figure 6. Sexual Assaults Visits in HCUP Sample

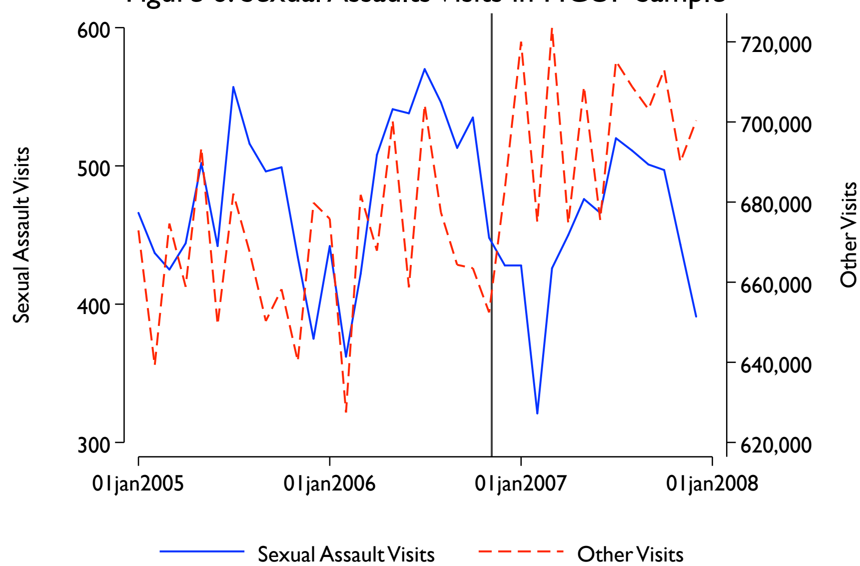

Table 1. State Laws

State or region	Pharmacy-Access Law	Pharmacy-Access Type	ED-Access Law	ED-Access Type
Alaska	25-Apr-2002	Collaborative practice		
Arkansas			9-Apr-2007	inform
California	1-Jan-2002	State-approved protocol*	1-Jan-2003	provide
Colorado			15-Mar-2007	inform
Connecticut			1-Oct-2007	provide
Hawaii	24-Jun-2003	Collaborative practice		
Illinois			1-Jan-2002	inform
Maine	3-Mar-2004	State-approved protocol**		
Massachusetts	15-Sep-2005	Collaborative practice	14-Dec-2005	provide
Minnesota			1-Aug-2007	provide
New Hampshire	15-Aug-2005	Collaborative practice		
New Jersey			20-Apr-2005	provide
New Mexico	15-May-2003	State-approved protocol	1-Oct-2003	provide
New York			31-Jan-2004	provide
Ohio			31-Mar-2003	recommendation†
Oregon			1-Jan-2008	provide
Pennsylvania			26-Jan-2008	provide*****
South Carolina			19-Jun-2005	pay (but not inform)†
Texas			1-Sep-2005	inform
Utah			25-Mar-2009	provide
Vermont	29-Mar-2006	Collaborative practice		
Washington	1-Jul-1997	Collaborative practice***	13-Jun-2002	provide
Washington, DC			25-Mar-2009	provide
Wisconsin			28-Mar-2008	provide
National	24-Aug-2006	18 and over only****		

* Legislation initially allowed collaborative practice only, but was expanded to state protocol 10/1/03.

** Hybrid model: collaboration required but not regulated. Listed as state protocol by Guttmacher.

*** Initially, a two-year pilot program building on state's existing collaborative practice law for some drugs.

**** Expanded to 17-year-olds on April 22, 2009.

***** Includes conscience exemption.

† These states are not considered "access" states by Guttmacher, and so we exclude in all specifications.

Note: Dates denote effective date if available, legislation signing date if effective date unknown, and adoption by legislature date if signing date unknown.

Sources: National Conference of State Legislatures; state legislative records; Guttmacher Institute; Lexis Nexis and Google news search.

Table 2: Effect of EC-Related Laws on Natality
Dependent Variable: The logarithm of births for the given sample

	(1)	(2)	(3)	(4)
Sample:	All Women	Women under age 18	Women aged 18-30	Women aged 30 plus
		A: State Law Changes, 1995-2006		
Passed ED-Access Law	- 0.004 (0.010)	- 0.020 (0.020)	0.003 (0.008)	- 0.004 (0.016)
Passed Pharmacy-Access Law	0.014 (0.008)	0.014 (0.020)	0.022** (0.007)	0.009 (0.013)
Average Births	6,574.8	269.5	4,257.9	2,047.0
R^2	0.999	0.989	0.999	0.998
		B: FDA Policy Change, 2004-2008		
No ED-Access Law X Post FDA	- 0.001 (0.011)	- 0.001 (0.023)	- 0.002 (0.012)	- 0.016 (0.020)
No Pharmacy-Access Law X Post FDA	- 0.006 (0.017)	0.036 (0.022)	- 0.018 (0.012)	0.008 (0.029)
Average Births	7,012.6	289.6	4,579.3	2,143.3
R^2	0.999	0.990	0.999	0.998

Note: For panel A, N = 7,344 and for panel B N=6,888. Standard errors in parentheses are robust to auto-correlation between observations from the same state. State fixed effects, month fixed effects, and state linear time-trends not shown. The sample consists of month by state totals of all births. The second panel excludes states which changed their legislation such that births in 2006-2008 would have been affected. * statistically significant at 10% level, ** statistically significant at 5% level.

Table 3: Effect of EC-Related Laws on Sub-Samples
Dependent Variable: The logarithm of births for the given sample

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	All married women	All unmarried women	White women, unmarried	Black women, unmarried	Black women, unmarried, ages 18-30	Black, unmarried, ages 18-30, High school diploma or less
			A: State Law Changes, 1995-2006			
Passed ED-Access Law	- 0.012	0.014	- 0.011	0.026	0.026	- 0.066
	(0.014)	(0.009)	(0.023)	(0.024)	(0.026)	(0.148)
Passed Pharmacy-Access Law	0.020*	0.006	0.088*	- 0.017	- 0.014	- 0.136
	(0.012)	(0.007)	(0.048)	(0.051)	(0.054)	(0.104)
Average Births	4,329.3	2,245.5	1,462.7	691.2	504.2	369.3
R^2	0.999	0.998	0.996	0.987	0.985	0.984
N	7,344	7,344	7,344	7,344	7,344	5,376
			B: FDA Policy Change, 2004-2008			
No ED-Access Law X Post FDA	- 0.010	- 0.015	- 0.001	- 0.014	- 0.034	- 0.338
	(0.013)	(0.012)	(0.028)	(0.043)	(0.045)	(0.207)
No Pharmacy-Access Law X Post FDA	- 0.013	0.013	0.009	0.076	0.086	0.348*
	(0.020)	(0.011)	(0.033)	(0.059)	(0.061)	(0.184)
Average Births	4,508.2	2,478.7	1,626.3	790.1	580.2	412.4
R^2	0.999	0.998	0.994	0.987	0.985	0.984
N	6,888	6,888	6,888	6,888	6,888	4,860

Note: Standard errors in parentheses are robust to auto-correlation between observations from the same state. State fixed effects, month fixed effects, and state linear time-trends not shown. The sample consists of month-by-state counts of all births. The second panel excludes states which changed their legislation such that births in 2006-2008 would have been affected. * statistically significant at 10% level, ** statistically significant at 5% level.

Table 4: Effect of EC-Related Laws on Abortions
Dependent Variable: The logarithm of abortions for the given sample

	(1)	(2)	(3)	(4)
Sample:	All Women	Women under age 20	Women aged 20-29	Women aged 30 plus
	A: State Law Changes, 1995-2005			
Passed ED-Access Law	- 0.052	-0.047	-0.017	- 0.071
	(0.059)	(0.057)	(0.066)	(0.049)
Passed Pharmacy-Access Law	0.038	-0.016	-0.002	- 0.005
	(0.075)	(0.074)	(0.060)	(0.049)
Avg. Abortions	16,666.8	3,171.8	9,312.7	4,279.3
R^2	0.987	0.986	0.988	0.990
N	505	498	503	503
	B: FDA Policy Change, 2004-2008			
No ED-Access Law X Post FDA	-0.117	-0.045	-0.008	-0.005
	(0.130)	(0.115)	(0.104)	(0.085)
No Pharmacy-Access Law X Post FDA	0.236*	0.231*	0.117	0.070
	(0.117)	(0.118)	(0.079)	(0.066)
Avg. Abortions	11,849.1	2,281.5	6,728.4	2,938.7
R^2	0.986	0.983	0.986	0.988
N	448	441	445	445

Note: Standard errors in parentheses are robust to auto-correlation between observations from the same state. State fixed effects, year fixed effects, and state linear time-trends not shown. The sample consists of year-by-state totals of all abortions, estimated by the CDC. The second panel excludes states which changed their legislation between 2006 and 2008. * statistically significant at 10% level, ** statistically significant at 5% level.

Table 5: Effect of EC-Related Laws on Reports of Assault

	(1)	(2)	(3)	(4)
Dependent Variable:	Logarithm of Sexual Assaults	Sexual Assaults per 10,000 People	Logarithm of Aggravated Assaults	Aggravated Assaults per 10,000 People
		A: State Law Changes, 1995-2005		
Passed ED-Access Law	0.042	0.215*	- 0.061	- 1.638
	(0.027)	(0.118)	(0.038)	(1.120)
Passed Pharmacy-Access Law	-0.093**	-0.308**	0.021	- 0.530
	(0.036)	(0.119)	(0.026)	(1.219)
Mean of Outcome	1,839.6	3.5	18,411.0	30.0
R^2	0.995	0.951	0.997	0.984
		B: FDA Policy Change, 2004-2008		
No ED-Access Law X Post FDA	0.036	- 0.102	0.038	- 0.121
	(0.054)	(0.222)	(0.032)	(1.108)
No Pharmacy-Access Law X Post FDA	0.011	0.279	- 0.006	0.190
	(0.055)	(0.308)	(0.046)	(1.477)
Mean of Outcome	1,797.9	3.5	18,771.6	31.5
R^2	0.994	0.941	0.995	0.977

Note: For panel A, N = 561, for panel B, N = 585. Standard errors in parentheses are robust to auto-correlation between observations from the same state. State fixed effects, year fixed effects, and state linear time trends not shown. The sample consists of year-by-state counts of all assaults reported to the FBI. The second panel excludes states which changed their legislation between 2006 and 2007. * statistically significant at 10% level, ** statistically significant at 5% level.

Appendix Table 1: Effect of EC-Related Laws on Natality Using Annual Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. Variable:	Logarithm of births				Birth per 1,000 women			
Sample:	All Women	Women under age 18	Women aged 18-30	Women aged 30 plus	All Women	Women under age 18	Women aged 18-30	Women aged 30 plus
	A: State Law Changes, 1995-2006							
Passed ED-Access Law	- 0.003	- 0.017	- 0.001	0.003	0.378	0.148	1.276	0.144
	(0.010)	(0.021)	(0.010)	(0.018)	(0.520)	(0.484)	(1.006)	(0.311)
Passed Pharmacy-Access Law	0.012	0.003	0.024**	0.005	1.104*	0.760*	2.480**	0.350
	(0.011)	(0.022)	(0.009)	(0.017)	(0.577)	(0.425)	(1.216)	(0.332)
Mean of Outcome	78,897.6	3,233.9	51,095.1	24,563.9	52.035	20.007	104.355	27.232
R^2	0.999	0.999	0.999	0.999	0.988	0.995	0.991	0.996
	B: FDA Policy Change, 2004-2008							
No ED-Access Law X Post FDA	0.003	0.011	0.003	- 0.008	- 0.418	- 0.161	- 1.138	- 0.364
	(0.013)	(0.022)	(0.014)	(0.020)	(0.434)	(0.411)	(0.808)	(0.292)
No Pharmacy-Access Law X Post FDA	- 0.010	0.010	- 0.022	0.006	- 0.884	- 0.701	- 1.941	- 0.261
	(0.019)	(0.023)	(0.015)	(0.030)	(0.614)	(0.419)	(1.464)	(0.354)
Mean of Outcome	84,150.9	3,475.1	54,951.4	25,720.2	52.737	20.555	106.393	26.958
R^2	0.999	0.999	0.999	0.999	0.981	0.993	0.986	0.995

Note: For panel A, N = 612 and for panel B N=574. Standard errors in parentheses are robust to auto-correlation between observations from the same state. State fixed effects, year fixed effects, and state linear time-trends not shown. The sample consists of year-by-state counts of all births in the first four columns and of births per 1,000 women in the last four columns. The second panel excludes states which changed their legislation such that births in 2006-2008 would have been affected. * statistically significant at 10% level, ** statistically significant at 5% level.

Appendix Table 2: Effect of EC-Related Laws on Abortions Using Rates
Dependent Variable: Abortions per 1000 women in given age group

	(1)	(2)	(3)	(4)
Sample	All Women	Women under age 20	Women aged 20-29	Women aged 30 plus
		A: State Law Changes, 1995-2005		
Passed ED-Access Law	-0.096	-0.220	-0.386	-0.188
	(0.433)	(0.597)	(1.093)	(0.145)
Passed Pharmacy-Access Law	0.190	-0.043	0.152	0.046
	(0.252)	(0.429)	(0.890)	(0.082)
Mean of Outcome	12.591	15.278	27.821	5.434
R^2	0.982	0.975	0.980	0.983
N	505	498	503	503
		B: FDA Policy Change, 2004-2008		
No ED-Access Law X Post FDA	0.506	0.732	2.319	0.519
	(1.293)	(1.652)	(1.644)	(0.313)
No Pharmacy-Access Law X Post FDA	0.165	0.280	-1.038	-0.316
	(1.227)	(1.443)	(1.408)	(0.302)
Mean of Outcome	10.753	13.057	23.717	4.515
R^2	0.960	0.951	0.960	0.957
N	448	441	445	445

Note: Standard errors in parentheses are robust to auto-correlation between observations from the same state. State fixed effects, year fixed effects, and state linear time-trends not shown. The sample consists of year by state totals of all abortions, estimated by the CDC. The second panel excludes states which changed their legislation between 2006 and 2008. * statistically significant at 10% level, ** statistically significant at 5% level.

Appendix Table 3: Effect of EC-Related Laws without Linear Time Trends
Dependent Variable: Births or abortions per 1,000 for the given sample

	(1)	(2)	(3)	(4)
Outcome:	Natality	Natality	Abortion	Abortion
Sample:	All Women	Ages 18 to 30	All Women	Ages 20 to 29
		A: State Law Changes, 1995-2006		
Passed ED-Access Law	- 0.339 (0.545)	- 1.054 (1.006)	-0.322 (0.403)	-0.573 (0.985)
Passed Pharmacy-Access Law	-0.732* (0.383)	-3.552** (0.663)	0.947 (0.608)	0.457 (1.161)
Mean of Outcome	52.035	104.355	11.074	24.351
R^2	0.957	0.961	0.965	0.963
N	612	612	505	503
		B: FDA Policy Change, 2004-2008		
No ED-Access Law X Post FDA	0.399 (0.995)	0.067 (1.916)	-0.544 (0.714)	-2.644** (1.012)
No Pharmacy-Access Law X Post	0.632 (1.338)	4.636* (2.557)	0.190 (0.775)	2.678** (1.259)
Mean of Outcome	52.737	106.393	10.473	22.748
R^2	0.939	0.940	0.910	0.911
N	574	574	448	445

Note: Standard errors in parentheses are robust to auto-correlation between observations from the same state. State and year-month fixed effects not shown. The sample consists of month-by-state counts of all births. The second panel excludes states which changed their legislation such that births in 2006-2008 would have been affected. * statistically significant at 10% level, ** statistically significant at 5% level.

Appendix Table 4: Effect of EC-Related Laws by Type of Law

Dependent Variable: The logarithm of births, abortions or sexual assault for the given sample

	(1)	(2)	(3)
Outcome:	Natality	Abortion	Sexual Assaults
Sample:	Ages 18 to 30	Ages 20 to 29	All Women
	A: State Law Changes, 1995-2006		
Passed ED-Access Law	0.002	-0.034	0.050*
	(0.008)	(0.071)	(0.029)
Passed Pharmacy-Access Law, Collaborative	0.02**	-0.038	-0.071*
	(0.009)	(0.082)	(0.037)
Passed Pharmacy-Access Law, Protocol	0.025**	0.038	-0.117*
	(0.010)	(0.109)	(0.060)
R^2	0.999	0.988	0.995
N	7,344	503	561
	B: FDA Policy Change, 2004-2008		
No ED-Access Law X Post FDA	- 0.007	-0.066	0.044
	(0.011)	(0.105)	(0.051)
No Pharmacy-Access Law-Collaborative X Post FDA	-0.031**	0.117	0.035
	(0.012)	(0.079)	(0.075)
No Pharmacy-Access Law-Protocol X Post FDA	0.003	0.232**	- 0.021
	(0.010)	(0.108)	(0.041)
R^2	0.999	0.986	0.994
N	6,888	445	585

Note: Standard errors in parentheses are robust to auto-correlation between observations from the same state. State fixed effects and month fixed effects not shown. The sample consists of month-by-state counts of all births. The second panel excludes states which changed their legislation such that births in 2006-2008 would have been affected. * statistically significant at 10% level, ** statistically significant at 5% level.