Assessing Plan B: The Effect of the Morning After Pill on Children and Women*

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July 14, 2014

Abstract

We examine the effect of quasi-experimental variation in the availability of the emergency contraceptive ("morning after") pill in Chile. Using censal data on all births and fetal deaths over the period 2005-2011 we show that the availability of the pill reduces pregnancy and early gestation fetal death, which we argue proxies for illegal abortion. These effects are particularly pronounced among teenagers and young women: point estimates suggest a 6.9% reduction in teenage pregnancy and 4.2% reduction for 20-34 year olds. We suggest that diffusion of the morning after pill between quasi treatment and control areas played an important role, and suggest a way to estimate unbiased treatment effects where the stable unit treatment value assumption does not hold locally. This paper is the first to provide censal evidence of the emergency contraceptive's effect, and the first to examine the technology in a country where no other (legal) post-coital fertility control options exist.

JEL codes: J13,J16,J18,I15,O15.

*We acknowledge the excellent support and advice of a number of members of the Government of Chile who provided extremely useful access to, and advice regarding, national databases. Principally, we thank Rodrigo Alarcón S., Andrés Álvarez A., Carlos Arce Mártinez, Ximena Carrasco and Nicolás Muñóz of the Ministries of Health, Social Development and Education. Much care was taken by all parties to respect all necessary privacy clauses, and data analysis was undertaken in line with Law 19.628 of Protection of Private Life (*Ley 19.628 de Protección de la Vida Privada*). We are grateful to Sonia Bhalotra, Lidia Casas, Rachel Cassidy, Julien Labonne, Jeanne Lafortune, Simon Quinn, Climent Quintana Domeque, Chris Roth, George Vega Yon and seminar audiences at ESPE Braga, LAGV Aix en Provence, and the Universidad de Chile for useful comments, without implicating them in any remaining shortcomings. We thank Daniela Gómez P. and Katharine Lauderdale for excellent research assistance. Full source code and data are publicly available for inspection at `https://github.com/damianclarke/morning-after-pill`.

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1 Introduction

Undesired pregnancy—particularly among young and adolescent women—is a considerable contributor to poor maternal and child outcomes, and to a lack of intergenerational mobility. The last half-century has seen a remarkable increase in contraceptive technology, with considerable impacts on rates of such undesired pregnancy and with far-reaching consequences for the social and productive structure of modern society. The widespread introduction of the oral contraceptive pill has brought with it lower birth rates, delays in childbearing and marriage, higher rates of human capital attainment and labour market participation for women (Angrist and Evans, 1996; Bailey, 2006; Goldin and Katz, 2002a,b), reductions in the gender wage differential (Bailey et al., 2012), and, theoretically at least, more empowered women (Chiappori and Oreffice, 2008). In the long-run, these outcomes have led to generations of children less likely to have divorced parents, and more likely to live with college educated mothers (Ananat and Hungerman, 2012).

While the contraceptive pill has had a remarkable impact on a woman's capability to control the timing of her fertility decisions, these treatments require an expensive ongoing investment, which is difficult or impractical for certain groups of women. In contrast to the rich literature on the effects of the contraceptive pill, very little evidence is available regarding the effects of post-coital (non-abortive) birth control. In this paper we examine the effect of fully-subsidised provision of the emergency contraceptive pill. This so called "morning after pill" offers an alternative form of contraception in cases where other forms were not used or failed during intercourse, or in the case of rape.

The scarce existing literature on this topic suggests that the emergency contraceptive (EC) pill may have had surprisingly little effect on both pregnancy and abortion (Gross et al., 2013; Durrance, 2013). Here we present considerable evidence that, at least in the case of Chile, access to emergency contraception does have significant effects on births and abortions, and that these effects are concentrated on teenagers and young women. We identify a plausibly exogenous policy decision in Chile which affects a woman's access to the (fully subsidised) emergency contraceptive pill. Using censal data on each woman's pregnancy status in each year, and the outcomes of each pregnancy in Chile, we demonstrate that the availability of the morning after pill reduced the likelihood of pregnancy and illegal abortion, and that this effect was transversal rather than being enjoyed overwhelmingly by one social class.

The reform under examination comes from a series of constitutional challenges between 2005-2008, which meant that the introduction of the emergency contraceptive pill in Chile was entirely controlled by the Supreme court and constitutional Tribunals. Legal challenges resulted in the

2008 finding that it would be illegal for all nationally run health centres and hospitals to prescribe the emergency contraceptive pill, however that in each of the 346 municipalities of Chile health centres were at liberty to do so. This resulted in a situation in which a woman's access to the pill entirely depended upon the decisions taken by her mayor. Due to this reform it is shown that around half the municipalities in Chile made the pill available, while the other half did not.

Using this reform, We estimate the effect that the staggered arrival of the emergency contraceptive had on women and children, including its effect on births and abortions. The arrival of this new technology is associated with significant reductions in these outcomes. Further, the effects identified are of considerable magnitude. It is estimated that among teenage girls, the widespread availability of emergency contraception reduces births by around 7%, and may more than halve rates of illegal abortion. Among older women the reductions in births and illegal abortion are more moderate, however still quantitatively important. For example, among 20-34 year olds, the emergency contraceptive pill reduces births by an estimated 4.2% and appears to reduce illegal abortion by around 20%.

Naive estimates of the effect of the morning after pill on pregnancies, abortions, and other outcomes, are based on the assumption that the arrival of the emergency contraceptive to approximately half of the women in the country had no effect on those women who did not live in areas where the pill was available. We examine the validity of this assumption by comparing women who live 'close' to areas where the pill was available to those who live considerably further away. It is shown that significant treatment spillovers may occur, and so suggested that naive estimates off the effect of the emergency contraceptive pill may significantly underestimate the true effect of the expansion of availability in Chile.

Given the spatial nature of these spillovers, a methodology is proposed which allows for the recovery of a consistent treatment effect even in the presence of local spillovers. It is shown that under certain assumptions regarding the nature of the stable unit treatment value assumption, the estimated treatment effect will be significantly attenuated if this consideration is not made. We then propose a number of ways to determine which control clusters should be considered 'close to' treatment clusters. It is shown that for the morning after pill, treatment spillover is a quantitatively important consideration, and that in some groups, diffusion may exist for anywhere up to 30km from a treatment location.

This study makes a number of contributions. Foremost, it is one of the first—if not the first—study of the effects of emergency contraception using censal microdata on a national scale. It is also the first large scale study of which the author is aware which addresses these questions in a country other than the United States. This is of considerable importance given that Chile, the country under study here, does not offer legal abortion, and so the emergency contraceptive pill is the first legal mechanism for post-coital fertility control.

The results of this study add to the nascent literature on the emergency contraceptive pill. Recent studies such as Gross et al. (2013); Durrance (2013) which have been the first to address this question in the economic literature have provided evidence to suggest that the effects of this technology may be minor. Here we offer considerable evidence to the contrary, suggesting that the expansion in the availability of emergency contraceptives may offer important effects in certain countries, with large impacts on pregnancy and abortion rates, especially among young women.

Finally, we raise a number of novel points regarding the estimation of treatment effects in the presence of spillovers between treatment and control clusters. This is fundamentally different to, despite sharing some characteristics with, the literature on estimating treatment effects in the presence of spillovers between treated and non-treated individuals within treatment clusters. We suggest that even in the presence of such spillovers, unbiased treatment effects can be estimated if these spillovers occur in a predictable way, as is likely to be the case when distance to treatment varies in an exogenous manner. We propose a number of flexible ways to consider estimating treatment effects in these circumstances.

In what remains of this paper we first provide background regarding the emergency contraceptive pill, and the reform under study in Chile in section 2. Section 3 discusses the censal data sets we will use to assess the effects of the reform, while 4 discusses identification and methodology. In section 5 we present results on the contraceptive's effect on births, abortions and aggregate human capital endowments. Finally, section 6 concludes.

2 The History of the Emergency Contraceptive Pill

The emergency contraceptive pill is a hormonal treatment which can be used within 5 days of an unprotected sexual relationship to reduce the probability of conception. There are a number of alternative types of emergency contraceptive pills, however principally these are composed of doses of the progestin levonogestrel, or a combined dose of estrogen and progestin. Typically these are taken as a single pill or two pills in a 12 hour period (von Hertzen et al., 2002), however similar doses of hormones can be obtained by combining normal birth control pills (Ellerson et al., 1998).

This form of contraception has been shown to be relatively effective at avoiding undesired pregnancy. Estimates of around 75%-85% effectiveness based on typical usage are common, depending upon the method of emergency contraception used.[1] The success of these treatments is dependent upon the delay between intercourse and taking the drug, so widespread—or at least quickly available—access is important in reducing undesired pregnancies. While most effective when taken within 12 hours after intercourse, effectiveness can continue when taken within as much as 120 hours (von Hertzen et al., 2002).

The emergency contraceptive pill is not an abortive agent, but rather is a 'postcoital contraceptive' which acts to prevent the implantation of the fertilised egg cell (or blastocyst), and hence the formation of the embryo (Morris, 1973). This contraceptive method has been of clinical interest since at least the late 1960s (Demers, 1971), however access to these methods, either by prescription or over the counter, is still not universal. The fact that emergency contraception is non-abortive has meant that it is available in many countries in which abortion is absolutely prohibited, or prohibited in all cases except where concerns for maternal survival exist. Some countries have made the EC pill available as early as the mid-1980s (UK Family Planning Association, 1996), while many more countries have legalised this method of contraception during the last decade.

2.1 The Morning After Pill in Chile

The introduction of the emergency contraceptive pill in Chile has followed a complicated path, with early legislation frequently blocked by conservative groups in office and in civil society.[2] While initial discussions and administrative inquiries took place in 2001, it was not until 2005 that significant advances in legislature were made. In December of this year the Chilean supreme Court determined that the Institute of Public Health—the pharmaceutical regularity body of Chile—was *not* acting unconstitutionally by approving the provision of an emergency contraceptive drug on the pharmaceutical register. However, this finding was quickly challenged by detractors, with cases presented before ordinary and Constitutional tribunals (Casas Becerra, 2008).

[1] The WHO's Task Force on Postovulatory Methods of Fertility Regulation (1998), for example suggests that a levonogestrel routine reduces pregnancy rates by 85%, with a 95% confidence interval of 74-93%.

[2] The Chilean political framework is marked by a strong conservative axis, and a constitution which favours the maintenance of the status quo in economic and valoric policies. This has been the case since the return to democracy in 1990, with an alliance of right wing parties (and some members of the presiding left wing coalition) who have "resisted more liberal changes in the poorly named value judgements" (Casas Becerra (2008), p.6, author's translation.)

These tribunals were followed by a number of years' worth of legislations and litigations, which resulted in sporadic availability of the morning after pill, occasionaly freely available from state clinics or by purchase in private pharmacies. However, these were generally short-lived and emergency contraception was not consistently stocked, with both political and economic ramifications for groups providing access to the pill.[3] Details regarding this intervening process and laws passed by parliament theoretically requiring the provision of emergency contraception are discussed more fully in appendix A.

The period of interest for this study follows a decision taken by the Chilean Constitional Tribunal in 2008. This finding, responding to a demand placed by 36 parlamentary deputies in 2006, made it expressly illegal for the centralised health system to distribute the emergency contraceptive. This requirement held for all centres under direct administration of the national Ministry of Health, but, fundamentally, provided all municipal-level centres and hospitals the freedom to distribute the pill. Given that these centres are administered by the mayor of each municipality (or *comuna*), the availability in each municipality was entirely under the control of the mayor (Dides C. et al., 2011, 2010; Dides et al., 2009).[4] This resulted in a situation in which around half of the municipalities in Chile distributed the morning after pill freely, while the remaining half refused to distribute it, or to distribute it only in a very restrictive set of circumstances. At the level of the woman, her municpality's treatment status was essentially exogenously determined, being based on the whim of the mayor or representative public health bodies in her area of residence. This strange policy environment endured for approximately four years, until a law was passed mandating that the emergency after pill must be available to all women who request it. This new law became operational in May of 2013.

The Chilean context is one in which emergency contraception may be expected to have particularly important effects on pregnancy and maternal health. Abortion is entirely illegal in Chile, meaning that in the absence of emergency contraception, undesired or accidental pregnancies must either be taken to term, or a woman must risk undertaking a dangerous and illegal clandestine abortion (Shepard and Becerra, 2007). Figures on the frequency and method of clandestine abortion are unclear, however Shepard and Becerra (2007) suggest that the primary method is by taking the abortive drug misoprostol, which can be legally prescribed for treatment of ulcers. However, the cost of accessing this drug without prescription is high. Dated (2007) figures suggest prices of 38,000-50,000 Chilean pesos, or around one third of the minimum monthly wage at this time.

[3]For example, the subsecretary of health was removed from cabinet due to his announcement in 2005 that emergency contraception would be available to all women who sought it.

[4]Of the 346 municipalities in Chile, 320 have their own health systems, while the remaining 26 depend entirely upon the Ministry of Health. These 320 municipalities make up 94% of the population of Chile. Municipal health centres make up the majority of health centres in Chile. Of the 2501 registered health centres and hospitals, 2049 are under the control of municipalities (DEIS Ministerio de Educación, Gobierno de Chile, 2013).

3 Data

The data of interest for this study comes from matched administrative data files recording all live births and fetal deaths in Chile. We use birth outcomes for all women aged between 15 and 49 (inclusive) at some point during 2006-2011. This is crossed with data recording all women in Chile and their municipality of residence, resulting in a record of each woman, and her pregnancy status in each period (live birth, fetal death or no pregnancy). Along with a woman's birth status, we observe her baby's birth weight and gestational length in the case that a birth or fetal death was recorded.[5]

This results in a total sample of 1,391,565 births and 11,387 fetal deaths. The number of births per year in Chile has remained relatively stable over the last decade. Figure 1 displays total births, along with total fetal deaths during the period under study (a similar plot for births per women is available in figure 3). Total births vary between around 220,000-250,000 per year, while total fetal deaths recorded in the Ministry of Health data (all fetal deaths in any hospitals or clinics in Chile), vary between 1700 and 2100.

Our measure for the pill is a binary variable which records whether the emergency contraceptive was freely available to a woman upon request at her municipal health centre in the year before her birth outcome is observed. We consult two sources to collect data on pill availability. First, in each of 2009, 2010 and 2011 an independent survey was conducted, asking health care workers from each municipality whether they were able to prescribe the morning after pill (Dides et al., 2009; Dides C. et al., 2010, 2011). This should directly reflect the decision by each mayor regarding whether his or her municipality could prescribe the pill after the 2008 Constitutional ruling. In each case, survey respondents were also asked to list the circumstances in which they could prescribe the pill. All municipalities which reported that they could prescribe the pill freely to women were recorded as treated, while all others were recorded as untreated.[6] Secondly, the Ministry of Health has made available administrative data on all pill requests and dispursements at municipality clinics and hospitals. This allows us to determine the veracity of the survey data discussed above, while also providing concrete numbers regarding the use of the emergency contraceptive pill following the reform of interest. However, we do not use pill disbursements as the main measure of treatment. We focus on reported availability, given that disbursements are

[5]Since 1999, each baby born in Chile (in public and private hospitals and in private homes with midwives) is weighed and measured, and their gestational time is recorded. This data is collated by the Ministry of Health, and is available as a public birth weight census. As well as the baby's characteristics, the mother's education, age, labour market status and municipality of residence is collected. Similar details are collected in the case that a woman enters hospital and suffers a miscarriage.

[6]A small number of municipalities reported that they could prescribe the emergency contraceptive, however that this was only following cases of rape. These municipalities were classed as *un*treated given the lack of widespread availability.

endogenous, and jointly determined by demand as well as supply.

In total, 224 of Chile's 346 municipalities report being able to prescribe the pill in at least one year after the 2008 Tribunal result (see table 1). Figure 2 displays the quantity of municipalities reporting pill disbursements over time. Here, the number of prescribers increases over time in line with greater awareness of the legality of distributing the emergency contraceptive pill. While less than half of all municipalities report pill availability in 2009, this has increased to around two thirds by 2011. Official records of pill prescriptions suggest reasonably large fluctuations over time. While nearly 8000 women were reported as requesting the pill in 2009, this fell to slightly under 4000 the following year. Recent figures suggest that this number has been stable at around 6000-7000 requests in 2010-2013 (the most recent two years have been omitted from this study, and from graphical output, given that official birth records for 2012 and 2013 are not yet finalised). Figure 4 displays the geographic variation of pill availabilitly. This suggests that the pill is available in all parts of the country. With the exception of the large and very sparsely populated southern region of the country (the 10^{th} region) which has no municipal health centres, no obvious spatial patterns exist.

We examine fetal deaths as a manner to proxy illegal abortion. While it is certainly not the case that all (or even the majority) of fetal deaths observed in administrative data are results of abortive drugs, there is some evidence that these are the result of abortion in some cases, although they are recorded in a number of different ways in official figures to avoid criminal charges against women (Shepard and Becerra, 2007). To avoid concerns that reductions in fetal deaths may be simply due to greater investments in public health, we examine a number of subgroups of interest. Firstly we focus on deaths occurring between 1-20 weeks of gestation, as this is the period in which nearly all abortions are conducted. Secondly, we remove deaths which, based on their ICD code,[7] are clearly not related to abortion, such as those due to congenital malformations, deformations and chromosomal abnormalities. By using this methodology, a clear validity check exists by comparing reductions in fetal deaths during 1-20 weeks (which may represent abortions and should respond to the morning after pill), to those occurring from week 21 and above, which should be largely or entirely unaffected by emergency contraceptive availability.

Full summary statistics are provided in Table 1. These statistics are subdivided by whether or not the municipality has the pill in a given year. We see that there are some differences between pill and non-pill municipalities, such as higher education and health spending in pill municipalities. However, this is largely due to the fact that all years in which the pill was observed occur after 2008 while non-pill status is observed over the entire time period under

[7]The ICD refers to the International Classification of Disease, and refers to a set of standardised codes by which deaths can be classed. All deaths on the birth register report this code, (the ICD-10).

study. Surprisingly, we see that municipalities in both groups are approximately balanced in terms of the 'conservativeness' of the party of the mayor, however we do see that female mayors are more likely to be associated with pill municipalities. Later in this study we describe pre-treatment trends in pill and non-pill municipalities.

4 Methodology

We take advantage of the quasi-random nature of the expansion of the availability of the morning after pill to women in Chile. A woman i, living in municipality j in time t is considered as treated if public health centres report that the pill is available upon request. A woman's child bearing status $birth_{ijt}$ is regressed on the availability of the pill ($pill_{jt}$) in the preceding year:

$$birth_{ijt} = \alpha + \delta \cdot \mathbb{I}\{Pill_{jt-1}\} + \phi_t + \eta_j + \eta_j \cdot t + X_{jt-1}\gamma + \varepsilon_{ijt}. \quad (1)$$

In (1), full municipality and year fixed effects are included, and municipality-specific time trends are allowed. Standard errors are estimated which allow for auto-correlation by municipality, and as a robustness check by municipality and time (Cameron et al., 2008). The identifying variation in availability of the pill is by municipality and year. Prior to the legal reform the pill was unavailable to all women, while posterior to the reform the pill was available to those women living in municipalities where the mayor did not restrict access. This provides a flexible differences-in-differences (hereafter diff-in-diff) framework, and allows us to causally estimate the effect of the morning after pill if we believe that typical diff-in-diff assumptions hold. Namely, we require that unobserved components ε_{ijt} in the above specification evolve similarly over time in the treated and untreated municipalities.

Given the geographically disperse, and, as discussed in previous sections, plausibly exogenous nature of the arrival of the morning after pill, we may be willing to accept that this assumption is valid. However, to minimise the potential that spurrious events confound the arrival of the pill, we progressively include higher order time trends and other factors that vary non-linearly over time across municipalities. These factors, X_{jt-1}, include controls for political and social outcomes such as the mayor's party (and implicitly the conservativeness of views), the degree of voter support for the mayor, the mayor's gender, health and education inputs including staffing and training investments, and time varying measures of female empowerment by municipality.

We are interested in determining whether the morning after pill affects fertility at both the extensive and the intensive margins. We thus measure pregnancy in a number of ways: firstly, at the intensive margin by examining whether a woman gives birth at any parity level, and secondly, only at the extensive margin by examining whether she moves from 0 to 1 births. Similarly, we are interested in determining the degree of heterogeneity of access by age groups, and look at teenagers (15-19 year olds), 20-34 year olds, and 35-49 year olds.

Similar estimations are run replacing $birth_{ijt}$ with $fetal\ death_{ijt}$, which—for certain subsets—we believe proxy illegal abortion (as discussed in section 3). After assessing the pill's impact on pregnancy, and abortion we estimate reduced form effects of the pill's arrival on various measures of mother and child outcomes. These include maternal education, employment status and marital status, and child birthweight and gestational length. While we don't believe that these regressions are demonstrating causality in the case of mother's outcomes, these regressions are a useful test to determine whether certain groups are more likely to access the pill leading to aggregate compositional change in the cohorts of women who give birth.

4.1 Identifying Spillovers Between Municipalities

Our diff-in-diff estimates in the previous section potentially underestimate the true effect of the morning after pill. Principally, we may be concerned that there exist spillovers between treatment and control clusters due to the porous nature of municipal boundaries. Given that a woman can access municipal health centres in neighbouring comunas, if she is denied access to the pill in her comuna she may travel to obtain it elsewhere, or otherwise rely on the close geographic distance between her municipality and a treatment municipality to gain access to the morning after pill[8]. This motivates the following specification:

$$y_{ijct} = \alpha + \delta \cdot \mathbb{I}\{Pill_{jt-1}\} + \sum_{c=0}^{C} \zeta_c \cdot close_{cdjt-1} + \phi_t + \eta_j + \eta_j \cdot t + X_{jt-1}\gamma + \varepsilon_{ijct}. \tag{2}$$

where

$$close_{cdjt} = \begin{cases} 1 & \text{if } dist_{jt} > c \wedge dist_{jt} \leq c + d \\ 0 & \text{if } dist_{jt} \leq c \vee dist_{jt} > c + d. \end{cases}$$

This specification is identical to that in (1), however here we include a number *close* controls (indexed by c). These *close* variables are designed to capture spillovers between the pill treatment areas and surrounding areas which may also be affected by this treatment status, but which were not themselves treated. By judiciously selecting an appropriate series of *close* dummy variables,

[8]This may be the case for example if women rely on friends or contacts in neighbouring municipalities to gain access.

the true effect of the morning after pill can be recovered even in the case that spillovers occur between certain treatment and control clusters.[9]

To see this, define y_{1ijt} as the potential outcome for a woman in the presence of treatment. Likewise, y_{0ijt} is the potential outcome for a woman in the absence of treatment. As is well known in the treatment effects literature[10], difference-in-differences will allow us to estimate the causal effect of treatment if we are willing to make a common trends assumption about treament and control municipalities. Implicitly, this common trends assumption nests an assumption about spillovers between treatment and control muncipalities: that no such spillovers may exist, as these will affect the pre-existing trend in the control state.[11] This is analogous to the Stable Unit Treatment Value Assumption (SUTVA) of the Rubin Causal Model.

Now, rather than dealing with the two potential outcomes statuses above, we define new outcomes. First, we define y_{0ijtc}, the potential outcome for woman i in untreated municipality j in time t and with close status c. For simplicity, in what follows we will consider c as binary, indicating whether j is 'close' or 'not close' to a treatment municipality, although the results for a categorical variable follow logically. Similarly, we define y_{1ijtc} as the potential outcome in a treated municipality with close status c.[12]

As is typical in a double-differences framework, an additive structure for y_{ijtc} is assumed which consists of a municipality effect, a time effect, an indicator for treatment (D_{jt}), and in our case, an indicator for being 'close' to treatment ($close_{jtc}$):

$$y_{ijtc} = \eta_j + \phi_t + \delta D_{jt} + \zeta close_{jtc} + \varepsilon_{ijtc} \tag{3}$$

Now, if we consider the single differences which make up a double-differences estimate, we have, for the treatment group:

$$E[y_{ijtc}|j = Pill, t = 2, c = 1] - E[y_{ijtc}|j = Pill, t = 1, c = 1] = \phi_2 - \phi_1 + \delta. \tag{4}$$

This is the traditional single difference which forms one half of a typical double-differences estimator. However, in the case of the control group, the single difference is no longer simple. It

[9] Thus, these controls are determined by c, the minimum distance to a treatment cluster, and $c+d$, the maximum distance to the treatment cluster. For example, $close_{0,15,jt}$ will take the value of 1 for any municipality which does not itself prescribe the EC pill, but is within (0,15] km of a treatment municipality. Similarly $close_{15,15,jt} \Rightarrow (15, 30]$.

[10] See for example Card and Krueger (1994).

[11] Fortunately for the naive estimates of treatment effects in this case, any estimates will be attenuated rather than overstated, given that the mixture of treated units with the control group will cause outcomes in control group to look more like those in the treatment group.

[12] However, in the case of treated municipalities c will always take the value of 0 given that these municipalities are themselves treated rather than simply close to a treated municipality.

will now be made up of two components: the difference over time in control municipalities who are 'close' to treatment municipalities:

$$E[y_{ijtc}|j = NoPill, t = 2, c = 1] - E[y_{ijtc}|j = NoPill, t = 1, c = 1] = \phi_2 - \phi_1 + \zeta \tag{5a}$$

and the difference over time for 'non-close' control municipalities:

$$E[y_{ijtc}|j = NoPill, t = 2, c = 0] - E[y_{ijtc}|j = NoPill, t = 1, c = 0] = \phi_2 - \phi_1. \tag{5b}$$

If we were to naively combine close and non-close control municipalities to make one large control group, we would have that our second difference consists of the weighted sum of (5a) and (5b). Were we then to combine the first difference (4) and the second difference (the weighted average of 5a and 5b) to form our double-differences estimator, this would give:

$$\begin{aligned} &\{E[y_{ijtc}|j = Pill, t = 2, c = 1] - E[y_{ijtc}|j = Pill, t = 1, c = 1]\}- \\ &\Bigg(\frac{N_c}{N_c + N_{nc}}\{E[y_{ijtc}|j = NoPill, t = 2, c = 1] - E[y_{ijtc}|j = NoPill, t = 1, c = 1]\}+ \\ &\frac{N_{nc}}{N_c + N_{nc}}\{E[y_{ijtc}|j = NoPill, t = 2, c = 0] - E[y_{ijtc}|j = NoPill, t = 1, c = 0]\}\Bigg) = \\ &\delta - \frac{N_c}{N_c + N_{nc}}\zeta. \end{aligned} \tag{6}$$

Here we clearly see that our naive estimator fails to recover the true parameter of interest δ.[13] Generally, we would suspect that if spillovers exist, then they are likely to be of the same direction as the effect of treatment, meaning that δ and ζ will have the same sign. If this is the case, the inclusion of $\frac{N_c}{N_{nc}+N_c}\zeta$ in the naive estimate attenuates the treatment effect.

However, in the above discussion, we are concerned that SUTVA has been violated in a specific manner. We are concerned that the treatment status of women in treatment municipalities spillover to those of control muncipalities 'close' to these treatment municipalities. Conversely then, we assume that women in control muncipalities 'far enough away' from those in treatment municipalities are not affected by their treatment status, and so SUTVA still holds in these cases. Specifically, imagine that our double-difference estimator is now only based upon those control municipalities which are not classed as belonging to *close*. In this case:

$$\begin{aligned} &\{E[y_{ijtc}|j = Pill, t = 2, c = 1] - E[y_{ijtc}|j = Pill, t = 1, c = 1]\}- \\ &\{E[y_{ijtc}|j = NoPill, t = 2, c = 0] - E[y_{ijtc}|j = NoPill, t = 1, c = 0]\} = \delta, \end{aligned} \tag{7}$$

[13]This estimator includes as a limiting case the typical diff-in-diff estimator, as in this case we assume that no spillovers are present and SUTVA holds, meaning that $\zeta = 0$. Similarly, if no municipalities were close enough to experience spillovers, we would have that $N_c = 0$, and once again δ would be recovered.

and using the sample analogue of these population parameters we are able to correctly recover the true effect of interest. Discussion of how to precisely determine which municipalities are and are not part of the *close* group is delayed until section 5.3.

Estimating (2) provides a flexible regression-based framework for (7). Both $Pill_{jt}$ and $close_{cjdt}$ switch on only in those municipalities who are affected by the pill (either directly or via spillover) in the date after the morning after pill has become available. In this case both δ and ζ (from 2) identify the effect of living in a pill or close-to-pill municipality by comparing them to treatment municipalities which are sufficiently far from the morning after pill that we can plausibly assume SUTVA. In the case of the coefficient on pill this is simply estimating our effect of interest (7), while the coefficient on *close* identifies the marginal effect of being close to the pill.[14] Given that we are assuming geographic dependence in these estimates, we use Conley's (1999) spatial standard errors. This involves defining a reasonably flexible covariance matrix which inversely weights observations to allow for dependence across individuals based on distance.

5 Results

5.1 The Effect of Emergency Contraception on Births

Table 2 provides estimates for specification (1). This has been estimated using a logistic regression, and all coefficients are cast as log odds. Here, we examine two fertility outcomes: the probability that a woman gives birth to any child (columns 1-4), and the probability that a woman gives birth to her first child (columns 5-8). The latter outcome captures just the effects of the emergency contraceptive pill at the extensive margin, while the prior outcome captures both extensive (first birth), and intensive (more births) effects.

In each case we estimate first the simple diff-in-diff specification without time-varying controls, and then gradually add time varying controls which may confound results of the original specification. Initial results suggest that the effect on pregnancies may be large, particularly so for teenagers. Point estimates on "All Births" for the 15-19 year old group suggest that the pill is associated with a highly significant 6.2% reduction in pregnancy (1-exp(-0.064)), or a

[14] In the framework above, this can be viewed as:

$$\{E[y_{ijtc}|j = NoPill, t = 2, c = 1] - E[y_{ijtc}|j = NoPill, t = 1, c = 1]\} - \\ \{E[y_{ijtc}|j = NoPill, t = 2, c = 0] - E[y_{ijtc}|j = NoPill, t = 1, c = 0]\} = \zeta.$$

4.0% reduction when including potentially confounding time-varying controls. The coefficients on these time varying controls are omitted from table 2 for the sake of clarity; however a full output for column (4) of each panel is provided in appendix table 7.

When we examine the effect only on first births, we see a somewhat smaller, but still important 3.5% reduction in births (or an imprecisely estimated 2.1% reduction when including controls for condom availability). This "First Births" column must necessarily be less than or equal to the effect of the morning after pill on all births, given that all births include first births, along with higher order births. The difference between the results in these two columns allows for a rough examination of the magnitude of extensive versus intensive effects on fertility. Were the entire effect of the pill working at the extensive (first birth) margin, we would expect that the coefficients for "First Births" should equal those on "All Births". As is, for the teenage group, we see that the coefficient on first births is 51% of that on all births, suggesting that while the extensive margin is important, the morning after pill also has important effects at the intensive margin.

The effects on older age groups are more moderate than the effect on teenagers, consistent with the fact that a greater proportion of teenage births are undesired. However, for 20-34 year olds we still see that access to the emergency contraceptive reduces pregnancy, by 3.0% for all births, and 2.1% for first births. In contrast to younger women, there appears to be no effect of the morning after pill on women aged 35 and above. All estimates for the 35-49 year old group are not significantly different to zero.

5.2 The Effect of Emergency Contraception on Abortions

In table 3, difference-in-difference estimates of the effect of the emergency contraceptive pill on fetal deaths are presented. Once again these are estimated using a logit model. In this case the denominator (or 0 in the outcome variable) is assigned to each live birth, while a fetal death is assigned a 1. All effects are thus interpreted as fetal deaths per live births. As discussed in section 3, by using certain subsets of fetal deaths we aim to proxy for illegal abortion. We expect that if the emergency contraceptive pill affects abortion, this should turn up in fetal deaths occurring from 0-20 weeks, however should not turn up in deaths occuring later in the gestational period, given that abortions rarely take place beyond the 20^{th} week.

Column (1) of table 3 presents the effect of the pill on *all* fetal deaths. We are, however, most intersted in columns (2) and (3), which present results for early (0-20 weeks), and late (21-39 weeks) respectively. In these columns we have removed from the sample any fetal deaths which have been classified in ICD class Q (a minority of fetal deaths), as these represent causes such as congenital defects, which are very unlikely to proxy abortion.

For the 15-19 year old group, significant evidence is found to suggest that the morning after pill may reduce the prevalence of (illegal) abortion. Some effect is found when examining the effect on all fetal deaths, however when this is examined by subgroups, the effect is entirely driven by early gestation deaths. The size of the coefficient is empirically very important: it suggests a reduction in early gestation deaths by 55%, which we interpret as strong evidence in favour of reductions of illegal abortion. When compared to the null effect on late-term deaths, this seems to provide more support to this claim.

A similar pattern is observed for the 20-34 year old group of women, however effects are smaller and somewhat less significant. While no significant effect is found when examining all births, there is evidence (at the 10% significance level), that the arrival of emergency contraceptive reduces early gestation deaths by 17%. Once again, no significant effect is found in late gestation fetal death.

The group of women aged 35 years and above is somewhat less clear, and, while the effect sizes of the coefficients follows the pattern outlined above, the significance on late gestation fetal deaths is somewhat surprising. Given that fetal death is much more common as maternal age increases, it is perhaps unsurprising that we find some effect for this group. One possible explanation for this finding is that the morning after pill allows less healthy women to select out of child bearing, although given the lack of covariates recording mother's health at the time of childbirth, this cannot be explored fully.

5.3 Municipality Spillover and Imperfect 'Compliance'

We augment our naive estimates from sections 5.1 and 5.2 to account for between-cluster spillovers in table 5. These results are estimated according to equation (2) using a logit regression. The discussion is section 4.1 proposes including controls for areas which are 'close enough' to treatment municipalities that they are likely to be affected by spillovers. However, discussion regarding how to determine the threshold has been putoff until this point. In this section we examine two related ways which this can be done. Both of these ways rely on the data and specific context of the treatment in question to determine the range over which municipalities should be considered as close.

The first method involves a series of consecutive regressions and tests on the coefficient $\widehat{\delta}$. First, a regression is run including no close controls and $\hat{\delta}^0$ is observed along with its standard errors (where superscript 0 refers to the number of close controls included). Then, a single close control is included for municipalities within d km of the treatment municipality (where d can be some small number). From this regression, we observe $\hat{\delta}^1$, and test for the equality of δ^0 and δ^1 using a t-test. If this test is rejected, we add another close control, this time indicating muncipalities located within d and $2d$ km of the treatment municipality. Again we run a t-test for the equality of δ^1 and δ^2. This iterative process is continued until the point that we cannot reject the test that $\delta^{C-1} = \delta^C$. At this point we accept that we have saturated our model with sufficient 'close' controls to recover a consistent estimate of δ, and assume:[15]

$$\left|\delta - \frac{c}{c+nc}\zeta\right| \simeq |\hat{\delta}^0| < |\hat{\delta}^1| < \cdots < |\hat{\delta}^{C-1}| = |\hat{\delta}^C| \simeq |\delta|. \tag{8}$$

The second method follows a similar iterative process, but rather than testing each δ^c against its predecesor, we run a t-test with the null: $\zeta = 0$. The logic in this case is that rather than assuming that we have a consistent estimate of δ once this coefficient is stable, we assume that we have included enough 'close' municipalities once spillover effects are no longer found in the marginal municipality.

In order for these methodologies to uncover a consistent estimate of δ, all we require is that there actually are at least *some* control municipalities far enough away from treatment municipalities in which no spillover effects are felt. As described in (7), these 'non-close' municipalities act as the control group for our diff-in-diff estimator, so if no non-close municipalities exist, no consistent estimator can be formed. Given the relatively large distance between some non-treated municipalities and their nearest treated counterpart in the Chilean context, this seems unlikely in this case, although we cannot reject this formally.

Panel A of table 5 estimates using this methodology. In this case, using either of the above methods results in an identical number of close controls. For both 15-19 year olds and 20-34 year olds, it appears that living within 30 km of a treatment municipality results in a spillover effect, while for the case of 35-49 year olds no spillover is observed. Now, based on these updated estimates it appears that the true effect of the morning after pill may be significantly higher than that estimated in section 5.1. Compared to the 4.1% reduction in teenage births estimated from specification (1), here we estimate a 6.9% reduction for women living in treated municipalities $(1-$

[15] One situation in which this will not provide a consistent estimate of δ is the case in which spillovers converge, but do not converge to zero. If for example beyond a certain distance C the effect of spillovers reach some fixed constant, then the null that $delta^{C-1} = \delta^C$ will not be rejected, even though the marginal ζ term is not equal to zero.

exp(−0.071)), with sizeable effects also found for those living in close, non-treated municipalities. Similar patterns are observed for the 20-39 year old group, however in this case estimates are increased in magnitude from 3.0% to 4.2%. Figures 5 and 6 provide graphical support of this methodology. Here, estimates are calculated based on a wide range of *close* controls, with a step size d of 2.5 km. In each case, the estimated $\hat{\delta}$ appears to converge when controlling for spillovers of up to 30km.

In Panel B, similar tests are run for fetal deaths. In this case it appears that spillovers act only over a shorter range or not at all, however this may owe partially to the fact that fetal death is a relatively uncommon event, so estimates are more imprecise. Once again however, the inclusion of close municipality controls act to increase the magnitude of treatment effects estimated. For 15-19 year olds, point estimates move from a 51.7% reduction in early-gestation fetal deaths, to a 56.6% reduction, and for 20-39 year olds these point estimates move from 13.0% to 16.1%, however it is worth noting that these changes are not statistically significant.

5.4 Emergency Contraception and Aggregate Human Capital at Birth

Table 4 examines the effect of emergency contraception on aggregate human capital indicators of pregnant women and newborn babies. While it is not suggested that the morning after pill itself will affect a woman's human capital attainment over such a short time frame, if certain subgroups of the population are more likely to access the contraceptive, it is likely that aggregate compositional changes will be seen in both maternal and child human capital outcomes. There is considerable evidence to this effect when considering access to the oral contraceptive pill (Bailey et al., 2012; Ananat and Hungerman, 2012; Chiappori and Oreffice, 2008), and the arrival of legal abortion (Whitaker, 2011; Ananat et al., 2009).

We examine three outcome variables for mothers: years of education, employment status, and a binary variable for marriage, and three outcome variables for newborns: weight at birth, weeks of gestation, and length (in cm) at birth.[16] Each model is estimated as outlined in (1) using OLS. Surprisingly, we find that the morning after pill has had no, or very little, effect on aggregate human capital indicators. This is the case among mothers, and consequently among newborn babies.

Panel A of table 4 presents estimates by age group. For both teenagers and 20-34 year olds, no effect is seen on any of the variables examined. In general, these results seem to suggest that

[16] These outcomes, particularly birthweight, have been shown to improve outcomes including educational attainment and income throughout life (Behrman and Rosenzweig, 2004)

access to the morning after pill is transversal, and is not centred on highly educated or employed women. Moving to the 35-49 year old group, some evidence exists to suggest that the aggregate education of women giving birth is slightly higher where the morning after pill is available. This would be consistent with less educated (and perhaps less healthy) women selecting out of child bearing in this age group, which is consistent with the results found for this age group in table 3.

Panel B provides estimates for all children born over the period under study. Once again, very little evidence is found to suggest that the emergency contraceptive pill has created large-scale compositional effects to birth cohorts. Given the lack of effect found in mothers, it is not surprising that similar results are found in babies. In each case, no effect is observed on birthweight, gestational period, or length at birth (with the exception of a very small reduction in gestational length for babies born to 20-34 year olds). Each of the reported significance levels is based on a two-tailed t-test. If we were to correct for multiple hypothesis testing using a Bonferroni correction, finding a significant result would be even less likely.

5.5 Placebo Tests

Robustness of the main estimates to the addition of time-varying controls and municipal-specific time trends provides some confidence in the results, however does not directly examine the differential trends assumption underlying diff-in-diff estimation. In order to examine this assumption more closely, we run a number of placebo tests. These placebo tests allow us to examine whether the results are driven by pre-existing differences or trends in treatment and control municipalities.

We thus run analogous tests to (1) and (2), however rather than looking at births following the introduction of the pill, we examine births *preceding* the introduction of the pill. The logic underlying these tests is that if is the arrival of the EC pill which reduces undesired births, then there should be no difference between trends in births in pill and non-pill municipalities in years preceding the reform. If however, the effects are due to general differences in trends in non-pill and pill municipalities, we may expect that an effect would be seen even in the absence of the EC pill. We this run the following series of tests:

$$birth_{ijt-l} = \alpha + \delta \cdot \mathbb{I}\{Pill_{jt}\} + \phi_t + \eta_j + \eta_j \cdot t + \varepsilon_{ijt}, \tag{9}$$

where l refers to a series of lags $l \in 3, 4, 5$ years. We choose lags of at least 3 years so that all births observed will occur entirely before the arrival of the EC pill in 2008.

These results are presented in table 6, both for specification (9) and an analogous specification where placebo close municipalities are defined. These placebo tests support the diff-in-diff specification estimated. In all but 3 of 30 coefficients, small and statistically insignificant results are observed on placebo treatments. In 3 of 30 cases, significant effects are found, although these are always on placebo 'close' treatments, and not on the main treatment itself. Generally this is quite strong evidence in favour of an absence of pre-treatment differential trends, as at 10% significance levels, it is expected that approximately 3 in 30 coefficients should be falsely accepted (ie a type I error should occur).

Along with these formal placebo tests, we can examine trends by eye based on full data on all births occurring in Chile in the past decade and a half. We present graphical results as appendix figures 7 and 8. These figures suggest that indeed, the sharp discontinuity in births occurs precisely following the arrival of the EC pill to Chile: further evidence in favour of these results owing to the morning after pill, rather than to alternative actions taken in pill and non-pill municipalities.

6 Conclusions

This study provides the first censal estimates of effect of the emergency contraceptive pill. In contrast to existing studies based on data from the United States, this study focuses on a reform in Chile, a country with high rates of teenage pregnancy and undesired childbearing, and where abortion is entirely outlawed. The lack of abortion or other post-coital birth control technologies means that the arrival of the emergency contraceptive pill heralded the first opportunity for women to control fertility in cases where alternative forms of birth control were not used or failed during intercourse.

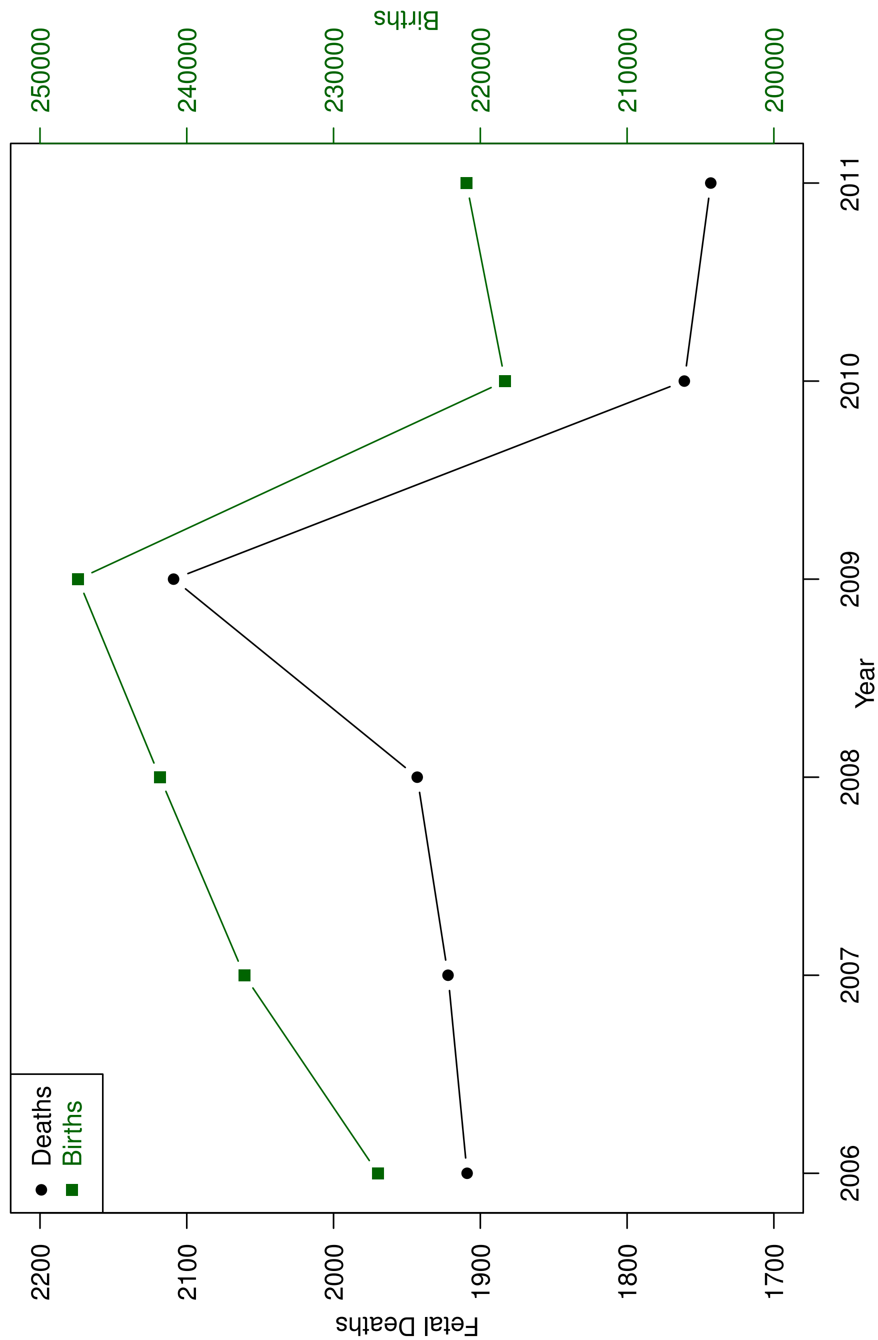
By taking advantage of a legal finding which left decisions regarding the availability of the morning after pill in the hands of the mayor of each of Chile's 346 municipalities, we estimate the effect of this technology on fertility, abortion and aggregate human capital outcomes. In contrast to the literature currently available, we find the emergency contraception has large and significant effect on births and early gestation fetal deaths. For teenagers, this effect is estimated to be a reduction of 6.9% and a remarkable 55% in births and early-gestation fetal deaths respectively, while for 20-34 year old women these figures are a smaller, but still significant 4.1% and 16.0%. It is argued that these early-gestation deaths proxy for illegal abortion, and comparisons with late term deaths add support to this claim.

Given the permeable nature of municipal boundaries within a country, we examine the possibility that the arrival of the pill to a given municipality is not restricted only to women who live within its boundaries. Results suggest that this may be the case, and that treatment spillovers may endure for as much as 30km. We propose an identification strategy which flexibly allows for such spillover effects to be accounted for, while simulatenously recovering consistent estimate of the effect of the treatment in the presence of contaminated (local) control groups.

All told, this paper provides considerable evidence that emergency contraception may play an important role in a woman's contraceptive behaviour. This finding is of particular importance to the country under study given that only recently has law been implemented making the morning after pill available to all. This also suggests that despite evidence to the contrary in the United States, the emergency contraceptive pill may be an important interim technology in the many countries which currently do not allow alternative forms of post-coital contraception.

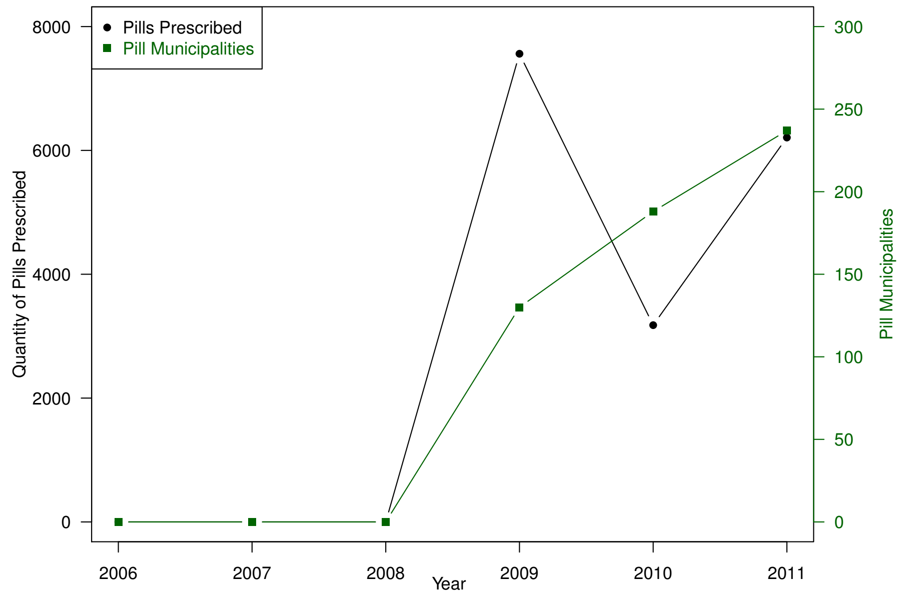
Figures

Figure 1: Total Recorded Births and Fetal Deaths, 2006-2011



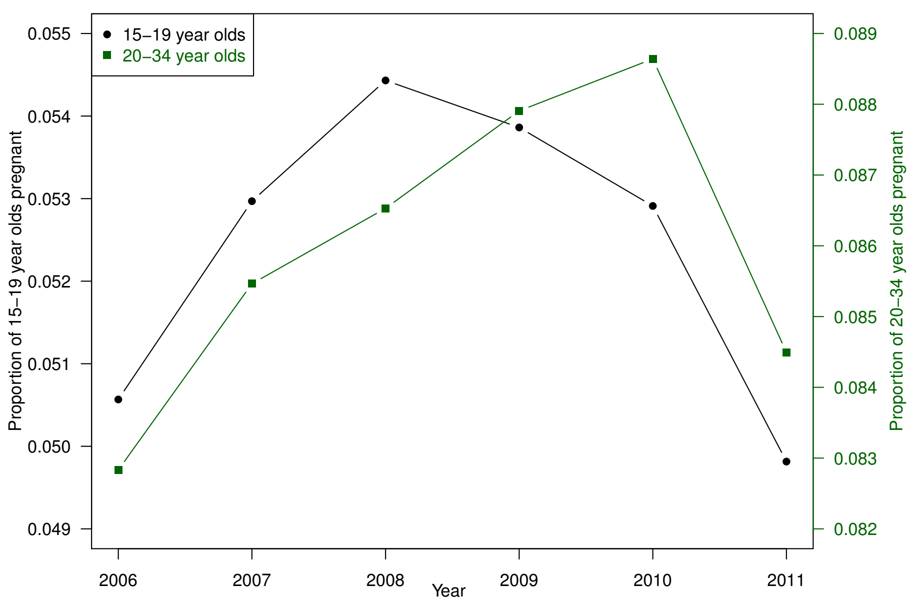
Note: Data on pregnancies and fetal deaths comes from the Ministry of Health's birth census

Figure 2: Pill Prescriptions and Availability by Time



Note: Prescription data is from the Ministry of Health's administrative data on medications and medical attention.
Municipality data is from an independent survey conducted by Dides et al, (2010;2011;2012).

Figure 3: Pregancies by Age Group and Time



Note: Data on pregnancies comes from the Ministry of Health's birth census

Figure 4: The Availability of the Pill by Geographic Region

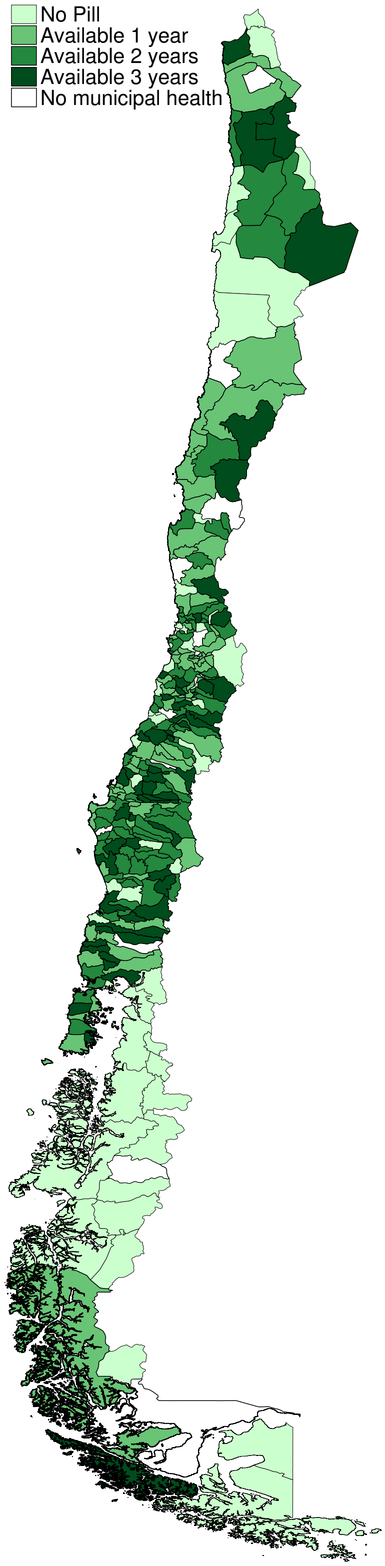


Figure 5: Estimates of $\hat{\delta}^c$ for Pregnancy (15-19)

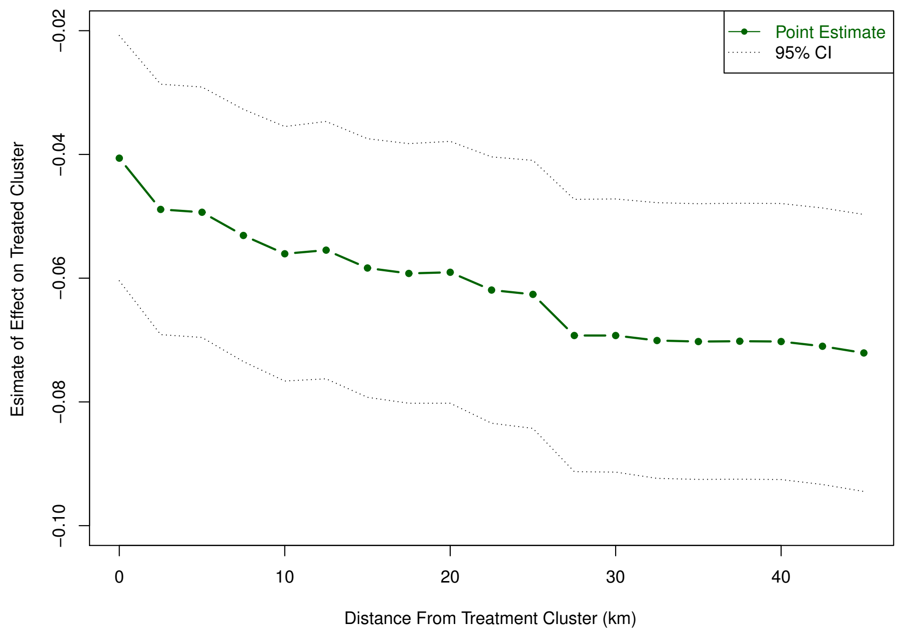
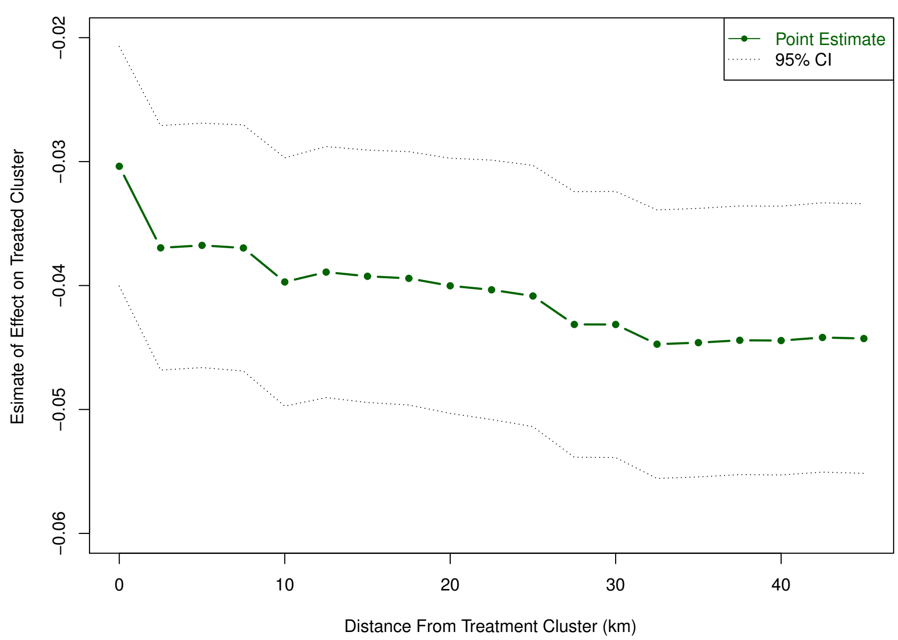


Figure 6: Estimates of $\hat{\delta}^c$ for Pregnancy (20-34)



Tables

Table 1: Summary Statistics

	No Pill Available	Pill Available	Total
Municipality Characteristics			
Poverty	16.4	17.0	16.5
	(7.48)	(7.71)	(7.52)
Conservative	0.285	0.291	0.286
	(0.451)	(0.454)	(0.452)
Education Spending	4,762	5,234	4,838
	(5,479)	(5,482)	(5,482)
Health Spending	1,842	2,333	1,921
	(2,595)	(2,830)	(2,640)
Out of School	4.07	3.99	4.06
	(3.17)	(3.10)	(3.15)
Female Mayor	0.119	0.135	0.121
	(0.323)	(0.342)	(0.326)
Female Poverty	60.4	60.7	60.5
	(10.61)	(9.64)	(10.5)
Pill Distance	5.11	0.00	4.29
	(16.2)	(0.0)	(15)
Individual Characteristics			
Live Births	0.054	0.054	0.054
	(0.226)	(0.226)	(0.226)
Fetal Deaths	0.0562	0.0457	0.0545
	(0.27)	(0.24)	(0.266)
Birthweight	3322.7	3334.3	3324.7
	(540.0)	(542.3)	(540.4)
Maternal education	11.92	12.03	11.94
	(2.967)	(2.894)	(2.955)
Percent working	0.295	0.395	0.312
	(0.456)	(0.489)	(0.463)
Married	0.340	0.309	0.335
	(0.474)	(0.462)	(0.472)
Age at Birth	27.05	27.15	27.07
	(6.777)	(6.790)	(6.779)
N Comunas	346	224	346
N Fetal Deaths	9,846	1,541	11,387
N Births	1,188,579	202,986	1,391,565

Notes: Group means are presented with standard deviations below in parentheses. Poverty refers to the % of the municipality below the poverty line, conservative is a binary variable indicating if the mayor comes from a politically conservative party health and education spending are measured in thousands of Chilean pesos, and pill distance measures the distance (in km) to the nearest municipality which reports prescribing emergency contraceptives. Pregnancies are reported as % of all women giving live birth, while fetal deaths are reported per live birth. All summary statistics are for the period 2006-2011.

Table 2: The Effect of the Morning After Pill on Pregnancy

	All Births				First Births			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
15-19 YEAR OLDS								
Morning After Pill	−0.064***	−0.065***	−0.057***	−0.041***	−0.036**	−0.041***	−0.036**	−0.021
	(0.015)	(0.015)	(0.015)	(0.016)	(0.015)	(0.016)	(0.016)	(0.016)
Observations	4,152,490	4,152,490	4,152,490	4,152,490	4,125,336	4,125,336	4,125,336	4,125,336
McFadden's R^2	0.670	0.670	0.671	0.673	0.633	0.634	0.636	0.637
20-34 YEAR OLDS								
Morning After Pill	−0.040***	−0.039***	−0.038***	−0.030**	−0.024*	−0.029**	−0.029*	−0.021
	(0.010)	(0.011)	(0.012)	(0.012)	(0.014)	(0.014)	(0.015)	(0.015)
Observations	11,022,111	11,022,111	11,022,111	11,022,111	10,458,703	10,458,703	10,458,703	10,458,703
McFadden's R^2	0.772	0.772	0.773	0.773	0.684	0.685	0.686	0.686
35-49 YEAR OLDS								
Morning After Pill	0.001	0.001	0.008	0.006	0.042	0.039	0.042	0.033
	(0.013)	(0.013)	(0.013)	(0.013)	(0.036)	(0.038)	(0.039)	(0.040)
Observations	10,572,196	10,572,196	10,572,196	10,572,196	10,376,895	10,376,895	10,376,895	10,376,895
McFadden's R^2	0.537	0.537	0.538	0.538	0.641	0.641	0.642	0.642
Trends & FEs	Y	Y	Y	Y	Y	Y	Y	Y
Political Controls		Y	Y	Y		Y	Y	Y
Health, Educ, Gender Controls			Y	Y			Y	Y
Condom Availability				Y				Y

NOTES: All Births and First Births are binary variables taking the value of 1 in the case that a women gives live birth and that this occurs at any birth order, or is her first birth (respectively). All models are estimated using logistic regression and include comuna and year fixed. Standard errors are clustered at the level of the comuna. All coefficients are reported as log odds and in each case Pill is a binary variable referring to the availability of the morning after pill in the woman's comuna and (lagged) year. Political controls include party dummies for the mayor in power, the mayor's gender, and the vote margin of the mayor. Health and education controls include the percent of girls out of highschool, education spending by both the municipality and the Ministry of Education and total health spending and health spending on staff and training. Gender controls are the percent of female heads of households living below the poverty line, and the percent of female workers in professional positions in the Municipality. *p<0.1; **p<0.05; ***p<0.01

Table 3: The Effect of the Morning After Pill on Fetal Deaths

	All Deaths	Early Gestation	Late Gestation
15-19 YEAR OLDS			
Morning After Pill	−0.131	−0.728***	−0.078
	(0.083)	(0.189)	(0.113)
Mean (deaths/live birth)	0.008	0.002	0.005
Observations	219,608	218,388	218,911
McFadden's R^2	0.233	0.379	0.254
20-34 YEAR OLDS			
Morning After Pill	−0.041	−0.139	−0.035
	(0.049)	(0.106)	(0.057)
Mean (deaths/live birth)	0.007	0.002	0.004
Observations	954,424	949,477	951,577
McFadden's R^2	0.199	0.386	0.171
35-49 YEAR OLDS			
Morning After Pill	−0.460***	−0.738***	−0.502***
	(0.081)	(0.216)	(0.101)
Mean (deaths/live birth)	0.012	0.003	0.007
Observations	228,920	227,029	227,781
McFadden's R^2	0.261	0.411	0.239

NOTES: Total fetal deaths for each group are 1,748, 7,014, and 2,625 for 15-19, 20-34 and 35-49 year olds respectively. All regressions include year and comuna fixed-effects, and comuna-specific trends. Each regression also includes the full set of time varying controls described in table 2. Standard errors are clustered by comuna. *p<0.1; **p<0.05; ***p<0.01;

Table 4: Emergency Contraception and Aggregate Human Capital

	15-19 year olds			20-34 year olds			35-49 year olds		
PANEL A: MOTHER CHARACTERISTICS	(1) Yrs Educ	(2) Working	(3) Married	(4) Yrs Educ	(5) Working	(6) Married	(7) Yrs Educ	(8) Working	(9) Married
Morning After Pill	0.022	-0.002	0.000	0.001	-0.004*	-0.003	0.061**	-0.004	-0.001
	(0.021)	(0.002)	(0.001)	(0.014)	(0.002)	(0.005)	(0.028)	(0.005)	(0.007)
Observations	131,605	131,746	131,614	896,230	897,363	896,318	198,885	199,472	198,906
R^2	0.02	0.01	0.01	0.14	0.04	0.17	0.21	0.03	0.247
PANEL B: CHILD CHARACTERISTICS	(1) Weight	(2) Gestation	(3) Length	(4) Weight	(5) Gestation	(6) Length	(7) Weight	(8) Gestation	(9) Length
Morning After Pill	-1.377	-0.020	0.039	-0.636	-0.023***	0.02	-4.923	-0.016	0.030
	(5.944)	(0.019)	(0.028)	(2.532)	(0.008)	(0.016)	(5.602)	(0.016)	(0.024)
Observations	131,746	131,471	129,880	897,363	895,671	885,932	199,472	198,745	195,863
R^2	0.01	0.01	0.03	0.01	0.01	0.03	0.09	0.01	0.03

NOTES: Each column represents an OLS regression, and full controls listed in table 2 are included. Working and Married are binary variables, Weight is measured in grams, Gestation in weeks, and Length in centimetres. Summary statistics for these variables are available in table 1. Standard errors are clustered at the level of the municipality. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 5: The Morning After Pill and Treatment Spillovers

	15-19 Year olds	20-34 Year olds	35-49 Year olds
PANEL A: BIRTHS			
Morning After Pill	-0.071^{***}	-0.043^{***}	0.014
	(0.017)	(0.014)	(0.015)
Close < 15 km	-0.077^{***}	-0.042^{***}	0.018
	(0.021)	(0.014)	(0.017)
Close 15-30 km	-0.057^{**}	-0.014	0.019
	(0.023)	(0.013)	(0.023)
Close 30-45 km	-0.037	-0.016	0.031
	(0.035)	(0.028)	(0.030)
Observations	4,152,490	11,022,111	7,117,890
McFadden's R^2	0.674	0.774	0.527
PANEL B: FETAL DEATHS			
Morning After Pill	-0.834^{***}	-0.175	-0.942^{***}
	(0.226)	(0.131)	(0.283)
Close < 15 km	-0.154	-0.022	-0.200
	(0.235)	(0.151)	(0.240)
Observations	218,388	949,477	194,327
McFadden's R^2	0.380	0.386	0.417

NOTES: All models are estimated using logistic regressions, and coefficients are reported as log odds. Each regression includes comuna and year fixed effects and comuna-specific trends, and the full set of time-varying controls described in table 2. Conley (1999) standard errors are reported. *p<0.1; **p<0.05; ***p<0.01;

Table 6: Placebo Tests

	Lag = 3 years		Lag = 4 years		Lag = 5 years	
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: 15-19 YEAR-OLDS						
Morning After Pill	0.006	−0.018	0.003	−0.004	0.006	−0.018
	(0.014)	(0.017)	(0.014)	(0.029)	(0.014)	(0.017)
Close < 15 km		−0.047**		0.021		0.049
		(0.022)		(0.036)		(0.041)
Close 15-30 km		−0.001		−0.046		0.038
		(0.022)		(0.034)		(0.040)
Close 30-45 km		−0.047*		−0.028		0.089**
		(0.025)		(0.035)		(0.041)
Observations	4,123,049	4,123,049	4,075,854	4,075,854	4,017,339	4,017,339
McFadden's R^2	0.235	0.235	0.235	0.235	0.239	0.239
PANEL A: 20-34 YEAR-OLDS						
Morning After Pill	0.000	0.002	−0.006	0.006	0.000	0.002
	(0.008)	(0.017)	(0.008)	(0.019)	(0.008)	(0.017)
Close < 15 km		0.008		0.027		−0.010
		(0.017)		(0.022)		(0.021)
Close 15-30 km		−0.005		0.002		0.009
		(0.018)		(0.021)		(0.019)
Close 30-45 km		−0.001		−0.004		0.030
		(0.019)		(0.024)		(0.020)
Observations	10,773,289	10,773,289	10,699,388	10,699,388	10,639,773	10,639,773
McFadden's R^2	0.232	0.232	0.221	0.221	0.219	0.219

NOTES: All specifications are identical to those estimated in tables 2 and 5. However, instead of using births 1 year subsequent to the reform the outcome variable in each case is births and preceeding the reform by lag= $l \in 3, 4, 5$ years, and hence entirely unaffected in both treatment and control municipalities. *p<0.1; **p<0.05; ***p<0.01.

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Appendices

A The Chilean Legislative Environment and the Adoption of Emergency Contraception

Discussions surrounding the introduction of emergency contraception in Chile have taken place since at least 1996, when the Chilean Institute of Reproductive Medicine (ICMER for its initials in Spanish) proposed the use of this method to avoid undesired pregnancies in a country where abortion was entirely outlawed (Dides Castillo, 2006). However, the first legislative attention given to this matter occurred when the aforementioned (see section 2.1) Institute of Public Health emitted a resolution allowing for the production and sale of 'Postinol', a drug containing levonogestrel by a Chilean laboratory in 2001. The Constitionality of this was quickly challenged, and the drug was prohibited by the Supreme Court.

The emergency contraceptive pill again entered legislative attention in 2004, following the Ministry of Health's publication of a guide suggesting that emergency contraception be used following cases of rape. Following this in 2005, the Subsecretary of Health Dr. Antonio Infante announced that emergency contraception would be freely available to *all* women who requested it, however the President of Chile and the Ministry of Health later declared that this was not the case, leading to removal of the Subsecretary from office.

In November of 2005, the Supreme Court of Chile provided the first constitutional support for the emergency contraceptive pill, voting 5-0 to reverse the decision taken in 2001, allowing emergency contraception to be provided in the case that the mother's life was in danger. Once again however, this finding was challenged shortly thereafter. The same non-governmental institution which had earlier raised a case against ICMER, now challenged the private commercial laboratory in charge of producing and distributing the drug. However, before this case could reach court, this laboratory voluntarily gave up their license to produce the drug, in a three line statement issued by the General Director of the company on February 14, 2006 (Casas Becerra, 2008).

In the same year, a group of 36 parliamentary deputies from conservative parties raised a case with the Constitional Tribunal, claiming that the provision of the emergency contraceptive pill contravened the "National Laws for the Regulation of Fertility", a set of rules issued by the Ministry of Health. This case was only resolved in 2008, with the Constitional Tribunal's finding in favour of this group, hence making illegal any provision by hospitals or health centres controlled by the Ministry of Health (and hence under the jurisdiction of the National Fertility Laws). Fundamentally however, this left the door open for Municipal health centres to distribute the pill freely to women. These Municipal Health Centres are run under the directive of the elected mayor of each Municipality, leaving all remaining legislation regarding the distribution of the pill up to the 346 mayors in Chile.

In this study We study the period surrounding this 2008 legislation as the cutoff of interest. However, even after this finding the emergency contraceptive pill has not been far from legislative action, with a number of other cases raised. These cases never entirely threatened the continuity of supply of the morning after pill by municipalities, however did cause some confusion for mayors and municipal health bodies in determining whether or not they were legally allowed to prescribe the contraceptive. These cases also resulted in the passing of a number of laws and standards. Most importantly, they resulted in national Law 20.418 which "creates standards for information, guidance and regulatory services in fertility" (author's translation), and the passing of a decree on March 3, 2013, which makes obligatory the provision of the morning after pill to women of any age in any health centre in Chile. This became operative on May 28, 2013, meaning that—at least officially—there are no longer any restrictions in place in the country.

B Data Appendix

With the exception of raw birth and fetal death data which requires that the user agree to a number of privacy clauses, all raw and processed data used in this paper is made available online at: `https://github.com/damiancclarke/morning-after-pill/tree/master/Data`. Birth and fetal death data can be downloaded online at: `http://www.deis.cl/descargar-bases-de-datos/` and we make available full processing scripts which convert this into the final dataset used here. In the remainder of this appendix, we provide further details regarding each data source used.

B.1 Main Data on Births and Fetal Deaths

Data on all births and deaths in Chile is publicly available for download at `http://www.deis.cl/descargar-bases-de-datos/`. This contains microdata registers of every birth and fetal death occurring Chile between 1999 and 2011. This is censal data, and is unlikely to miss any births given the importance of registering every child born with authorities in order to receive a national identity number used in all contact with public and private organisations including hospitals and schools. The main analysis in this paper is based on births and fetal deaths occurring between 2005 and 2011 (see table 1), however in placebo tests earlier birth data is also used.

B.2 Population Data

In order to link the number of births to the number of women of fertile age in each municipality and time period, we consult data from the National Institute of Statistics of Chile (INE). This is made available at `http://www.ine.cl/canales/chile_estadistico/familias/demograficas_vitales.php` and provides full demographics by age, municipality, and gender.

B.3 Time-Varying Municipality and Region Controls

Time-varying municipal controls such as education and health spending, and the number of females working in public government is downloaded from the National System of Municipal Information (SINIM). This provides data as far back as 2005, and is freely available for download online at `http://www.sinim.gov.cl/indicadores/busq_serie.php`.

Data on municipal elections, mayor's gender, party and vote share is accessed from the Electoral Service of Chile (SERVEL). This provides all electoral results from municipal elections for the full time period of this study. Raw data is available online at `http://www.servel.cl/ss/site/mobile/padron_electoral_comunal_por_ano_informe_comunal_anual.html` or processed as one line per municipality at the data page of the author's website linked to above.

Finally, we calculate data for alternative contraceptive use based on a series of regionally representative surveys collected every 3 years beginning in 1994. The National Survey of Youth asks respondents whether they use any method of contraception in both their first and most recent sexual activity. In the case that they did not use a condom, they are asked whether this is because they did not have access. Based on this survey, access to condom is calculated as an additional time-varying control. However, it should be noted that this variable can only be calculated at the level of the region (one level above the municipality), given that this data survey is not representative at the level of the municipality. Once again, processed data and processing scripts are made available at the data section of the author's site, and, if desired, raw data is available on the web: `http://extranet.injuv.gob.cl/Encuesta_Nacional_de_la_Juventud/contenido/index.php`.

C A Back of The Envelope Consistency Check of Effect Sizes

Using the official Ministry of Health data on the number of pills distributed in each year, we are able to determine whether the effect sizes identified in this study seem to be of reasonable magnitude. These calculations should of course be taken as illustrative only, given that we do not know if all pills distributed were taken by the recipient, nor the rates of pregnancy avoidance conditional upon taking the pill.

According to the adminstrative medications data, 16,857 emergency contraceptive pills were prescribed (in total) in 2009, 2010 and 2011. Of these, 5,736 were prescribed to women 18 years of age and younger, while the remaining 11,121 we prescribed to women over that age of 18. In order to have a rough idea of whether the estimates we find are reasonable, we can compare the approximate reduction in pregnancy estimated from our preferred specification, with the number of pills given out over the period of interest.

Given that the Ministry of Health's administrative data on prescriptions only records the ages of women accessing the pill as 18 and under and 19 and over, we estimate our specification for these two subgroups. We also calculate the total number of pregnancies in treated (and close to treated) municipalities during the period in which the pill was available. These figures are displayed in table 9. In order to determine the reduction in pregnancies which these estimates imply, we compare the theoretical number of pregnancies without the pill, to the number recorded with the pill. For example, in the case of the 18 and under group, the pill acts to reduce pregnancies by $1 - \exp(-0.069) = 0.067$, or 6.7%. So, we inflate the total number of pregnancies for this group (which was 20,713), suggesting that the total number of pregnancies without the pill would have been 22,612 (which we calculate as $\frac{20{,}713}{1-0.067}$). Thus, the approximate effect of the pill for this group is estaimated as a reduction of 22,200-20,713=1,487 pregnancies. Similar calculations can be run for each subset, to calculate the total estimated effect in each age group.

Based on this methodology, our estimates suggest that the pill accounted for 3,212 fewer pregnancies in the 18 and under age group, and 11,742 fewer pregnancies in the 18 and over age group.[17] Comparing these to total pill disbursements of 5,736 and 11,121, the estimated effects seem to be quite close to actual data on pills acquired. Although the estimates are slightly higher than expected for the 19 and over group (implying -0.86 births per pill dispursed) and perhaps

[17]The full calculation for the 18 and under group is:

$$\left(\frac{20,713}{1-0.067} - 20,713\right) + \left(\frac{10,370}{1-0.072} - 10,370\right) + \left(\frac{6,141}{1-0.048} - 6,141\right) = 2,596$$

slightly lower than expected for the 18 and under group (-0.45 births per pill dispursed), this back of the envelope consistency check performs remarkably well, and when considering the standard errors on our estimates, certainly falls within the margins that we would expect given the number of pill requests.[18]

If we instead compare the total pill disbursements over the period to the total estimated reduction in pregnancies,[19] this implies an efficiency rate of 71.9% (or that 71.9% of pills should result in an avoided pregnancy to account for the reduction in births. For reference, the United States FDA reports an effectiveness rate of 89% based on typical usage.

and a similar calculation for the 19 and over group gives

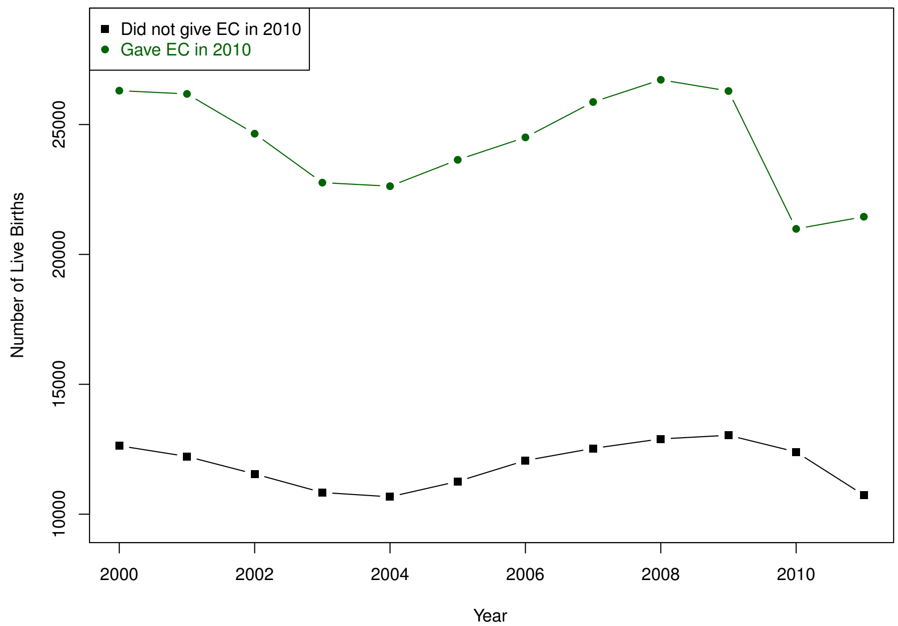
$$\left(\frac{172,557}{1-0.032} - 172,557\right) + \left(\frac{100,749}{1-0.032} - 100,749\right) + \left(\frac{48,756}{1-0.013} - 48,756\right) = 9,525.$$

[18]Further, when considering that pills may have be transferred between women who received the prescription and women who ultimately took the pill, we may be more interested in overall rates for both age groups.

[19]It seems reasonable to make such a comparison given the the spillover effects estimated in this paper suggest that the person accessing the pill may not be the same as the person using the pill.

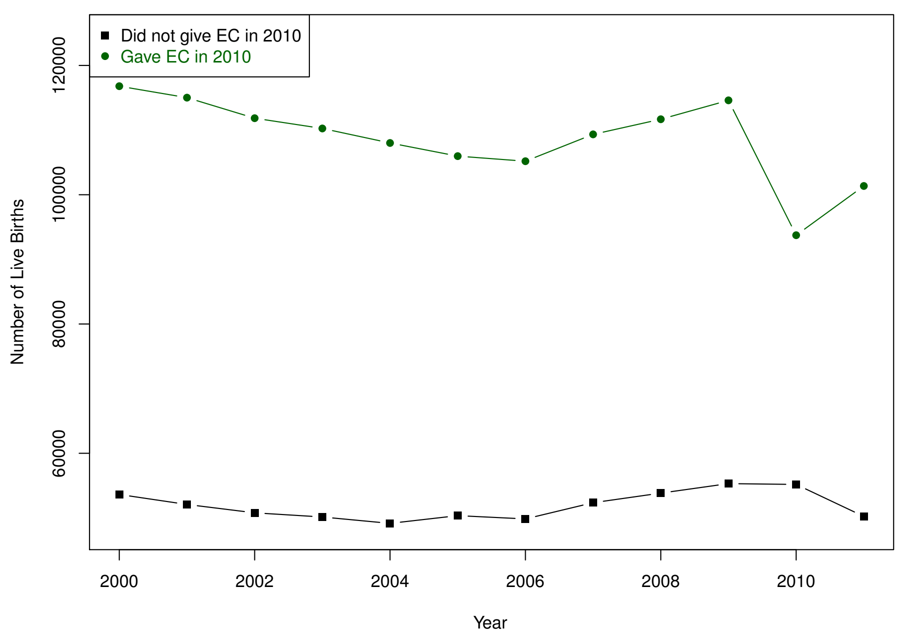
D Appendix Figures

Figure 7: Birth Trends 2000-2011: 15-19 Years



Note: Some municipalities which did not give the EC pill in 2010 did give the EC pill in 2011 (and vice versa).

Figure 8: Birth Trends 2000-2011: 20-34 Years



Note: Some municipalities which did not give the EC pill in 2010 did give the EC pill in 2011 (and vice versa).

E Appendix Tables

Table 7: The Morning After Pill and Pregnancy: Full Covariates

	Pregnancy		
	15-19 year olds	20-34 year olds	35-49 year olds
	(1)	(2)	(3)
Morning After Pill	-0.041***	-0.030***	0.006
	(0.010)	(0.005)	(0.010)
Female Mayor	0.016	-0.005	-0.007
	(0.026)	(0.013)	(0.026)
Mayor's Support	0.054	0.017	-0.129
	(0.084)	(0.042)	(0.085)
Out of School	-0.004	-0.001	-0.001
	(0.003)	(0.001)	(0.003)
Total Education Spending	0.001*	-0.00004	0.001**
	(0.0003)	(0.0002)	(0.0003)
Municipal Education Spending	-0.004***	-0.001**	-0.001**
	(0.001)	(0.0003)	(0.001)
Health Spending	-0.0002	0.0001	-0.0005
	(0.001)	(0.0003)	(0.001)
Health Training	-0.080***	-0.036***	0.005
	(0.024)	(0.012)	(0.025)
Health Staff	0.001	0.001***	-0.0004
	(0.001)	(0.0005)	(0.001)
Female Poverty	-0.0004	-0.0001	0.002***
	(0.001)	(0.0003)	(0.001)
Female Workers	-0.001	-0.0005	0.001
	(0.001)	(0.0004)	(0.001)
Years × Municipality	1,929	1,934	1,934

NOTES: Each model is identical to column (4) of table 2. A description of each variable is also provided in table 2. Municipality dummies and trends and political party dummies have been omitted for clarity. *p<0.1; **p<0.05; ***p<0.01

Table 8: The Morning After Pill and Fetal Death: Full Covariates

	Fetal Death (0-20 Weeks)		
	15-19 year olds	20-34 year olds	35-49 year olds
	(1)	(2)	(3)
Morning After Pill	-0.815***	-0.189*	-0.776***
	(0.237)	(0.113)	(0.217)
Female Mayor	0.987*	0.096	-0.270
	(0.593)	(0.293)	(0.528)
Mayor's Support	1.861	1.168	-0.416
	(1.886)	(0.989)	(1.783)
Out of School	-0.005	-0.003	0.074
	(0.083)	(0.032)	(0.064)
Total Education Spending	0.0001	-0.00003	-0.00003
	(0.0001)	(0.00004)	(0.0001)
Municipal Education Spending	0.0004*	0.0001	0.0001
	(0.0002)	(0.0001)	(0.0001)
Health Spending	-0.0001	0.0002**	0.0001
	(0.0002)	(0.0001)	(0.0001)
Health Training	0.005	-0.003	0.005
	(0.004)	(0.002)	(0.004)
Health Staff	0.00004	0.00004	0.0001
	(0.0002)	(0.0001)	(0.0002)
Female Poverty	0.017	0.002	0.002
	(0.016)	(0.007)	(0.013)
Female Workers	0.008	-0.006	0.018
	(0.019)	(0.008)	(0.014)
Years × Municipality	1,887	1,912	1,891
Akaike Inf. Crit.	2,594.943	4,244.940	2,811.065

NOTES: Each model is identical to column (2) of table 3. A description of each variable is also provided in table 2. Municipality dummies and trends and political party dummies have been omitted for clarity. *p<0.1; **p<0.05; ***p<0.01

Table 9: Back of the Envelope Calculation of Effect Sizes

	18 & Under (1)	19 & Over (2)
Morning After Pill	-0.069^{***}	-0.032^{***}
	(0.020)	(0.011)
Close < 15 km	-0.075^{**}	-0.032^{***}
	(0.029)	(0.012)
Close 15-30 km	-0.049^{*}	-0.013
	(0.030)	(0.012)
N Preg (pill)	20,713	172,557
N Preg (close 15)	10,370	100,749
N Preg (close 30)	6,141	48,756
Pills Disbursed	5,736	11,121

NOTES: Regression coefficients and standard errors are calculated in line with specification (2). The number of pills disbursed is calculated from administrative data described in figure 2, and number of avoided pregnancy is based on regression estimates and total births in administrative data. Further details are provided in appendix C. *p<0.1; **p<0.05; ***p<0.01