

ASSESSING PLAN B: THE EFFECT OF THE MORNING AFTER PILL ON CHILDREN AND WOMEN*

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We test whether the availability of the emergency contraceptive ('morning after') pill in the absence of legalised abortion can have effects similar to those of other large-scale contraceptive reforms. To do so, we examine a quasi-experimental policy reform occurring in Chile in 2008. Using vital statistics covering all births and foetal deaths over the period 2006–12, we show that the availability of the emergency contraceptive pill reduces pregnancy and early gestation foetal death, which we argue proxies for illegal abortion. Our results suggest that in the context of Chile, a country with among the most restrictive abortion laws in the world, the emergency contraceptive pill had effects around a third as large as various abortion reforms observed in other contexts.

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 (Goldin and Katz, 2002; Bailey, 2006, 2009), 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 article, we examine the effect of fully-subsidised provision of the emergency

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contraceptive (EC) 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 EC pill may have had surprisingly little effect on both pregnancy and abortion (Durrance, 2013; Gross *et al.*, 2014). Compared to the large body of evidence suggesting that contraceptive technologies such as the oral contraceptive and legal abortion can have significant and important effects on pregnancy rates, empirical studies of the effect of the EC pill have found small or non-existent shifts in fertility outcomes despite high rates of use.¹ However, existing analyses of the effect of the EC pill on birth rates have been limited in terms of the contexts examined. Existing quasi-experimental studies in the literature are focused on the US, where abortion is legal and hence the scope of the EC on net birth rates is likely to be limited, especially if abortion and the EC pill act as substitutable technologies.²

However, in many countries and regions, abortion is highly regulated, allowed in only extreme cases, or even outlawed entirely. Recent figures suggest that only one third of the world’s governments currently allow elective abortion or abortion for economic or social reasons, with this number being considerably lower in developing regions with high rates of fertility (United Nations, 2014). For areas in which abortion is not decriminalised, it is of considerable interest to determine whether the arrival of the EC pill in the absence of abortion is sufficient to provide similarly remarkable changes in fertility rates as those observed with the historical arrival of the first wave of contraceptive technologies to other areas.

A considerable literature on the effect of both the oral contraceptive pill and abortion, in the US and in other countries, suggests that both may have direct effects on rates of teen births of around -4% to -9% . Table 1 provides a summary of the range of estimated effects found in economic studies of contraceptive reforms. Although not a comprehensive meta-analysis, these quasi-experimental studies examining different contexts and national reforms often find broadly similar effects, suggesting that both abortion and the oral contraceptive pill alone resulted in significant reductions in births. In the US, these estimates suggest that both methods reduce childbearing at a young age by about 5.5% (mean estimate), while in Romania and Nepal, the effects are closer to 7% . In this article, we examine whether the EC pill alone (where legal abortion is not available) could have effects of similar magnitude and importance.

¹ For example, the CDC reports that in the United States between 2006 and 2010, 11% (5.8 million) women aged between 15 and 44 used the emergency contraceptive pill at least once. Among 20–24 year olds, the rate is even higher, at 23% (Daniels *et al.*, 2013).

² Indeed, this is explicitly suggested, although untested, in the economic literature. Gross *et al.* (2014), in one of the few quasi-experimental studies of the EC pill, present a model of contraceptive use where use of the EC pill depends on the outside cost of abortion. They find small effects in the USA, however state:

This paper studies the effect of EC during a time period in which abortion was legal. The effect of EC might be very different [were] abortion to be illegal (Bailey *et al.*, 2013; Joyce, 2013) (p. 73).

Table 1
The Estimated Effect of Reforms on Fertility (selected studies)

Author	Effect (SE)	Note
Panel (a): the contraceptive pill		
Bailey (2006)	-0.074 (0.057)	* ($x = 22$)
Guldi (2008)	-0.085 (0.041)	
Bailey (2009)	0.028 (0.048)	* ($x = 22$)
Kearney and Levine (2009)	-0.071 (0.024)	†
Bailey (2012)	-0.042 (0.019)	
Ananat and Hungerman (2012)	-0.088 (0.023)	
Panel (b): abortion		
Angrist and Evans (1996)	-0.012 (0.004)	* ($x = 19$)
Levine <i>et al.</i> (1996)	-0.019 (0.007)	‡
Gruber <i>et al.</i> (1999)	-0.059 (0.005)	§
Ananat <i>et al.</i> (2007)	-0.068 (0.012)	* ($x = 25$)
Bailey (2006)	-0.093 (0.043)	* ($x = 22$)
Guldi (2008)	-0.100 (0.054)	
Bailey (2009)	-0.012 (0.007)	* ($x = 22$)
Ananat <i>et al.</i> (2009)	-0.085 (0.020)	
Pop-Eleches (2010)	-0.068 (0.012)	¶
Ananat and Hungerman (2012)	-0.043 (0.015)	
Valente (2014)	-0.073 (0.027)	**
Panel (c): the morning after pill		
Gross <i>et al.</i> (2014)	-0.020 (0.020)	
Durrance (2013)	0.006 (0.036)	††

Notes. All figures report the results of short-term access of a national fertility reform on birth rates of teenage (15–19 years old) women unless otherwise specified in notes. *Binary model with outcome 1 = first birth by age x . †Estimate with no state trends reported. With state trends: $-0.047(0.013)$. ‡Estimate expressed as births per woman. Mean rate is 0.110. §Estimate for states adopting 1974–5. Estimate for 1971–3 is $-0.021(0.005)$. ¶This is the effect for 20–24 year old uneducated mothers. **This is the effect for all mothers. When expressed as a rate, it is -8.1% . ††State-specific analysis. Estimate is per additional % of participating pharmacies.

In order to examine the effect that the EC pill can have on mothers and children when abortion is not available, we turn to a particular empirical example. We focus on a plausibly exogenous policy decision in Chile which affects a woman's access to the

fully subsidised emergency contraceptive pill. We find evidence that, at least in the case of Chile, (a country with no access to legal abortion in any circumstances),³ access to emergency contraception does have an effect on birth rates, and that these effects are concentrated on teenagers and young women. We also find suggestive evidence that EC pill availability results in a reduction in the likelihood that women resort to illegal and risky clandestine abortions. With the arrival of the EC pill, we observe a reduction in early term foetal deaths in reform areas and no similar reduction in non-reform areas, while no similar effect is observed for late term foetal deaths. In the case of both births and deaths, the observed effects appear to be transversal rather than being centred on more highly educated women.

The reform under examination comes from a series of constitutional challenges between 2005 and 2008, which meant that the introduction of the emergency contraceptive pill in Chile was entirely controlled by the Supreme Court and Constitutional Tribunal. 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 EC 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 EC pill available, while the other half did not. Using administrative birth data and censal population data, we observe each pregnancy in the country leading to a birth or recorded foetal death, as well as municipal-level birth and foetal death rates.

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 foetal deaths. The arrival of this new technology is associated with reductions in these outcomes. It is estimated that among teenage girls, the widespread availability of emergency contraception reduces births by around 2.7% (though this estimate is not statistically significant), and reduces rates of early term foetal death (which we argue may reflect illegal abortion) by approximately 40%. Among older women and the entire country, the reductions in rates of births are more moderate, however still quantitatively important. For example, among 20–34 year olds, the emergency contraceptive pill reduces births by an estimated 1.8%, and the general fertility rate in the country is estimated to fall by 1.6%.

Naive estimates of the effect of the emergency contraceptive 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 EC pill was available. We examine the validity of this assumption by comparing women who live 'close' to areas where the EC pill was available to those who live considerably further away. Using non-parametric tests to detect spillovers up to a distance of 30 km, we find relatively little evidence to suggest that spillovers exist, at least when considering rates of pregnancy.

³ In Chile abortion is illegal in all circumstances; clandestine abortions causing hospitalisation result in incarceration in some cases (Shepard and Casas Becerra, 2007). Chile is one of only six countries in which abortion is criminalised in all circumstances, including the cases of foetal inviability, risk to the life of the mother and rape (United Nations, 2014).

This finding supports the identifying assumptions required for our difference-in-difference estimates.

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 complete microdata on births and deaths at a national scale. It is also the first large-scale study of which the authors are aware that addresses these questions in a country other than the US. 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 context studied here offers lessons on the nature of contraceptive reforms and whether emergency contraceptives in a context of high rates of undesired pregnancy and few outside options are sufficient to have effects over observed birth rates.

The results of this study add to the nascent literature on the emergency contraceptive pill. Recent studies such as Durrance (2013) and Gross *et al.* (2014) 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 some evidence to the contrary, suggesting that the expansion in the availability of emergency contraceptives may offer significant effects in certain countries, with notable impacts on pregnancy and abortion rates, especially among young women.

1. The History of the Emergency Contraceptive Pill

1.1. *The Emergency Contraceptive Pill*

The emergency contraceptive pill is a hormonal treatment which can be used within five 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 oestrogen 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.⁴ 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 ovulation (Novikova *et al.*, 2007; Noé *et al.*, 2011). 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

⁴ 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%.

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 the EC pill during the last decade.

1.2. *The History of the EC 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.⁵ 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 2005 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).

These tribunals were followed by a number of years' worth of legislation and litigation, which resulted in sporadic availability of the emergency contraceptive pill, occasionally 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 EC pill.⁶ Details regarding this intervening process and laws passed by parliament theoretically requiring the provision of emergency contraception are discussed more fully in the online Appendix to this article.

The period of interest for this study follows a decision taken by the Chilean Constitutional Tribunal in 2008. This finding, responding to a demand placed by 36 parliamentary 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 EC pill. Given that these centres are administered by the mayor of each municipality, the availability in each municipality was entirely under the control of the mayor (Dides *et al.*, 2009, 2010, 2011).⁷ This resulted in a situation in which around half of the municipalities in Chile distributed the EC pill freely, while the remaining half refused

⁵ 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 social 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.)

⁶ For example, the subsecretary of health was removed from the cabinet due to his announcement in 2005 that emergency contraception would be available to all women who sought it.

⁷ 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 2,501 registered health centres and hospitals, 2,049 are under the control of municipalities (DEIS Ministerio de Salud, Gobierno de Chile, 2013).

to distribute it, or to distribute it only in a very restrictive set of circumstances. At the level of the woman, her municipality'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. We provide further discussion of the mayor and municipality characteristics and EC provision in Section 3. This idiosyncratic policy environment endured for approximately four years, until a law was passed mandating that the emergency contraceptive 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 Casas Becerra, 2007). Figures on the frequency and method of clandestine abortion are unclear, however Shepard and Casas 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. Further discussion related to the contraceptive environment in Chile preceding and posterior to the reform are available in online Appendix B.

2. Data

Population data from Chile come from two main sources. The first is vital statistics data, which records all births and foetal deaths, and is provided by the Ministry of Health of the Government of Chile. These data provide microdata records covering >99% of all births and foetal deaths reported in aggregate data in the country (Bharadwaj *et al.*, 2013). Each entry records the occurrence of a birth or foetal death, characteristics of the mother (and if present the father) including her age, education and municipality of residence, as well as a number of characteristics of the birth (including birthweight, gender, gestation and birth order) or the foetal death (weeks of gestation, birth order, cause of death). These data have been collected and reported in Chile since 1982 and, at the time of writing, are publicly available up to the year 2012. These vital statistics data for birth and foetal deaths thus provide data on all pregnant women in the country who either give live birth, have a birth leading to foetal death or who miscarry in any hospital in the country.

Data on all women of reproductive age come from the National Institute of Statistics of Chile (INE). The INE provides estimates of the number of women of each age living in each municipality in each year. These estimates are based on the decennial census, as well as net migration each year, and vital statistics on all births and deaths occurring to residents in the municipality (Instituto Nacional de Estadísticas, 2014). These two sources of data provide information on the number of women of fertile age living in each municipality, and the number of women pregnant (where pregnancy leads to a birth or observed foetal death) in each municipality during the time period of interest. They can be merged at the level of

the municipality, resulting in counts of total number of births and women, and also the calculation of municipality level rates of pregnancy (births/total women) and foetal deaths (deaths/total births). We provide additional details regarding the vital statistics and population data and their structure and matching in the online appendices provided with this article.

This results in a total sample of 1,605,300 births and 13,063 foetal deaths occurring between 2006 and 2012 (inclusive). The number of births per year in Chile has remained relatively stable over the last decade. Figure 1 displays total births, along with total foetal deaths during the period under study. Total births vary between around 220,000–250,000 per year, while total foetal deaths recorded in the Ministry of Health data (all foetal deaths occurring in any hospital or clinic in Chile), vary between 1,700 and 2,100.

Our measure for the EC 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 EC 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 EC pill (Dides *et al.*, 2009, 2010, 2011). This should directly reflect the decision by each mayor regarding whether his or her municipality could prescribe the EC pill after the 2008 Constitutional ruling. In each case, healthcare representatives were also asked to list the circumstances in which they could prescribe the EC pill. All municipalities which reported that they could prescribe the EC pill freely to women were recorded as treated, while all others

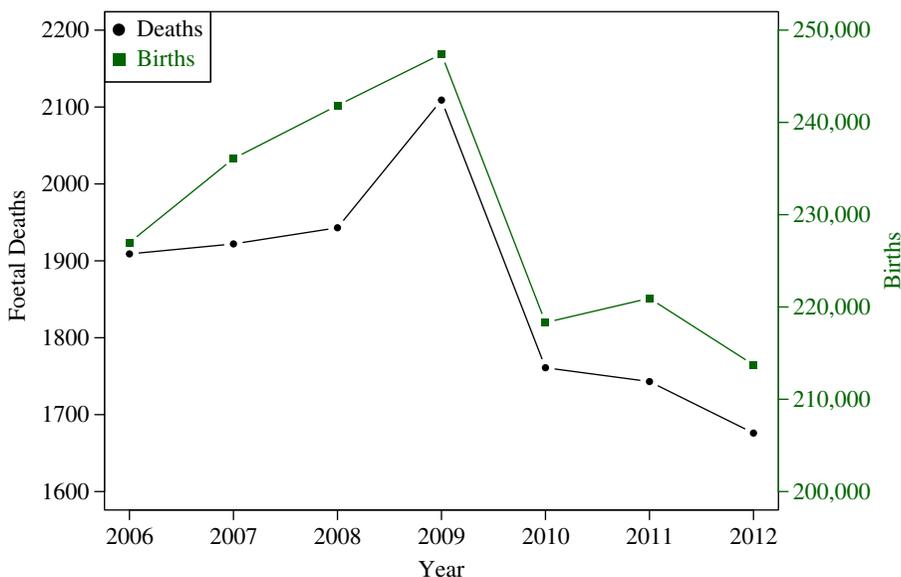


Fig. 1. Total Recorded Births and Foetal Deaths, 2006–12

Note. Data on pregnancies and foetal deaths comes from the Ministry of Health’s birth census.

were recorded as untreated.⁸ Secondly, the Ministry of Health has made available administrative data on all EC pill requests and disbursements 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 EC pill disbursements as the main measure of treatment. We focus on reported availability, given that disbursement are endogenous and jointly determined by demand as well as supply.⁹

In total, 280 of Chile's 346 municipalities report being able to prescribe the EC pill in at least one year after the 2008 Tribunal result (see Table 2). Figure 2 displays the quantity of municipalities reporting EC 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 EC pill availability in 2009, this has increased to around two thirds by 2011. Official records of EC pill prescriptions suggest reasonably large

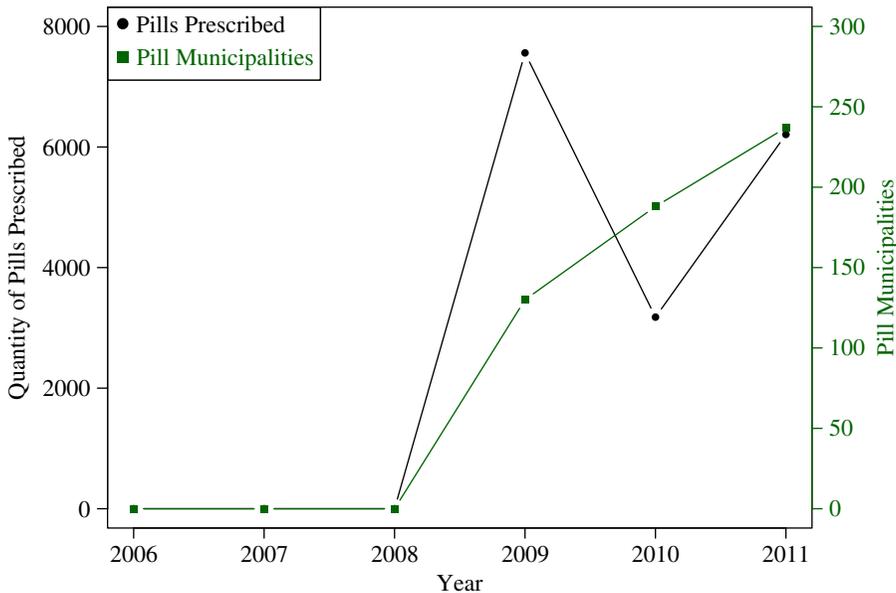


Fig. 2. *Pill Prescriptions and Availability by Time*

Notes. 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.* (2009, 2010, 2011).

⁸ 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 untreated given the lack of widespread availability. Similarly, a small number of municipal health centres could not be reached or refused to answer in each year (Dides *et al.*, 2009, 2010, 2011). These municipalities were not included in the analysis.

⁹ However, when we define EC pill municipalities based on disbursements rather than reported willingness to prescribe, we find effect sizes which are largely similar. These results are presented in online Appendix Tables.

fluctuations over time. While nearly 8,000 women were reported as requesting the EC pill in 2009, this fell to slightly under 4,000 the following year. Recent figures suggest that this number has been stable at around 6,000–7,000 requests in 2011–3 (the most recent two years have been omitted from this study and from graphical output, given that official birth records for 2013 are not yet finalised). Figure 3 displays the geographic variation of EC pill availability. This suggests that the EC 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 10th region) which has no municipal health centres, no obvious spatial patterns exist.

We examine foetal deaths as a manner to proxy illegal abortion. While it is certainly not the case that all (or even the majority) of foetal 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 Casas Becerra, 2007). To avoid concerns that reductions in foetal 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 and 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,¹⁰ 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 foetal deaths during 1–20 weeks (which may represent abortions and should respond to the EC pill), to those occurring from week 21 onwards, which should be largely or entirely unaffected by emergency contraceptive availability.

Full summary statistics are provided in Table 2. These statistics are subdivided by whether or not the municipality reports having the EC pill available in a given year. We observe some differences between EC pill and non-EC 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 EC pill was observed occur after 2008 while non-EC pill status is observed over the entire time period under study. In order to test this more formally, in Table 3 each municipality's EC pill status is regressed on all observed municipality characteristics, mayor characteristics, as well as year and region fixed effects. Unlike the raw summary statistics, this Table suggests that those municipalities where the EC pill was prescribed do not look remarkably different from areas where the EC was not prescribed, with two exceptions. Of the observed time-varying controls, differences (at a 5% level) are observed along the conservativeness of the mayor's party (which is negatively related to EC pill status) and by the degree to which condoms are also used in the region (which is positively related to EC pill availability). As we discuss later in the article, we address concerns that municipalities which do and do not prescribe the EC pill may differ in a way which threatens identification in a number of ways. Firstly, we estimate full event studies to examine the plausibility of parallel trends, secondly, we weight using inverse-propensity scores and, thirdly, we include these time-varying controls in our regressions.

¹⁰ The ICD refers to the International Classification of Disease, and refers to a set of standardised codes by which deaths can be classified. All deaths in the vital statistics data report this code, (the ICD-10).

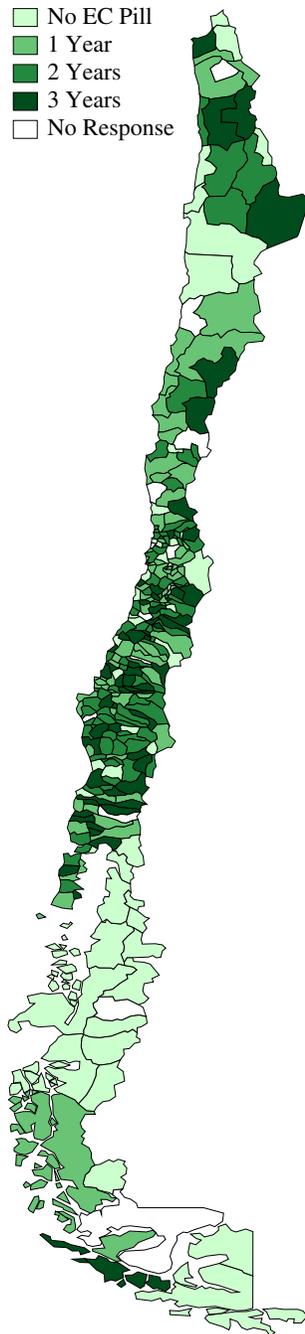


Fig. 3. *The Availability of the Pill by Geographic Region*

Longer time-series trends of the number of pregnancies in Chile are presented in Figures 4 (births to 15–19 year old mothers) and 5 (for 20–34 year olds). These Figures display the counts of total numbers of pregnancies, split by whether or not the

Table 2
Summary Statistics

	No pill available	Pill available	Total
<i>Municipality characteristics</i>			
Poverty	16.4 (7.47)	17.0 (7.56)	16.6 (7.49)
Conservative	0.286 (0.452)	0.267 (0.443)	0.281 (0.45)
Education spending (total)	4,817 (5,649)	5,980 (6,216)	5,108 (5,818)
Education spending (municipal)	430,625 (873,448)	525,143 (858,240)	454,232 (870,635)
Health spending	1,866 (2,635)	2,788 (3,381)	2,096 (2,867)
Out of school	4.07 (3.16)	3.98 (3.06)	4.05 (3.13)
Female mayor	0.120 (0.325)	0.134 (0.341)	0.123 (0.329)
Female poverty	60.5 (10.64)	62.0 (9.48)	60.8 (10.4)
Condom use	0.466 (0.0506)	0.536 (0.0401)	0.483 (0.0571)
Pill distance	204 (5,229)	0.00 (0.00)	153 (4,530)
<i>Individual characteristics</i>			
Live births	0.054 (0.226)	0.053 (0.224)	0.054 (0.226)
Foetal deaths	0.0558 (0.269)	0.0513 (0.256)	0.0547 (0.266)
Birthweight	3,322.7 (540.0)	3,334.3 (542.3)	3,324.7 (540.4)
Maternal education	11.92 (2.967)	12.03 (2.894)	11.94 (2.955)
Percent working	0.295 (0.456)	0.395 (0.489)	0.312 (0.463)
Married	0.340 (0.474)	0.309 (0.462)	0.335 (0.472)
Age at birth	27.05 (6.777)	27.15 (6.790)	27.07 (6.779)
No. of municipalities	336	280	336
No. of foetal deaths	9,999	3,064	13,063
No. of births	1,214,088	391,212	1,605,300

Notes. Group means are presented with standard deviations below in parentheses. Poverty refers to the percentage of the municipality below the poverty line, conservative is a binary variable indicating if the mayor comes from a politically conservative party (UDI or RN), health and education spending are measured in thousands of Chilean pesos, and pill distance measures the distance (in kilometres) to the nearest municipality which reports prescribing emergency contraceptives. Pregnancies are reported as percentage of all women giving live birth, while foetal deaths are reported per live birth. All summary statistics are for the period 2006–12.

municipality reported giving the EC pill in 2010. While there are year-by-year fluctuations, from 2004 onwards there is an increasing trend in the absolute number of pregnancies in both age groups. This tendency reverts following the availability of the EC pill in municipal health centres. While this reversion is seen in the whole country, it is extremely sharp in municipalities which report that the EC pill was available when requested by women. In the following Sections, we examine this effect econometrically.

Table 3
Commune Characteristics and Pill Decisions

	EC pill × 100 (1)	EC pill × 100 (2)	Close × 100 (3)	Close × 100 (4)
Out of school	0.08 (0.15)	0.06 (0.15)	-0.05 (0.14)	0.02 (0.14)
Health spending	-0.10 (0.19)	-0.07 (0.19)	-0.22 (0.17)	-0.22 (0.18)
Health staff	0.16 (0.26)	0.14 (0.26)	0.40* (0.24)	0.33 (0.24)
Education spending	-0.01 (0.02)	-0.02 (0.02)	-0.04* (0.02)	-0.00 (0.02)
Education level	0.01 (0.10)	0.04 (0.11)	0.19* (0.10)	0.06 (0.11)
Female poverty	0.12** (0.06)	0.02 (0.07)	0.00 (0.05)	-0.02 (0.06)
Female workers	0.03 (0.07)	0.02 (0.08)	0.02 (0.07)	-0.04 (0.07)
Urban	-1.24 (1.45)	-1.05 (1.61)	2.14 (1.35)	1.46 (1.49)
Condom use	17.73 (17.53)	73.04** (29.79)	11.52 (16.34)	-56.97** (27.57)
Condom availability	2.02 (15.58)	16.55 (24.62)	-16.96 (14.52)	26.17 (22.79)
Female mayor	1.12 (1.88)	2.18 (1.90)	-1.85 (1.75)	-1.29 (1.76)
Conservative mayor	-3.45** (1.40)	-3.32** (1.43)	2.11 (1.31)	1.89 (1.32)
Vote margin	5.44 (5.66)	5.89 (5.81)	2.12 (5.28)	4.70 (5.38)
R ²	0.57	0.57	0.29	0.31
Observations	2,210	2,210	2,210	2,210
Year FE	Y	Y	Y	Y
Region FE		Y		Y

Notes. Each column presents the results of an OLS regression of a municipality’s treatment status (prescribes the EC pill, or close to a municipality which prescribes the EC pill), on municipal level characteristics. Each outcome variable is binary and has been multiplied by 100 for presentation. Year fixed effects are always included. Independent variables and their descriptive statistics are described in Table 2 and this Section of the article. Standard errors clustered at the level of the municipality are displayed in parentheses. *p < 0.1; **p < 0.05; ***p < 0.01.

3. Methodology

We take advantage of the quasi-experimental nature of the expansion of the availability of the EC pill to women in different municipalities in Chile. Municipality j in year t is considered as treated if public health centres report that the EC pill is available upon request. Municipal-level birth rates $BirthRate_{jt}$ are regressed on the availability of the EC pill ($ECpill_{jt}$) in the preceding year:

$$BirthRate_{jt} = \alpha + \delta \times \mathbb{1}\{ECpill_{jt-1}\} + \phi_t + \eta_j + \eta_j \times t + \mathbf{X}_{jt-1}\gamma + \varepsilon_{jt}. \tag{1}$$

In (1), full municipality and year fixed effects are included, and municipality-specific time trends are allowed in some specifications. In our main specification crude birth rates for each municipality are used, although population-weighted results are also

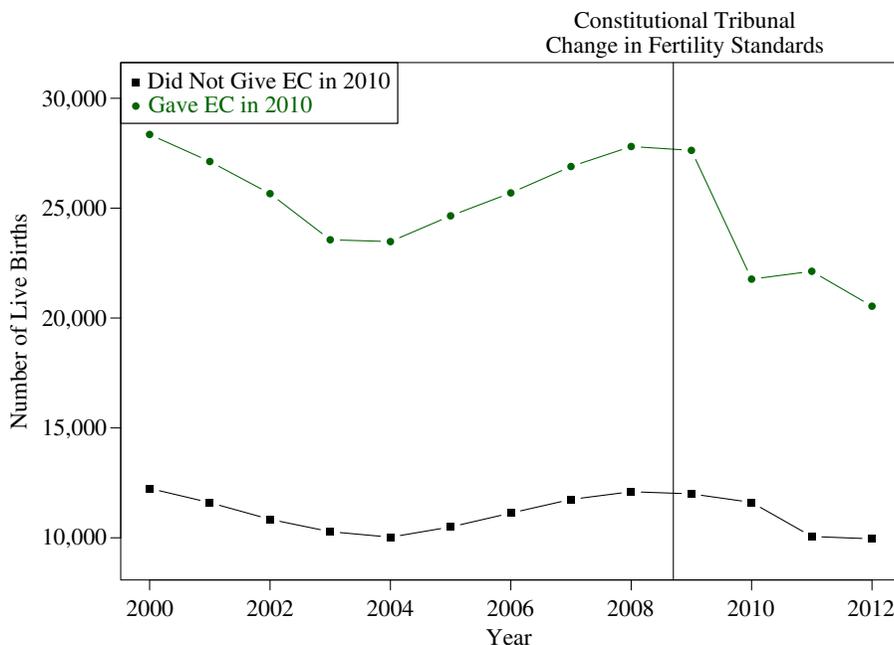


Fig. 4. Birth Trends 2000–12: 15–19 Year Olds

Notes. Some municipalities which did not give the EC pill in 2010 did give the EC pill in 2011 (and vice versa). All data comes from The Ministry of Health of The Government of Chile’s vital statistics records. The total number of births for women are presented by whether or not they occurred in a municipality which stated that it gave the pill in 2010, from survey data collected by Dides *et al.* (2009, 2010, 2011). For further details, see Section 2 of this article.

estimated. Standard errors are clustered by municipality. The identifying variation in availability of the EC pill is by municipality and year. Prior to the legal reform the EC pill was unavailable to all women, while following the reform the EC pill was available to those women living in municipalities where the mayor did not restrict access.¹¹ This provides a flexible difference-in-differences (hereafter diff-in-diff) framework, and allows us to estimate the effect of the EC pill causally if we believe that typical diff-in-diff assumptions hold. Namely, we require that unobserved components ε_{jt} 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 (with respect to the woman) nature of the arrival of the EC pill, we may be willing to accept that this assumption is valid. However, to minimise the potential that spurious events confound the arrival of the EC pill, we progressively include municipality-specific linear time trends and other factors that vary non-linearly over time across municipalities. These factors, X_{jt-1} , include controls for political and social

¹¹ Prescribing the EC pill in a given year after the reform does not imply that municipalities necessarily prescribe the EC pill in the years following. For example, in some cases, the municipalities switched from 0 (pre-reform), to 1 (during reform), back to 0 (during reform), and then to 1 definitively (post-reform) after the EC pill was made legal in the whole country in 2013. Our identification strategy takes advantage of this switching, given that $ECpill_{jt}$ varies by municipality and time.

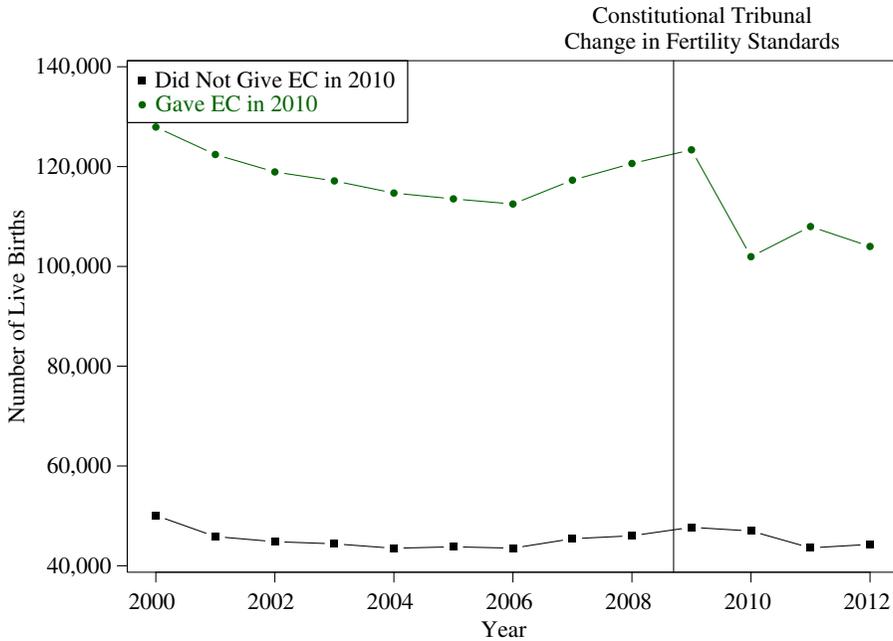


Fig. 5. Birth Trends 2000–12: 20–34 Year Olds

Note. See notes to Figure 4.

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, rates of reported condom usage over time, and measures of female empowerment (including women in public positions) by municipality.

In our principal specification, we estimate (1) using OLS and birth rates defined as births per 1,000 women. From our data, we observe the number of women who give birth, as well as the total number of women of each age in each municipality. When calculated for all fertile-aged women (ages 15–49) this measure is known as the general fertility rate (GFR). We estimate both weighted and unweighted regressions, and also a number of alternative measures of fertility as robustness checks. These measures are the absolute number of births, $Birth_{jt}$, and the log of the number of births plus 1, $\log(Birth_{jt} + 1)$. We begin by examining the effect of the reform on all women of child-bearing age (15–49), before testing for heterogeneity of effects by age groups, focusing separately on teenagers (15–19 year olds), 20–34 year olds, and 35–49 year olds. These age-specific birth rates are referred to as age-specific fertility rates (ASFR).

Similar regressions are run replacing $BirthRate_{jt}$ with $FoetalDeathRate_{jt}$, which is a measure of the number of observed foetal deaths per 1,000 live births. As discussed in Section 2, we believe that certain types of foetal deaths may be a reasonable proxy for illegal abortion. Finally, after assessing the EC pill’s impact on pregnancy and foetal deaths, we estimate the reduced form effect of the EC 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 do not believe that

these regressions are demonstrating causality in the case of mother's outcomes, they are a useful test to examine whether certain groups are more likely to access the EC pill, leading to aggregate compositional change in the cohorts of women who give birth.

3.1. *Identifying Spillovers Between Municipalities*

Our diff-in-diff estimates in the previous Section potentially underestimate the true effect of the emergency contraceptive pill. Principally, we may be concerned that there are 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 municipalities, if she is denied access to the EC pill in her municipality, 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 EC pill.¹² This motivates the following specification:

$$y_{jt} = \alpha + \delta \times \mathbb{1}\{ECpill_{jt-1}\} + \mathbf{close}_{jt-1}\boldsymbol{\beta} + \phi_t + \eta_j + \eta_j \times t + \mathbf{X}_{jt-1}\boldsymbol{\gamma} + \varepsilon_{jt} \quad (2)$$

This specification is identical to that in (1), however here we include a vector of close controls (\mathbf{close}_{jt-1}). These variables allow for us to estimate spillover effects between the EC pill treatment areas and surrounding areas which may also be affected by this treatment status, but which were not themselves treated, as long as spillovers only occur locally.

These close variables are specified as a vector of mutually exclusive binary variables, each indicating whether a municipality is located within a certain (small) minimum and maximum distance from the nearest municipality which gives the EC pill but does not itself give the EC pill. In practice, we define these variables to pick up spillovers in bins of 10 kilometres, and examine whether individuals who live up to 30 kilometres from the nearest EC pill municipality are affected by the EC pill. This results in three close controls: one which switches on for municipalities (0,10] kilometres from the nearest treatment municipality, another for those located (10,20] kilometres from the nearest EC municipality, and a final for those located at (20,30] kilometres. By definition, these variables take 0 for all treatment municipalities. In order to measure distance, we calculate point-to-point (Euclidean) distance from the centre of each municipality to each other municipality. As robustness checks, we also calculate distance over roads, and travel time in cars. A full description of this methodology, the distance data and its construction is provided in online Appendix A2.

4. Results

4.1. *The Effect of Emergency Contraception on Births*

Table 4 provides estimates for specification (1). The mean of each dependent variable is presented at the bottom of each panel: on average in Chile the GFR is found to be 53.87 (per 1,000 births) in panel (a), while panel (b) calculates the adolescent fertility rate as 52.00 per 1,000 births. This is in agreement with values from the World Bank

¹² This may be the case, for example, if women rely on friends or contacts in neighbouring municipalities to gain access.

Table 4
The Effect of the Emergency Contraceptive (EC) Pill on Birth Rates

	Birth rate (1)	Birth rate (2)	Birth rate (3)	Birth rate (4)
Panel (a): All women				
Emergency contraceptive pill	-0.882* (0.515)	-0.288 (0.461)	-0.814 (0.527)	-0.336 (0.949)
Observations	2,210	2,210	2,210	2,210
Mean birth rate	53.87	53.87	53.87	53.87
Panel (b): 15–19 year-olds				
Emergency contraceptive pill	-1.422 (1.092)	-1.068 (0.990)	-1.431 (1.092)	-1.552 (1.881)
Observations	2,205	2,205	2,205	2,205
Mean birth rate	52.00	52.00	52.00	52.00
Panel (c): 20–34 year-olds				
Emergency contraceptive pill	-1.602* (0.939)	-0.096 (0.843)	-1.512 (0.971)	-1.607 (1.752)
Observations	2,210	2,210	2,210	2,210
Mean birth rate	85.49	85.49	85.49	85.49
Panel (d): 35–49 year-olds				
Emergency contraceptive pill	-0.038 (0.340)	-0.144 (0.366)	-0.035 (0.331)	0.817 (0.539)
Observations	2,210	2,210	2,210	2,210
Mean birth rate	21.40	21.40	21.40	21.40
Year & municipality FEs	Y	Y	Y	Y
Municipal-specific linear trends		Y		
Time varying controls			Y	Y
Spillovers				Y

Notes. Each panel presents difference-in-difference results for a regression of age-specific fertility rates (ASFR) on the EC reform for the age group in each municipality. ASFR is defined as the number of births per 1,000 women. In the case of all women, this is called the general fertility rate (GFR). All models are estimated by OLS. Time-varying controls included in the regression consist of party dummies for the mayor in power, the mayor's gender, the vote margin of the mayor, the percentage of girls out of highschool, education spending by both the municipality and the Ministry of Education, total health spending and health spending on staff and training, the percentage of female heads of households living below the poverty line, the percentage of female workers in professional positions in the Municipality, and condom availability (measured at the level of the region). Wild bootstrap standard errors are clustered at the level of the municipality. *p < 0.1; **p < 0.05; ***p < 0.01.

DataBank, which lists the adolescent fertility rate in Chile as 52 per 1,000 births in 2008 (the year of the reform).

We first estimate 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, though not precisely estimate. Point estimates of the effect of the EC pill for the 15–19 year old group suggest that its arrival is associated with a reduction in the birth rate of 1.4 births per 1,000 women, and a similar value is estimated when including potentially confounding time-varying controls. However, these estimates are not statistically significant at typical levels for the adolescent group. The coefficients on other time-varying controls are omitted from Table 4 for the sake of clarity; however a full output for column (4) is provided in the

online Appendix Tables. Cast in terms of absolute birth rates among adolescents in the country, these point estimates are relatively large. When compared to the ASFR of 52 births per 1,000 women, this is a 2.7% reduction in rates of teen birth.

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. Nevertheless, results for older women are estimated with more precision. For 20–34 year olds we see that access to the emergency contraceptive reduces pregnancy, by 1.9% for all births when full controls are included (1.6 fewer births per 1,000 women, *versus* an average fertility rate of 85.49 for this group). In contrast to younger women, there appears to be no effect of the EC pill on women aged 35 and above. All estimates for the 35–49 year old group are not significantly different to zero, and indeed turn marginally positive when full controls are included. The global effect of the reform pooled for women of all fertile ages (15–49) is, as expected, a weighted average of these estimates. In areas where the EC pill became available as a result of the reform, the reduction in pregnancies associated with its arrival for all women was approximately 1.6% (0.88 births *versus* an average of 53.87 births per 1,000 women), as displayed in column (1) of the top panel of Table 4.

We address concerns regarding particular specification and estimation decisions in Table 5. Overall, the results from Table 5 suggest that our results for younger women are universally negative across methodologies, though often not at a statistically significant level. For all groups, estimating without a state-specific time-trend (column (1)) produces results of a similar magnitude, and not statistically distinguishable to the main result with full controls (reproduced for simplicity in column (2)). Similarly, specifications estimated with and without municipal weights, and results which weight using inverse propensity score consistently result in negative point estimates. In the case of inverse propensity score weights, estimated standard errors are considerably larger and levels of statistical significance smaller, though on the whole magnitudes of point estimates are larger. Finally, columns (5) and (6) estimate using only birth data. In column (5), the count of births are used instead of the birth rate, and in column (6) the log of the number of births (plus 1): $\ln(\text{Birth}+1)$. These results agree in magnitude with our main results. The log birth results suggest approximately a 1.6% reduction in teen births, and 1.2% reduction in all births. Full results for each of these specifications with and without trends and controls are included in the online Appendix to this article.

4.2. *The Effect of Emergency Contraception on Abortions*

In Table 6, diff-in-diff estimates of the effect of the emergency contraceptive pill on foetal deaths are presented. Once again the main specification is estimated using average rates, and weighted results are presented in the online Appendix. In this case the denominator is the total number of live births occurring in the municipality and year, while the total number of foetal deaths is assigned to the numerator. This value is multiplied by 1,000 to express the results as foetal deaths per 1,000 live births. The total number of observations in this case is fewer, as in various cases no births are observed in a particular municipality and year.

Table 5
Alternative Specifications – Emergency Contraceptive (EC) Pill and Births

	Double diff. (1)	Full controls (2)	Popln weights (3)	Inv PS (4)	OLS count (5)	OLS ln(Birth) (6)
Panel (a): All women						
Emergency contraceptive pill	-0.882* (0.515)	-0.336 (0.949)	-0.430 (0.527)	-0.884 (2.252)	-6.095 (6.517)	-0.012 (0.011)
Observations	2,210	2,210	2,210	2,210	2,210	2,210
Mean of dep. var.	53.87	53.87	53.87	53.87	2,632.67	7.37
Panel (b): 15–19 year olds						
Emergency contraceptive pill	-1.422 (1.092)	-1.552 (1.881)	-0.373 (1.111)	-3.859 (5.557)	-2.283 (1.460)	-0.016 (0.017)
Observations	2,205	2,205	2,205	2,205	2,205	2,205
Mean of dep. var.	52.00	52.00	52.00	52.00	386.14	5.46
Panel (c): 20–34 year olds						
Emergency contraceptive pill	-1.602* (0.939)	-1.607 (1.752)	-0.640 (0.959)	-2.860 (4.695)	-3.482 (4.792)	-0.010 (0.012)
Observations	2,210	2,210	2,210	2,210	2,210	2,210
Mean of dep. var.	85.49	85.49	85.49	85.49	1,833.11	7.02
Panel (d): 35–49 year olds						
Emergency contraceptive pill	-0.038 (0.340)	0.817 (0.539)	-0.250 (0.339)	-0.366 (1.092)	-0.310 (1.614)	0.006 (0.016)
Observations	2,210	2,210	2,210	2,210	2,210	2,210
Mean of dep. var.	21.40	21.40	21.40	21.40	435.78	5.55

Notes. Column (1) replicates the diff-in-diff result from Table 4 column (1). Column (2) estimates the same specification, however with full controls. Column (3) presents weighted results of the DD model, and column (4) weights using the inverse propensity score of the municipality's treatment status. Columns (5) and (6) present results using alternative outcome measures. These are the absolute number of births, and the log number of births plus 1 (respectively). All standard errors are estimated using wild bootstrapping, and are clustered at the level of the municipality. Full results with and without controls and trends for each specification are included in online appendix tables. *p < 0.1; **p < 0.05; ***p < 0.01.

Table 6
The Effect of the Emergency Contraceptive (EC) Pill on Foetal Death Rates

	All deaths	Early gestation	Late gestation
Panel (a): All women			
Emergency contraceptive pill	0.199 (0.559)	0.023 (0.275)	0.096 (0.437)
Observations	2,189	2,189	2,189
Mean (foetal deaths/live birth)	8.14	2.40	4.76
Panel (b): 15–19 year-olds			
Emergency contraceptive pill	1.124 (1.445)	–1.043* (0.617)	2.054 (1.269)
Observations	2,157	2,157	2,157
Mean (foetal deaths/live birth)	8.02	2.42	4.83
Panel (c): 20–34 year-olds			
Emergency contraceptive pill	0.052 (0.646)	0.522 (0.320)	–0.400 (0.556)
Observations	2,184	2,184	2,184
Mean (foetal deaths/live birth)	7.37	2.21	4.35
Panel (c): 35–49 year-olds			
Emergency contraceptive pill	0.196 (1.654)	–0.662 (1.165)	0.420 (1.292)
Observations	2,159	2,159	2,153
Mean (foetal deaths/live birth)	11.47	3.21	6.23

Notes. Each panel presents difference-in-difference results for a regression of the foetal death rate (deaths per 1,000 live births) on the EC reform for the age group in question. All models are estimated by OLS, and include the controls documented in Table 4. Standard errors are clustered at the level of the municipality. *p < 0.1; **p < 0.05; ***p < 0.01.

As discussed in Section 2, by using certain subsets of foetal deaths we aim to proxy for illegal abortion. We expect that if the emergency contraceptive pill affects abortion, this should turn up in foetal deaths occurring between 0–20 weeks of gestation, however, it should not turn up in deaths occurring later in the gestational period, given that abortions rarely take place beyond the 20th week.

Column (1) of Table 6 presents the effect of the EC pill on all foetal deaths. We are, however, most interested in columns (2) and (3), which present results for early (0–20 weeks), and late (≥ 21 weeks) respectively. In these columns, we have removed from the sample any foetal deaths which have been classified in ICD class Q (a minority of foetal deaths), as these represent causes such as congenital defects, which are very unlikely to proxy abortion.

For the 15–19 year old group, evidence is found (at the 10% significance level) to suggest that the EC pill may reduce the prevalence of (illegal) abortion. When examining the effect of the EC pill, it only has an effect on early gestation foetal deaths. Although not precisely estimated, the size of the coefficient is empirically very important: it suggests a reduction in early gestation deaths by approximately 40% (1.04 fewer foetal deaths per 1,000 live births *versus* an average rate of 2.42 per 1,000 births). We interpret this as suggestive evidence in favour of reductions of illegal abortion for this age group. When compared to the insignificant (and even weakly positive) effect on late-term deaths, this seems to provide more support to this claim. These effects, while somewhat

Table 7
The Emergency Contraceptive Pill and Aggregate Human Capital

	15-19 year olds			20-34 year olds			35-49 year olds		
	(1) Years of education	(2) Working	(3) Married	(4) Years of education	(5) Working	(6) Married	(7) Years of education	(8) Working	(9) Married
Panel (a): mother characteristics									
Emergency contraceptive pill	0.047 (0.035)	-0.003 (0.002)	0.000 (0.002)	-0.010 (0.023)	-0.001 (0.002)	0.006 (0.005)	0.028 (0.043)	-0.001 (0.006)	0.013 (0.010)
Observations	131,605	131,746	131,614	896,230	897,363	896,318	198,885	199,472	198,906
Mean of dep. var.	9.933	0.023	0.013	12.175	0.337	0.332	12.191	0.531	0.420
Panel (b): child characteristics									
Emergency contraceptive pill	-4.964 (7.539)	-0.015 (0.025)	0.009 (0.029)	-3.611 (3.290)	-0.022 (0.018)	-0.025 (0.023)	-1.344 (9.169)	-0.011 (0.031)	-0.037 (0.037)
Observations	131,493	131,471	129,880	895,660	895,671	885,932	198,733	198,745	195,863
Mean of dep. var.	3,267.031	38.669	49.546	3,336.382	38.575	49.661	3,309.71	38.236	49.528

Notes: Each column presents an OLS regression, and full controls listed in Table 4 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 2. Standard errors are clustered at the level of the municipality. *p < 0.1, **p < 0.05, ***p < 0.01.

imprecisely estimated, appear to be focused on younger women. When turning to women aged 20–34 and 35 and over, effects are smaller and not statistically significant.

While the suggestive estimates on the effect of the EC pill on abortion are large, particularly for young women, these are not implausible given the steep costs of abortion in Chile. Clandestine abortion in Chile is both dangerous and risky. According to Shepard and Casas Becerra (2007) (based on figures from the mid-2000s) approximately one third of maternal deaths in Chile are classified as ‘due to abortion’ and around 40 women per year were prosecuted for abortion. Further anecdotal evidence suggests some cases of physical and emotional abuse in hospitals, or even refusal of treatment if women were suspected of accessing clandestine abortions prior to hospitalisation.

4.3. *Emergency Contraception and Aggregate Human Capital at Birth*

Table 7 examines the effect of emergency contraception on aggregate human capital indicators of pregnant women and newborn babies. While it is not suggested that the EC 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 of this effect when considering long-term outcomes following access to the oral contraceptive pill (Ananat and Hungerman, 2012; Bailey *et al.*, 2012) and the arrival of legal abortion (Ananat *et al.*, 2009; Whitaker, 2011) in the US and in Romania (Pop-Eleches, 2006).

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.¹³ Each model is estimated as outlined in (1) using OLS. In the short term, we find that the emergency contraceptive 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 7 presents estimates by age group. For women of all ages, no effect is seen on any of the variables examined. In general, these results seem to suggest that access to the EC pill is transversal and is not centred on highly educated or employed women. 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, at least over the short time frame of the reform. Given the lack of effect found in mothers, it is not surprising that similar results are found for their babies. In each case, no effect is observed on birthweight, gestational period, or length at birth. While point estimates suggest that babies affected by the EC reform are on average lighter, potentially suggesting a negative selection into motherhood, none of these estimates are significant, even at the 10% level. Each of the reported significance levels is based on a two-tailed t-test.¹⁴

¹³ These outcomes, particularly birthweight, have been shown to improve outcomes including educational attainment and income throughout life (Behrman and Rosenzweig, 2004).

¹⁴ In the reported results, we test the effect of the reform on various outcome measures. Given that the results are largely insignificant, we do not correct for multiple hypothesis testing. If we were to correct using stepdown testing, we would be even less likely to reject the null of no effect on maternal or birth cohorts.

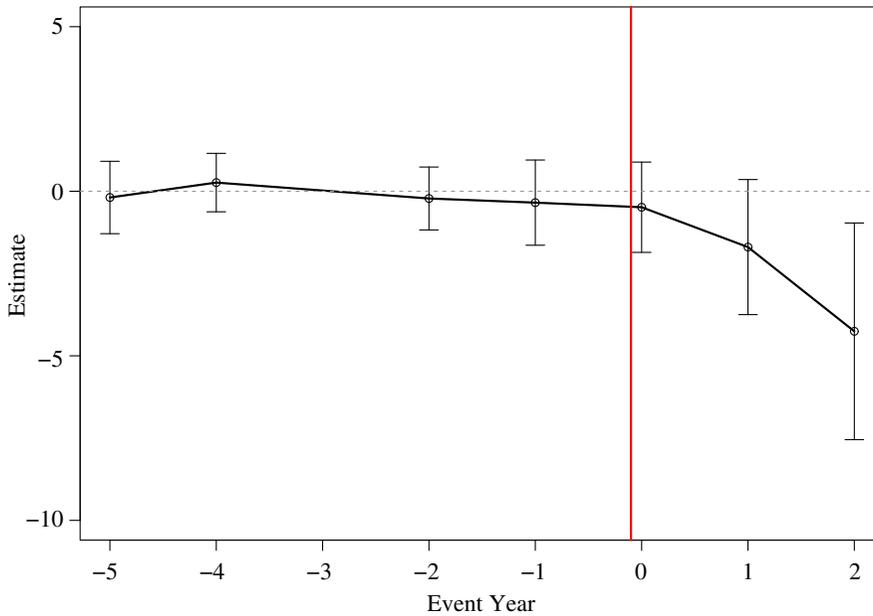


Fig. 6. *Event Study: 15–19 Year Olds*

Notes. Points and confidence intervals represent estimates for a full event study. Each point represents an indicator for the treatment group (pill municipality) interacted with n years prior or posterior to the reform; error bars represent 95% confidence intervals. On the x axis, 0 represents the first year the reform arrived, -1 represents one year prior to the arrival of the reform, 1 represents one year posterior to the reform and so forth. The omitted base in each case is three years prior to the reform, given that for the last emergency contraceptive (EC) pill adopting municipalities, three years prior to the reform there were no EC pills anywhere in Chile. Further discussion is provided in subsection 4.4.

4.4. *Event Study Analysis*

The similarity of the main estimates when adding time-varying controls, using inverse propensity score weights, and municipal-specific time trends provides some confidence in the results, however does not directly examine the parallel trends assumption underlying diff-in-diff estimation. Unbiased estimates of the reform require that the EC pill and non-EC pill municipalities would have followed similar trends in the absence of the reform. In order to examine this assumption more closely, we estimate an event study surrounding the arrival of the EC pill. This test allows for us to examine whether any difference in trends emerges before the arrival of the EC pill between treated and non-treated areas, and also the evolution of outcomes after the date of the reform.

We present event studies for 15–19 and 20–34 year olds in Figures 6 and 7. Point estimates and confidence intervals display the estimates on interactions between lags (or leads) interacted with whether the municipality gave the EC pill or not. If the only difference between EC pill and non-EC pill municipalities emerged after the reform (rather than prevailing differences in trends prior to the reform), estimates should only be significant in post-reform years. Given the time-varying nature of the reform, in order to form the event study, we omit as the base case the lag three years before the

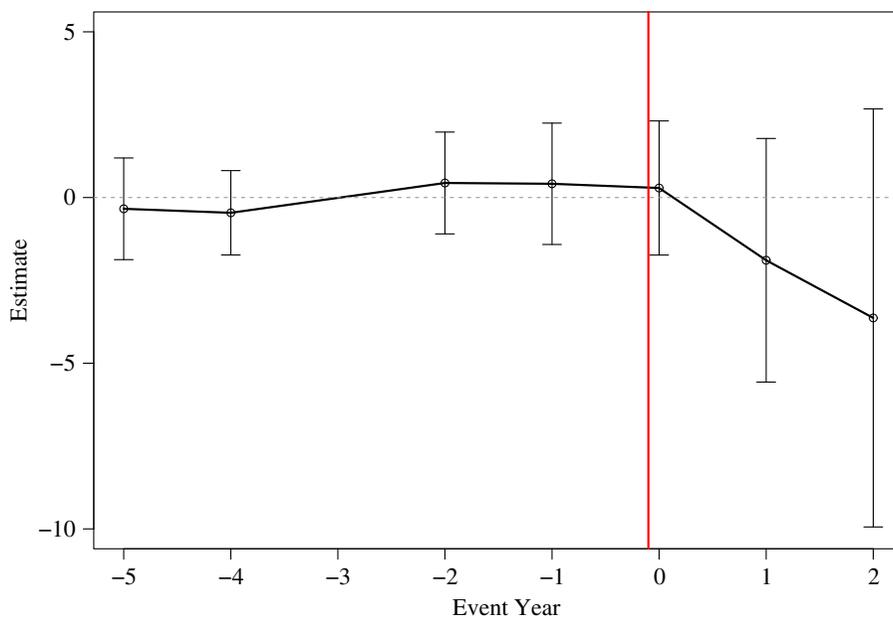


Fig. 7. *Event Study: 20–34 Year Olds*

Note. See notes to Figure 6.

start of the reform. This is necessary given that for a municipality which adopts the EC pill in 2011 (the final reform year), a lag of three years is 2008, entirely before the EC pill arrived to Chile.

As the raw trends presented in Figures 4 and 5 suggest, formal event studies show that differences between EC pill and non-EC-pill municipalities do occur after the arrival of the reform, though at times these are imprecisely estimated. All lags suggest that no significant difference in trends between treated and control areas exist before the reform. However, after the reform differences do emerge and these are particularly pronounced after the reform has been in place for some time (1 or 2 years). In the online Appendix, similar event studies are presented for all women, and also using the log of the number of births as an outcome variable; qualitatively identical results are found.

4.5. *Municipality Spillover and Imperfect ‘Compliance’*

Our results from subsection 4.1 present the effect of the EC reform only on municipalities which themselves received treatment. However, as we lay out in (2) and Section 3, we may be interested also in the effect on women who live close to, but not precisely in, treatment municipalities. If these women are able to travel the short distance and access the EC pill in nearby areas, close areas may themselves be affected by the reform. In order for this regression to uncover a consistent estimate of δ , we require that there are at least some control municipalities far enough away from treatment municipalities in which no spillover effects are felt. These ‘non-close’ municipalities then act as the control group for

Table 8
The Emergency Contraceptive (EC) Pill and Treatment Spillovers

	All women	15–19 year-olds	20–34 year-olds	35–49 year-olds
Emergency contraceptive pill	–0.336 (1.096)	–1.552 (2.339)	–1.607 (2.082)	0.817 (0.725)
Close <10 kilometres	1.588 (1.373)	–0.490 (2.232)	1.530 (2.470)	1.901* (0.992)
Close 10–20 kilometres	1.115 (1.335)	–0.602 (2.288)	0.517 (2.455)	1.569* (0.839)
Close 20–30 kilometres	0.010 (1.217)	0.332 (2.717)	–1.744 (2.188)	0.944 (0.890)
Observations	2,210	2,205	2,210	2,210
R ²	0.820	0.649	0.770	0.601

Notes. All models are estimated using difference-in-difference models, and coefficients are expressed as births per 1,000 women. Each regression includes municipal and year fixed effects, and the full set of time-varying controls described in Table 4. Standard errors clustered at the level of the municipality are displayed in parentheses. *p < 0.1; **p < 0.05; ***p < 0.01.

our diff-in-diff estimator. Given the relatively large distance between some non-treated municipalities and their nearest treated counterpart in the Chilean context, and the time sensitive nature of the EC pill, it seems likely that women far from treatment will not themselves be able to access treatment.

Results for specification (2) are presented in Table 8. In general, we do not find that areas close to EC municipalities had large changes in rates of pregnancy following the reform. While for 15–19 year-olds we do observe negative point estimates in near-by municipalities, these are both small and insignificant. Similarly, in the case of 20–34 year olds we find small and insignificant effects in contiguous municipalities, even when focusing on areas within 10 kilometres of a treated area. These estimates, while not suggestive of appreciable geographic spillovers up to a distance of 30 kilometres do reduce the precision of our main diff-in-diff estimates. In general, the lack of geographic spillovers suggests that the assumptions underlying diff-in-diff are reasonable in this case. In online Appendices we run similar tests using the alternative distance measures of distance over roads and travel time in vehicle, and find broadly similar results.

5. Conclusions

In many countries around the world, access to contraceptives is complicated or limited, and this is particularly the case when considering abortion. However, in those areas where contraceptives including the oral contraceptive pill and abortion are widely available, the economic literature suggests that these technologies can have significant effects on rates of fertility, particularly among young women. Evidence from historical reforms in the US and around the world suggest that these reforms may have effects on the fertility rates of young women approaching reductions of 10%. The evidence on the effectiveness of the emergency contraceptive pill is much more limited. In this article, we provide the first national-level study of its effect using complete vital

statistics, in an environment where abortion is completely outlawed. We find that in these circumstances, the EC pill may be as, or nearly as, effective as abortion at reducing rates of teenage pregnancy.

By taking advantage of a legal finding which left decisions regarding the availability of the emergency contraceptive 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. We find that in Chile, emergency contraception has large and significant effect on births and early gestation foetal deaths. For teenagers, this effect is estimated to be a reduction of 2.7% and 40% in births and early-gestation foetal deaths respectively. It is argued that these early-gestation deaths proxy for illegal abortion; comparisons with late term deaths add support to this claim. For 20–34-year-old women the estimated effect of the EC pill is a smaller but statistically significant (in the case of births) 1.8% reduction. When considering the general fertility rate, the EC pill is estimated to reduce fertility by 0.88 births per 1,000 women; or 1.6% of the average GFR in the country.

In the case of Chile, where women previously had no legal recourse to access post-coital contraceptives, the EC pill heralded a large change in their ability to control their fertility. There appears to be significant demand for these contraceptives. Immediately following the reform, rates of request approached 8,000 EC pills per year in the public system, a value which has remained steady in recent years.

All told, this article provides some evidence that emergency contraception may play an important role in a woman's contraceptive behaviour in Chile. Using a range of estimation techniques and methodologies suggests that in the absence of the EC pill, teenage childbearing in Chile would be approximately 1.5–3% higher, or that the arrival of the EC pill resulted in around 1 fewer birth per 1,000 15–19-year-old women during these years. These estimates suggest that the arrival of the EC pill prevented nearly 500 births to teen mothers in Chile over this time period. This effect is not insubstantial, particularly in a context where high rates of teenage pregnancy have been quite slow to fall over time. This finding is of particular importance to the country under study given that only recently a law has been implemented making the emergency contraceptive pill available to all. On a broader scale, these results also suggest that despite less evidence in the United States, the emergency contraceptive pill may act as a useful interim technology in the two thirds of the world's countries which currently restrict access to abortion.

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Additional Supporting Information may be found in the online version of this article:

Appendix A. Data.

Appendix B. The Chilean Legislative Environment and the Adoption of Emergency Contraception.

Appendix C. Alternative Specifications and Tests.

Appendix D. Alternative Event Study Specifications and Results.

Appendix E. A Back of The Envelope Consistency Check of Effect Sizes.

Data S1.

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Correction note: article corrected on 26 June 2017 after initial online publication on 20 April 2017. The authors were made aware of a coding error in the their original preparation of the microdata which resulted in incorrect estimates in the accepted version of this article. When reading in microdata to convert to municipal averages, the authors transposed two variable names meaning that year fixed effects (but not municipal by year trends) were incorrectly coded in municipal-level regressions. Correction of the code resulted in updated municipal-level regression estimates. Individual-level regression estimates remain unchanged. This version of the article has been corrected; the discussion in the article reflects the estimates in the corrected regression tables and event studies, resulting in changes in the introduction, results and concluding sections where effect sizes are discussed. This has resulted in a reduction in the magnitude of results reported in municipal-level analysis, while maintaining the direction of the effects. The online Technical Appendix and replication files, which are available in the online version of the article as supporting information, have also been corrected.

For clarity, a copy of the of the original version of the article, published online on 20 April 2017, a copy of the original online Technical Appendix and the uncorrected replication code are also available as Supporting Information to allow for comparison with the corrected versions. See Supporting Information file `Original_uncorrected_files_20April2017.zip`