The Children of the Missed Pill*

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Abstract

We assess the impact of exogenous variation in oral contraceptives prices—a yearlong decline followed by a sharp increase due to collusion—on fertility decisions and newborns' outcomes. Despite symmetric effects on Pill's consumption, we find stable weekly birth rates as prices declined, but significant increases (3.2%) as they skyrocketed. Interestingly, the incidence of low birth weight and fetal/infant deaths increased (declined) as birth control pills' costs rose (fell). We report inferior school outcomes as the 'extra' children reached school age. Our evidence suggests these 'extra' conceptions were more likely to face adverse conditions during critical periods of development.

Keywords: Fertility, newborn health, impact of collusion.

JEL codes: J13, I11, I18, D18

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1 Introduction

Existing evidence shows that having more control over fertility decisions allows women and families to alter their life choices more freely.[1] In 2019, 76% of women of reproductive age who have their need for family planning satisfied used modern contraceptive methods (United Nations, 2019a), with the contraceptive Pill being the preferred method of choice almost all over the world (United Nations, 2019b). The worldwide efforts to raise awareness for contraception and the absence of cheaper alternatives of reversible birth control suggest the prevalence of this method will expand (United Nations, 2015). However, to a large extent, access to the Pill is determined by dynamic market forces. As such, individuals may opt in or out of this product depending on price fluctuations, which in turn could affect the odds of conception as well as the conditions under which a potential pregnancy develops.

This paper quantifies the Pill's role in fertility and child outcomes using a sequence of events in which unexpected shocks affected the supply of oral contraceptives. In particular, we exploit a well-established case of anti-competitive behavior in the pharmaceutical market, which—after a year-long price war between the three largest pharmaceutical retailers in Chile—triggered a sharp and unexpected increase in birth control pills prices.

The price war took place between December 2006 and December 2007, and it effectively reduced the prices of medicines across the board. In particular, prices of oral contraceptives fell by 24% during that year. By the end of 2007, the three largest pharmacies agreed to end the price war and engaged in a collusion scheme in which they strategically increased the prices of 222 medicines. Oral contraceptives were included in this group, experiencing price increases ranging from 30 to 100 % in just a few weeks (45% on average in the first three weeks). We use daily information on prices and quantities sold in the country by the three companies from almost 40 million transactions to determine the date when the changes in prices for birth control pills took place. Based

[1] Access to contraceptive methods is associated with lower fertility of married and unmarried women as well as delayed marriage and first births (Bailey, 2006, 2010). Also, the literature has documented that it increases women's human capital accumulation, labour force participation and hours worked (Goldin and Katz, 2002; Bailey, 2006; Guldi, 2008; Ananat and Hungerman, 2012).

on it, we implement an interrupted time-series analysis (Cauley and Iksoon, 1988; Dee and Jacob, 2011), which takes into account the seasonality of births, the general trends of fertility as well as dynamics that arise because it takes time for the menstrual cycle to be fully regulated after discontinuing the Pill's intake. We complement the pharmacies' transaction data with administrative information on birth and death certificates for the period 2005 and 2008, and administrative records of school enrollment for the period 2013-2016. Our empirical strategy considers two different treatments: one stemming from a sustained and steady decline in prices, and another one from a massive and sudden increase.

Our estimates suggest consumers are reactive to increases and decreases in the price of contraceptives. The consequences of those reactions are asymmetric. By the end of the year-long price war, the demand for the Pill increased 28%. However, the subsequent skyrocketing increment in prices caused a sharp decrease in contraceptive use. Within four months after the price increase, consumption of oral contraceptives was back to pre-price war levels. We show that as a result, at the peak of the effect of the price increase, between 139 and 146 additional individuals were born in Chile per week, a 3.2% increase in the weekly birth rate. That contrasts with the lack of changes in fertility in response to the steady and incremental price reduction observed during 2007. We find, on the other hand, significant effects of the price increase on the numbers of children born out of wedlock and from women in their early twenties. And although the price war did reduce the incidence of underweight births, fetal and infant deaths; the price spike that followed led to increases in these dimensions that more than exceed the gains achieved while contraceptive prices were falling.[2] It is worth noting that we do not find significant impacts among teenage mothers or households located in deprived areas, as was expected due to their typically low usage of oral contraceptives (Ministerio de Salud, 2007).

Lastly, we analyze the long-term consequences of the unexpected price increase of 2008. In particular, we estimate its impact on kindergarten, first- and second-grade

[2]Throughout the paper, we refer to both miscarriages and stillbirths as *fetal deaths*. This is just for the sake of simplicity, and through it, we do not intend to take sides on the debate on when into the pregnancy should a conception be considered as a bearer of life. A distinction between miscarriages and stillbirths is made below.

enrollment for the period 2013-2016, when the children born in 2008 reached school age. We find that children conceived shortly after the price shock were less likely to enroll relative to those conceived during the 2007 price war. Furthermore, conditional on enrolling, these children were more likely to attend education programs with intellectual disabilities.

Our findings are consistent with the hypothesis that those conceived during the first weeks of 2008 were relatively more likely to face less favorable conditions during critical periods of development, as extensive literature has shown that the development of healthy children and adults is greatly affected by economic and environmental deprivation while *in-utero* (Black et al., 2007; Kiernan and Huerta, 2008; Eriksson et al., 2009; Almond and Currie, 2011b, among others), stress, depression and emotional hardship during pregnancy (Huttunen and Niskanen, 1978; Kiernan and Huerta, 2008; Class et al., 2011; Black et al., 2016), and maternal behavior (Currie and Moretti, 2003; Jayachandran and Pande, 2017). In our context, such lack of timely complementary investments in the health of the pregnancy may be explained by increases in the numbers of unwanted and/or cryptic—when a woman does not find out she is pregnant until 20 weeks along or later—pregnancies, as there is uncertainty regarding when ovulation starts again after stopping the intake of the Pill.

However, there are reasons to believe that unintended pregnancies are more likely to suffer from such deprivations. Women and families carrying an unintended pregnancy may be unprepared to deal with the behavioral, social, economic and health-related changes that a pregnancy entails.[3] According to public health and medical studies, they are less likely to abandon unhealthy habits (Hellerstedt et al., 1998; Dott et al., 2009), may delay prenatal care (Mayer, 1997), and the pregnancy itself may become a source of stress and anxiety (Biaggi et al., 2016). In consequence, children born from unintended pregnancies

[3]In cryptic pregnancies, the lack of timely investments may be because mothers are unaware they are pregnant, rather than the unpreparedness or the unwillingness to adopt healthy behaviors. Regardless of the distinction, the fact is that the lack of timely investments affects the development of the fetus. Thus, just like other unintended pregnancies, cryptic ones may be more likely to face deprivation while *in-utero* than intended pregnancies. Indeed, Del Giudice (2007) states that "...cryptic pregnancy appears to reduce the costs of pregnancy, both energetic and ecological (mobility, dependence on kin/mate, etc.), thus favoring the mother at the expense of the fetus."

may be on average less healthy than those born from intentional ones (Bustan and Coker, 1994; Sharma et al., 1994).

This paper extends several branches of the literature. First, it contributes to the limited evidence on the relationship between prices and demand for contraception, and its effects on fertility.[4] Second, it provides new evidence linking changes in the Pill's availability to shifts in the average health of the children conceived, which highlights the role of selection into and out of the consumption of the Pill due to changes in its affordability.[5] Third, our analysis pins down a channel by which anti-competitive agreements between competitors can cause substantial long-lasting harm even if they are stopped by antitrust enforcement (Baker, 2003; Levenstein and Suslow, 2006). In fact, to the best of our knowledge, we are the first to quantify the short- and long-term impacts of anti-competitive behavior in the pharmaceutical industry on both consumers and their descendants.[6] As a consequence, our findings should serve as a cautionary tale for current cases of market failures in the pharmaceutical sector all around the world, most notably in the US where cases like the EpiPen, insulin and Daraprim—just to name a few—have caught the public's attention.[7]

The paper is organized as follows. Section 2 describes the price war and the collusion case, the events triggering exogenous variations in prices. Sections 3 presents our

[4]This literature has found that subsidizing contraceptives decreases fertility by about 3 to 6 percent in Indonesia (Molyneaux and Gertler, 2000), and 9 percent for women in a relatively high-income bracket in the US (Kearney and Levine, 2009). To the best of our knowledge, ours is the first paper to study the case in which birth control Pills become substantially more expensive while remaining widely available.

[5]From this perspective, our work relates to Ananat and Hungerman (2012) who document short- and long-term changes in the composition of the pool of women who become mothers after young women (under 21) gained access to the Pill. It also connects to the literature on the access to abortion which suggests that its legalization yielded cohorts born in relatively less economically deprived households (Gruber et al., 1999), who as adults had higher educational attainment and were less likely to end up being welfare recipients (Ananat et al., 2009).

[6]While some work analyzes the role of pharmaceutical companies' market power in determining drug prices (Howard et al., 2015), no work has linked such market failures to long-lasting consequences.

[7]The EpiPen's price went from \$100 to \$608 after Mylan—a pharmaceutical company—bought the brand and later found itself in a near-monopolistic position as its only competitor withdrew from the market, due to a recall (Willingham, 2016). Insulin's price tripled in about a decade due to a lack of competition from generics (Rappold, 2018). The price of Daraprim, a drug used to fight toxoplasmosis in those with weak immune systems like AIDS or cancer patients, increased 5455% overnight, after the brand was acquired by Turing Pharmaceuticals (Pollack, 2015). Prices of Isuprel—a bronchodilator used to treat heart failure—and Nitropress—a vasodilator used to treat heart failure and life-threatening hypertension—increased by 3000% and 1500% respectively after the brands were sold twice to different laboratories (Thomas, 2016).

methodology and identification strategy, while Section 4 describes our data. Section 5 presents the main results as well as falsification tests and robustness checks. Section 6 concludes.

2 The Collusion Case

In January of 2008, daily average prices of birth control pills in the three main pharmacies in Chile—Farmacias Ahumada (FASA), Cruz Verde (CV), and Salcobrand (SB), who control between 92 to 97 percent of the market[8]—increased by about 75 percent within just a few weeks. Figure 1 displays this sharp increase as well as the evolution of the average daily contraceptive prices by chain between January 2006 and January 2009.

The massive and widespread price increase between the last week of 2007 and the first weeks of 2008 was the result of a secret plan to collude and coordinate an "expressive, simultaneous and uniform" raise of prices for 222 prescription and over-the-counter (OTC) drugs orchestrated between the three pharmacies (Fiscalía Nacional Económica, 2008). According to lawsuit documents, SB was the one that led the price increases (Tribunal de Defensa de la Libre Competencia, 2012). This was the direct consequence of a change in ownership of SB that took place in April 2007, and the new owner's decision of abandoning the existing pricing policy after receiving—in October 2007, just two months before collusion started—the reports of a business consultancy firm who advised for a "de-commoditization" of the industry.[9] Hence, it is very unlikely that consumers could have anticipated the price increase, much less its timing, rendering it exogenous to the consumers' fertility decisions. It responded to changes in corporate policy, triggered by the arrival of new managers. Thus, we treat it as an unexpected shock to consumers who had been facing a year-long spell of a steady price decline.

[8]The *Fiscalía Nacional Económica* (National Economic Prosecutor, FNE) estimates the joint market share of the three retailers at 92%. This market concentration has been accompanied by a long tradition of anticompetitive practices in the industry over the last 20 years. In 1995, the same drug retailers were sanctioned for price-fixing, and episodes of price wars and unfair competition accusations were not uncommon. Not surprisingly, Chile has the highest share of out-of-pocket family expenditures on medical care out of all OECD member nations (OECD, 2013). Drugs are the largest component of that spending.

[9]See http://www.salcobrand.cl/cl/empresas-salcobrand/ and http://www.jec.cl/articulos/?p=6528. According to the FNE, coordination between the retailers was facilitated by the fact that the SB's new owner actively recruited executives from FASA and CV during 2007.

In April 2008, the price coordination stopped after the National Economic Prosecutor (FNE) called drug retailers' executives to question regarding the price increases. In December 2008, the FNE filed a lawsuit at the *Tribunal de Defensa de la Libre Competencia, TDLC* (Bureau of Competition) against the pharmaceutical companies for price-fixing (Fiscalía Nacional Económica, 2008). In March 2009, the Competition Court delivered a settlement between the FNE and FASA, in which FNE dropped the charges against the pharmacy. This settlement established a US$1 million fine for FASA, together with a statement that disclosed the coordination mechanisms and exchanges of information that had allowed the concerted price rises. After more than two years of trials, on April 23, 2011, the TDLC unanimously decided that the pharmaceutical companies were "guilty" of price-fixing. It imposed a fine of US$19 million, the largest fine set in the Chilean antitrust history at that time. The Supreme Court ratified this sentence after an appeal process in 2013.[10]

3 Empirical Strategy

In principle, the sharp exogenous increase in birth control pills prices triggered by the collusion could offer the opportunity to estimate the short-run price elasticity of contraceptives and the causal effect of the Pill's availability on fertility and birth-related outcomes using a Regression Discontinuity Design (RDD). This as women eligible to conceive right before and right after the changes might be identical except for the fact that the latter group faced different prices than the ones faced by the former. Thus, any discontinuity in the conditional distribution of outcomes, such as number of births or the health of the babies conceived after the price increases could be interpreted as the effect of contraceptive prices.

However, both the nature of the treatment examined as well as the outcomes of interest entail dynamic considerations. Regarding the former, dynamics come from the fact that conceptions in Chile (and elsewhere) have a secular trend and a marked seasonality. The

[10]See http://www.economist.com/blogs/americasview/2012/02/competition-chile and http://www.law360.com/articles/376729/chilean-high-court-backs-40m-pharmacy-price-fixing-fines

declining oral contraceptive prices observed throughout 2007 exacerbate this concern (see Figure 1). Regarding the latter, medical evidence shows that the probability of conception increases with the time elapsed after the suspension of contraceptives' intake, because the contraceptive medication progressively wears off, and the menstrual cycle is gradually regulated (Gnoth et al., 2002). RDD would thus identify the average causal effect of the treatment only *at* the discontinuity point (Lee and Lemieux, 2010; Card et al., 2017). Hence, ignoring the dynamic elements of outcome and treatment variables invalidates the use of discontinuity methods. Formally, the exogenous price increase does not secure the strict exogeneity assumption on the conditioning variable.

Our empirical strategy takes these considerations into account. To avoid the confoundedness caused by dynamics in conceptions, we control for trends and seasonality.

$$Y_t = \alpha + \tau_{PW} d_{PW} + \tau_C d_C + f_{PW}(t) + f_C(t) + \gamma t + \sum_{w=1}^{51} \omega_w \times S_w + \varepsilon_t, \quad (1)$$

where $d_{pW} = 1\left[t_1 < t \leq t_2\right]$, is a dummy variable for the *price war* period ($t_1 < t \leq t_2$), $d_C = 1\left[t > t_2\right]$ is a dummy variable for the collusion period ($t > t_2$), $f_{PW}(t) = f\left(\beta_{PW}, t_1 < t \leq t_2\right)$ and $f_C(t) = f\left(\beta_C, t > t_2\right)$ are flexible polynomial for the price war and collusion periods respectively. Lastly, t is a linear trend, and $\sum_{w=1}^{51} \omega_w \times S_w$ represents the week-of-the-year fixed-effects. [11] In order to allow for flexible dynamic responses of the outcome to the price change, we favor specifications with different parameterizations for $f\left(\beta_{PW}, t_1 < t \leq t_2\right)$ and $f\left(\beta_C, t > t_2\right)$ at each side of the discontinuity where β_{PW} (price war) and β_C (collusion) represent two distinct parametric configurations. Moreover, we highlight the dynamic consequences of the price increase by presenting the estimated impacts at different moments throughout each treatment period. That is, we present the deviation relative to a scenario without the price shocks. Thus, we implement an interrupted time-series (ITS) estimator with two structural breaks in the time dimension: the year long price war and the sudden price increase that followed. See Cauley and Iksoon

[11] Note that t_1 and t_2 in equation (1) needs not to be the first week of 2007 and 2008 respectively. We can use this specification to allow for a "donut-hole" approach as in Cohodes and Goodman (2014) and Barreca et al. (2011), but not because of the conventional argument of manipulation of the running variable. Instead, this strategy could help us to deal with the fact that sudden price changes may not translate into instantaneous adjustments of consumption.

(1988) and Dee and Jacob (2011) for implementation of ITS in other contexts.

4 Data

We gather and combine three unique sources of individual-level information.

Births, Mothers' Characteristics, Fetal and Infant Deaths. The main source of information for our empirical analysis is Chile's Health Information and Statistics Department (*Departamento de Estadísticas e Información de Salud*, DEIS). The DEIS records information on the date of birth, weight at birth, place of birth, gestation length (in weeks) and characteristics of the mother of every newborn in the country. Our main empirical analysis is carried out with data on all births in Chile during 2005-2008. The availability of gestation length allows us to calculate the conception date by subtracting the number of gestation weeks from the week of birth.

In addition to date on live births, the DEIS collects data on all deaths, including those of unborn and newborn children.[12] By studying these cases, we are able to include in our analysis pregnancies that end up in fetal deaths, as well as inquiere about the impact of pill price changes on infant mortality (i.e., children under the age of one).[13] Importantly, the census of infant deaths records the dates of birth and death as well as the medical reason for death classified using the International Statistical Classification of Diseases and Related Health Problems (ICD-10) of the World Health Organization.

Chile's official statistics report 739,390 live births in the country between 2007 and 2009, 90.6% of which took place in urban areas and 40.54% took place in the Santiago region.[14] Our data also indicate that there were 6,582 miscarriages and stillbirths during

[12]The precise definition of fetal death used by the Chilean Health Information and Statistics Department is the death that happened before the complete expulsion or extraction of a conceived being from the mother's body, regardless of the length of the pregnancy. A body is considered dead if after such separation the fetus does not breath nor shows any sign of life such as heartbeat, umbilical cord pulsing, or effective movement of voluntary muscles.

[13]The fetal death registry is built based on the collection of the *Certificados de Defunción y Estadística de Mortalidad Fetal* (Death Records and Fetal Mortality Registry) that a physician or a midwife should fill in every time they are able to identify the "product of the conception", regardless of the length of the pregnancy. It the physician or the midwife do not identify the "product of the conception"—because, for instance, the abortion happened outside the hospital—they should not fill the fetal mortality forms (see the relevant legislation at http://deis.minsal.cl/deis/codigo/neuw/norma_fetales.asp).

[14]We supplement the births and deaths censuses with information on local characteristics (e.g., income

that time. Furthermore, the death records show around 2,000 infant deaths per year, half of which happen within their first week of life.

Table 1 presents basic summary statistics on weekly births in 2007 for different groups in our sample. As can be seen, there was an average of 4,626 births per week, most of them out of wedlock (2,921), and 45% of them are their mother's first child. About 16% are from teenage mothers. The infant mortality rate is approximately 8 deaths per thousand live births. The main causes of infant deaths are congenital malformations and complications within the perinatal period, accounting for 35% and 44.8% of deaths, respectively.

Conceptions in Chile, on the other hand, have a clear trend and a marked seasonality (see Figure 2). As the fertile-aged population grows, so does the number of conceptions per week. This causes a positive trend of about 3 additional conceptions each week. Fertility patterns also vary seasonally. Conceptions peak during the last three weeks of the year, where summer vacations and end-of-year holidays coincide. These patterns are crucial for our estimation procedures and the interpretation of results.

Contraceptives: Consumption, prices and the collusion case. We supplement the information on births and fetal and infant deaths with data from Chile's Bureau of Competition (TDLC). This agency examined the evidence of collusion by the pharmaceutical companies, gathering detailed information on around 40 million transactions involving more than 220 medicines during 2006 to 2008. From these data, we are able to precisely observe the medicines' daily prices and the quantities purchased. In particular, we analyze the data for birth control pills (oral contraceptives) for the three most important drugstore franchises, which control between 92% and 97% of the market share. For these companies, we also have information on the number and location of stores over time.

The TDLC data contains rich information describing important features of the contraceptives' market in Chile and its dynamics. Retailing contraceptives in Chile is a sizable business.[15] According the the TDLC data, 10,773,126 out of the 39,476,571 transactions

and poverty levels) obtained from different waves of The Socio-economic Characterization surveys.

[15]Data on the prevalence of different contraceptive methods in Chile is scant and dispersed. However, using different sources of partial information one can paint a picture of Chileans' contraceptive use. In 2005, there were 5,932,000 women above the age of 15 in Chile (CEPAL and INE, 2005), out of which

(i.e., 27.3%) included contraceptives. In 97% of the transactions that involved contraceptives, the costumers purchased the contraceptives and nothing else. On average, 10,542 units of contraceptives were sold each day between 2006 and 2008.[16] Revenues from sales of contraceptives at these three pharmacies was of around US$35.7 million in 2006, US$34.7 million in 2007 and US$47.6 million in 2008.[17]

As Figure 1 illustrates, the TDLC data is detailed enough for us to identify each stage behind the changes affecting the contraceptives market during 2007 and 2008 (price war and price fixing) as well as the synchronicity of contraceptive price increases by drug retailers (substantial, sudden, and across the board price increases).

The data also allows us to explore contraceptive brand dominance and substitutability, because we are able to observe the brand choices made by the consumers. Chileans have several alternatives for oral contraceptives; in fact, pharmacies report selling 24 different brands of contraceptives. Our data shows that the choices of contraceptives were remarkably stable across time, even after the spike in prices of January of 2008. Such stability in the market share of each brand is partly due to the fact that all of the contraceptive brands saw their prices increase starting in January 2007. Therefore, the scope of the substitutability that might have taken place was hampered by the fact that all contraceptives became more expensive. However, we cannot rule out that some degree of substitution may have occurred. As discussed below, if this occurred, substitution would

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48.9% declare that they do not use any contraceptive method, 8.8% had gone through an sterilization procedure in the past, and 2.2% use a so called *natural* method (i.e., periodic abstinence, breastfeeding and *coitus interruptus*) (Ministerio de Salud, 2007). Of the 2,379,000 women that use a modern contraceptive method, 869,000 take contraceptive pills. That is 36.5% of the non-sterilized women that use a modern contraceptive method take the Pill, 19.8% of the women in fertile age (15 to 49). On the other hand, the Chilean government claims that 53.4% of the population that uses any kind of modern family planning does so through the public heath system (Ministerio de Salud, 2006). Hence, around 1,100,000 women get their contraceptives from private vendors. Our data shows that around 340,000 women purchased oral contraceptives each month from pharmacies in 2007. Therefore, 31% of the women that use modern methods of contraception and do not get them through the public health system buy the Pill. That figure matches up very well with the prevalence of the Pill among the users of the public health system 38% (FLACSO-Chile et al., 2008) and its overall prevalence of 36.5% among those who use modern methods of contraception.

[16] We understand as "unit of contraceptives" the dosage of medicaments that supply contraceptive capability for one full feminine cycle. This precision needs to be made as the number of pills provided in a packet of contraceptives varies across brands. However, regardless of the number of pills a woman needs to take monthly, the packets in question provides contraception for one cycle.

[17] During the 2007 price war, pharmacy revenues fell by US$ 1 million despite the fact they sold 629,254 units more than the year before, only to rebound while they were colluding.

bias our econometric results towards not finding an effect. In that respect, our results can be considered as a lower bound.

That same reasoning holds for the potential substitution with condoms, although such substitution would have a limited scope as the prevalence of condom use among Chileans is only of 5.5% (Ministerio de Salud, 2007). Furthermore, the literature recognizes that very little is known about condom access and fertility partly because condom availability and use are very difficult to observe (Buckles and Hungerman, 2018). Regardless, we address that concern by analyzing pharmacies' daily data on condom prices and quantities sold, and government procurement data on 588 purchase orders for the 2007-2008 period that include condoms for distribution in public hospitals, public health facilities, and municipalities. Our findings indicate that prices of condoms and the quantities demanded did not respond to skyrocketing increases in oral contraceptive prices due to the collusion of drug retailers. The evidence presented in Appendix A shows that pharmacies did not change their condom pricing policy—that entailed raising prices at the beginning of Summer and keeping them at that level until next Summer—nor they sold more units after the contraceptives price increase. In fact, Figure A.1 shows a drop in the number of condoms sold contemporaneous to the contraceptive price increase, attesting against the fact that consumers substituted away from the Pill in favor of condoms. Furthermore, Figure A.2 shows that public procurement of condoms did not change after contraceptives price increased indicating that there was no reaction from public providers to the contraceptives price shock.

School Outcomes between 2013 and 2016. Our final source of information comes from public records on school attendance for the years 2013 and 2016. The Ministry of Education of Chile reports individual-level school attendance on a monthly basis for all students attending schools receiving public funding.[18] The public records contain students' exact date of birth for the years 2013 and 2014 and month/year of birth for the years 2015 and 2016, grade attended, type of program including those for students with

[18]During the period of analysis, the proportion of schools not receiving public funding did not exceed 9% of the total number of schools in the country. All public and private subsidized schools received public funding.

disabilities, school location, and days of school attendance per month. In March of 2013 and 2014 (beginning of academic year), the files contain data for 3,246,945 and 3,331,326 students, respectively.

5 Results

5.1 Prices and Quantities

We begin by analyzing the responses of contraceptive purchases to the price changes. Figure 3 suggests that consumers are very reactive to the prices of oral contraceptives. In 2006, when prices were relatively stable—with a very small increasing trend—consumption of contraceptives stayed flat. If anything they show a non-statistically different from zero upward trend of 50 extra boxes per week. The price war ended with that stability. As it evolved lowering prices week after week, consumption of contraceptives raised steadily. The unexpected price increase of January 2008 caused a sizable reduction in the amount of contraceptives purchased per week.[19] In particular, Figure 3 suggests that within the first four months consumption levels were back to those that existed before the price war.

Table 2 confirms and quantifies this association. It reports the estimated effects obtained from a regression model in the same spirit of equation (1), when the outcome variable is the total number of units (boxes) of oral contraceptives sold per week. It shows that by the end of 2007's price war, between 18,000 and 21,000 extra units were sold. These represent a massive relative increase of 28.6% to 33% in the contraceptives' sales. However, the price increase of January 2008 was so large and sudden that by week 20 after the price shock, pharmacies were selling the number of units they would have sold if the price war had not happened. By mid-2008, weekly sales were 5,000 units (8%) below

[19] It is important to note that the increase of the contraceptives' prices was large, sudden and unanticipated. In consequence, there are no reasons to believe that people could strategically be stockpiling contraceptives as anticipation to price changes (Simonsen et al., 2015). Our data show that between 2006 and 2008 96.4% of the contraceptive purchases were *single purchases* in that costumers only bought one box. Therefore, stockpiling was negligible in the first place, and the proportion of single purchases remained fairly stable between 94.4% and 96.6% throughout 2007. Our analysis in Appendix III shows that if there was any stockpiling, it was not as a strategic response to prices, but due to pharmacy availability.

the pre-price war levels. By the end of 2008, weekly sales had fallen by 11,000 units. That means that by then, the price increase due to collusion reduced weekly sales of contraceptives by about 30,000 units from the peak of the effect of the price war. Figure 4 depicts the estimated effects and associated standard errors reported in Table 2.

We can use the point estimates to construct the price elasticity. Considering an average price increase of 45 percent during the first weeks after the shock, a Wald estimate of the contraceptives' price elasticity ranges between -0.11 and -0.16, which is in the upper end of those found in the literature (between 0 and -0.15).[20]

5.2 Live Births

Our first set of results on conceptions is presented in Table 3 and comes from the estimation of equation (1) when the outcome variable is the total number of conceptions per week. In particular, those that resulted in live births later on. Specifically, the table displays the estimated impact on live births after 1, 8, 15, 22, 29, 36, 44 and 52 weeks since the start of the price war or the collusion respectively. Interestingly, the lower prices in 2007 and consequent increases in oral contraceptives consumption did not translate to significant changes in the number of live births. This contrasts the significant increases in births caused by the price increase of January 2008.[21] Just ten weeks after the price shock, we document an increase of about 54 births per week; five weeks later, there are on average 106 more births. As expected, and in line with medical evidence, the risk of conception increases with time as the effect of past contraceptive medication wears off and the natural menstrual cycle is progressively restored by gradually extending the length of the luteal phase, which improves the chances of a successful pregnancy (Gnoth et al., 2002). The effect on total conceptions peaks during mid-year at around 146 extra births per week, which represents 3.2% of the births that take place in the average week and yields a price elasticity of 0.066. Figure 5a depicts the patterns emerging from the para-

[20]A more thorough analysis of the contraceptive demand elasticity can be found in Appendix B

[21]In the estimations, we include all weeks after the price increase, although we should not find any effect during the first two weeks after the price shock. This is because women would need *at least* half of a menstrual cycle after stopping the Pill intake to conceive and the soonest a woman can be affected by the price shock is on January 1^{st} of 2008.

metric model and it illustrates the extent to which the the massive price increases—and not the price reductions—caused a significant change in births.

The asymmetry in the fertility responses to changes in the price of oral contraceptives is remarkable and provides a key insight about the self-selection into and out of the Pill's consumption. Existing literature shows the existence of asymmetric demand responses to price changes in different goods (Bidwell et al., 1995; Gerlach et al., 2006; Vespignani, 2012), most notably in the energy market (Gately and Huntington, 2002; Moosa et al., 2003; Adeyemi and Hunt, 2014). It is well stablished in this literature that consumers are more sensitive to price increases than to price decreases (Bidwell et al., 1995). Several reasons could explain such asymmetry. Evidence from theoretical and experimental economics show that agents may react differently to price increases than to price decreases due to psychological features like loss aversion and the role of reference points in determining utility (Tversky and Kahneman, 1991). Another reason could be the availability of information (Abaluck and Adams, 2017). Current consumers of the good are aware that the price goes up and they adjust their consumption immediately. Instead, in order for price decreases to yield an effect on consumption, those who are not the usual consumers of the good need to become aware of the price change, which could potentially take more time.

Our asymmetric results on overall fertility are remarkable because we find that the expansion of the Pill's consumption during the price war was innocuous, while its contraction during the collusion period had large effects that materialized even before consumption fell below pre-price war levels (we find impacts on extra conceptions by week 10 after the price increase even though we do not observe contraceptive sales retreating to pre-price war levels until week 20). Thus, the asymmetry must be the result of who is selecting into the pool of consumers during the price war and who is being priced out by the price increase during the collusion. We will revisit this issue below.

Our parametric results can be biased if the functional forms assumed are misspecified. To check for this we also estimate a non-parametric model whose results we present in

Figure 5b.[22] They corroborate our findings in Table 3 (and Figure 5a). They show an increasing effect that peaks during mid-year reaching a year-to-year increase of 164 conceptions per week, and the returns to pre-price war levels.[23]

The fact that the effect of the price increase peaks during mid-year is in line with medical evidence that shows that women continue to experience cycle disturbances that may prevent a conception for five to nine months after they discontinue the use of oral contraceptives (Gnoth et al., 2002). The effect of the price shock persists through September, after which we no longer observe extra weekly conceptions. This feature suggests that the increased pregnancy risk was later on counteracted either by behavioral changes that took some time to materialize (e.g., substitution to a different birth control mechanism), or by dynamic selection out of the pool of women that could get pregnant—given that that those who conceived were no longer able to do so in later weeks.

Another possible explanation is the introduction of emergency contraception (i.e., the morning after pill) which was illegal until mid 2008. However, Bentancor and Clarke (2017) show that such introduction of emergency contraception did not have significant effects on birth rates (see Table 4 in Bentancor and Clarke, 2017). Furthermore, in Figure 6 we report the non-parametric estimates of the effect of price changes on weekly births in the municipalities that did not allow emergency contraception to be distributed in 2008.[24]

[22]In the non-parametric procedure we control for the distance to the shock using Local Linear Regressions (LLR) for the practical estimation of the parameter of interest τ^{BA}, since it minimizes bias when estimating regression functions at the boundary and allows wide flexibility for exploring the treatment dynamics (Fan and Gijbels, 2000). More specifically, we solve:

$$min_{\alpha_t^l \beta_t^l} \sum_{t<s^*} (\nabla \nu_t - \alpha_t^l - \beta_t^l (t-t^0))^2 K\left(\frac{t-t^0}{h}\right) \text{ and } min_{\alpha_t^r \beta_t^r} \sum_{t \geq s^*} (\nabla \nu_t - \alpha_t^r - \beta_t^r (t-t^0))^2 K\left(\frac{t-t^0}{h}\right)$$

where, K is the kernel function, h is the bandwidth chosen following Calonico et al. (2014) and $\nabla \nu_t = \nu_t - \nu_{t-52}$ is the de-trended and de-seasonalized transformation of the outcome variable, where ν_t comes from regressing conceptions at time t, Y_{it}, on a time trend (i.e., $Y_t = \alpha + \gamma t + \nu_t$). This way, we compare weekly conceptions between the same week of each year, absorbing the peaks and nadirs of conceptions within the calendar year. Therefore, the estimated average treatment effect at time t is given by $\hat{\tau}_t^{BA} = \hat{\alpha}_{s^*}^r - \hat{\alpha}_t^l$

[23]For more non-parametric results, please refer to Appendix I. The complete set of non-parametric estimates for all the subsamples are available upon request from the authors.

[24]The distribution of emergency contraception found an unexpected route to legality in Chile in 2008 after the Supreme Court and the Constitutional Tribunal deemed illegal its prescription by for all nationally run health establishments. Inadvertently, the ruling allowed its prescription by *locally* run heath establishments. Hence, emergency contraception's availability depended on the choice made by the each municipality's mayor (Bentancor and Clarke, 2017).

The Figure shows the same inverse U shape for the effect of the price increase in January 2008 we observe in the overall results. The effect faded away even in municipalities were emergency contraception was not introduced. Therefore, the introduction of emergency contraception cannot be responsible for the decline in extra births during the second half of 2008.

Effects by individual characteristics. Results in Table 4 show a significant increase in the number of weekly out-of-wedlock births. Fifteen weeks after the 2008's price increase, we find 85.5 extra conceptions from unmarried mothers. This number goes up to around 120 by mid-year. This represents about 4.2% of the out-of-wedlock births that take place within the average week. Table 4 also shows that if we split the total effect on fertility by the age of the mother, we see the largest effects among women in their early twenties (20-24). During the peak of the effect, we register 70 extra weekly births from mothers in this age group. That represents a 6.5% increase; twice the size of the overall effect on fertility and the effect we find on births from women in the 25-29 age bracket.

Our results show that the price war drew nulliparae women into the consumption of the Pill. Table 4 reports up to 69 less conceptions from women without children in the second half of 2007, when the price was the lowest. That represents a 3.4% decrease with respect to 2006 levels. However, the trend reversed after January 2008's price increase and the reductions in first-child conceptions achieved during the price war reversed very fast. By week twenty after the price increase, there were around 30 more first-child conceptions, and by week 29, the effect reached 37.5 additional first-child conceptions relative to the pre-price war period.

Interestingly, we find no effect among teenage mothers or among households that live in poorest municipalities. The results presented in Table 4 confirm our hypothesis that the observed changes in the number of births is the result of a behavioral response to the rise in prices. Poor households and teenagers, most likely not using birth control pills as a contraceptive method, do not respond to the price change as much as the rest of the population did. Poor households not only have a significantly lower rates of Pill use (Ministerio de Salud, 2007), but are also more likely to get contraceptives through the public health system.[25] Use of the Pill is also very low among teenagers. Only 6.6% of teenagers use oral contraceptives, in contrasts with 19.8% of all women in fertile age.[26]

Births, weight at birth and mother's education. Table 5 shows that the slow price decline during 2007 did not have fertility responses in women from any education group. However, it shows that the sudden price increase in January 2008 did have an effect on fertility among less educated women. While we see no significant effects among college educated women and a small—relative to the overall response—increase in fertility among women with high school degrees (64.2 extra births, representing a relative increase of 2.3%), at the peak of the response, high school dropout women increased their fertility by 6.2%, almost doubling the overall effect we detect.

Table 5 also shows that conceptions that resulted in underweight newborns were falling on a year-to-year during the period when contraceptive prices were falling as the pharmacies engaged in a price war. Then, the price spike that resulted from the pharmacies colluding broke the trend and weekly underweight births drastically increased (see Figure I.1 in the Appendix for non-parametric estimates). The increase of the contraceptive prices caused a significant increase in the number of underweight births (measured as the proportion of newborns with low birth weight for gestational age, as indicated by Mikolajczyk et al., 2011). At the peak of the effect, there were 15.4 more underweight births per week. This figure represents a 9.5% increase in the weekly average underweight births relative to the pre-treatment period (three times the relative size of the impact from total live births). Despite our empirical difficulties to separate mistimed, unplanned or unwanted pregnancies from cases in which a woman is unaware she is pregnant during a critical period of prenatal development, such large effect is in line with medical literature linking unintended pregnancies with the incidence of underweight newborns (Sharma et al., 1994). However, and regardless of the circumstances, the result raises

[25] Using data on government procurement of contraceptives, we found that the government did not react to the price increases by acquiring more contraceptives in order to supply them to the public.

[26] The 6.6% prevalence rate come from the following facts: only 40% of teenagers aged 15 to 19 have been sexually initiated (Ministerio de Salud, 2007), only two-thirds of those use any kind of protection in their sexual relations, and among the latter only one in four use the Pill (while two-thirds use condoms) (INJUV, 2009).

concerns as weight at birth represents a good proxy for child endowments (Almond and Currie, 2011a). It is the outcome of genetic background, the extent to which parents were involved in pre-natal care and mother's previous health and habits (Currie, 2011). As such, it has been shown to be a determinant of cognitive development (Torche and Echevarría, 2011; Figlio et al., 2014), school attainment (Oreopoulos et al., 2008) and even future earnings (Behrman and Rosenzweig, 2004; Black et al., 2007).

Finally, the bottom panel of Table 5 shows that underweight babies were born overwhelmingly to high school educated mothers. At the peak of the effect 13.8% of all the 'extra' births induced by the price increase in this group were underweight, while in comparison virtually no underweight babies were born to college educated women and 8.5% of the 'extra' births among high school dropouts were underweight. These heterogenous responses across education levels may be the results of a trade-off between lower taste for childbearing (i.e., less inclined to adopt healthy behaviors) and wealth effects (i.e., more likely to be willing to pay the costs of adopting healthier behaviors). Our findings are consistent with the idea that college educated women may comprise a subgroup of the population for which the wealth margin could improve faster the average health quality of the 'extra' newborns than the deterioration resulting from the taste-for-children margin (the overall effect depends mainly on the marginal returns to health inputs). Hence, the health of the babies conceived does not deteriorate as it does in other subgroups. This further relates to the fact that college educated women may be more prone to perform some remedial investments after cryptic pregnancies are revealed.

5.3 Fetal Deaths

Just like underweight newborns, miscarriages and stillbirths were consistently falling on a year-to-year basis during the price war period.[27] Table 6 and Figure 7 show that on average there were 15.5% less fetal deaths per week. However, when pharmacies

[27] The difference between miscarriages and stillbirths is based on the length of gestation until the product of conception leaves the mother's body, being miscarriages early fetal deaths and stillbirths late fetal deaths. The threshold after which a fetal death is considered a stillbirth is still in contention in the medical literature. Thresholds vary from 18 to 28 weeks, being 22 and 28 the most commonly used (Lawn et al., 2011).

colluded and increased contraceptive prices, fetal deaths increased significantly. Weekly conceptions that resulted in miscarriages and stillbirths increased by around 5.3 during their peak in mid-year relative to pre-treatment levels.[28] Such an increase represents a 12.3% growth in the average weekly fetal deaths, around four times the relative effect the price shock had on live births.[29] This disproportionate effect on fetal deaths relative to live births may be the product of either a biological response to going off the Pill, or a behavioral response to an unintended pregnancy.

Regarding the biological response, medical literature shows that women who take contraceptives for a very long time face an increased probability of miscarriages when they become pregnant (García-Enguídanos et al., 2005). However, we do not find differential effects of the price shock across age groups of nulliparous women. If we assume that older nulliparous women are more likely to have longer exposure to hormonal contraceptives than younger nulliparous women, we would expect more fetal losses among older women. Regressions in Table I.1 in Web Appendix I show no evidence of differential effects of the price shock on fetal losses across the two age groups. Furthermore, non-parametric results presented in Figure I.2 show that even though there are effects for first-time pregnant women in all age groups, these effects are not statistically different across age groups. That is, the confidence intervals of the four estimates always overlap with one another.[30]

That leaves us with the behavioral response hypothesis. That is, the possibility that these pregnancies were more likely to be unintended and women were not prepared to

[28] The non-parametric estimates in Figure 7 show a year-to-year increase of 11 fetal deaths after the collusion. The difference from these estimates and the ones in Table 6 is that while the latter reports the causal estimates in reference to pre-treatment levels (i.e., before 2007), the non-parametric estimates present year-to-year changes. Therefore, the 11 fetal deaths by mid-2008 reported in the non-parametric estimates are equivalent to the sum of the effects reported by the parametric estimation in mid-2007 and mid-2008 (i.e., 6.9+5.2).

[29] Table 6 shows that the effect is the largest on women from municipalities in the third quartile of the income distribution. Hence, there is a reason to believe that middle-class women were the most affected by the price shock. While women in low-income municipalities might not be able to afford contraceptives in the first place and women in high-income municipalities are able to afford contraceptives even after the price increase, middle-class women find the price increase binding, ergo the pregnancies.

[30] Fertility literature has linked mother's age with the likelihood of miscarriages and stillbirths (see, for example, Andersen et al., 2000). They show that the likelihood of these events remain under 20% for women younger than 35 years old, but starts increasing rapidly after that age, reaching 84% for women above 45. This feature does not play a confounding role in our comparisons across ages because in each of our estimates of the effect we are comparing people within the same age group before and after the discontinuity.

invest in the healthy development of the pregnancy, potentially neglecting or interrupting it. This is consistent with research showing that granting access to contraceptives can drastically reduce the incidence of abortion. For instance, Peipert et al. (2012) found that providing at-risk women with free access to long-acting reversible contraceptives in St. Louis reduced the abortion rate by half. Due to a lack of information, and given the unlawfulness of abortion in Chile, we are not able to disentangle fetal deaths due to poor health from intentional abortions—reports estimate there are around 70,000 yearly clandestine abortions in Chile (Casas and Vivaldi, 2013). Furthermore, because of the way fetal death records are collected, the results we provide on the impact of the contraceptives' price increase on fetal deaths serve as a lower bound. As explained in Section 4, they are recorded only if the physician or the midwife identifies the "product of the conception". Therefore, fetal deaths at early stages of the pregnancy are less likely to be recorded.[31] In fact, when we analyze stillbirths and miscarriages separately, we find that the effect on fetal deaths is due almost exclusively to a year-to-year increase in stillbirths and not in miscarriages. Such findings suggest that the contraceptives' price increase caused an increase in the number of unhealthy fetuses. The next section explores whether this holds for live births as well.

5.4 Infant Deaths

Just as the contraceptives' price changes caused significant shifts in the incidence of fetal deaths, they could also have resulted in changes in the number of unhealthy babies born alive. Having shown that the number of underweight newborns fell due to lower contraceptive prices, and then they dramatically increased due to the price shock after the collusion, we now turn to infant mortality (i.e., the number of children that were born alive and died before they completed their first year of life), an even more stringent margin.

Prior to 2007, there were around 2,000 infant deaths per year in Chile (representing an infant mortality rate of about 8 per 1,000 live births) due to numerous causes. In fact, our

[31] Pop-Eleches (2010) provides evidence on the fact that intentional abortion is a relatively common birth control mechanism that has significant impacts on fertility.

data shows that physicians list 589 different diagnoses as causes of the infant deaths that range from congenital malformations to infections and trauma. However, almost 80% of the infant deaths we observe can be classified in two broad categories: congenital malformations and conditions originating in the perinatal period (i.e., the time immediately before and after birth). We focus on analyzing deaths that are due to conditions related to the mother's health and habits, prenatal care, or failed attempts to end the pregnancy. In particular, we are interested in diagnoses that reflect unpreparedness of the expectant mother, a lack of healthy habits, or exposure to toxic substances while in-utero. Namely, babies born extremely small or immature to sustain life, perinatal complications, brain malformations, and malformations likely caused by exposure to harmful environments.

Providing further evidence on the role the Pill plays in shifting the average health of the babies conceived, the results presented in Table 7 show that just like fetal deaths and underweight births (i.e., indications of poor fetal health), infant mortality fell as contraceptives became cheaper and their consumption increased—just to bounce back when prices increased and consumption retreated in 2008. We estimate that infant deaths fell by about 18% due to the increased consumption of contraceptives during the price war. When pharmacies started colluding, those gains were lost and overall infant mortality stopped its decline. In fact, we find almost symmetric increases in infant mortality after the first week of 2008, although not statistically significant. However, when we limit the analysis to conditions related to unpreparedness of the expectant mother, a lack of healthy habits, or exposure to toxic substances while in-utero, we find a clearer picture on the effect of contraceptives' price changes on newborn health.

Table 7 indicates that the effect of contraceptives' price changes on weekly infant mortality due to conditions arising during the perinatal period account for almost the entirety of the effect we find on total infant mortality. We find they fell on average by 28.7% due to the steady decline in contraceptives' prices in 2007.[32] However, the sudden

[32]The conditions generated in the perinatal period explored in Table 7 include newborn affected by maternal factors and by complications of pregnancy, labour and delivery, disorders related to length of gestation and fetal growth, birth trauma, respiratory and cardiovascular disorders specific to the perinatal period, infections specific to the period, hemorrhagic and hematological disorders of the newborn, transitory endocrine and metabolic disorders specific to the newborn, digestive system disorders, conditions involving the integument and temperature regulation of the newborn, and other unclassified

price increase in 2008 led to significant increases in the number of this type of infant deaths. On average, about 7 'extra' infants died to these causes when contraceptives' consumption fell. This represents a massive increase in such deaths of about 40% relative to pre-treatment levels.

When we analyze in more detail the effect on the diagnoses that are bundled together as perinatal conditions, we see contraceptive consumption shifts affect infant mortality mainly by decreasing or increasing the number of extremely premature or immature babies, in line with our findings of low birth weight and fetal deaths. In fact, infants deaths due to extreme immaturity of the baby (i.e., babies with low gestational age or born too small and weak to survive) went up by 50% due to the contraceptives' price increase. Such uptick in the number of extreme immature newborns is also evident in the increased number of deaths due to intracraneal nontraumatic hemorrhage, a condition highly prevalent in extremely premature babies. We find that deaths due to this condition doubled as a consequence of contraceptives becoming more expensive in 2008. In the same way, we find that weekly infant deaths due to necrotizing enterocolitis of the newborn—a condition closely related to fetal immaturity that typically develops among premature babies, especially those that are formula fed, and is the second most common cause of death among premature infants (Panigrahi, 2006)—decreased by about 75% among babies conceived as contraceptives' prices were declining, especially in the second half of 2007. This is further evidence supporting the idea that the contraceptives' price changes and subsequent consumption shifts led to reductions (in 2007) and increases (in 2008) in the number of babies that ended up being conceived in environments lacking the necessary resources for their adequate development.[33]

disorders originating in the period like convulsions of the newborn, neonatal cerebral ischemia, feeding problems of the newborn and disorders of muscle tone.

[33]We implement the empirical strategy outlined in Section 3 to a different data source containing information on mothers and births for the relevant period, The Early Childhood Longitudinal Survey (Encuesta Longitudinal de Primera Infancia or ELPI). In line with findings in the public health and medical literatures that indicate that women with unintended pregnancies tend to have a harder time quitting unhealthy behaviors even after they know they are pregnant (Dott et al., 2009), we find that women who conceived after the price hike were twice more likely to drink alcoholic beverages during pregnancy than those who became pregnant before the contraceptives' price shock. Also, they end breastfeeding one month earlier (Jayachandran, 2014), and the babies conceived were 5 percentage points more likely to have below-median cranial circumferences.

Regarding anatomical malformations, we see a significant change in the number of deaths related to cardiac malformations. We find that the increased consumption of oral contraceptives in 2007 caused the weekly infant mortality rate due to malformations of the cardiac chambers, connections and valves to drop by 80% during the second half of 2007.

Overall, the estimated impacts on infant mortality show that oral contraceptives' take-up affects the average health of the babies born by preventing the conception of children that are less likely to have adequate resources for their development. The nature and causes of the conditions we identify as most likely to be influenced by the Pill's take-up, together with our findings on miscarriages, are suggestive of the relation between oral contraceptives' consumption and the number of unintended/unknown and neglected pregnancies (in congruence with the medical literature on the topic) (Bustan and Coker, 1994).

Our findings regarding the year-to-year increase in the numbers of mothers in the early twenties; out-of-wedlock, first-born, underweighted births; fetal and infant deaths suggest an increase in the number of unintended pregnancies due to the pharmacies' colluding practices. Due to the skyrocketing prices of contraceptives, people who otherwise would have avoided pregnancy ended up conceiving. This resulted not only in a significant increase in weekly births, but also in the arrival of less healthy babies. Overall, these findings are consistent with the idea that a sudden interruption of accessibility to contraceptives can increase the number of unintended or cryptic pregnancies, preventing mothers' healthy behaviors and impacting the health of the newborns. We further explore this in Section 5.6.

5.5 Falsification and Robustness Checks

Dropping 2007 conceptions. To assess the robustness of our empirical strategy, we re-estimated the model but now dropping the conceptions from 2007. The results are presented in panel A of Table 8, and they follow closely what was originally reported in Table 3. Thus, our findings remain robust to this change.

Modifying the discontinuity date. We examine a placebo situation in which we set the discontinuity one year prior to the price increase. Figure 1 shows that, unlike the first weeks of 2008, during the first weeks of 2005 prices did not change dramatically compared to those at the end of the previous year. Therefore, if our results identify behavioral responses to the contraceptives' prices change, we should see no differential effects on weekly conception just before and after January 2007.

Panel B of Table 8 presents the results when the discontinuity is timed at January 1^{st} of 2005. These results confirm the distinctive patterns in live births documented for 2008, and support the hypothesis that these are attributed to exogenous variations in birth control pills prices. Thus, there are no differences in the year-to-year growth of weekly conceptions, providing evidence that our results do come from behavioral responses to the price increase in 2008 and not from mechanical features of the estimation procedure. Our Web Appendix II presents further results from exercises, all providing evidence in support of our main hypothesis.

Pharmacy Availability. Web Appendix III explores the effect of the exogenous increase in birth control Pill's price by the availability of pharmacies in a given *comuna*. This is important as consumers facing scarcity of pharmacies might be less exposed to price shifts. To test this hypothesis, we identify *comunas* with high and low density of pharmacies using the number of stores per capita as a proxy. Our findings indicate that contraceptives' price increase had distinctive effects across these groups, with earlier effects in high density *comunas* relative to low density comunas (see Table III.1 for details).

Alternative empirical strategy. In principle, every women in the country experienced the exogenous changes in birth control Pill's prices during 2007 and 2008. As a consequence, the implementation of a conventional Difference-in-Difference strategy exploring the behavioral responses of two groups (control and treatment) before and after the changes is not suitable in this case. Despite of this fact, in Web Appendix IV we pursue such strategy using an ad-hoc taxonomy delivering treatment and control groups.

The former is comprised of women younger than 20 (low consumption of oral birth control Pill) whereas the latter of women older than 19 (high consumption). Data from Chile's National Study of Youth of 2006 justifies this classification based on pre-2007 Pill's consumption (see Web appendix for further details). As usual, the second difference exploits the timing of the collusion. The results are presented in Table IV.1. The estimated impacts are similar to those obtained using the interrupted time series approach (see Table 3). This confirms the robustness of our findings to different empirical strategies.

5.6 Long Term Outcomes

We now investigate the long-term impact of the price hike of 2008. In particular, we examine differences in school enrollment at least five years after the price increase between two groups: students conceived before and after the 40th week of 2008. In the context of our previous findings, differences in favor of the former group could suggest that those conceived right after the price hike effectively faced more deprived early development (Black et al., 2007; Currie and Moretti, 2007). To this end, we use publicly available administrative information on school attendance for the academic years 2013 to 2016. The empirical strategy follows a simple difference-in-difference model where we use the cohort of students conceived at the end of 2006 or beginning of 2007 (before the price war) as control group. We focus on enrollment in kindergarten, first and second grades.

Some institutional background is needed before presenting our results. In Chile, first grade enrollment is compulsory for children who turned six years of age by March 31st of a given year. Therefore, this group is expected to be attending a school by March of that year (the academic year goes from the first week of March to beginning of December). Nevertheless, the Ministry of Education, upon parent request, might authorize children turning six before June 30th to enroll as well.[34] On the other hand, although similar entry age requirements apply to kindergarten, enrollment in this level was not compulsory for the cohort of individuals potentially affected by the collusion (it became mandatory only

[34] Requests to enroll children not old enough are common in Chile. For example, out of the 242,041 children attending first grade in March 2016, 10.65% reported as month of birth April (4.88%), May (3.53%), or June (2.24%) of 2010. Only 177 first graders, equivalent to 0.03%, reported a date of birth beyond June 30th of 2010.

after 2015). Therefore, children turning five by March 31st of a given year could enroll in kindergarten at public or private subsidized schools offering the level.[35]

We first focus on kindergarten enrollment (academic years 2013 and 2014).[36] Table 9 presents the diff-in-diff results. Column (1) does so for the sample of children in the treatment (control) group born between weeks 34-48 of 2008 (2007) and Column (2) for those born between weeks 27-55. The comparison across columns informs about whether the results remain robust as we move away from the 40th week. We find negative and statistically significant effects. The price increase reduced kindergarten enrollment five years later in at least -0.84 per each 1000 births. At first glance this might seem a small number. However, this is expected as we are capturing differences emerging from "extra" births resulting from the price hike.[37]

The rest of Table 9 shows that the reduction in kindergarten enrollment was larger among middle-income municipalities (columns (3) and (4)), and municipalities of the Metropolitan Region with high density of pharmacy locations (columns (5) and (6)). The point estimates are negative and larger than those reported under columns (1)-(2) providing additional information on the type of families that were less likely to enroll their children in kindergarten.[38] Nonetheless, since kindergarten enrollment was voluntary,

[35]Our empirical strategy remains very similar to the one used throughout the paper. The only difference is that we need to incorporate the details of the timing of school enrollment. The children that could potentially be affected by the collusion case were those eligible to attend kindergarten by March 2014. Among them, we distinguish two groups: those more likely to have been conceived before the price increase (born before September 2008) and those more likely to have been conceived after the hike (born during or after September 2008). Thus, September defines the treatment threshold. As before, we use the children conceived one year earlier (i.e., those eligible to enroll in kindergarten by March 2013) to control for seasonality.

[36]We proxy enrollment with the report of at least one day of school attendance during the first quarter of the year the student became eligible to enroll.

[37]Unlike all the data used so far, we do not observe gestation length in the school enrollment records. Therefore, although we observe the exact birthdate of enrollees for the years 2013 and 2014, we have some uncertainty on the precise conception week. In consequence, we include specifications that exclude individuals born within few weeks of the 40th week of the year. These mimic a donut-hole specification. Moreover, to examine how sensitive our results are to the downward trend in prices observed during 2007, we present results covering two time periods: all births from weeks 34 to 48 of a given year, and all births from week 27 of a year and week 3 of the following year (we label it "week 55"). As we better capture differences between those conceived before and after the effect of collusion if we exclude the uncertainty surrounding week 40 of 2008, we expect the negative effects to magnify in the donut-hole specifications. These findings are presented in Table V.1 of our Web Appendix. Indeed, excluding weeks before and after the 40th week leads to large effects on kindergarten enrollment.

[38]The estimated effects for high-income municipalities are small and positive, while among poor municipalities they are not conclusive (they change sign depending on the weeks considered). The full set of results for municipalities from the Metropolitan Region are presented in Table V.2 in the Web

underinvestment in the "extra" post price conceptions might not be the only factor behind these findings. Selection into pre-primary education emerging from, for example, potential changes in family size and its impact on child's education (Black et al., 2005), could also explain the results.

To mitigate concerns arising from voluntary enrollment, we repeat the analysis but now for the academic years 2015 and 2016. This allows us to examine mandatory enrollment in first and second grades. Table 10 presents the results. While we estimate a small and non-significant negative effect on first grade enrollment, we find that the 2008 price increase reduced second-grade enrollment by 5.70 students per each 1000 births seven years later. The differences across grades suggest higher grade retention in second grade among those individuals conceived after the price hike relative to those conceived before the event.[39]

We further use administrative records to inquire about the effects that the 2008 price hike might have had on enrollment in programs dedicated to supporting the educational needs of students with disabilities in different areas. These include hearing impairment, vision impairment, speech-language impairment, physical impairment, autism spectrum disorder, and intellectual disability. However, most of the children with educational needs report attending a program for students with intellectual disability, so we focus on those programs.[40] And as before, we interpret any attendance during the first quarter of the academic year as a proxy for overall enrollment. We report the point estimates in Table 11. Our findings reveal a positive association between the price increase of 2008 and enrollment in special programs six and seven years later. In particular, among first and second graders, the results disclose increases of 0.47 and 0.83 students with special needs

Appendix.

[39] For the academic years 2015 and 2016 only month and year of birth are reported. This prevents us from estimating specifications that exclude children born between specific weeks of a given year. Instead, in our Web Appendix we report results excluding those children born between September and October. Panel A of Table V.3 displays the results for the whole sample. The point estimates in Panel B confirm the negative impacts for both grades after excluding individuals born between September and October. In this case, the point estimates are -5.33 and -13.15 for first and second grade, respectively.

[40] Among first graders in our cohort of interests, 79.13% and 78.59% of the children reporting educational needs in 2014 and 2015 attended a program for students with intellectual disability. For second graders in 2015 and 2016, the figures are 92.57% and 91.44%, respectively

(intellectual disability) per each 1000 births.[41]

Overall, we interpret these findings as an indication that those conceived during the first weeks of 2008 (post price increase) were more likely to face adverse conditions during critical periods of development, which—in line with economic evidence (Currie and Hyson, 1999; Behrman and Rosenzweig, 2004; Currie and Moretti, 2007)—resulted in worse early education outcomes.

6 Conclusions

This paper exploits exogenous price variation in birth control pill prices arising from market failures in the pharmaceutical sector in Chile as a natural experiment to study fertility responses and their consequences on newborns' outcomes. We document sizable and asymmetric short-run responses. While our estimates show no fertility reactions to slow and continuous decreases in contraceptives' prices of 2007 (price war), we find large responses to the sharp and unexpected price increase documented during the first weeks of 2008 (collusion case). In fact, we estimate the price hike produced between 139 and 167 additional births per week in Chile during the relevant period. Moreover, we document larger fertility responses among unmarried and primiparae women, and as expected, we do not find significant impacts among poor households and teenage mothers. We provide several falsification tests and robustness checks. Our results are robust to different settings.

Unlike overall fertility, when analyzing newborns' health, we find that poor health indicators improved during 2007, but that trend dramatically changed in 2008. Following the sudden oral contraceptives' price increase agreed by the firms, we document a disproportionate increase in the total number of underweight births and of miscarriages and infant deaths. Overall, the evidence points to the deterioration of average newborns' health (birth weight/mortality) among those conceived in the first months of 2008, suggesting this group was disproportionally more likely to face adverse conditions

[41] Table V.4 in the Web Appendix presents the results aggregating all special education programs. The results confirm our findings. Moreover, Table V.5 in the same appendix shows that the results in Table 11 increase in magnitude after excluding students born in September of October (panel B).

during critical periods of fetal development. Furthermore, this hypothesis is reinforced by our assessment of the impact of the price hikes on long-term outcomes. As 'extra' children reached school age, we document inferior school outcomes: lower kindergarten and second-grade enrollment rates, as well as with an increase in the number of children requiring special education several years after the event.

Our findings suggest that the interruption of the Pill intake could have increased both the number of unintended pregnancies and the number of women unaware they were expecting during a critical period (first trimester), impacting the short- and long-term health of the newborns. In this context, this paper presents new evidence that anti-competitive behaviors can cause substantial and long-lasting harm to consumers (and their descendants).

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Figure 1: Average Daily Contraceptive Prices by Pharmacy: January 2006-January 2009

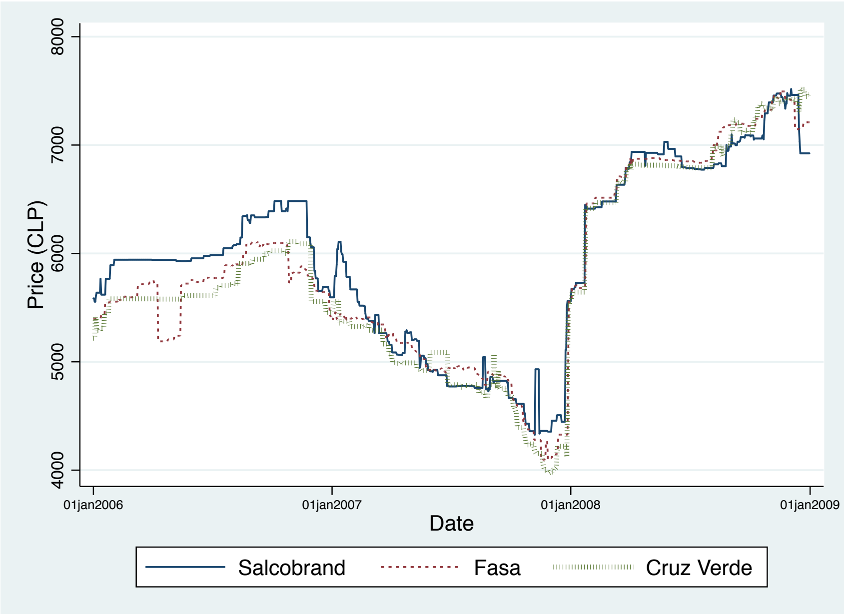


Figure 2: Conceptions per Week

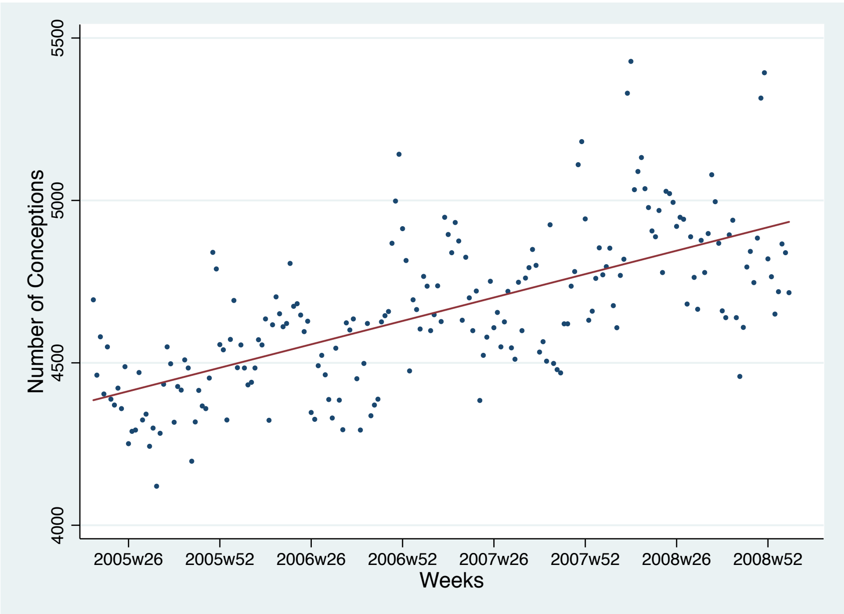
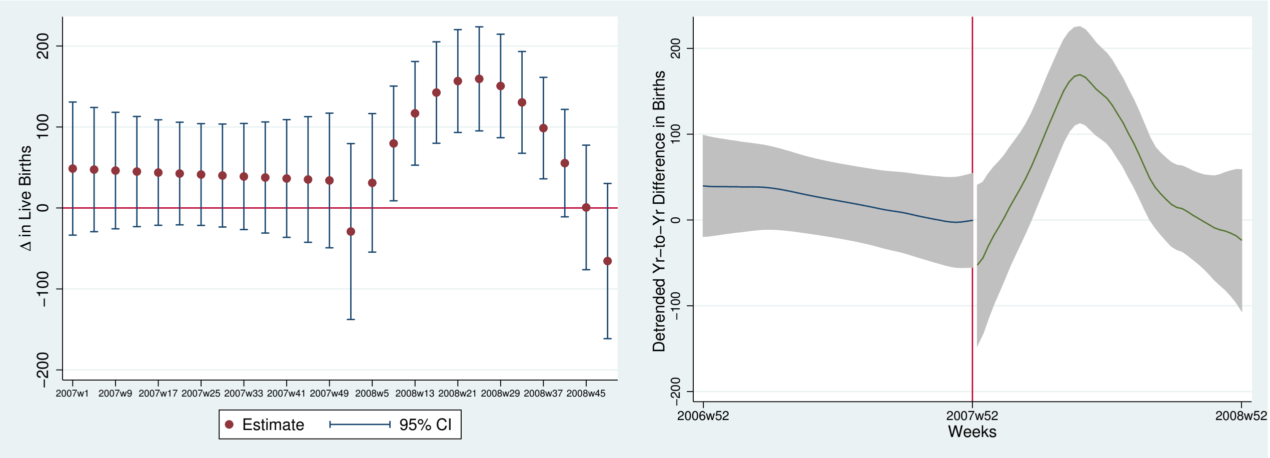


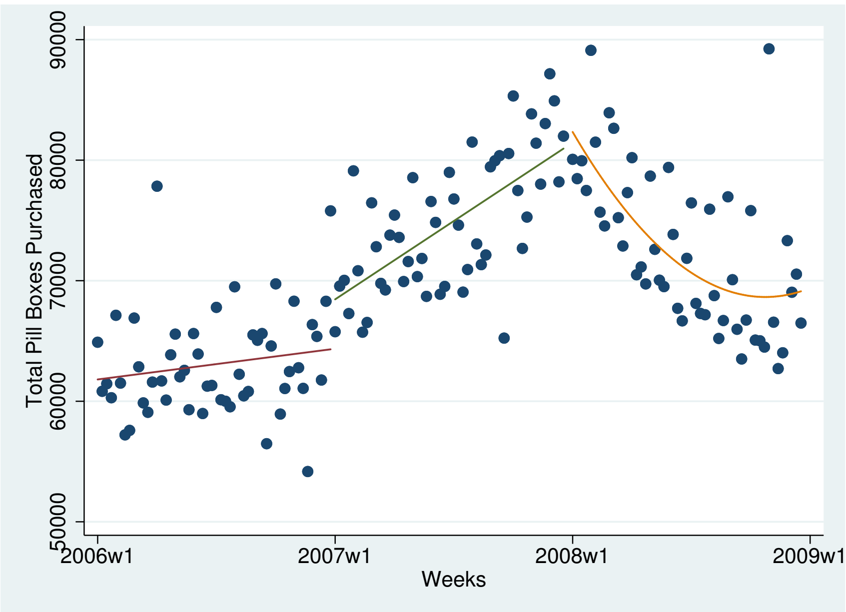
Figure 5: The Effect of the Birth Control Pills' Price War and Later Increase on Weekly Live Births by Conception Week

(a) Parametric Method **(b)** Non-parametric Method



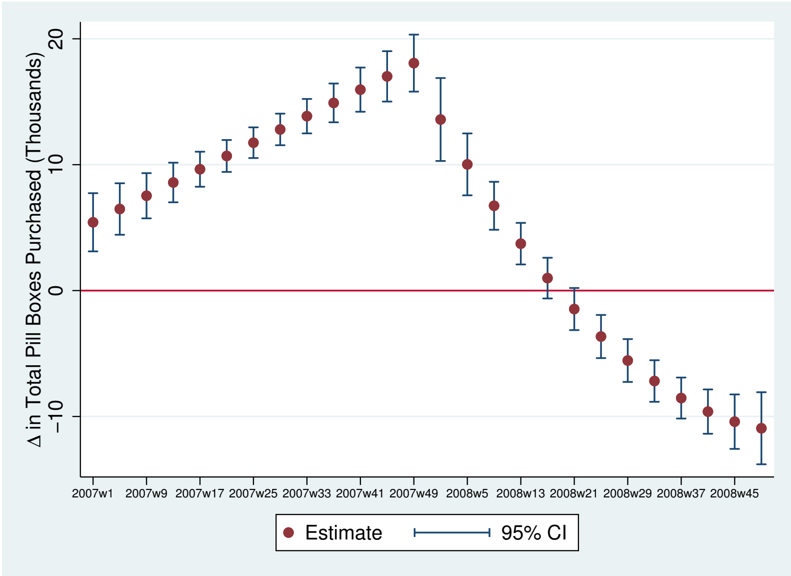
Note: Panel (a) presents the estimated effects of increases in birth control pills prices on weekly live births. Point estimates and associated standard errors come from Table 3 (quadratic polynomial specification). Panel (b): Births series are first de-trended (linear trend) and de-seasonalized by standard methods (dummies per week of year). Then, year-to-year differences in weekly births are computed. The conception week is obtained by subtracting the pregnancy length to the birth date.

Figure 3: Weekly Contraceptives Purchases: January 2006-January 2009



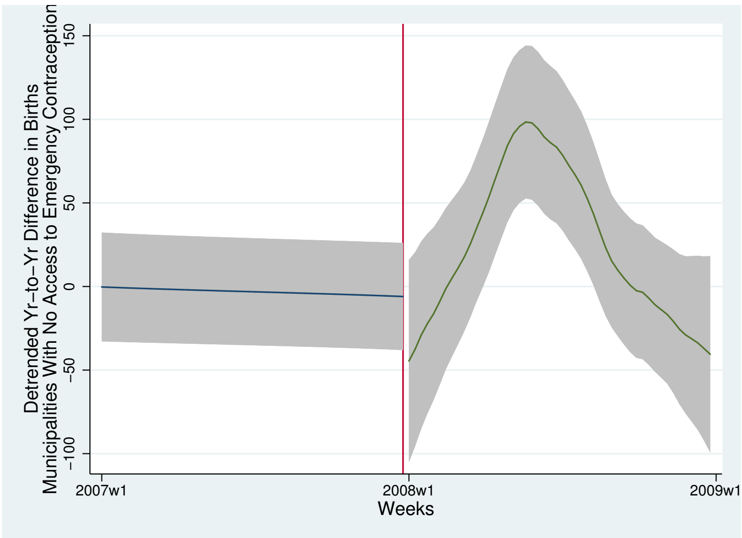
Note: Lines represent fitted values.

Figure 4: The Effect of the Birth Control Pills' Price War and Later Increase on Weekly Units of Birth Control Pills Sold



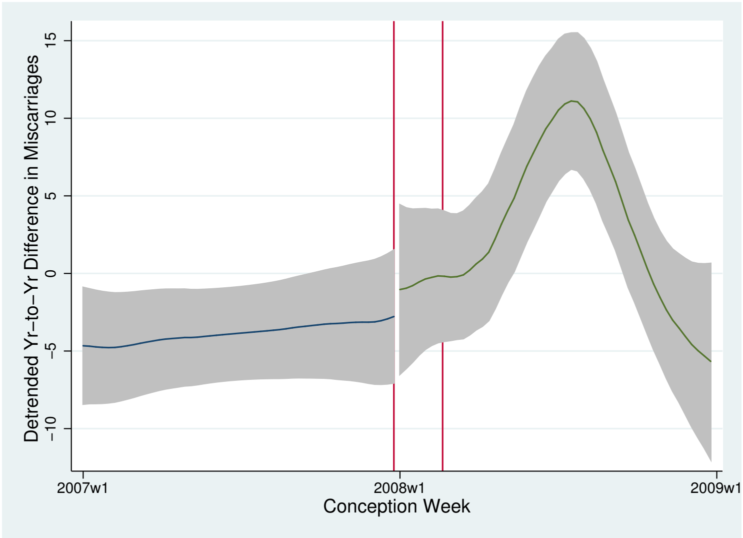
Note: This figure plots the estimated effects of increases in birth control pills prices on the units of contraceptives sold (thousands of boxes per week). Point estimates and associated standard errors come from Table 2 (quadratic polynomial specification).

Figure 6: The Effect of the Birth Control Pills' Price War and Later Increase on Weekly Live Births in Municipalities Where Emergency Contraception Was not Allowed in 2008, by Conception Week
(Non-parametric Specification)



Note: Births series are first de-trended (linear trend) and de-seasonalized by standard methods (dummies per week of year). Then, year-to-year differences in weekly births are computed. The conception week is obtained by subtracting the pregnancy length to the birth date.

Figure 7: The Effect of the Birth Control Pills' Price War and Later Increase on Weekly Miscarriages and Stillborns by Conception Week
(Non-parametric Specification)



Note: Births series are first de-trended (linear trend) and de-seasonalized by standard methods (dummies per week of year). Then, year-to-year differences in weekly births are computed. The conception week is obtained by subtracting the pregnancy length to the birth date.

Table 1: Weekly Live Births and Deaths in 2007

	Total Number		**Mortality Rate**
	Average	Std. Err	
Live Births			
Total	4626.32	32.14	
Out of Wedlock	2921.29	19.91	
Low Birth weight	161.92	2.09	
1^{st} Child	2121.83	15.55	
Teen Mom	743.27	6.93	
By Mothers Age			
20-24	1083.36	5.42	
25-29	1084.88	5.93	
30-35	1116.18	4.96	
>35	579.47	2.88	
By Mothers Education			
College	1088.07	137.10	
High School	2709.88	189.07	
< High School	785.52	90.75	
Deaths			
Fetal	42.03	0.71	
Infant	38.6	0.48	7.970
Infant Deaths by Diagnosis			
Perinatal	16.55	4.12	3.577
Nervous System	0.677	0.77	0.146
Brain Malformations	0.400	0.50	0.086
Cardiac Malformations	0.926	1.05	0.200
Entorocolitis	1.451	1.08	0.314
Non-Inf. Nervous System	0.456	0.37	0.099

Note: Total number of live births in 2007: 240,569. Total number of fetal deaths in 2007: 2,165. Total number of infant deaths in 2007: 1,966. *Fetal Deaths* comprise miscarriages and stillborns, while *Infant* stands for children who were born alive but died before turning one year old. Weekly infant mortality rates should be interpreted in terms of 1,000 live births. They were calculated based on the 4,626.32 average weekly live births that took place in 2007. *Non-Inf Nervous System* stands for non-inflammatory nervous system diseases.

Table 2: The Effect of the Birth Control Pills' Price War and Later Increase on the Number of Units of Birth Control Pills Sold (in thousands)

	Week of the Year							
	1	8	15	22	29	36	44	52
Quadratic Polynomial Specification								
2007	7.97***	8.23***	8.94***	10.09***	11.69***	13.75***	16.64***	20.12***
	(1.59)	(0.93)	(0.74)	(0.81)	(0.82)	(0.75)	(0.88)	(1.59)
2008	12.74***	7.54***	2.91***	-1.14	-4.61***	-7.51***	-10.12***	-11.98***
	(1.59)	(0.93)	(0.74)	(0.81)	(0.82)	(0.75)	(0.88)	(1.59)
Cubic Polynomial Specification								
2007	7.02***	8.41***	9.42***	10.36***	11.54***	13.29***	16.38***	21.08***
	(2.07)	(0.97)	(0.99)	(0.89)	(0.85)	(0.98)	(0.95)	(2.07)
2008	12.32***	7.62***	3.13***	-1.02	-4.68***	-7.71***	-10.23***	-11.55***
	(2.07)	(0.97)	(0.99)	(0.89)	(0.85)	(0.98)	(0.95)	(2.07)
Linear Before 2008, Quadratic Polynomial Afterwards								
2007	6.03***	7.69***	9.36***	11.03***	12.70***	14.36***	16.27***	18.18***
	(1.10)	(0.88)	(0.70)	(0.58)	(0.57)	(0.66)	(0.86)	(1.10)
2008	13.71***	7.81***	2.70***	-1.60**	-5.11***	-7.82***	-9.93***	-11.00***
	(1.50)	(0.93)	(0.74)	(0.76)	(0.77)	(0.73)	(0.88)	(1.50)

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. All estimations include a linear trend. We fit a different polynomial $f(\beta, |t - t^*|)$ to each side of the cutoffs.

Table 3: The Effect of the Birth Control Pills' Price War and Later Increase on the Number of Weekly Births by Week of Conception

	Week of the Year							
	1	8	15	22	29	36	44	52
Total Births								
Quadratic Polynomial Specification								
2007	1.29	14.38	23.00	27.15	26.84	22.05	11.11	-5.66
	(57.82)	(41.55)	(37.62)	(38.87)	(39.19)	(37.81)	(40.34)	(57.82)
2008	-77.92	30.97	103.49***	139.65***	139.44***	102.86***	16.52	-117.33*
	(60.59)	(38.93)	(33.17)	(35.05)	(35.52)	(33.47)	(37.20)	(60.59)
Cubic Polynomial Specification								
2007	19.39	11.00	13.90	22.13	29.73	30.72	16.13	-23.76
	(70.10)	(41.95)	(42.47)	(40.19)	(39.45)	(42.20)	(41.59)	(70.10)
2008	-164.20**	47.06	146.85***	163.57***	125.66***	61.53	-7.41	-31.05
	(76.00)	(39.63)	(40.37)	(37.12)	(36.05)	(39.99)	(39.13)	(76.00)
Linear Before 2008, Quadratic Polynomial Afterwards								
2007	20.67	19.71	18.76	17.81	16.85	15.90	14.81	13.72
	(44.95)	(40.24)	(36.68)	(34.64)	(34.37)	(35.93)	(39.65)	(44.95)
2008	-90.84	27.41	106.32***	145.88***	146.09***	106.96***	14.05	-130.25**
	(55.44)	(38.27)	(32.67)	(32.98)	(33.19)	(32.50)	(36.83)	(55.44)

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. All estimations include week of the year fixed-effects and at least a linear trend. Sample includes conceptions that took place in the period 2005-2008. The conception week is obtained by subtracting the pregnancy length to the birth date.

Table 4: The Effect of the Birth Control Pills' Price War and Later Increase on the Number of Weekly Births by Week of Conception and Individual Characteristics

	Week of the Year							
	1	8	15	22	29	36	44	52
Out of Wedlock								
2007	19.18	16.79	14.39	12.00	9.61	7.21	4.48	1.74
	(31.66)	(28.35)	(25.84)	(24.40)	(24.22)	(25.31)	(27.94)	(31.66)
2008	-44.91	32.43	85.54***	114.42***	119.07***	99.49***	47.45*	-36.25
	(39.05)	(26.96)	(23.02)	(23.23)	(23.38)	(22.90)	(25.95)	(39.05)
Mom Age 20-24								
2007	12.71	13.43	14.16	14.88	15.61	16.33	17.16	17.99
	(16.86)	(15.09)	(13.76)	(12.99)	(12.89)	(13.47)	(14.87)	(16.86)
2008	-0.87	30.94**	53.26***	66.10***	69.45***	63.31***	44.68***	13.66
	(20.79)	(14.35)	(12.25)	(12.37)	(12.45)	(12.19)	(13.81)	(20.79)
Mom Age 25-29								
2007	19.08	14.71	10.33	5.96	1.59	-2.78	-7.77	-12.77
	(19.14)	(17.13)	(15.62)	(14.75)	(14.63)	(15.30)	(16.88)	(19.14)
2008	-49.50**	-10.45	16.35	30.90**	33.21**	23.26*	-3.11	-45.48*
	(23.60)	(16.29)	(13.91)	(14.04)	(14.13)	(13.84)	(15.68)	(23.60)
First Child								
2007	5.94	-4.37	-14.67	-24.97	-35.28*	-45.58**	-57.35**	-69.13***
	(25.29)	(22.64)	(20.64)	(19.49)	(19.34)	(20.21)	(22.31)	(25.29)
2008	-92.68***	-32.37	9.44	32.73*	37.51**	23.79	-14.57	-77.10**
	(31.19)	(21.53)	(18.38)	(18.55)	(18.67)	(18.28)	(20.72)	(31.19)
Poor 10%								
2007	-2.30	-2.65	-3.00	-3.35	-3.70	-4.05	-4.45	-4.85
	(3.09)	(2.77)	(2.52)	(2.39)	(2.37)	(2.47)	(2.73)	(3.09)
2008	0.84	1.75	2.17	2.11	1.56	0.52	-1.26	-3.68
	(3.82)	(2.64)	(2.25)	(2.27)	(2.29)	(2.24)	(2.54)	(3.82)
Teen Mom								
2007	-3.21	-3.50	-3.79	-4.08	-4.37	-4.66	-5.00	-5.33
	(12.71)	(11.38)	(10.37)	(9.79)	(9.72)	(10.16)	(11.21)	(12.71)
2008	-28.14*	-11.43	-1.87	0.53	-4.23	-16.14*	-38.51***	-70.23***
	(15.67)	(10.82)	(9.24)	(9.32)	(9.38)	(9.19)	(10.41)	(15.67)

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. All estimations include week of the year fixed-effects and at least a linear trend. Sample includes conceptions that took place in the period 2005-2008. We fit a different polynomial to each side of the cutoffs. We present the results of the model using a linear specification on the polynomials before 2008 and a quadratic polynomial specification afterwards. We do so because that is the specification that better fits the data. The conception week is obtained by subtracting the pregnancy length to the birth date. *Poor 10%* stands for births from mothers with less than high school in municipalities with income in the bottom 10% of the distribution. *Teen Mom* stands for the mother being a teenager.

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Table 5: Effect of Birth Control Pills' Price War and Later Increase on the Number of Weekly Births and Underweight Births by Week of Conception and Mother's Education

	Week of the Year							
	1	8	15	22	29	36	44	52
Total Briths by Mother's Education								
College								
2007	5.26	7.00	8.74	10.48	12.22	13.96	15.94	17.93
	(17.63)	(15.78)	(14.39)	(13.59)	(13.48)	(14.09)	(15.55)	(17.63)
2008	-11.41	7.20	18.76	23.28*	20.74	11.15	-8.44	-37.23*
	(21.74)	(15.01)	(12.81)	(12.94)	(13.02)	(12.75)	(14.45)	(21.74)
High School								
2007	15.39	10.29	5.19	0.09	-5.01	-10.12	-15.95	-21.78
	(30.97)	(27.73)	(25.28)	(23.87)	(23.68)	(24.76)	(27.32)	(30.97)
2008	-75.03**	-7.78	37.85*	61.86***	64.24***	45.00**	-3.46	-80.17**
	(38.20)	(26.37)	(22.51)	(22.73)	(22.87)	(22.39)	(25.38)	(38.20)
Less Than High School								
2007	-3.51	-1.10	1.31	3.72	6.13	8.54	11.29	14.05
	(14.27)	(12.78)	(11.65)	(11.00)	(10.92)	(11.41)	(12.59)	(14.27)
2008	-11.00	19.00	39.01***	49.03***	49.07***	39.12***	15.53	-21.11
	(17.60)	(12.15)	(10.38)	(10.47)	(10.54)	(10.32)	(11.69)	(17.60)
Total Underweight Births								
2007	-14.37**	-12.14**	-9.91**	-7.68*	-5.45	-3.23	-0.68	1.87
	(5.83)	(5.22)	(4.76)	(4.50)	(4.46)	(4.66)	(5.15)	(5.83)
2008	-8.92	2.99	11.01***	15.15***	15.40***	11.77***	2.86	-11.13
	(7.20)	(4.97)	(4.24)	(4.28)	(4.31)	(4.22)	(4.78)	(7.20)
Underweight Births by Mother's Age								
College								
2007	-3.49	-2.85	-2.21	-1.57	-0.93	-0.28	0.45	1.18
	(2.87)	(2.57)	(2.34)	(2.21)	(2.20)	(2.29)	(2.53)	(2.87)
2008	-1.62	0.01	1.36	2.42	3.19	3.68*	3.88*	3.71
	(3.54)	(2.44)	(2.09)	(2.11)	(2.12)	(2.08)	(2.35)	(3.54)
High School								
2007	-6.13	-5.26	-4.39	-3.52	-2.65	-1.78	-0.78	0.21
	(4.46)	(3.99)	(3.64)	(3.43)	(3.41)	(3.56)	(3.93)	(4.46)
2008	-4.19	2.38	6.63**	8.54***	8.13**	5.39*	-0.60	-9.62*
	(5.50)	(3.79)	(3.24)	(3.27)	(3.29)	(3.22)	(3.65)	(5.50)
Less Than High School								
2007	-4.66**	-3.96*	-3.26*	-2.56	-1.87	-1.17	-0.37	0.43
	(2.29)	(2.05)	(1.86)	(1.76)	(1.75)	(1.83)	(2.02)	(2.29)
2008	-3.54	0.33	2.90*	4.18**	4.16**	2.86*	-0.22	-4.98*
	(2.82)	(1.95)	(1.66)	(1.68)	(1.69)	(1.65)	(1.87)	(2.82)

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. All estimations include week of the year fixed-effects and at least a linear trend. Sample includes conceptions that took place in the period 2005-2008. We fit a different polynomial to each side of the cutoffs. We present the results of the model using a linear specification on the polynomials before 2008 and a quadratic polynomial specification afterwards. We do so because that is the specification that better fits the data. The conception week is obtained by subtracting the pregnancy length to the birth date.

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Table 6: The Effect of the Birth Control Pills' Price War and Later Increase on the Number of Weekly Fetal Deaths by Week of Conception and Municipality Income Level (Quartile)

	Week of the Year							
	1	8	15	22	29	36	44	52
Fetal Deaths								
2007	-8.01**	-7.64***	-7.27***	-6.89***	-6.52***	-6.15**	-5.72**	-5.29*
	(3.19)	(2.86)	(2.60)	(2.46)	(2.44)	(2.55)	(2.82)	(3.19)
2008	-10.61***	-2.74	2.53	5.21**	5.29**	2.78	-3.27	-12.70***
	(3.94)	(2.72)	(2.32)	(2.34)	(2.36)	(2.31)	(2.62)	(3.94)
Bottom 50%								
2007	-1.64	-1.84	-2.04*	-2.24**	-2.44**	-2.64**	-2.87**	-3.09**
	(1.40)	(1.26)	(1.14)	(1.08)	(1.07)	(1.12)	(1.24)	(1.40)
2008	-3.72**	-2.52**	-1.61	-1.01	-0.70	-0.69	-1.05	-1.80
	(1.73)	(1.19)	(1.02)	(1.03)	(1.03)	(1.01)	(1.15)	(1.73)
Top 50%								
2007	-6.38**	-5.80**	-5.23**	-4.66**	-4.08*	-3.51	-2.86	-2.20
	(2.99)	(2.68)	(2.44)	(2.30)	(2.29)	(2.39)	(2.64)	(2.99)
2008	-6.89*	-0.23	4.14*	6.21***	5.99***	3.47	-2.22	-10.91***
	(3.69)	(2.55)	(2.17)	(2.19)	(2.21)	(2.16)	(2.45)	(3.69)
Top 25%								
2007	-2.80	-2.24	-1.69	-1.13	-0.57	-0.01	0.62	1.26
	(2.20)	(1.97)	(1.79)	(1.69)	(1.68)	(1.76)	(1.94)	(2.20)
2008	0.26	1.91	2.75*	2.80*	2.04	0.48	-2.29	-6.11**
	(2.71)	(1.87)	(1.60)	(1.61)	(1.62)	(1.59)	(1.80)	(2.71)
50-75%								
2007	-3.58**	-3.56**	-3.54**	-3.53***	-3.51***	-3.50**	-3.48**	-3.46*
	(1.77)	(1.59)	(1.45)	(1.37)	(1.36)	(1.42)	(1.56)	(1.77)
2008	-7.15***	-2.14	1.39	3.42***	3.95***	3.00**	0.07	-4.80**
	(2.19)	(1.51)	(1.29)	(1.30)	(1.31)	(1.28)	(1.45)	(2.19)

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. All estimations include week of the year fixed-effects and at least a linear trend. Sample includes conceptions that took place in the period 2005-2008. We fit a different polynomial to each side of the cutoffs. We present the results of the model using a linear specification on the polynomials before 2008 and a quadratic polynomial specification afterwards. We do so because that is the specification that better fits the data. The conception week is obtained by subtracting the pregnancy length to the birth date.

Table 7: The Effect of the Birth Control Pills' Price War and Later Increase on Infant Mortality by Week of Conception (per 1,000 live births)

	Week of the Year							
	1	8	15	22	29	36	44	52
Infant Mort.								
2007	-3.20	-5.19**	-6.43***	-6.92***	-6.65***	-5.62**	-3.53	-0.45
	(3.54)	(2.55)	(2.31)	(2.38)	(2.40)	(2.32)	(2.47)	(3.54)
2008	-0.65	1.00	2.75	4.60	6.54	8.57	11.01	13.58
	(3.71)	(2.36)	(2.42)	(3.64)	(5.27)	(7.16)	(9.65)	(12.58)
Perinatal								
2007	1.13	-1.80	-3.83***	-4.97***	-5.21***	-4.56***	-2.73*	0.28
	(2.27)	(1.63)	(1.48)	(1.53)	(1.54)	(1.48)	(1.58)	(2.27)
2008	-0.23	0.94	2.55*	4.62**	7.13**	10.10**	14.04**	18.57**
	(2.38)	(1.51)	(1.55)	(2.33)	(3.38)	(4.58)	(6.18)	(8.06)
Extreme Immaturity								
2007	0.51	0.64	0.77	0.90	1.03	1.16	1.31	1.46
	(0.96)	(0.86)	(0.79)	(0.74)	(0.74)	(0.77)	(0.85)	(0.96)
2008	1.43	1.59*	1.59**	1.42**	1.09	0.59	-0.18	-1.17
	(1.19)	(0.82)	(0.70)	(0.71)	(0.71)	(0.70)	(0.79)	(1.19)
Intracranial Nontraumatic Hemorrhage								
2007	0.25	0.25	0.25	0.25	0.25	0.25	0.25	0.25
	(0.46)	(0.41)	(0.38)	(0.35)	(0.35)	(0.37)	(0.41)	(0.46)
2008	1.79***	1.25***	0.83**	0.53	0.36	0.31	0.41	0.67
	(0.57)	(0.39)	(0.34)	(0.34)	(0.34)	(0.33)	(0.38)	(0.57)
Enterocolitis								
2007	0.31	0.03	-0.26	-0.55	-0.83**	-1.12**	-1.45***	-1.77***
	(0.54)	(0.49)	(0.44)	(0.42)	(0.42)	(0.43)	(0.48)	(0.54)
2008	-0.28	-0.47	-0.54	-0.49	-0.32	-0.02	0.46	1.10
	(0.67)	(0.46)	(0.40)	(0.40)	(0.40)	(0.39)	(0.45)	(0.67)
Sudden Death Syndrome								
2007	-0.08	0.06	0.20	0.33	0.47	0.61	0.77	0.93
	(0.60)	(0.53)	(0.49)	(0.46)	(0.46)	(0.48)	(0.53)	(0.60)
2008	1.46**	1.21**	1.01**	0.87**	0.79*	0.76*	0.80	0.91
	(0.73)	(0.51)	(0.43)	(0.44)	(0.44)	(0.43)	(0.49)	(0.73)
Malformations								
2007	-2.65	-2.32	-1.99	-1.66	-1.32	-0.99	-0.61	-0.23
	(1.70)	(1.52)	(1.39)	(1.31)	(1.30)	(1.36)	(1.50)	(1.70)
2008	-0.98	-0.21	0.26	0.40	0.23	-0.26	-1.21	-2.58
	(2.09)	(1.45)	(1.23)	(1.25)	(1.25)	(1.23)	(1.39)	(2.09)
Cardiac Malf.								
2007	-0.57	-0.62	-0.68*	-0.73**	-0.79**	-0.84**	-0.91**	-0.97**
	(0.47)	(0.42)	(0.38)	(0.36)	(0.36)	(0.38)	(0.42)	(0.47)
2008	-0.28	-0.17	-0.07	0.02	0.11	0.18	0.26	0.32
	(0.58)	(0.40)	(0.34)	(0.35)	(0.35)	(0.34)	(0.39)	(0.58)

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. All estimations include week of the year fixed-effects and at least a linear trend. Sample includes conceptions that took place in the period 2005-2008. We fit a different polynomial to each side of the cutoffs. We present the results of the model using a linear specification on the polynomials before 2008 and a quadratic polynomial specification afterwards. We do so because that is the specification that better fits the data. The conception week is obtained by subtracting the pregnancy length to the birth date.

Table 8: Falsification Tests and Robustness Check
The Effect of the Birth Control Pills' Price Increase by Week of Conception.

	Week of the Year After the Cutoff								
	1	5	9	13	17	21	25	29	33
Month Week	JanW1	FebW1	MarW1	MarW5	AprW4	MayW3	JunW3	JulW3	AugW3
Panel A: Dropping 2007									
Total Births									
	-99.68	-24.25	38.56	88.76*	126.35**	151.33***	163.69***	163.45***	150.59***
	(63.98)	(55.95)	(51.66)	(50.16)	(50.20)	(50.70)	(50.95)	(50.65)	(49.91)
Panel B: 2005 vs. 2008									
Total Births 2006									
	-68.82	-60.34	-51.85	-43.37	-34.88	-26.40	-17.91	-9.43	-0.95
	(43.98)	(40.06)	(36.38)	(33.00)	(30.03)	(27.62)	(25.91)	(25.05)	(25.12)
Total Births 2008									
	-50.57	16.18	71.80**	116.30***	149.68***	171.94***	183.08***	183.10***	172.00***
	(53.61)	(42.58)	(36.04)	(33.50)	(33.44)	(34.13)	(34.44)	(33.89)	(32.61)

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. All estimations include week of the year fixed-effects and at least a linear trend. We fit a different polynomial $f(\beta, |t - t^*|)$ to each side of the cutoff. Panel A: It displays the results when data from 2007 is excluded from the analysis. Panel B: It compares the parametric estimates of equation (1) on weekly conceptions in 2005 and 2008. The specification for Total Births 2008 contains a quadratic polynomial, while the specification for Total Births 2005 contains a linear one. The estimates on 2008 are the same as in the first panel in Table 3. Included for comparison. The conception week is obtained by subtracting the pregnancy length to the birth date.

Table 9: The Effect of the Birth Control Pills' Price Increase on School Attendance in Kindergarten

	Full Sample		Middle class Municipalities		Municipalities (R.M.) with high density of pharmacies	
Born between weeks:	34-48	27-55	34-48	27-55	34-48	27-55
(week 1=first week of reference year)	(1)	(2)	(3)	(4)	(5)	(6)
Effect (Diff-in-Diff)	-1.587***	-0.843***	-3.261***	-1.817***	-14.07***	-20.59***
	(0.074)	(0.056)	(0.175)	(0.132)	(0.755)	(0.483)
Cohort	7.283***	7.284***	6.385***	8.335***	16.05***	14.03***
	(0.054)	(0.041)	(0.127)	(0.0958)	(0.546)	(0.349)
Time	-13.60***	-15.94***	-14.05***	-16.20***	5.407***	2.168***
	(0.053)	(0.040)	(0.124)	(0.0937)	(0.535)	(0.343)
Constant	761.6***	762.6***	794.2***	793.0***	597.5***	601.4***
	(0.038)	(0.029)	(0.0901)	(0.0679)	(0.386)	(0.247)
Number of Births	143,286	276,169	62,987	121,478	23,433	45,362
R-squared	0.559	0.590	0.360	0.396	0.037	0.065

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. Sample size (number of births) corresponds to two cohorts (treatment and control groups), i.e. the number of births over two years. We combine individual-level administrative information on daily attendance and birthdates. The dependent variable is defined as the proportion of individuals born in a specific week reporting at least one day of school attendance during the first quarter of the year they are eligible to enroll in kindergarten times 1000. Children turning 5 by March 31st of a given year are eligible to enroll in kindergarten in March of that year. However, the Ministry of Education can authorize children turning 5 until June 30th to enroll as well. The children that could potentially be affected by the price increase are those who turned five between July 2013 and January of 2014, so they could attend kindergarten starting March 2014. The reference group corresponds to those who turned five between July 2012 and January of 2013, so they could attend kindergarten starting March 2013. We first normalize the first week of 2007 as week 1. The time dummy is defined as 1 if week of birth is 41 or later, and 0 otherwise. Each column displays results for different sub-samples within these dates. Column (1): From control group those born between 8/20/07 (first day of week 34 of 2007) and 12/2/07 (last day of week 48 of 2007), while from treatment group those born between 08/18/08 (first week of week 34 of 2008) and 11/30/08 (last day of week 48 of 2008). Column (2): From control group those born between 7/2/07 (first day of week 27) and 1/20/08 (last day of week 55), while from treatment group those born between 06/30/08 (first day of week 27) and 01/18/09 (last day of week 55). Columns (3) to (4) repeat the analysis but for the sample of individuals born in municipalities with average income levels between the 25th and 75th percentiles of the income distributions. Columns (5) and (6) repeat the analysis but for the sample of children attending schools located in municipalities of the Metropolitan Region with high density of pharmacy locations. A *municipality* is said to have high density of pharmacies if the number pharmacies per capita exceeds the median number of pharmacies per capita in R.M.'s *municipalities*.

Table 10: The Effect of the Birth Control Pills' Price Increase on School Attendance in 1st and 2nd Grade

	1st Grade	2nd Grade
Diff-in-Diff	-0.002	-5.698***
	(0.652)	(0.597)
R-squared	0.001	0.003
Mean at baseline$^{(a)}$	879.96	849.41
Number of births	371,916	

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. Sample size (number of births) corresponds to two cohorts (treatment and control groups), i.e. the number of births over two years. We combine individual-level administrative information on daily school attendance and birthdates. The dependent variable is the defined as the proportion of individuals born in a specific month reporting at least one day of school attendance during the first quarter of the year they are eligible to enroll first or second grade (times 1000). Children turning 6 by March 31st of a given year are eligible to enroll in first grade by March of that year. However, the Ministry of Education might authorize children turning 6 until June 30th to enroll as well. The children that could potentially be affected by the price increase are those who turned six between July 2014 and January of 2015, so they could attend first grade starting March 2015. The reference group corresponds to those who turned six between July 2013 and January of 2014, so they could attend first grade starting March 2014. Since the effect of collusion on prices became visible during the first week of 2008, we define those children born between July 2007 and March of 2008 as part of the control group. Likewise, children born between July of 2008 and March of 2009 belong to the treatment group. The time dummy is equal to 1 if birth month is July or August, and 0 otherwise. For first grade attendance we use data for the years 2014 and 2015. For second grade attendance we use data for the same months for the years 2015 and 2016. (a): The means at baseline represent the monthly averages of the dependent variable for 2014 (first grade) and 2015 (second grade) for those born one year before the price increase.

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Table 11: The Effect of the Birth Control Pills' Price Increase on Enrollment in Special Education Programs (for children with intellectual disabilities) in 1st and 2nd Grade

	1st Grade	2nd Grade
Diff-in-Diff	0.469***	0.834***
	(0.002)	(0.003)
R-squared	0.440	0.313
Mean at baseline$^{(a)}$	2.31	4.19
Number of births	371,916	

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. Sample size (number of births) corresponds to two cohorts (treatment and control groups), i.e. the number of births over two years. We combine individual-level administrative information on daily school attendance and birthdates. The dependent variable is the defined as the proportion of individuals born in a specific month reporting at least one day of school attendance in services for children with intellectual disabilities during the first quarter of the year they are eligible to enroll first or second grade. Children turning 6 by March 31st of a given year are eligible to enroll in first grade by March of that year. However, the Ministry of Education might authorize children turning 6 until June 30th to enroll as well. The children that could potentially be affected by the price increase are those who turned six between July 2014 and January of 2015, so they could attend first grade starting March 2015. The reference group correspondes to those who turned six between July 2013 and January of 2014, so they could attend first grade starting March 2014. Since the effect of collusion on prices became visible during the first week of 2008, we define those children born between July 2007 and March of 2008 as part of the control group. Likewise, children born between July of 2008 and March of 2009 belong to the treatment group. The time dummy is equal to 1 if birth month is July or August, and 0 otherwise. For first grade attendance we use data for the years 2014 and 2015. For second grade attendance we use data for the same months for the years 2015 and 2016. (a): The means at baseline represent the monthly averages of the dependent variable for 2014 (first grade) and 2015 (second grade) for those born one year before the price increase.

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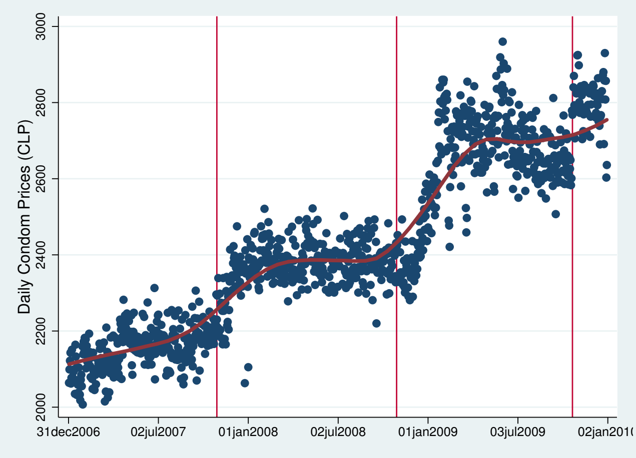
Appendix

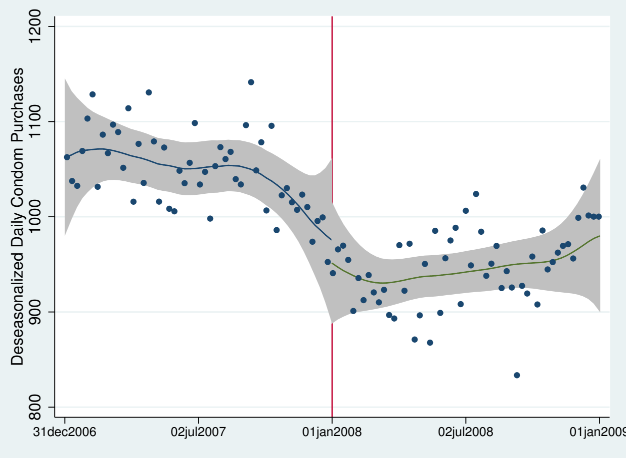
A The Condom as a Substitute

Figure A.1: Daily Prices and Quantities Sold of Condoms in Pharmacies

(a) Price of Packet of Condoms (3 Units)



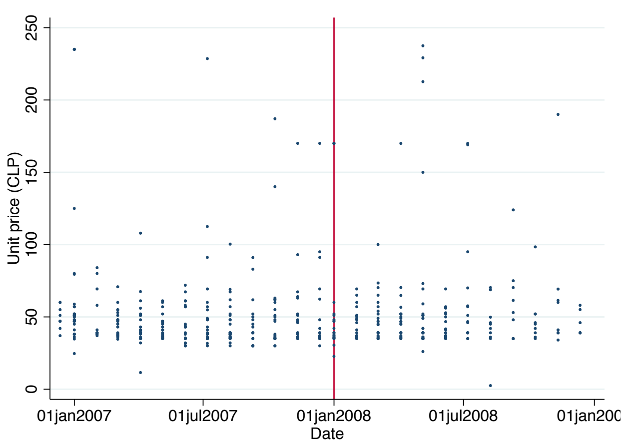
(b) Units Sold



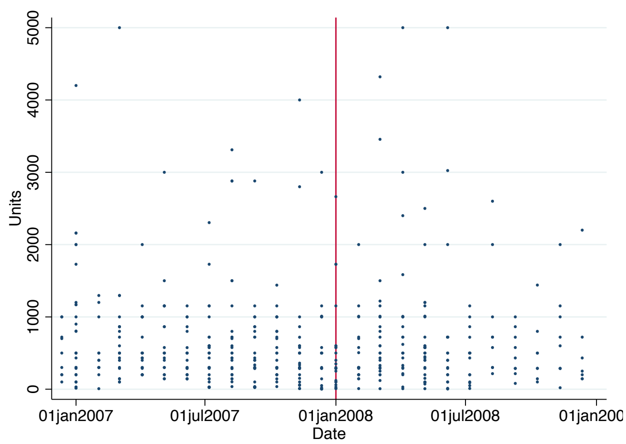
Note: Data from Salcobrand pharmacies. (a) Vertical lines indicate the beginning of Summer. (b) Non-parametric approximations estimated using daily data. Scatter plots weekly averages. The quantities series followed two seasonal patterns. One that goes with the yearly seasons, where more quantities are sold in warmer months. And a second pattern related to the day of the week, where more units are sold during Fridays and Saturdays that during the rest of the days.

Figure A.2: Condoms Purchases in Procurement Data

(a) Price of Unit of Condoms



(b) Quantities Purchased of Units of Condoms



Note: Prices obtained from the Chilean government procurement data.

B Estimating Contraceptive Demand Elasticities

Table B.1: Price elasticity

	(1) OLS	(2) IV (straight)	(3) IV (hole)	(4) IV (weight)
β	-0.18***	-0.11**	-0.13***	-0.16***
	(0.039)	(0.04)	(0.04)	(0.04)
First stage				
δ	–	0.36***	0.37***	0.35***
Cragg-Donald	–	377.13	428.9	455.4

Note: Standard errors in parentheses.

Studies that relate contraceptive prices and demand find relatively small sensitivity of the demand of oral contraceptives to price increases ranging from 0 to 15 percent (Ciszewski and Harvey, 1995; Janowitz and Bratt, 1996; Matheny, 2004; Collins and Hershbein, 2013, among others). The price changes analyzed are typically small (with the exception of Collins and Hershbein, 2013), and the scale (amount of women affected by these changes) is reduced. We, on the contrary, are able to analyze a nationwide shock in which prices increased overnight by an average of 45%. Therefore, in this Appendix we further our analysis of the contraceptives' price elasticity by estimating a 2SLS model the following triangular system

$$
\begin{aligned}
\ln(q_t) &= \alpha + t + \beta \ln(p_t) + u_t \\
\ln(p_t) &= \gamma + t + \delta 1\left[t > t^*\right] + v_t
\end{aligned}
$$

where p_t and q_t are weekly prices and quantities and $1\left[t > t^*\right]$ is dummy variable equal to one after the collusion date t^*. The parameter of interest is β which corresponds to the contraceptive price elasticity. We run four different specifications presented in Table B.1. In column (1), for comparison purposes, we present the OLS estimate that yields a parameter of -0.18. Columns (2) to (4) present the 2SLS results. Column (2) presents the results when we do not exclude the eight first weeks of the year (i.e., without the donut-hole), while column (3) present the estimates that incorporate the donut-hole. Column (4) use a kernel weight (gaussian) that gives more weight to observations near the collusion date and decreasing for dates further away. As can be seen, in the 2SLS strategy elasticities varies form -0.11 to -0.16 which agrees with the rough Wald estimate obtained from Figure 1.[42]

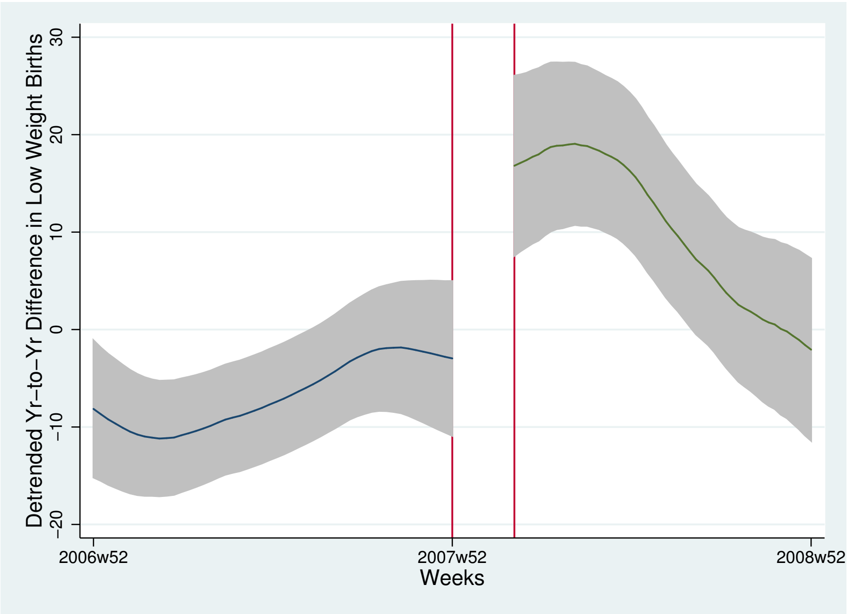
[42]The lower panel of Table B.1 shows that the first stage in each 2SLS estimation is very strong considering the high value of the Cragg-Donald statistics.

Web Appendix

I Additional Non-Parametric Estimations

I.1 More Results on Live Births

Figure I.1: Year-to-Year difference in Weekly Low Weight Births by Conception Week



Note: births series are detrended (linear trend) and deseasonalized by standard methods (dummies per week of year). Then, the year-to-year difference in weekly births are computed. The conception week is obtained by subtracting the pregnancy length to the birth date.

I.2 Additional Results on Fetal Losses

Table I.1: Discontinuity Regressions of Fetal Losses of Nulliparous Women

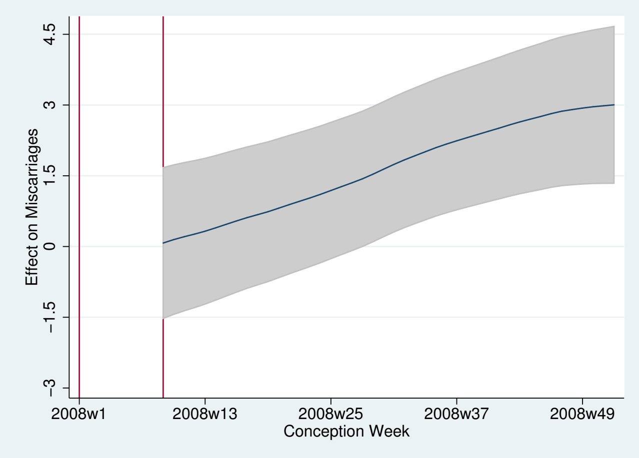
VARIABLES	(1) First Preg.	(2) First Preg.	(3) First Preg.	(4) First Preg.
Treatment	0.7525	7.7520	9.6857	-24.0518
	(29.946)	(30.324)	(29.715)	(490.244)
Mother Age	-0.6722	-0.6807	-0.8763	27.9635
	(0.685)	(0.682)	(0.689)	(24.229)
Mother Age^2				-0.5184
				(0.435)
Dist to Cutoff	0.1670***	0.2144***	0.4546*	0.5286**
	(0.048)	(0.060)	(0.240)	(0.246)
Dist to Cutoff^2			-0.0047	-0.0062
			(0.005)	(0.005)
Treat×Age	-0.0582	-0.2023	-0.3764	1.8791
	(1.068)	(1.070)	(1.066)	(35.129)
Treat×Age^2				-0.0371
				(0.629)
Treat×Dist to Cutoff		-0.1284	0.3578	0.3175
		(0.099)	(0.382)	(0.386)
Treat×Dist to Cutoff^2			-0.0123	-0.0117
			(0.008)	(0.008)
Constant	15.2672	14.2944	17.7542	-383.0051
	(19.179)	(19.124)	(18.870)	(337.072)
Observations	96	96	96	96
R-squared	0.150	0.165	0.230	0.256

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. The dependent variable in all regressions are detrended (using a linear trend) and de-seasonalized (using the difference with the same week of last year) series.

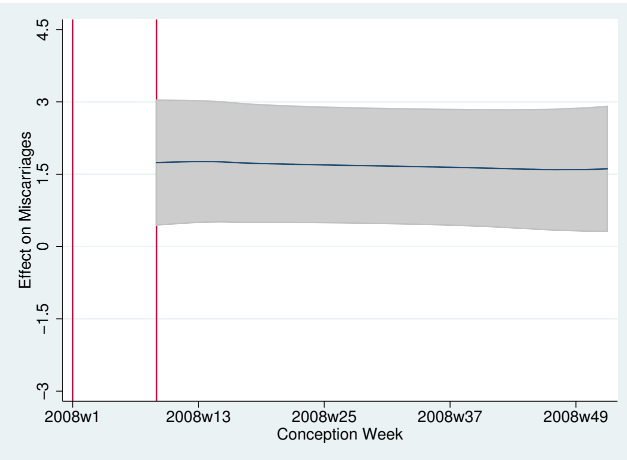
Figure I.2: Effect on Weekly Fetal Losses of Nulliparous Women by Conception Week (By Mother's Age)

(a) 20-24 year old mothers

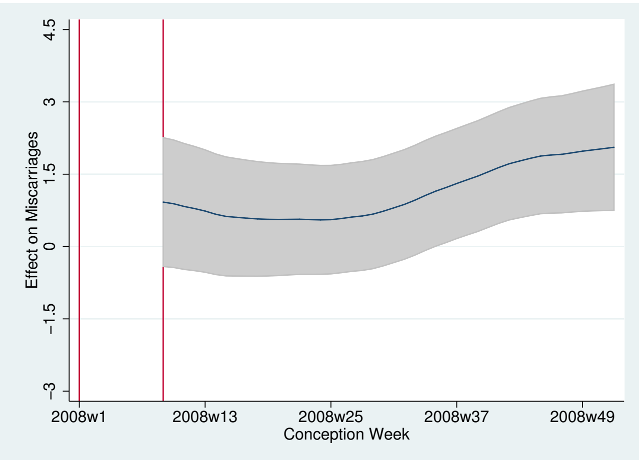
(b) 25-29 year old mothers



(c) 30-35 year old mothers



(d) 36-60 year old mothers



Note: Non-Parametric estimates of the effect of price increase of the weekly fetal deaths. Calonico et al. (2014) bandwidth and the shaded area corresponds to the 95% confidence interval for such effect. Note that these figures do not plot in there vertical axis the levels of the variable of interest. Instead we plot the estimated effect itself.

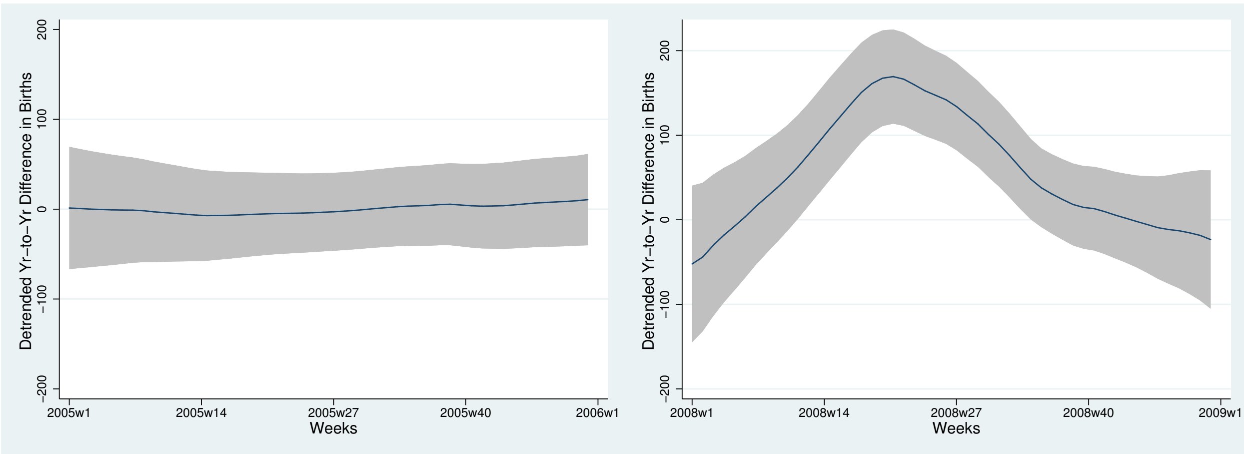
II Additional Falsification Test: 2005 vs. 2008

We follow our empirical strategy but now applied to pre 2006 data. The purpose of this exercise is to further evaluate the extent to which our results for 2008 are triggered by the exogenous increase in birth control pills prices and not the result of common patterns.

Figure II.1 compares the results for 2005 (Panel A) and 2008 (Panel B). The non-parametric models are estimated using weekly information on total live births data covering a period of 156 weeks (up to December of the respective year). Importantly, during 2004 and 2005 control pills prices remain stable. Panel A shows an almost flat profile for live births. Panel B displays our main result for 2008. Figure II.2 repeats the analysis but using the parametric specification. The results are similar to those obtained using the non-parametric models. The comparison of 2005 and 2008 provides additional evidence in support of our main hypothesis and the empirical strategy we employ to test it.

Figure II.1: De-trended Year-to-Year Differences of Weekly Live Births by Conception Week
Non-parametric Specification

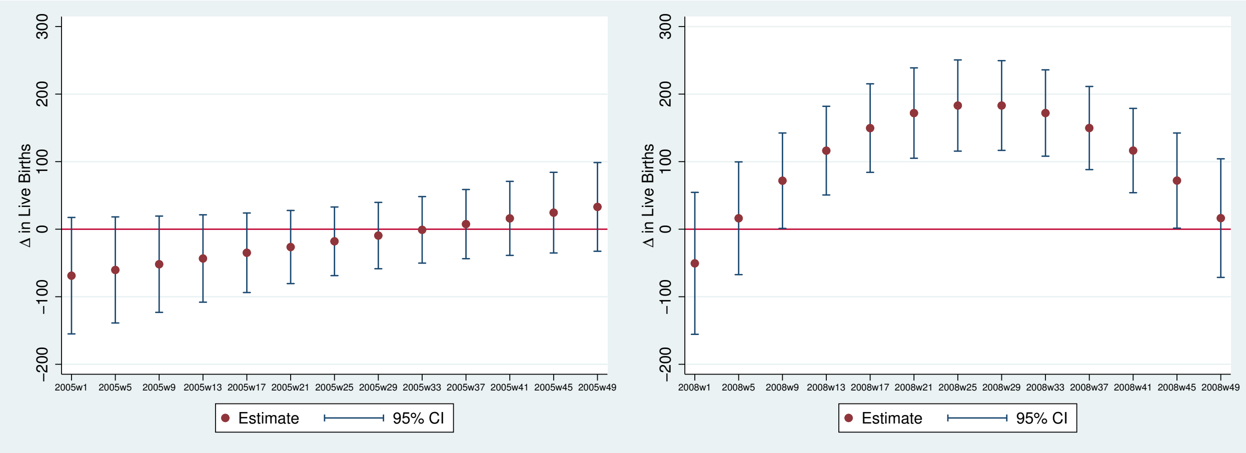
(a) 2005 **(b)** 2008



Note: This figure plots the the non-parametric estimates of the de-trended year-to-year differences in weekly live births in 2005 and 2008 by conception week. The conception week is obtained by subtracting the pregnancy length to the birth date.

Figure II.2: De-trended Year-to-Year Differences of Weekly Live Births by Conception Week
Parametric Specification

(a) 2005 **(b)** 2008



Note: This figure presents the parametric estimates of equation (1) weekly live conceptions. Point estimates and associated standard errors come from Panel B of Table 8. The conception week is obtained by subtracting the pregnancy length to the birth date.

III Pharmacy Availability

In this Appendix, we explore the effect of the price jump by the availability of pharmacies in a given *comuna*. Although we can only obtain results for the Santiago region, due to data availability, we find suggestive evidence in favor of the fact that there were different impact depending on such availability. One would expect that consumers that have easier access to pharmacies would stock less quantities of medicine as they can easily purchase the monthly dosage needed. On the contrary, consumers that face scarcity of pharmacies might visit them less often and therefore, be less exposed to price shifts. To test this hypothesis, we identify *comunas* with high and low density of pharmacies using the number of stores per capita as a proxy. Those *comunas* above the median are considered to be high density *comunas*, and consequently, those below the median are considered low density *comunas*.

Table III.1 shows evidence in favor of the contraceptives' price increase having an earlier effect in high density *comunas* than in low density comunas. In fact, high density *comunas* start feeling the effect as early as the first week after the two-cycle period, while the low density *comunas* start feeling the effect of the price increase around two months after the two-cycle period. Interestingly, although the effects have different timing, they are of similar sizes: around 35 to 45 extra births per week.

Figure III.1: Geographical distributions of Drugstores - Santiago

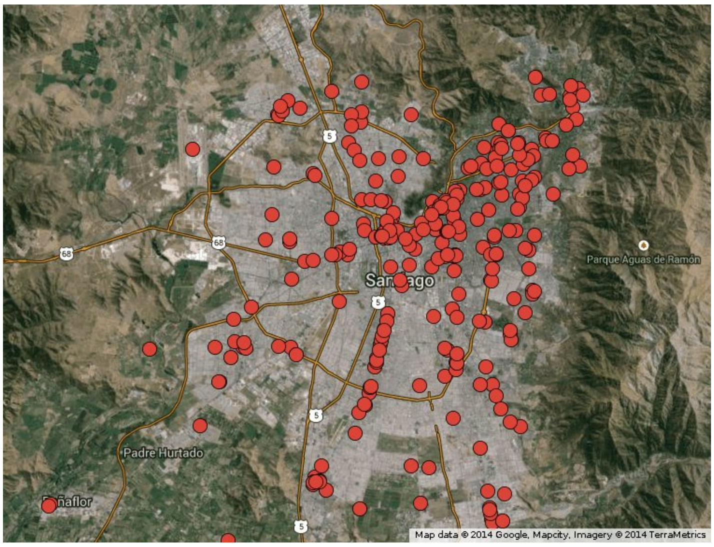


Table III.1: Estimated impact of price increase on births by Week of Conception

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Week After 2-cycle Gap						
	0	4	8	12	16	20	24
HD of Pharm							
Non-parametric	40.08**	41.54***	40.24***	40.11***	39.97***	39.43***	38.42***
	(16.31)	(15.57)	(14.98)	(14.49)	(14.14)	(13.96)	(13.98)
Parametric	45.76*	38.48**	31.57**	25.02*	18.84	13.02	7.57
	(26.29)	(18.47)	(14.46)	(14.10)	(15.43)	(16.66)	(17.02)
LD of Pharm							
Non-parametric	11.36	21.36	31.53**	37.70**	44.65***	49.42***	41.81**
	(17.85)	(16.46)	(15.57)	(15.66)	(16.12)	(16.67)	(17.30)
Parametric	15.24	22.53	27.48	30.09*	30.36	28.29	23.87
	(31.63)	(22.22)	(17.39)	(16.96)	(18.57)	(20.04)	(20.47)

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. *HD of Pharm* stands for conceptions that took place in Santiago's *comunas* with high density of pharmacies and *LD of Pharm* stands for conceptions that took place in *comunas* with low density of pharmacies. A *comuna* is said to have high density of pharmacies if the number pharmacies per capita exceeds the median number of pharmacies per capita in Santiago's *comunas.* Series are detrended (linear trend) and deseasonalized by standard methods (dummies per week of year). Then, the year-to-year difference in weekly births is computed. The conception week is obtained by subtracting the pregnancy length to the birth date. Rows titled *Non-parametric* present the estimates using local polynomial regressions in which the effect is the difference between the mean of the outcome at the last week of 2007 and the mean—given by the local polynomial regression—of the outcome at each particular week after the 2-period cycle. Rows titled *Parametric* present the results from the estimations presented in Specification 1 in which $f(\beta, |t - t^*|)$ fits a different quadratic polynomial to each side of the cutoff.

IV A Difference-in-Difference Estimator

The National Study of Youth of 2006, a nationally representative survey administered by the National Youth Service of Chile (Instituto Nacional de la Juventud or INJUV), collected data on the use of birth control methods. Among women younger than 18, only 10% reported using the Pill. Among 19 yrs old 16%, 20 yrs old 22%, 21 yrs old 24%, and for those older than 21 between 25% and 30%.

We use these figures to re-estimate the impact of the birth control Pill's war and its later sharp increase using a double difference strategy. To this end, we first define the following taxonomy: women younger than 20 (low consumption of birth control Pill group) and women older than 19 (high consumption of birth control Pill group). Exploiting the administrative records, we then construct the weekly number of births of mothers within each category. Finally, we generate the weekly difference between these two groups and implement a difference-in-difference type of model using data for the period 2005-2008. Table displays the results. The estimated impacts are similar to those obtained using the interrupted time series approach (see Table 3 in our paper). This confirms the robustness of our findings to different empirical strategies.

Table IV.1: The Effect of the Birth Control Pill's Price War and Later Sharp Increase on the
Number of Weekly Births, by Week of Conception

	Week of the Year							
	1	8	15	22	29	36	44	52
Dif: Mom Age $> 19 - < 20$								
2007	27.09	26.71	26.34	25.97	25.60	25.23	24.80	24.38
	(39.24)	(35.13)	(32.02)	(30.24)	(30.01)	(31.36)	(34.61)	(39.24)
2008	-34.57	50.27	110.06***	144.83***	154.55***	139.24***	91.08***	10.21
	(48.39)	(33.41)	(28.52)	(28.79)	(28.97)	(28.37)	(32.15)	(48.39)

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. The specification includes week of year fixed-effects and a linear trend. Sample includes conceptions that took place in the period 2005-2008. The conception week is obtained by subtracting the pregnancy length from the birth date.

V Long-Term Outcomes

Table V.1: The Effect of the Birth Control Pills' Price Increase on School Attendance in Kindergarten

Donut-hole:	None		± 2 weeks		± 3 weeks		± 4 weeks	
Born between weeks: (week 1=first week of reference year)	34-48	27-55	34-48	27-55	34-48	27-55	34-48	27-55
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effect	-1.587***	-0.843***	-7.807***	-3.130***	-6.400***	-2.252***	-6.358***	-1.886***
	(0.074)	(0.056)	(0.089)	(0.062)	(0.096)	(0.065)	(0.116)	(0.069)
Cohort	7.283***	7.284***	6.784***	7.063***	7.540***	7.386***	10.72***	8.351***
	(0.054)	(0.041)	(0.062)	(0.045)	(0.067)	(0.046)	(0.082)	(0.049)
Time	-13.60***	-15.94***	-13.49***	-16.42***	-14.86***	-17.13***	-16.72***	-17.92***
	(0.053)	(0.040)	(0.063)	(0.044)	(0.068)	(0.046)	(0.082)	(0.049)
Constant	761.6***	762.6***	762.6***	763.1***	761.3***	762.7***	761.2***	762.8***
	(0.038)	(0.029)	(0.044)	(0.032)	(0.048)	(0.033)	(0.058)	(0.035)
Number of Births	143,286	276,169	95,387	228,270	77,728	210,611	56,846	189,729
R-squared	0.559	0.590	0.636	0.616	0.663	0.629	0.707	0.647

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. We combine individual-level administrative information on daily attendance and birthdates. The dependent variable is defined as the proportion of individuals born in a specific week reporting at least one day of school attendance during the first quarter of the year they are eligible to enroll in kindergarten times 1000. Children turning 5 by March 31st of a given year are eligible to enroll in kindergarten in March of that year. However, the Ministry of Education can authorize children turning 5 until June 30th to enroll as well. The children that could potentially be affected by the price increase are those who turned five between July 2013 and January of 2014, so they could attend kindergarten starting March 2014. The reference group corresponds to those who turned five between July 2012 and January of 2013, so they could attend kindergarten starting March 2013. We first normalize the first week of 2007 as week 1. The time dummy is defined as 1 if week of birth is 41 or later, and 0 otherwise. Each column displays results for different sub-samples within these dates. Column (1): From control group those born between 8/20/07 (first day of week 34 of 2007) and 12/2/07 (last day of week 48 of 2007), while from treatment group those born between 08/18/08 (first week of week 34 of 2008) and 11/30/08 (last day of week 48 of 2008). Column (2): From control group those born between 7/2/07 (first day of week 27) and 1/20/08 (last day of week 55), while from treatment group those born between 06/30/08 (first day of week 27) and 01/18/09 (last day of week 55). Columns (3) to (8) display the results from different donut-hole regressions around week 41. Column (3): Control group those born between 8/20/07- 9/23/07 (last day of week 38 of 2007) and 10/29/07 (first day of week 44)-12/2/07, while treatment group those born between 8/18/08- 9/21/ 08 (last day of week 38 of 2008) and 10/27/08 (first day of week 44)-11/30/08. Column (4): Control group those born between 7/02/07-9/23/07 and 10/29/07-1/20/08, while treatment group those born between 6/30/08-9/21/08 and 10/27/08-1/18/09. Column (5): Control group those born between 8/20/07 - 9/16/07 (last day of week 37) and 11/5/07 (first week of week 45) - 12/2/07, while treatment group those born between 8/18/08 - 9/14/08 (last day of week 37) and 11/3/08 (first week of week 45) -11/30/08. Column (6): Control group those born between 7/2/07 -9/16/07 and 11/5/07-1/20/08, while treatment group those born between 6/30/08-9/14/08 and 11/3/08-1/18/09. Column (7): Control group those born between 8/20/07 - 9/9/07 (last day of week 36) and 11/12/07 (first day of week 46) -12/2/07, while treatment group those born between 8/18/08 - 9/7/08 (last day of week 36) and 11/10/08 (first day of week 46) - 11/30/08. Column (8): Control group those born between 7/2/07-9/9/07 and 11/12/07-1/20/08, while treatment group those born between 6/30/08-9/7/08 and 11/10/08-1/18/09.

Table V.2: The Effect of the Birth Control Pills' Price Increase on School Attendance in Kindergarten

	Metropolitan region (R.M.)		Municipalities with low density of pharmacies		Municipalities with high density of pharmacies	
Born between weeks: (week 1=first week of reference year)	34-48	27-55	34-48	27-55	34-48	27-55
	(1)	(2)	(3)	(4)	(5)	(6)
Effect	-4.125***	-7.218***	3.124***	3.456***	-14.07***	-20.59***
	(0.284)	(0.175)	(0.403)	(0.276)	(0.755)	(0.483)
Cohort	5.464***	8.829***	-1.948***	5.025***	16.05***	14.03***
	(0.206)	(0.127)	(0.293)	(0.200)	(0.546)	(0.349)
Time	–13.63***	-13.58***	-28.08***	-25.73***	5.407***	2.168***
	(0.201)	(0.124)	(0.285)	(0.195)	(0.535)	(0.343)
Constant	692.2***	690.9***	758.6***	753.2***	597.5***	601.4***
	(0.145)	(0.0898)	(0.207)	(0.141)	(0.386)	(0.247)
Number of Births	57,177	110,969	33,744	65,607	23,433	45,362
R-squared	0.184	0.281	0.340	0.334	0.037	0.065

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. The sample is restricted to individuals attending schools in the Metropolitan Region (Santiago). We combine individual-level administrative information on daily attendance and birthdates. The dependent variable is defined as the proportion of individuals born in a specific week reporting at least one day of school attendance during the first quarter of the year they are eligible to enroll in kindergarten times 1000. Children turning 5 by March 31st of a given year are eligible to enroll in kindergarten in March of that year. However, the Ministry of Education can authorize children turning 5 until June 30th to enroll as well. The children that could potentially be affected by the price increase are those who turned five between July 2013 and January of 2014, so they could attend kindergarten starting March 2014. The reference group corresponds to those who turned five between July 2012 and January of 2013, so they could attend kindergarten starting March 2013. We first normalize the first week of 2007 as week 1. The time dummy is defined as 1 if week of birth is 41 or later, and 0 otherwise. Each column displays results for different sub-samples within these dates. Column (1): From control group those born between 8/20/07 (first day of week 34 of 2007) and 12/2/07 (last day of week 48 of 2007), while from treatment group those born between 08/18/08 (first week of week 34 of 2008) and 11/30/08 (last day of week 48 of 2008). Column (2): From control group those born between 7/2/07 (first day of week 27) and 1/20/08 (last day of week 55), while from treatment group those born between 06/30/08 (first day of week 27) and 01/18/09 (last day of week 55). Columns (3) to (4) repeat the analysis but for the sample of children attending schools located in municipalities of the Metropolitan Region with a low density of pharmacy locations (four or less). Columns (5) and (6) repeat the analysis but for the sample of children attending schools located in municipalities of the Metropolitan Region with a high density of pharmacy locations (five or more). A *municipality* is said to have high density of pharmacies if the number pharmacies per capita exceeds the median number of pharmacies per capita in R.M.'s *municipalities.*

Table V.3: The Effect of the Birth Control Pills' Price Increase on Enrollment in Special Education Programs (for children with intellectual disabilities) by Grade
First two years of formal education

	1st Grade	2nd Grade
A. Full Sample:		
Diff-in-Diff	0.461***	0.806***
	(0.002)	(0.003)
R-squared	0.434	0.320
Number of births	371,916	
B. Excluding those born in September or October:		
Diff-in-Diff	0.501***	0.985***
	(0.002)	(0.003)
R-squared	0.450	0.320
Number of births	285,485	
Mean at baseline[(a)]	2.31	4.19

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. Sample sizes correspond to two cohorts (treatment and control groups), i.e. the number of births over two years. We combine individual-level administrative information on daily school attendance and birthdates. The dependent variable is the defined as the proportion of individuals born in a specific month reporting at least one day of school attendance in services for children with intellectual disabilities during the first quarter of the year they are eligible to enroll first or second grade. Children turning 6 by March 31st of a given year are eligible to enroll in first grade by March of that year. However, the Ministry of Education might authorize children turning 6 until June 30th to enroll as well. The children that could potentially be affected by the price increase are those who turned six between July 2014 and January of 2015, so they could attend first grade starting March 2015. The reference group correspondes to those who turned six between July 2013 and January of 2014, so they could attend first grade starting March 2014. Since the effect of collusion on prices became visible during the first week of 2008, we define those children born between July 2007 and March of 2008 as part of the control group. Likewise, children born between July of 2008 and March of 2009 belong to the treatment group. The time dummy is equal to 1 if birth month is July or August, and 0 otherwise. For first grade attendance we use data for the years 2014 and 2015. For second grade attendance we use data for the same months for the years 2015 and 2016. (a): The means at baseline represent the monthly averages of the dependent variable for 2014 (first grade) and 2015 (second grade) for those born one year before the price increase.

Table V.4: The Effect of the Birth Control Pills' Price Increase on Enrollment in Special Education Programs (for children with intellectual disabilities) by Grade
First two years of formal education - Aggregating Special Education Programs

	1^{st} Grade	2^{nd} Grade
A. Full Sample:		
Diff-in-Diff	0.920***	1.232***
	(0.009)	(0.006)
R-squared	0.097	0.164
Number of births	371,916	
B. Excluding those born in September or October:		
Diff-in-Diff	0.920***	1.423***
	(0.008)	(0.006)
R-squared	0.285	0.296
Number of births	285,485	
Mean at baseline*	7.06	6.87

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. Sample sizes correspond to two cohorts (treatment and control groups), i.e. the number of births over two years. We combine individual-level administrative information on daily school attendance and birth dates. The dependent variable is the defined as the proportion of individuals born in a specific week reporting at least one day of school attendance in any special education program during the first quarter of the year they are eligible to enroll first or second grade. These include: (211) Hearing Disability (*Educación Especial Discapacidad Auditiva*), (212) Intelectual Disability (*Educación Especial Discapacidad Intelectual*), (213) Visual Disability (*Educación Especial Discapacidad Visual*), (214) Language Disability (*Educación Especial Trastornos Específicos del Lenguaje*), (215) Motor Disability (*Educación Especial Trastornos Motores*), (216) Autism (*Educación Especial Autismo*), (217) Social and Communication Disabilities (*Educación Especial Discapacidad Graves Alteraciones en la Capacidad de Relación y Comunicación*), and (299) Program for Integration in School (*Opción 4 Programa Integración Escolar*). Children turning 6 by March 31st of a given year are eligible to enroll in first grade in March of that year. However, the Ministry of Education can authorize children turning 6 until June 30th to enroll as well. The children that could potentially be affected by the price increase are those who turned six between July 2014 and January of 2015, so they could attend first grade starting March 2015. The reference group corresponds to those who turned six between July 2013 and January of 2014, so they could attend first grade starting March 2014. Since the effect of collusion on prices became visible during the first week of 2008, we define those children born between July 2007 and March of 2008 as part of the control group. Likewise, children born between July of 2008 and March of 2009 belong to the treatment group. The time dummy is equal to 1 if birth month is July or August, and 0 otherwise. For first grade attendance we use data for the years 2014 and 2015. For second grade attendance we use data for the same months for the years 2015 and 2016. (a): For first and second grade, the means at baseline are represent the monthly averages of the dependent variable computed among those born between July and August of 2014 and 2015, respectively.

Table V.5: The Effect of the Birth Control Pills' Price Increase on Enrollment in Special Education Programs (for children with intellectual disabilities) by Grade
First two years of formal education

	1st Grade	2nd Grade
A. Full Sample:		
Diff-in-Diff	0.469***	0.834***
	(0.002)	(0.003)
R-squared	0.440	0.313
Number of births	371,916	
B. Excluding those born in September or October:		
Diff-in-Diff	0.501***	0.985***
	(0.002)	(0.003)
R-squared	0.450	0.320
Number of births	285,485	
Mean at baseline$^{(a)}$	2.31	4.19

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. Sample sizes correspond to two cohorts (treatment and control groups), i.e. the number of births over two years. We combine individual-level administrative information on daily school attendance and birthdates. The dependent variable is the defined as the proportion of individuals born in a specific month reporting at least one day of school attendance in services for children with intellectual disabilities during the first quarter of the year they are eligible to enroll first or second grade. Children turning 6 by March 31st of a given year are eligible to enroll in first grade by March of that year. However, the Ministry of Education might authorize children turning 6 until June 30th to enroll as well. The children that could potentially be affected by the price increase are those who turned six between July 2014 and January of 2015, so they could attend first grade starting March 2015. The reference group correspondes to those who turned six between July 2013 and January of 2014, so they could attend first grade starting March 2014. Since the effect of collusion on prices became visible during the first week of 2008, we define those children born between July 2007 and March of 2008 as part of the control group. Likewise, children born between July of 2008 and March of 2009 belong to the treatment group. The time dummy is equal to 1 if birth month is July or August, and 0 otherwise. For first grade attendance we use data for the years 2014 and 2015. For second grade attendance we use data for the same months for the years 2015 and 2016. (a): The means at baseline represent the monthly averages of the dependent variable for 2014 (first grade) and 2015 (second grade) for those born one year before the price increase.