The Children of the Missed Pill*

Tomás Rau Miguel Sarzosa Sergio Urzúa

September 18, 2017

Abstract

We use sharp, massive and unexpected price increases of oral contraceptives—product of a documented case of collusion among pharmaceutical retailers in Chile—as a natural experiment to estimate the impact of access to oral contraceptives on fertility and newborn health. Our empirical strategy combines multiple sources of information and takes into account the seasonality of conceptions and the general trends of fertility, as well as the dynamics that arise after interrupting Pill's intake. Our estimates suggest that due to the price hike, the weekly birth rate increased by 4%. We show large effects on the number of children born to unmarried mothers, from mothers in their early 20's, and to primiparae women. Moreover, we find evidence of significant deterioration of newborn health as measured by the incidence of low birthweight and infant mortality. We suggest that the "extra" conceptions faced dire conditions during gestation as a result of mothers' unhealthy behaviors. In addition, we document a disproportional increase of 27% in the weekly miscarriage and stillbirth rates, which we interpret as manifestations of active efforts of termination in a country where abortion was illegal. As the "extra" children reached school age, we find lower school enrollment rates and higher participation in programs for children with special needs. Our results suggest that access to contraceptives may improve the average quality of the children conceived as it prevents the conception of ones that will turn out to be less healthy.

Keywords: Fertility, newborn health, impact of collusion.

JEL codes: J13, I11, I18, D18

* *Corresponding author*: Miguel Sarzosa, Department of Economics, Krannert School of Management, Purdue University, email: msarzosa@purdue.edu. Tomás Rau, Instituto de Economía, Pontificia Universidad Católica de Chile; email: trau@uc.cl. Sergio Urzúa, Department of Economics, University of Maryland and NBER; email: urzua@econ.umd.edu. We would like to thank Fernando Alvarado, MD for his valuable input. We are indebted to the useful comments of Melissa Kearney, Jeanne Lafortune, Santiago Truffa, Sarah Okoampah, John Casterline, Monica Deza, Jackie Buhrmann and seminar participants at the World Bank (2014, D.C.), LACEA (2014, São Paulo), SECHI (2014, Viña del Mar), UC Berkeley (2015, Berkeley, CA), Universidad del Rosario (2015, Bogota), University of Western Ontario (2016, London, Canada), University of Maryland (2016, College Park, MD), Essen Health Conference (2016, Essen, Germany), Purdue University (2016, West Lafayette, IN), PopPov Conference (2016, Washington, DC), Eastern Economic Association Meetings (2017, New York), and PAA Meetings (2017, Chicago). Tomás Rau thanks the funding granted by Fondecyt, Project No.1171128. Additionally, this research was supported by the National Institutes of Health under award number NICHD R01HD065436. The content is solely the responsibility of the authors and does not necessarily represent the official views of the National Institutes of Health.

1 Introduction

The introduction of the contraceptive pill was one of the most significant technological innovations in the twentieth century as it provided a practical and reliable tool to avoid unintended pregnancies, giving women and families greater control over the timing of the pregnancies and the number of children they would bear (Bailey, 2006, 2010). An extensive literature has shown that having more control over fertility decisions allowed women and families to alter their life choices more freely. For instance, marriage and first births were delayed, and women's human capital accumulation, labor force participation and hours worked increased (Goldin and Katz, 2002; Bailey, 2006). However, few studies have examined the extent to which the Pill's availability affects the average quality of the conceived children by avoiding pregnancies that are more likely to be unintended, and therefore less likely to receive adequate biological, economic, and emotional resources.[1]

In this article, we examine the Pill's role in avoiding the conception of potentially less healthy children, therefore improving the average health of infants. In the process, we extend the scarce literature on the relationship between contraceptive prices and fertility. We develop a model that provides foundations for the empirical strategy we use. It relates contraceptive prices to health outcomes of the conceived. It predicts that the direction in which changes in contraceptive prices affect newborn health is ambiguous, and depends on parental characteristics, their taste for having children at the time, and their price elasticity of demand for contraceptives. We use a sharp, unexpected, and overnight increase in the price of contraceptives to analyze changes in their use and the consequent impact on the number of children conceived and on fetal, newborn, and infant health. We also identify factors that make women more vulnerable to contraceptives' price shocks.

Our empirical strategy exploits an exogenous variation in contraceptive prices, resulting from a massive collusion case involving the three largest pharmaceutical retailers in Chile—together they hold a market share of 92 percent. Between December 2007 and April 2008 they strategically increased the prices of 222 medicines. Oral contraceptives were included in this group, experiencing price increases ranging from 30 to 100 percent in just a few weeks (45 percent on average in just

[1] Part of the reproductive health literature considers *unintendedness*, unwantedness and mistimed to be different concepts regarding pregnancies (Petersen and Moos, 1997). Although they have very relevant distinctions, in this paper as in most dealing with these types of pregnancies (see, for instance, Gonen, 1999; Finer and Zolna, 2011), we bundle all of them together and label them as 'unintended' as we have no way to identify them separately in the data, and we believe our findings are extensive to all of them.

three weeks). The pharmacies established a system, called *the chronogram*, to raise the prices. In practice, it implied that one pharmacy, generally the largest, moved first; the other two followed, leaving the price of the products equal in all pharmacies in less than a week (Nuñez et al., 2010). 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 rise in prices for birth control pills took place. Based on it, we implement a version of a Before-After estimator, which takes into account the seasonality of births, the general trends of fertility in the country, as well as dynamics that arise because it takes time for the contraceptive medication to wear off 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 2007, as well as with administrative records of school enrollment for the period 2013-2016.

Our estimates suggest that the skyrocketing increment in prices caused a sharp decrease in contraceptive use, yielding a price elasticity that ranges between -0.11 and -0.16 (the upper limit reported in the literature). As a result, between 183 and 265 "extra" individuals were born in Chile per week, a 4% increase in the weekly birth rate. We show large effects on the numbers of children born out of wedlock, from nulliparous women, and from women in their early twenties. We provide evidence in favor of the argument that the price spike caused a significant increase in unintended pregnancies as we find year-to-year increases in the number of fetal deaths, the number of underweight births, and the number of infant deaths that exceed the increase of the overall birth rate.[2] Additionally, we do not find significant impacts among teenage mothers or households located in poor areas, as was expected due to their typically low usage of oral contraceptives.

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 enrollment for the period 2013-2016, when the children affected by price hike reached school age. We find that children conceived shortly after the price shock were less likely to enroll relative to those conceived before the shock. However, the same children were more likely to attend special education programs for students with special needs, the majority helping individuals with intellectual disabilities. This

[2]Throughout the paper, we will refer to both miscarriages and stillbirths as *fetal deaths*. This is just for the sake of brevity, 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 will be made when needed.

evidence is consistent with the hypothesis that those conceived during the first weeks of 2008 faced dire conditions during critical developmental periods.

The paper is organized as follows. The next section provides a literature review. Section 3 describes the collusion case, the event generating the required exogenous variation in prices. Section 4 describes our multiple sources of information. Section 5 develops a conceptual framework motivating our empirical strategy. Sections 6 and 7 present the empirical analysis and main results, respectively. Section 8 concludes.

2 Brief literature review

Since its introduction in the US in the late 1950's, social scientists have tried to disentangle the impact of access to oral contraceptives from the economic and social changes taking place at that time (Bailey, 2010). For example, economists have argued that evidence shows a significant fertility decline even before the introduction of oral contraceptives, corresponding to fundamental changes in the demand for children that are closely related to women's increasing access to the labor market (Becker, 1981). To address the confoundedness issue, researchers used exogenous legal variations that affected access to the Pill for particular subgroups of women. They found that the Pill reduced the fertility in unmarried young women and contributed to strengthening the social changes that granted them more access to the market economy (Goldin and Katz, 2002; Bailey, 2006). The results on the reduction of fertility due to the introduction of the Pill in the US were later extended to include married women (Bailey, 2010).

To understand the effect of oral contraceptive access on the fertility of all women instead of just on subgroups, we have to look for literature outside the US. Not surprisingly, just like in the US, the confoundedness driven by social forces also diluted the impact of family planning programs on fertility behavior in developing countries, where the impacts found were modest (Freedman and Freedman, 1992; Pritchett, 1994). However, more recent evidence using exogenous sources of variation shows that access to contraceptives has a significant impact on fertility in the developing world. For instance, fertility fell by 12 percent among women who gained access to the Pill due to the expansion of its distribution network in Indonesia (Molyneaux and Gertler, 2000). Conversely, a city-wide ban on oral contraceptives in Manila increased family size (Dumas and Lefranc, 2013).

Such pronatalist policies were also found to increase fertility rates by about 0.5 children in Romania (Pop-Eleches, 2010). In our case, the sudden, steep, and nationwide increase in oral contraceptive prices due to the collusion of drug retailers can be interpreted as a pronatalist unexpected change. Unlike the existing literature, where the mechanism is variation in the Pill accessibility due to exterior factors like laws or distribution channels, we are able to explore behavioral reactions due to price changes. Certainly, the effects of this change on fertility will depend on the price elasticity of contraceptives.

Therefore, our paper also relates to the literature that studies the relationship between contraceptive prices and demand (Ciszewski and Harvey, 1995; Janowitz and Bratt, 1996; Matheny, 2004; Collins and Hershbein, 2013, among others). With different identification strategies, these studies find relatively small sensitivity of the demand of oral contraceptives to price increases ranging from 0 to 15 percent. 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. Our paper, on the contrary, analyzes a nationwide shock in which prices increased overnight by an average of 45%. In that regard, our paper goes beyond the analysis of the relation between prices and contraceptive use and contributes to the scarce literature on the relationship between contraceptive prices and fertility. Findings indicate 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 contraceptives become substantially more expensive while remaining widely available.

Furthermore, our paper is the first to link the Pill's availability and its use to fetal and infant health. 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, 2011, among others), stress, depression and emotional hardship during pregnancy (Kiernan and Huerta, 2008; Black et al., 2016), and maternal behavior (Currie and Moretti, 2003). There is reason to believe that unintended pregnancies are more likely to suffer from all of these 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. According to public health and medical studies, they may be 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 may be on average less healthy than those born from intentional ones (Bustan and Coker, 1994; Sharma et al., 1994). In that context, the Pill, as a mechanism of avoiding unintended pregnancies, may play a role in determining the average quality of the beings that get to be conceived. In this regard, the economic literature has remained silent. Using the contraceptives price shock, we are able to explore the causal effect of an unintended pregnancy on the health of the product of conception. In the process, we contribute to the scarce literature on contraceptives and abortion in two ways: considering abortion as a contraceptive method as in Pop-Eleches (2010), and considering abortion as an indicator of poor health of unintended pregnancies.

3 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[3]—increased by about 75 percent within just few weeks (See Figure 1). This massive and widespread price increase was unexpected by the consumers who had been enjoying a year long spell of a steady price decline. The price shock 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).[4] 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,[5] 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

[3] The *Fiscalía Nacional Económica* (National Economic Prosecutor, FNE) estimates the joint market share of the three retailers at 92%. Durán and Kremerman (2007) calculates it closer to 97%. 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.

[4] The price increases did not occurred simultaneously, but rather implemented in successive waves of increases, which began in December 2007 with 62 medicines. Then, they added 70 drugs in January 2008, 31 February 2008, 40 in March 2008 and 19 medications in April 2008.

[5] http://www.salcobrand.cl/cl/empresas-salcobrand/

a business consultancy firm who advised for a "de-commoditization" of the industry.[6,7] 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.

In December 2008, the National Economic Prosecutor (FNE) filed a lawsuit at the *Tribunal de Defensa de la Libre Competencia* (Competition Tribunal, TDL) against the three major pharmaceutical companies in Chile for price fixing (Fiscalía Nacional Económica, 2008). According to the FNE, the three retailers immersed themselves in a price war during 2007, which resulted in a reduction in marketing margins.[8] The price war was put to an end in the last weeks of 2007, when the pharmaceutical companies decided to coordinate their pricing strategies (Fiscalía Nacional Económica, 2008).[9] In April 2008, the price coordination stopped after the FNE called drug retailers' executives to question regarding the price increases.

In March 2009, the Competition Court delivered a settlement between the FNE and FASA, in which FNE dropped the charges against the pharmacy.[10] 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,[11] the largest fine set in the Chilean antitrust history. This sentence was ratified by the Supreme Court after an appeal process in 2013.[12]

4 Data

Births, Mothers' Characteristics and 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

[6] http://www.jec.cl/articulos/?p=6528

[7] 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.

[8] This situation, evident in Figure 1, motivated FASA to present charges against CV for unfair competition.

[9] The FNE argued that they used the retail price suggested by the laboratories to coordinate the price increases.

[10] Even though at that time there was no leniency program.

[11] See http://www.economist.com/blogs/americasview/2012/02/competition-chile

[12] See http://www.law360.com/articles/376729/chilean-high-court-backs-40m-pharmacy-price-fixing-fines

the date of birth, weight at birth, place of birth, 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.

In addition to date on live births, the Chilean Health Information and Statistics Department collects data on all deaths, including those of unborn and newborn children.[13] Using them, we are able to include in our analysis pregnancies that end up in fetal deaths, as well as inquire about effects on infant mortality (i.e., children under the age of one).[14] 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.[15]

The Chilean Health Information and Statistics Department reports 739,390 live births in Chile between 2007 and 2009, 90.6% of which took place in urban areas and 40.54% took place in the Santiago region. Our data also indicate that there were 6,582 miscarriages and stillbirths during 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. Our data indicate an infant mortality rate of about 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.

Figure 2 shows that conceptions in Chile have a clear trend and a marked seasonality.[16] As the Chilean fertile-aged population grows, so does the number of conceptions per week. This causes

[13]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.

[14]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).

[15]See http://apps.who.int/classifications/icd10/browse/2016/en.

[16]We calculate the conception date by subtracting the gestation length (in weeks) from the week of birth

a positive trend of about 3 additional conceptions each week. In addition to the positive trend, Chilean fertility patterns 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, their market, and the collusion case. We supplement the information on births and fetal and infant deaths with data from the Competition Tribunal of Chile (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: Salcobrand, Fasa and Cruz Verde (who control between 92% and 97% of the market share). For these companies, we also have information on the number and locations of their stores over time.[17]

The TDLC data allow us to inquire about important features of the contraceptives' market in Chile and its dynamics. Retailing contraceptives in Chile is a sizable business.[18] According the the TDLC data, 10,773,126 out of the 39,476,571 transactions (i.e., 27.3%) included contraceptives.[19] On average, 10,542 units of contraceptives were sold each day between 2006 and 2008.[20] Revenues from sales of contraceptives at these three pharmacies was of around US$35.7 million in 2006,

[17]We also supplement the births and deaths censuses and the TDLC data with information on local characteristics (e.g., income and poverty levels) obtained from different waves of The Socio-economic Characterization surveys.

[18]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 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.

[19]It is important to note that 97% of the transactions that involved contraceptives were contraceptives-only sales, meaning that costumers purchases the contraceptives and nothing else.

[20]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 will provide contraception for one cycle.

US$34.7 million in 2007 and US$47.6 million in 2008 (see Figure A.1 in the Appendix). During the 2007 price war, pharmacy revenues fell by US$ 1 million despite the fact they sold 629,254 units more than the year before, only to rebound by around US$13 million while they were colluding.

As we observed in Figure 1 the TDLC data are detailed enough for us to clearly see each stage of the change in the structure of the contraceptives market (price war, collusion, and competition). The data also allow us to analyze the synchronicity of contraceptive price increases by store. Figure 3 compares the median contraceptive price by store during the last week of 2007 to the same price two weeks later (i.e., second week of 2008). It shows that the price increases were substantial, sudden, and across the board. Within two weeks, around 85% of stores were charging higher prices, and many of them—particularly, those that were on the lower side of the price distribution at the end of 2007—were charging over 100% more than they had charged a fortnight before.

Our data also allow us to explore contraceptive brand dominance and substitutability, because we are able to observe the brand choices made by the Chilean public. Chileans have several alternatives for oral contraceptives; in fact, pharmacies report selling 24 different brands of contraceptives. Of those 24 brands, only five sustain market shares above 5%, and very rarely one of them surpasses a market share of 15%.[21] Hence, the contraceptive market is not highly concentrated brand-wise; the top five brands account for 45% to 50% of the units sold in a given month. This feature of the Chilean pharmaceutical market is easily seen in Figure 4, where we plot the shares of the market for the main brands across time. The Figure 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. In Figure A.2 in the Appendix, we can observe sharp price spikes even at the brand level.[22] 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. If it occurred, substitution would bias our econometric results towards not finding an effect. In that respect, our results can be considered as a lower bound.

[21]Here we measure market share with respect to units sold, where a unit is defined in footnote 20

[22]In addition, Figure A.2 shows that the price fixing behavior was across the board and very precise. We present the price trajectories of the five main brands. We see, even at this disaggregate level, that prices across pharmacies do not differ and they move almost simultaneously. Such feature attests to the exact coordination of the pharmacies—the reader should note that these are *daily* average prices.

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).[23] 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 B 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 B.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 B.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 attendance years 2013 to 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.[24] The public records contain student's 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 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 Conceptual Framework

In principle, exogenous changes in contraceptive prices produce ambiguous effects on the average quality of the children conceived. Suppose that the propensity of agent i to use contraceptives at

[23] 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, 2016).

[24] 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.

time s is given by the price at that moment P_s, a vector of observable characteristics $Z_{is} \in [\underline{Z}, \bar{Z}]$ (e.g., age, marital status, income and education), and an unobserved component that represents the agent's taste for conceiving children at time s, $\theta_{is} \sim F_\theta(\cdot)$ and $\theta_{is} \perp P_S$. Thus, we can write the latent variable determining the propensity of agent i to purchase contraceptives at time s as

$$C^*_{is} = \gamma Z_{is} - \psi P_s - \theta_{is}$$

where γ and ψ are positive vectors. Consequently, the agent will use contraceptives if and only if $C^*_{is} > 0$. Let C_{is} be a dummy variable capturing this decision: $C_{is} = 1$ if $C^*_{is} > 0$, and $C_{is} = 0$ otherwise. All in all, people who greatly dislike having children at time s will most likely purchase contraceptives. Also, agents with low values of Z_{is} (e.g., poor or teenage agents) will most likely never use contraceptives, and agents with high values of Z_{is} must have a higher taste for children to abandon contraceptive use. Of course, in this context a price change—from P_s to P'_s with $P'_s > P_S$—does not modify the contraceptive purchasing behavior of all agents. Formally, the group responding to the change can be described by

$$N^{\Delta P}_{C_s=0} = \int\limits_{\underline{Z}}^{\bar{Z}} f(Z) \int\limits_{\gamma Z - \psi P'_s}^{\gamma Z - \psi P_s} f(\theta) d\theta dZ. \tag{1}$$

Thus, as basic economic intuition suggests, larger price changes would induce more agents to switch their contraceptive use behavior, modifying to a greater extent the characteristics of the individuals, θ and Z, on each side of the decision. In fact, equation (1) implies that $E[\theta|C = 0, P'_s] < E[\theta|C = 0, P_s]$ after a price increase. Likewise, for each level of θ, those responding to a price change will come from the group with relatively higher realizations of Z compared to the ones already choosing $C = 0$. Therefore, a price increase implies that $E[Z|C = 0, P'_s] > E[Z|C = 0, P_s]$.

Let's consider now the latent propensity of agent i to conceive at time s, Y^*_{is}, which we describe as

$$Y^*_{is} = \mu_{w(s)} - bC_{is}(Z_{is}, P_s, \theta_{is}) + \tau X_{is} + \xi_{is}, \tag{2}$$

where the idiosyncratic shock ξ_{is} is an iid random variable with distribution $F_\xi(\cdot)$. Therefore, agent i will conceive at time s (i.e., $Y_{is} = 1$) if $Y^*_{is} > 0$. Equation (2) states that the propensity to conceive at time s is affected by the use of contraceptives $C_{is}(P_s, Z_{is}, \theta_{is})$, individual characteristics X_{is}, and the thickness of the sexual/reproductive market at that time $\mu_{w(s)}$. This last term captures the fact that conception requires finding an appropriate mate and/or deciding to have sexual relations with a mate. As Figure 2 demonstrates, these events might have seasonal components. In low seasons, finding a mate and/or having sex with a mate occur with less frequency, therefore conception is less likely. $w(s)$ indicates which part of the yearly cycle time s belongs to. Thus, under this framework, access to contraceptives will not only decrease the probability of conceiving by an amount proportional to a positive constant b, but also that even in the absence of contraceptives, conception is not guaranteed as the agent could face a low $\mu_{w(s)}$ (or obtain a low draw of ξ_{is}).

For simplicity, we assume that agents are myopic in that they make their choices at time s using the information they have at that time, and do not create any expectation about the future. This rules out any strategic behavior regarding inter-temporal decisions.[25] Thus, the conditional probability of agent i conceiving at time s is given by

$$\Pr\left(Y_{is} = 1 | \mu_{w(s)}, C_{is}\left(Z_{is}, P_s, \theta_{is}\right) = c_{is}, X_{is} = x_{is}\right) = \mathcal{P}_{s,x,c,\theta}.$$

To obtain the unconditional probability of an agent conceiving at time s we integrate the conditional probability over the space of conditioning variables:

$$\mathcal{P}_s = \frac{Y_s}{N_s} = \int\int\int \mathcal{P}_{s,\nu,\zeta,\varrho} dF_X(\nu) dF_Z(\zeta) dF_\theta(\varrho) = \Pr\left(Y_s = 1 | \mu_{w(s)}, \bar{C}_s\left(P_s\right), \alpha\right)$$

where the variables with a bar indicate the average at time s, and α captures our assumption that the distributions of X_i, Z_i and θ_i do not change with time. Consequently, the characteristics of the *overall* pool of agents are always the same.

On the other hand, the total number of conceptions at time s can be written as

$$Y_s = N_s\left(\alpha + \mu_{w(s)} - b\bar{C}_s\left(P_s\right)\right) = N_s\alpha + \mu'_{w(s)} - b\sum_{i=1}^{N_s} C_s\left(P_s\right). \tag{3}$$

Thus, the total number of conceptions at time s depends on the number of people in fertile age at

[25]This is not an implausible assumption as the price increase we analyze in this paper was massive and sudden. Furthermore, before unexpected the price shock, prices had been declining steadily for about a year (see Figure 1). In consequence, past prices gave little information about future prices when the price increase arrived.

that time, which is effectively a time trend; a time fixed-effect that captures seasonality, and the number of people using contraceptives at time s, which in turn depends on the price of contraceptives at that time. This expression identifies the determinants of time-specific fertility, a result we will later take to the empirical estimation.

Finally, let the health of a being conceived at time s, H_{is}, be a random variable with associated distribution $F_H\left(\lambda_{is}, \sigma_H^2\right)$. Parents can affect the conceived being's health by adopting healthier behaviors before and during the pregnancy. That is, if parents choose healthier behaviors, the baby's realized health H_{is} will be drawn from a distribution that is centered around a higher mean (i.e., higher λ_{is}). We further assume that the adoption of healthier behaviors during pregnancy positively correlates with the taste for children θ and socioeconomic characteristics Z at time s. In particular, we assume $\lambda_{is} = g(\theta_{is}, Z_{is})$, where $g(\cdot,\cdot)$ is a differentiable increasing function in both arguments. Thus, the average health of the conceived at time s can be written as

$$
\begin{aligned}
E[g(\theta_{is}, Z_{is})|Y_{is}=1] = \Pr(C_{is}=0)E[g(\theta_{is}, Z_{is})|\xi_{is} > -\mu_{w(s)} - \tau X_{is}, C_{is}=0]& \\
+ \Pr(C_{is}=1)E[g(\theta_{is}, Z_{is})|\xi_{is} > -\mu_{w(s)} + b - \tau X_{is}, C_{is}=1]&
\end{aligned}
$$

Given that conception while taking the Pill is a very low probability event, we focus on the first term. Since $\theta_{is} \perp \xi_{is}$,

$$
E[g(\theta_{is}, Z_{is})|Y_{is}=1] \approx \int\limits_{\underline{Z}}^{\bar{Z}} \int\limits_{\gamma Z - \psi P_s}^{\infty} g(\theta_{is}, Z_{is}) f(\theta) f(Z) d\theta dZ,
$$

and the change in the expected health of the conceived, due to a chance in P_s, is given by

$$
\begin{aligned}
\frac{\partial E[g(\theta_{is}, Z_{is})|Y_{is}=1]}{\partial P_s} \approx \int\limits_{\underline{Z}}^{\bar{Z}} \int\limits_{\gamma Z - \psi P_s}^{\infty} \left[\frac{\gamma}{\psi}\frac{\partial g(\theta_{is}, Z_{is})}{\partial Z_s} - \psi \frac{\partial g(\theta_{is}, Z_{is})}{\partial \theta_s}\right] f(\theta) f(Z) d\theta dZ& \\
+ \int\limits_{\underline{Z}}^{\bar{Z}} \int\limits_{\gamma Z - \psi P_s}^{\infty} g(\theta_{is}, Z_{is}) \left[f'(\theta) f(Z) + f(\theta) f'(Z)\right] d\theta dZ + \psi h(Z)&
\end{aligned}
$$

where $h(Z) = \int_{\underline{Z}}^{\bar{Z}} g(Z, \gamma Z - \psi P) dZ$. Therefore, our model shows that a contraceptives price increase can have an ambiguous effect on the average health of the children conceived. The direction of the effect will depend on whether $\frac{\gamma}{\psi}\frac{\partial g(\theta_{is},Z_{is})}{\partial Z_s}$ is greater or less than $\psi\frac{\partial g(\theta_{is},Z_{is})}{\partial \theta_s}$, and on the distributions of θ and Z in the population. These results indicate that the direction of the change in average health of the conceived will depend in part on the marginal returns to health inputs, mediated by the parameters that measure how responsive switchers are to changes in P. If ψ is large, small changes in P will induce many infra-marginal agents in terms of θ to change their contraceptive choice. However, these switchers would not have substantially different Z relative to those already choosing $C_s = 0$. Therefore, the size of the decrease in the average θ among all those who decide $C_s = 0$ after the price increase is greater than the size of the increase in the average Z. The results also show that the average health of the children conceived will be more likely to decrease due to a price increase if income is highly concentrated ($f'(Z) < 0$ holds for a great portion of its domain).

In sum, our model shows that those who alter their behavior due to price changes have different traits from those who leave their choices unaltered. This affects the pool of agents that conceive after the price increase, which in turn translates to a different average health of the babies conceived. The direction of that change is ambiguous. It depends on the tests for having children at the time, their socioeconomic and demographic characteristics, and the price elasticity of demand for contraceptives of those who end up switching their behavior. By documenting the impact of price hikes across different groups, our empirical analysis seeks to determine the relative relevance of these underlying factors.

6 Empirical Strategy

Now we translate our simple model to an estimation strategy. In principle, the sharp and completely exogenous—from the point of view of the consumer—increase in contraceptive prices in just a few days offers an opportunity to estimate the price elasticity of contraceptives and the causal effect of the Pill's availability on births and birth-related outcomes using simple OLS regressions. The intuition is that, due to the exogeneity of the price shock, the population of women before and after the price increase are comparable except for the fact that one group of women faces contraceptive prices 30% to 100% higher in a matter of days. This relates to the assumption made in the model, in which the distributions of X_i, Z_i and θ_i do not change across time. As a result, any discontinuity in the conditional distribution of outcomes, such as births or the health of newborn babies that were conceived after the price increases could be interpreted as the effect of contraceptive prices. Hence, we would be evaluating the difference in the outcomes between pre and post price shock periods.

More formally, let us call s^* the week in which the price of contraceptives increased, given that prices remained high, we can define a treatment indicator $D_i = \mathbf{1}\{t \geq s^*\}$. Therefore, the estimation would take the form:

$$Y_i = \alpha + \beta D_i + \varepsilon_i \tag{4}$$

where β would be our parameter (i.e., the average causal effect of the treatment) of interest given that $D_i \perp \varepsilon_i$.

However, the nature of the treatment and the outcomes we explore entails a significant amount of dynamics. Regarding the former, we have shown in equation (3) and in Figure 2 that conceptions in Chile have a secular trend and a marked seasonality. Regarding the latter, medical evidence shows that the probability of conception increases with the time elapsed after the suspension of contraceptive intake, due to the fact that the contraceptive medication progressively wears off and the menstrual cycle is gradually regulated (Gnoth et al., 2002). Hence, ignoring the dynamic elements of outcomes and treatment invalidates equation (4) as the correct empirical strategy because $D_i(t) \not\perp \varepsilon_i(t)$.[26]

To avoid the confoundedness caused by the dynamics in conceptions, and following our model, we need to control for a trend and seasonality in the conceptions series. Empirically, if we ignore the trend, we will overestimate our results since more conceptions happen as the fertile population in the country grows. If we ignore the seasonality, the comparison between the conceptions pre and post price shock will be misleading, because the number of conceptions fluctuate predictably throughout the year. Hence, we consider the following specification:

$$Y_t = \alpha + \tau 1\left[t > t^*\right] + f\left(\beta, |t - t^*|\right) + \beta_t t + \sum_{w=1}^{51} \omega_w S_w + \varepsilon_t \tag{5}$$

[26]Dynamics are also the main reason for not using the sharp increase of the contraceptives' prices to implement a Regression Discontinuity Design (RDD). RDD identifies the average casual effect of the treatment *at* the discontinuity point $\tau_{RD} = E[Y_i(1) - Y_i(0)|t = t^*]$ (Lee and Lemieux, 2010).

where t^* is the week when we start observing the treatment, t is a linear trend, and $\sum_{w=1}^{51} \omega_w S_w$ represents the week-of-the-year fixed-effects.[27]

We need to make two choices regarding the slopes of the regression lines before and after the cutoff (i.e., $f(\beta, |t - t^*|)$). First, we need to choose wether we force those slopes to be the same or to differ depending on the side of the discontinuity they lie. For instance, if we choose an specification in which $f(\beta, |t - t^*|) = \beta p(|t - t^*|)$, we impose the restriction for both slopes to be the same. On the other hand, if we allow the pre-treatment and treatment slopes to differ, the specification for $f(\cdot)$ would be $f(\beta, |t - t^*|) = \beta_b p(|t - t^*|) + (\beta_a - \beta_b) 1[t > t^*] p(|t - t^*|)$. Second, we need to choose the degree of the polynomial with which we are going to approximate the true regression lines (i.e., $p(|t - t^*|)$).

To provide as much flexibility as possible, we favor specifications that allow a different quadratic functional form for $f(\beta, |t - t^*|)$ in each side of the discontinuity.[28] We highlight the importance of the dynamic component of the effect by presenting results at different moments throughout the year; evaluating the estimated polynomial at different values of t. That is, the effect on week $t = s > t^*$ is given by

$$Y_{s>t^*} - Y_{t^*} = \tau + \beta_a p(|t - t^*|)\,|_{t=s>t^*}$$

7 Results

As explained in the previous section, our empirical strategy relies on a version of a Before-After estimation strategy that uses an exogenous positive shock in the price of contraceptives. The discontinuity is in the time dimension, as we compare the year-to-year difference in conception before and after the rise in the contraceptive prices.

[27] Note that t^* in equation (5) need not be the first week of 2008. We can use this specification to allow for a "donut-hole" approach (i.e., $t^* > s^*$) as in Cohodes and Goodman (2014) and Barreca et al. (2011), but not for the usual reason of manipulation of the running variable. Instead, we could potentially do so to deal with the fact that a sudden price increase of contraceptives does not imply that their consumption will immediately be modified, because women's Pill stocks will take time to deplete before the new prices must be faced, and then in order to conceive, a new menstrual cycle should commence after the withdrawal from the Pill.

[28] In some cases, we show robustness to the inclusion of cubic polynomials in $f(\beta, |t - t^*|)$. More of these results can be provided upon request.

7.1 Prices and Quantities.

We analyze first the responses of contraceptive purchases. Given that the expected cost of not taking the contraceptives outweighs the price spike, one would expect contraceptives to be very inelastic to price. However, our first result, presented in Figure 5, and quantified in Table 2 indicates that the exogenous price increase caused a sizable reduction in the amount of contraceptives purchased weekly. By week five after the price shock, pharmacies were selling 4,500 units less (a 5.5% reduction), and by mid-2008 that drop had reached 14,500 units (a 17.6% reduction). In that sense, we show that contraceptive prices are relevant in determining their consumption. Putting together Figures 1 and 5, we see that consumption of contraceptives was rising steadily as prices were decreasing throughout 2007. However, as the price spiked in January 2008, consumption receded.[29] Then, 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).[30] One explanation of this relatively high elasticity (when compared to others found in the international literature) is that abortion is illegal in Chile and the distribution of emergency contraceptives, such as the morning after pill, was very restricted until mid 2008 (Bentancor and Clarke, 2017).

In the remainder of this section, we analyze the impact of the significant reduction in contraceptive purchases on weekly conceptions, fetal deaths and infant deaths.

7.2 Results on Live Births

Our first set of results on conceptions is presented in Table 3. We collect the dynamic estimations of the impact of the price increase on conceptions that took place in different weeks after the price increase and resulted in live births.[31] Table 3 and shows the extent to which the sizable reduction

[29] 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 D shows that if there was any stockpiling, it was not as an strategic response to prices, but due to pharmacy availability.

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

[31] In the estimations we include all week after the price increase, although it should be noted that we should find no positive effect on the first two weeks after the price shock. This 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.

in contraceptive purchases—itself due to the massive price increases—caused a significant increase in births. In fact, just nine weeks after the price shock, there is an increase of about 71 births per week; four weeks later, there are on average 116 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.[32] The effect on total conceptions peaks during mid-year at a year-to-year increase of around 183 births per week, which represents 4% of the births that take place in the average week and yields a price elasticity of 0.089. The fact that the effect 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 the extra weekly conceptions return to pre-treatment levels. This interesting feature suggests that the increased pregnancy risk was counteracted either by behavioral changes that took some time to materialize, 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.

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 6.[33] They corroborate our findings in Table 3. 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-treatment levels.[34]

Having calculated the overall effect of pharmacy collusion in terms of total births, we now

[32]In particular by extending the length of the luteal phase, which improves the chances of a successful pregnancy (Gnoth et al., 2002)

[33]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 $\hat{\tau}_t^{BA} = \hat{\alpha}_{s^*}^r - \hat{\alpha}_t^l$

[34]For more non-parametric results, please refer to Appendix E. The complete set of non-parametric estimates for all the subsamples con be provided upon request.

proceed to analyze the impact across different groups.

If we split the effect described in Figure 6 by the age of the mother, as in Figures E.1 in the Appendix, we see that the effect followed different patterns for older women (age 30 to 35) versus younger women. The effect on older women took place immediately after the price shock, as the number of weekly conceptions increased by about 40, representing a relative increase of 3.6%. On the other hand, the effect on younger women increased over time, peaking by mid-year with about 60 extra weekly conceptions. This represents about 5.5% of the average weekly conceptions for that age range. Our evidence suggests that although the effect of the price increase is present for women of all ages, it was a bigger burden for women in their early twenties.

The second set of results in Table 3 shows a significant increase in the number of weekly out-of-wedlock births (see also Figure E.4a in the Appendix). Nine weeks after the price shock, we find a year-to-year increase of around 94.6 conceptions. This number goes up to around 197 by mid-year. This represents about 6.5% of the out-of-wedlock births that take place within the average week. Our estimations, reported in Figures E.3 in the Appendix, show that the significant increase in out-of-wedlock conceptions occurred among women of all fertile ages. The impact of the price increase on out-of-wedlock births follow the same overall pattern as the impact on all conceptions, presented in Figure 6. That is, these births increase until mid-year and then gradually return to pre-treatment levels. This should not be a surprise, given that two-thirds of the weekly births come from unmarried women.

The third set of estimates presented in Table 3 also show that the increase of the contraceptive prices caused a significant increase in the number of underweight births (measured as the proportion of newborns with low birthweight for gestational age, as indicated by Mikolajczyk et al. (2011)). At the peak of the effect, there were 18.5 more underweight births per week. They represent about 11.4% of the weekly average underweight births in the pre-treatment period. This effect is almost three times grater than the overall effect, in relative terms. Such large effect is in line with medical literature linking unintended pregnancies with the incidence of underweight newborns (Sharma et al., 1994). This should be a major concern given the well documented fact that weight at birth is a significant 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). Economic and early childhood development literatures consider weight at birth to be a measure of initial endowments product of not only genetic background and the extent to which the parents were involved in pre-natal care, but also the mother's previous health and habits (Currie, 2011).

Figure E.4b in the Appendix also shows that conceptions that resulted in underweight newborns were falling on a year-to-year basis prior to the price shock, during the period when contraceptive prices were falling as the pharmacies engaged in a price war. Sadly, the price spike that resulted from the pharmacies colluding broke the trend in the reduction of weekly underweight births.

The last set of results in Table 3 and Figure E.4c show the impact of the price spike on first-child conceptions. Before the pharmacies colluded, the first-child conceptions were falling on a year-to-year basis. In particular, first-child conceptions among young women (i.e., between 20 and 24 years of age) were falling by more than 30 conceptions per week (see Figures E.2 in the Appendix), congruent with the ongoing pattern of delaying fertility among women in middle and high income societies. However, the trend reversed after the price shock. By week nine after the price increase, there were around 59 more first-child conceptions, and by week 29, the effect reached 130 additional first-child conceptions. This represents an increase of 6% relative to the average week during the pre-treatment period.

Effects on poor households and teenage pregnancies. Interestingly, we find little or no effect among teenage mothers or among households that live in poorest municipalities. In fact, the barely significant positive effects we find are proportionally smaller than the overall effects we find on total conceptions. The results presented in Table 4 confirms 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.[35] 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.[36]

[35] 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.

[36] 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,

7.3 Fetal Deaths

Just like first-child conceptions, miscarriages and stillbirths were consistently falling on a year-to-year basis prior to the price shock, at which point they increased significantly.[37] Table 5 and Figure 7 show that weekly conceptions that resulted in miscarriages and stillbirths increased by around 11 during their peak in mid-year. Such an increase represents a 27% growth in the average weekly fetal deaths prior to the price shock, around 7 times the proportional effect the price shock had on live births. There can be three explanations to such disproportionate effect on fetal deaths. First, fetal deaths may be the product of unintended and therefore neglected pregnancies. Since abortion is illegal in Chile, some part of this increase could reflect efforts to end the pregnancy. Second, they may be from women who, due to health reasons, could not bring a pregnancy to term and were avoiding it by taking oral contraceptives. And third, it is possible 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).

To shed light on which of these possible explanations may be driving the result, we analyze the effect of the price increase on fetal deaths across different subsamples. First, we split the sample according to the average income in the mother's municipality. Table 5 shows that the effect is exclusive to females who live in municipalities with above median income. Among these, the greatest effect is on women from municipalities in the third quartile of the income distribution. Hence, there is 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—or the abortion later on—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. However, middle class women may have had the means to attempt to end them. 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

and among the latter only one in four use the Pill (while two-thirds use condoms) (INJUV, 2009).

[37] 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).

rate by half.

The differential effect across average income of the municipalities also suggests that the second explanation does not drive the results, since it is unlikely that the women who are unable to bring a pregnancy to term for health reasons are clustered in municipalities belonging to the third quartile of the income distribution.

To explore the possibility that the effect of the price increase worked through a lasting effect of past exposure to contraceptives, we analyze the incidence of miscarriages and stillbirths among women women who are pregnant for the first time. If we assume that older nulliparous women are more likely to have a longer exposure to hormonal contraceptives than younger nulliparous women, we would expect more fetal losses among older first-time pregnant women. However, regressions in Table E.1 in the Appendix show no evidence of differential effects of the price shock on fetal losses across the two age groups of first-time pregnant women. Furthermore, Figure E.5 in the Appendix shows that even though there are effects for first-time pregnant women in all age groups, except those in the 20-24 age bracket, these effects are not statistically different across age groups. That is, the confidence intervals of the four estimates always overlap with one another.[38]

Taken together, this evidence suggests that the only hypothesis that we cannot refute as an explanation for the increase in fetal deaths due to the price increase, is the possibility of mothers continuing with unhealthy behaviors during pregnancy, intentional neglect of the pregnancy, or interruption of unintended pregnancies. 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 heath from abortions. 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, fetal deaths are recorded only if the physician or the midwife identify the "product of the conception". Therefore, fetal deaths at early stages of the pregnancy—the ones more likely to be have been abortions—are less likely to be recorded.[39] 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-

[38]Fertility literature has linked mother's age with the likelihood of miscarriages and stillbirths (Andersen et al., 2000, see for example). 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 will 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.

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

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.

7.4 Infant Deaths

Just as the increase in unintended pregnancies due to the contraceptives' price increase boosted the incidence of fetal deaths, it could also have resulted in an increase in the number of unhealthy babies born alive, either because of poor health and habits of the now expectant mother, neglected pregnancies or as a consequence of failed pregnancy interruption efforts. Having shown a large effect of the price shock on the number of underweight newborns, 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). We explore whether there is a deterioration of newborn health after the contraceptives' price increase in an even more stringent margin than the one we explored with low birthweight. Prior to 2008, 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 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 will focus in 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, perinatal complications, nervous system diseases, brain malformations, malformations of the cardiac chambers, connections and valves, and enterocolitis. These conditions, listed in Table 6 where we provide the average number of weekly deaths and the mortality rate associated with each cause, account for 55% of all infant deaths in a given week.

The results presented in Table 7 suggest the price shock had a positive, although not statistically significant, year-to-year impact on the total infant mortality rate of around 0.7 permille points during the first two months after the two-cycle gap. Such difference represents a 9% increase in total infant mortality. The lack of significance may be attributable to the fact that infant deaths can happen for a variety of reasons, many of which are not related to the mother's health and habits, prenatal care, or failed attempts to end the pregnancy. Therefore, the effect of the price shock on total infant mortality may be diluted. 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 that, infant deaths due to these conditions were falling on a year-to-year basis prior to the price increase, but spiked up among the babies that were conceived after the contraceptives' price increase.

Table 7 indicates that the weekly infant mortality rate due to conditions arising during the perinatal period increased by 0.85 permille points, for those conceived during the second month after the price shock. This represents a year-to-year growth of 24% in the weekly infant mortality of that kind.[40] In particular, we find a proportionally large effect on weekly infant deaths due to necrotizing enterocolitis of the newborn, a condition 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). Our results indicate a year-to-year increase in weekly infant mortality due to necrotizing enterocolitis of about 73.25% among babies conceived after the price increase.[41]

Likewise, when we analyze the change in the weekly infant mortality rate due to malformations of the cardiac chambers, connections and valves, we find a stable year-to-year increase of 0.15 permille points. This represents a 75% growth in that particular infant mortality rate.

The weekly infant mortality rate due to diseases of the nervous system grew by around 0.15 permille points among infants conceived in the first six months after the contraceptives' price increase, a 103% year-to-year increase in the infant mortality rate associated with nervous diseases.[42] In an effort to distinguish the effect on *structural* nervous system diseases, we exclude diseases that

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

[41]See also non-parametric results in Figure E.6a in the Appendix.

[42]The diseases of the newborns' nervous system explored in Table 7 include inflammatory diseases of the central nervous system (e.g., meningitis), spinal muscular atrophy and related syndromes, degenerative diseases of the nervous system, epilepsy, primary disorders of muscles, cerebral palsy and other unclassified disorders of the nervous system like brain damage due to lack of oxygen.

arise due to contagion, like meningitis. Our findings, show that the weekly infant mortality rate due to non-inflammatory nervous system diseases increased by 141.5% on a yearly basis among babies conceived after the contraceptives' price increase.

Among infant deaths due to brain malformations, we find that the impact of the contraceptives' price increase is greater five to six months after the price shock. By this time, the weekly infant mortality rate due to brain malformations increased by 116.3%.[43] It is worth noting that most of the diseases associated with brain malformations have been linked to exposure of teratogens, substances that can disturb the development of the embryo or fetus resulting in congenital malformations. Croen et al. (2000) find that babies of mothers who take insulin, aspirin, alcohol or cigarettes, among other substances during pregnancy have higher incidence of brain malformations.[44]

Overall, the estimated impacts on infant mortality show that the pharmacies' collusion increased the number of conceptions as well as the conception of babies that were more likely to die as a result of cardiac malformations, perinatal complications like those caused by the immaturity of the digestive system, and congenital structural problems of the brain and the nervous system. The nature and causes of these diseases, together with our findings on miscarriages, are suggestive of not only an increase in the number of unintended and neglected pregnancies (in congruence with the medical literature on the topic) (Bustan and Coker, 1994), but also an increase in early termination attempts in a country where abortion is illegal and the morning-after pill was not available.

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. In light of the theoretical model, all these results indicate that, among

[43]The diseases considered under the category of brain malformations include malformations of the corpus callosum, holoprosencephaly, defective development or absence of part of the brain, lissencephaly (i.e., lack of brain fold and grooves), hydranencephaly, congenital cerebral cysts, macrocephaly, and malformation that affect facial appearance.

[44]We implement the empirical strategy outlined in Section 5 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). We find that, compared to women who became pregnant before the contraceptives' price shock, women who conceived after the price hike were twice more likely to drink alcoholic beverages during pregnancy, on average they ended breastfeeding one month earlier, and the babies conceived were 5 percentage points more likely to have below-median cranial circumferences. This goes 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).

those priced out by the shock, the effect of having relatively lower preferences for children at the time dominates that of having relatively better socioeconomic and demographic characteristics. In consequence, they provide evidence in favor of the notion that contraceptives, by preventing the conception of unexpected babies, truncate from the left the health distribution we observe among children. We will further explore this by analyzing the consequences of the price shock on long run outcomes.

7.5 Long Term Outcomes

We use publicly available administrative information on school attendance for the academic years 2013 to 2016 to investigate the long term consequences of the unexpected, sharp and significant price hike of oral contraceptives of 2008.

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.[45] 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 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.[46]

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-

[45]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 Abril (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.

[46]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.

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").

First, we first focus on kindergarten enrollment.[47] Table 8 presents the results. Columns (1) and (2) display the point estimates without excluding those born in the vicinity of week 40. Column (1) does so for the sample of children born between weeks 34-48 and Column (2) for those born between weeks 27-55. We find that the price increase reduced kindergarten enrollment five years later in at least -0.84 per each 1000 births. 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. The next two columns confirm this. In particular, excluding two weeks before and after week 40 leads to point estimates of -7.81 (column 3) and -3.13 (column 4). Columns (5)-(6) and (7)-(8) repeat the exercise but excluding three and four weeks, respectively. Overall, we find robust evidence that the price increase of oral contraceptives of 2008 had a negative effect on voluntary enrollment in kindergarten.

Since kindergarten enrollment was not compulsory, we need to be careful in interpreting the negative coefficients reported in Table 8. They might not only suggest that the "extra" children had a deprived early development because of being product of unexpected pregnancies. 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 further investigate the impact on enrollment, we extent the analysis to the academic years 2014, 2015 and 2016. This allows us to examine mandatory enrollment in first and second grades. For these years, however, 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, we report results excluding those children born between September and October. Table 9 presents the results. Panel A does so for the full sample (no donut-hole). In it we report that while we estimate a small and non-significant negative effect on first grade enrollment, we find that the 2008

[47]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.

price increase reduced second-grade enrollment by 5.70 students per each 1000 births seven years later. The differences between first and second grade might suggest higher grade retention among those individuals conceived during 2008 relative to those conceived in 2007. However, the results in Panel B confirm 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.

Administrative records are detailed enough to allow us to observe attendance to special education programs. In particular, those 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.[48]

To examine the impact of the price of contraceptive on enrollment in special education programs, we use the proportion of children (born in a specific month) attending programs for students with intellectual disabilities as the dependent variable. 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 10. 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 an second graders, panel A discloses increases of 0.46 and 0.81 students with special needs (intellectual disability) per each 1000 births. As expected, the point estimates increase in magnitude after excluding students born in September of October (panel B). Table E.2 in the Appendix presents the results aggregating all special education programs. The results confirm our findings.

As school enrollment in first and second grade is mandatory, we interpret the findings reported in Table 9 as an indication that those conceived during the first weeks of 2008 (post price increase) were more likely to face dire conditions during critical periods of development, supporting the hypothesis—outlined by the theoretical model—of an increase in the number of unexpected children. The results in Table 10 seem to confirm this interpretation. They go in line with the evidence on health outcomes of fetuses and infants presented above.

[48] Among first graders in our cohort of interests, 79.13% and 78.59% of the 4880 and 4882 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

7.6 Falsification Tests

Geographical Variation. In order to further explore the behavioral response to a sharp price increase, we use the geographical discontinuity provided by the international border between Chile and Peru. Arica is the northernmost city in Chile, and is the closest Chilean urban center to Peru. On the Peruvian side of the border, there is Tacna, a city characterized by its cheap medical services intended to attract inhabitants to this border city. Arica is just 57kms (35.6 miles) away from Tacna, and anecdotal evidence suggests that Chileans from Arica go to Tacna when they intend to purchase medical services, including prescription drugs.[49] The availability of cheaper prescription drugs should dilute the effect the contraceptives' price increase on the fertility of women living in Arica. We explore this in Table 11, and find that the price increase of contraceptives in Chilean pharmacies had no effect on conceptions in this border city.

Changing discontinuity date. We test whether our non-parametric estimation strategy is mechanically identifying effects where there are none by creating 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 2007 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. Table 11 shows our results when the discontinuity is timed at the 1^{st} of January of 2007. 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.

We perform the same test using data on miscarriages and fetal deaths. Congruent with what we described above, Table 11 shows that both miscarriages and fetal deaths where falling on a year-to-year basis up until 2007. Therefore, when we use the 1^{st} of January of 2007 as cutoff, we find negative results. In the absence of the contraceptives' price increase of January of 2008, the downward trends in fetal and infant deaths would have continued. Instead, they peaked to unprecedented levels.

[49]While Arica is 57kms (35.6 miles) away from Tacna, it is 311kms (193.3 miles) away from the closest Chilean city, Iquique.

8 Conclusions

This paper presents direct estimates of the behavioral responses of women to the price of the birth control pills. Specifically, we analyze the impact of unexpected, sharp and massive price hikes of oral contraceptives on the number of births as well as on the health of fetuses and newborns. Our identification strategy exploits the proved and documented case collusion of pharmaceutical companies in Chile, which resulted in a substantial exogenous change in prices between December 2007 and April 2008 (as we documented above, the prices increased between 30% and 100% during this period).

We find positive and statistically significant results for overall births. Our estimates suggest that as a result of skyrocketing prices, between 183 and 265 "extra" individuals were born per week in Chile during 2008. We show large effects on the number of children born out of wedlock and from mothers in their early twenties. We also show a disproportionate year-to-year increase in the number of underweight births and the numbers of miscarriages and infant deaths. Furthermore, we find evidence linking the price shock with a decrease in kindergarten, first and second-grade enrollment and an increase in the number of children requiring special education several years after the event. Hence, we provide evidence consistent with that the Pill is not only a birth control mechanism but also a tool that may improve newborn health by avoiding unintended pregnancies that are more likely to be affected by economic, social, biological and emotional deprivation. As expected, we do not find significant impacts among poor households and teenage mothers.

References

Almond, D. and Currie, J. (2011). Killing Me Softly: The Fetal Origins Hypothesis. *Journal of Economic Perspectives*, 25(3):153–172.

Andersen, A.-M. N., Wohlfahrt, J., Christens, P., Olsen, J., and Melbye, M. (2000). Maternal age and fetal loss: population based register linkage study. *BMJ*, 320(7251):1708–1712.

Bailey, M. J. (2006). More power to the pill: the impact of contraceptive freedom on women's life cycle labor supply. *The Quarterly Journal of Economics*.

Bailey, M. J. (2010). "Momma's Got the Pill": How Anthony Comstock and Griswold v. ConnecticutShaped US Childbearing. *American Economic Review*, 100(1):98–129.

Barreca, A. I., Guldi, M., Lindo, J. M., and Waddell, G. R. (2011). Saving Babies? Revisiting the effect of very low birth weight classification. *The Quarterly Journal of Economics*, 126(4):2117–2123.

Becker, G. S. (1981). *A Treatise on the Family, Enlarged Edition.* Harvard University Press, Cambridge, Massachusetts.

Behrman, J. R. and Rosenzweig, M. R. (2004). Returns to birthweight. *Review of Economics and Statistics*, 82(2):586–601.

Bentancor, A. and Clarke, D. (2017). Assessing Plan B: The Effect of the Morning after Pill on Children and Women. *The Economic Journal*, Forthcoming.

Biaggi, A., Conroy, S., Pawlby, S., and Pariante, C. M. (2016). Identifying the women at risk of antenatal anxiety and depression: A systematic review. *Journal of Affective Disorders*, 191(C):62–77.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2005). The more the merrier? the effect of family composition on children's education. *The Quarterly Journal of Economics*, 120(2):669–700.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2007). From the Cradle to the Labor Market? The Effect of Birth Weight on Adult Outcomes. *The Quarterly Journal of Economics*, 122(1):409–439.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2016). Does grief transfer across generations? bereavements during pregnancy and child outcomes. *American Economic Journal: Applied Economics*, 8(1):193–223.

Buckles, K. and Hungerman, D. M. (2016). The incidental fertility effects of school condom distribution programs.

Bustan, M. N. and Coker, A. L. (1994). Maternal attitude toward pregnancy and the risk of neonatal death. *American Journal of Public Health*, 84(3):411–414.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica*, 82(6):2295–2326.

CEPAL and INE (2005). CHILE: Proyecciones y Estimaciones de Población. Total País 1950-2050. CEPAL, Santiago.

Ciszewski, R. L. and Harvey, P. D. (1995). Contraceptive price changes: the impact on sales in Bangladesh. *International Family Planning Perspectives*, 21(4):150.

Cohodes, S. R. and Goodman, J. S. (2014). Merit Aid, College Quality, and College Completion: Massachusetts' Adams Scholarship as an In-Kind Subsidy. *American Economic Journal: Applied Economics*, 6(4):251–285.

Collins, E. G. and Hershbein, B. J. (2013). The Impact of Subsidized Birth Control for College Women: Evidence from the Deficit Reduction Act.

Croen, L. A., Shaw, G. M., and Lammer, E. J. (2000). Risk factors for cytogenetically normal holoprosencephaly in California: A population-based case-control study. *American Journal of Medical Genetics*, 90(4):320–325.

Currie, J. (2011). Inequality at Birth: Some Causes and Consequences. *American Economic Review*, 101(3):1–22.

Currie, J. and Moretti, E. (2003). Mother's education and the intergenerational transmission of human capital: Evidence from college openings. *The Quarterly Journal of Economics*.

Dott, M., Rasmussen, S. A., Hogue, C. J., and Reefhuis, J. (2009). Association Between Pregnancy Intention and Reproductive-health Related Behaviors Before and After Pregnancy Recognition, National Birth Defects Prevention Study, 1997–2002. *Maternal and Child Health Journal*, 14(3):373–381.

Dumas, C. and Lefranc, A. (2013). 'Sex in Marriage is a Divine Gift': For Whom? Evidence from the Manila Contraceptive Ban. *IZA Discussion Paper Series*, (7503).

Durán, G. and Kremerman, M. (2007). Informe Farmacias. Technical Report 3, Fundación Sol, Santiago.

Eriksson, J. G., Kajantie, E., Osmond, C., Thornburg, K., and Barker, D. J. P. (2009). Boys live dangerously in the womb. *American Journal of Human Biology*, 22(3):330–335.

Fan, J. and Gijbels, I. (2000). *Local Polynomial Fitting*. Approaches, Computation, and Application. John Wiley & Sons, Inc., Hoboken, NJ, USA.

Figlio, D., Guryan, J., Karbownik, K., and Roth, J. (2014). The Effects of Poor Neonatal Health on Children's Cognitive Development. *American Economic Review*, 104(12):3921–3955.

Finer, L. B. and Zolna, M. R. (2011). Unintended pregnancy in the United States: incidence and disparities, 2006. *Contraception*, 84(5):478–485.

Fiscalía Nacional Económica (2008). Requerimiento en contra de Farmacias Ahumada S.A., Cruz Verde S.A. y Salcobrand S.A. Technical report, Santiago.

FLACSO-Chile, Programa de Género y Equidad, Dides, C., Morán, J. M., Benavente, C., and Pérez, S. (2008). *Salud Sexual y Reproductiva en Chile 2007: actualización de datos estadísticos*. FLACSO, Santiago.

Freedman, R. and Freedman, D. (1992). The Role of Faily Planning Programms as a Fertility Determinant. In Phillips, J. and Ross, J., editors, *Family Planning Programmes and Fertility*. Oxford.

García-Enguídanos, A., Martínez, D., Calle, M. E., Luna, S., Valero de Bernabé, J., and Domínguez-Rojas, V. (2005). Long-term use of oral contraceptives increases the risk of miscarriage. *Fertility and Sterility*, 83(6):1864–1866.

Gnoth, C., Frank-Herrmann, P., Schmoll, A., and Freundl, G. (2002). Cycle characteristics after discontinuation of oral contraceptives. *Gynecol Endocrinol*, 16(4):307–317.

Goldin, C. and Katz, L. F. (2002). The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions. *Journal of Political Economy*, 110(4):730–770.

Gonen, J. S. (1999). Managed care and unintended pregnancy: testing the limits of prevention. *Women's Health Issues*, 9(2):26S–35S.

Hellerstedt, W. L., Pirie, P. L., and Lando, H. A. (1998). Differences in preconceptional and prenatal behaviors in women with intended and unintended pregnancies. *American Journal of Public Health*, 88(4):663–666.

INJUV (2009). *V Encuesta Nacional de Juventud*. Instituto Nacional de la Juventud, Santiago.

Janowitz, B. and Bratt, J. H. (1996). What do we really know about the impact of price changes on contraceptive use? *International Family Planning Perspectives*, 22(1):38.

Kearney, M. S. and Levine, P. B. (2009). Subsidized contraception, fertility, and sexual behavior. *The Review of Economics and Statistics*, 91(1):137–151.

Kiernan, K. E. and Huerta, M. C. (2008). Economic deprivation, maternal depression, parenting and children's cognitive and emotional development in early childhood1. *The British Journal of Sociology*, 59(4):783–806.

Lawn, J. E., Blencowe, H., Pattinson, R., Cousens, S., Kumar, R., Ibiebele, I., Gardosi, J., Day, L. T., and Stanton, C. (2011). Stillbirths: Where? When? Why? How to make the data count? *The Lancet*, 377(9775):1448–1463.

Lee, D. S. and Lemieux, T. (2010). Regression Discontinuity Designs in Economics. *Journal of Economic Literature*, 48(2):281–355.

Matheny, G. (2004). Family planning programs: getting the most for the money. *International Family Planning Perspectives*, 30(3):134–138.

Mayer, J. P. (1997). Unintended Childbearing, Maternal Beliefs, and Delay of Prenatal Care. *Birth*, 24(4):247–252.

Mikolajczyk, R. T., Zhang, J., Betran, A. P., Souza, J. P., Mori, R., Gulmezoglu, A. M., and Merialdi, M. (2011). A global reference for fetal-weight and birthweight percentiles. *The Lancet*, 377(9780):1855–1861.

Ministerio de Salud (2006). *Normas Nacionales Sobre la Regulación de la Fertilidad*. Ministerio de Salud, Santiago.

Ministerio de Salud (2007). *II Encuesta de Calidad de Vida y Salud Chile 2006*. Ministerio de Salud, Santiago.

Molyneaux, J. W. and Gertler, P. J. (2000). The impact of targeted family planning programs in Indonesia. *Population and Development Review*, 26(Supplement: Population and Economic Change in East Asia):61–85.

Nuñez, J., Rau, T., and Rivera, J. (2010). *Informe pericial sobre el Requerimiento de la FNE en contra de Farmacias Ahumada SA y otros.* Rol C No. 184-2008. Santiago.

OECD (2013). Health at a glance 2013. OECD Publishing.

Oreopoulos, P., Stabile, M., Walld, R., and Roos, L. L. (2008). Short-, Medium-, and Long-Term Consequences of Poor Infant Health: An Analysis Using Siblings and Twins. *Journal of Human Resources*, 43(1):88–138.

Panigrahi, P. (2006). Necrotizing Enterocolitis. *Pediatric Drugs*, 8(3):151–165.

Peipert, J. F., Madden, T., Allsworth, J. E., and Secura, G. M. (2012). Preventing Unintended Pregnancies by Providing No-Cost Contraception. *Obstetrics & Gynecology*, pages 1–7.

Petersen, R. and Moos, M. K. (1997). Defining and measuring unintended pregnancy: issues and concerns. *Women's Health Issues*, 7(4):234–240.

Pop-Eleches, C. (2010). The Supply of Birth Control Methods, Education, and Fertility Evidence from Romania. *Journal of Human Resources*, 45(4):971–997.

Pritchett, L. H. (1994). Desired fertility and the impact of population policies. *Population and Development Review*, 20(1):1–55.

Sharma, R., Synkewecz, C., Raggio, T., and Mattison, D. R. (1994). Intermediate variables as determinants of adverse pregnancy outcome in high-risk inner-city populations. *Journal of the National Medical Association*, 86(11):857.

Simonsen, M., Skipper, L., and Skipper, N. (2015). Piling Pills? Forward-looking Behavior and Stockpiling of Prescription Drugs.

Torche, F. and Echevarría, G. (2011). The effect of birthweight on childhood cognitive development in a middle-income country. *International Journal of Epidemiology*, 40(4):1008–1018.

Tribunal de Defensa de la Libre Competencia (2012). Sentencia No. 119/2012. pages 1–333.

9 Tables

Table 1: Average Births and Deaths per Week in 2007

Live Births					Deaths	
Total	OutWed	UWeight	1^{st} Child	TeenMom	Fetal	Infant
4626.32	2921.29	161.92	2121.83	743.27	42.03	38.6
(32.14)	(19.91)	(2.09)	(15.55)	(6.93)	(0.71)	(0.48)

Births By Mothers Age				Fetal Losses By Mothers Age			
20-24	24-29	30-35	>35	20-24	24-29	30-35	>35
1082.36	1084.88	1116.18	579.47	8.25	7.88	9.14	8.24
(5.42)	(5.93)	(4.96)	(2.88)	(0.30)	(0.31)	(0.30)	(0.30)

Note: Standard errors in parentheses. 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. *OutWed* stand for births out of wedlock. *UWeight* stands for babies with low birthweight for gestational age. 1^{st} *Child* stands for mother for which this is her first child. *TeenMom* stands for the mother being a teenager. *Fetal* stands for miscarriages and stillborns, while *Infant* stands for children who were born alive but died before turning one year old.

Table 2: Estimated impact of price increase on contraceptives units sold (in thousands)

	Week of 2008								
	1	5	9	13	17	21	25	29	33
Panel A: Overall, No Dynamics									
(1) -9.90***									
(1.90)									
Panel B: Dynamic Effects, Quadratic									
(2)	-1.17	-4.53**	-7.43***	-9.89***	-11.90***	-13.46***	-14.57***	-15.23***	-15.44***
	(2.57)	(2.12)	(1.88)	(1.78)	(1.79)	(1.82)	(1.84)	(1.83)	(1.80)
Panel C: Dynamic Effects, Cubic									
(3)	-3.74	-4.58*	-6.03***	-7.89***	-9.94***	-12.00***	-13.86***	-15.32***	-16.18***
	(3.31)	(2.48)	(2.30)	(2.33)	(2.32)	(2.27)	(2.23)	(2.24)	(2.30)

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 cutoff.

Table 3: Estimated impact of price increase on births by Week of Conception

		Week of 2008								
		1	5	9	13	17	21	25	29	33
Total Births										
Panel A: Overall, No Dynamics										
(1)	61.72**									
	(26.93)									
Panel B: Dynamic Effects, Quadratic										
(2)		-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)
Panel C: Dynamic Effects, Cubic										
(3)		-8.78	97.18*	174.16***	225.57***	254.81***	265.27***	260.37***	243.49***	218.04***
		(71.78)	(51.88)	(46.82)	(46.56)	(45.63)	(43.54)	(41.71)	(41.30)	(41.92)
Out of Wedlock										
Panel A: Overall, No Dynamics										
(1)	69.86***									
	(18.46)									
Panel B: Dynamic Effects, Quadratic										
(2)		11.20	55.97*	94.59***	127.06***	153.37***	173.54***	187.56***	195.43***	197.15***
		(37.70)	(29.94)	(25.34)	(23.56)	(23.51)	(24.00)	(24.22)	(23.83)	(22.93)
Underweight										
Panel A: Overall, No Dynamics										
(1)	10.22***									
	(3.47)									
Panel B: Dynamic Effects, Quadratic										
(2)		-8.27	-0.95	5.21	10.20**	14.01***	16.66***	18.13***	18.43***	17.57***
		(7.09)	(5.63)	(4.77)	(4.43)	(4.42)	(4.51)	(4.56)	(4.48)	(4.31)
First Child										
Panel A: Overall, No Dynamics										
(1)	7.78									
	(15.89)									
Panel B: Dynamic Effects, Quadratic										
(2)		-12.30	26.38	58.96***	85.45***	105.83***	120.12***	128.31***	130.41***	126.40***
		(31.20)	(24.78)	(20.98)	(19.50)	(19.46)	(19.87)	(20.05)	(19.73)	(18.98)

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. The conception week is obtained by subtracting the pregnancy length to the birth date.

Table 4: Estimated impact of price increase on births by Week of Conception

	Week of 2008								
	1	5	9	13	17	21	25	29	33
Poor 25%									
Panel A: Overall, No Dynamics									
(1) 8.51									
(14.44)									
Panel B: Dynamic Effects, Quadratic									
(2)	27.37	36.98*	44.52**	49.98***	53.37***	54.67***	53.90***	51.06***	46.13***
	(24.88)	(20.42)	(17.89)	(16.93)	(16.88)	(17.08)	(17.08)	(16.69)	(15.95)
Poor 10%									
Panel A: Overall, No Dynamics									
(1) 3.24									
(8.82)									
Panel B: Dynamic Effects, Quadratic									
(2)	18.43	19.58	20.20*	20.30*	19.86*	18.90*	17.41	15.39	12.84
	(15.57)	(12.78)	(11.19)	(10.59)	(10.56)	(10.69)	(10.69)	(10.44)	(9.98)
Teen Mom									
Panel A: Overall, No Dynamics									
(1) -13.33*									
(7.55)									
Panel B: Dynamic Effects, Quadratic									
(2)	-6.28	2.12	8.70	13.44	16.37*	17.47*	16.75*	14.21	9.84
	(15.16)	(12.04)	(10.19)	(9.47)	(9.46)	(9.65)	(9.74)	(9.59)	(9.22)

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. The conception week is obtained by subtracting the pregnancy length to the birth date. *Poor 25%* stands for births in municipalities with income in the bottom 25% of the distribution. *Poor 10%* stands for births in municipalities with income in the bottom 10% of the distribution. *Teen Mom* stands for the mother being a teenager.

Table 5: Estimated impact of price increase on fetal deaths by week of conception and income quantile

	Week of 2008								
	1	5	9	13	17	21	25	29	33
Fetal Deaths									
Panel A: Overall, No Dynamics									
(1) 1.56									
(1.95)									
Panel B: Dynamic Effects, Quadratic									
(2)	-9.14**	-3.36	1.45	5.27**	8.12***	9.98***	10.87***	10.77***	9.69***
	(3.81)	(3.02)	(2.56)	(2.38)	(2.37)	(2.42)	(2.44)	(2.41)	(2.31)
Bottom 50%									
Panel A: Overall, No Dynamics									
(1) -0.35									
(0.80)									
Panel B: Dynamic Effects, Quadratic									
(2)	0.11	0.58	0.97	1.29	1.54	1.71	1.80*	1.82*	1.76*
	(1.66)	(1.32)	(1.11)	(1.04)	(1.03)	(1.06)	(1.06)	(1.05)	(1.01)
Top 50%									
Panel A: Overall, No Dynamics									
(1) 1.91									
(1.80)									
Panel B: Dynamic Effects, Quadratic									
(2)	-9.26***	-3.94	0.47	3.98*	6.58***	8.28***	9.07***	8.95***	7.93***
	(3.53)	(2.81)	(2.38)	(2.21)	(2.20)	(2.25)	(2.27)	(2.23)	(2.15)
Top 25%									
Panel A: Overall, No Dynamics									
(1) 0.18									
(1.26)									
Panel B: Dynamic Effects, Quadratic									
(2)	-4.00	-2.00	-0.36	0.90	1.79	2.32	2.48	2.28	1.70
	(2.60)	(2.06)	(1.75)	(1.62)	(1.62)	(1.65)	(1.67)	(1.64)	(1.58)
50-75%									
Panel A: Overall, No Dynamics									
(1) 1.73									
(1.09)									
Panel B: Dynamic Effects, Quadratic									
(2)	-5.26**	-1.94	0.84	3.08**	4.79***	5.95***	6.58***	6.67***	6.23***
	(2.12)	(1.68)	(1.43)	(1.32)	(1.32)	(1.35)	(1.36)	(1.34)	(1.29)

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. The conception week is obtained by subtracting the pregnancy length to the birth date.

Table 6: Weekly Infant Deaths and Infant Mortality Rates before 2008 by Diagnosis

Diagnosis	Deaths	Mort. Rate	Diagnosis	Deaths	Mort. Rate
Total	36.87	7.970	Cardiac Malf.	0.926	0.200
Perinatal	16.55	3.577	Enterocolitis	1.451	0.314
Nervous Sys.	0.677	0.146	Non-Inf Nrv. Sys.	0.456	0.099
Brain Malf.	0.400	0.086			

Note: 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 before 2008. *Non-Inf Nrv. Sys.* stands for non-inflammatory nervous system diseases.

Table 7: Estimated impact of price increase on Infant Mortality by Week of Conception (per 1,000 live births)

	Week of 2008								
	1	5	9	13	17	21	25	29	33
Infant Mort.									
Panel A: Overall, No Dynamics									
(1)	0.70								
	(0.46)								
Panel B: Dynamic Effects, Quadratic									
(2)	0.70	0.62	0.55	0.47	0.38	0.30	0.21	0.11	0.01
	(0.82)	(0.67)	(0.59)	(0.56)	(0.56)	(0.56)	(0.56)	(0.55)	(0.53)
Perinatal									
Panel A: Overall, No Dynamics									
(1)	0.34								
	(0.30)								
Panel B: Dynamic Effects, Quadratic									
(2)	0.94*	0.88**	0.81**	0.73**	0.64*	0.54	0.42	0.30	0.18
	(0.54)	(0.44)	(0.39)	(0.37)	(0.37)	(0.37)	(0.37)	(0.36)	(0.35)
Nervous Sys.									
Panel A: Overall, No Dynamics									
(1)	0.08								
	(0.06)								
Panel B: Dynamic Effects, Quadratic									
(2)	0.16	0.16*	0.15**	0.15**	0.14*	0.13*	0.12	0.10	0.08
	(0.11)	(0.09)	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Cardiac Malf.									
Panel A: Overall, No Dynamics									
(1)	0.17**								
	(0.08)								
Panel B: Dynamic Effects, Quadratic									
(2)	0.16	0.16	0.15	0.15	0.15	0.15	0.15	0.14	0.14
	(0.14)	(0.11)	(0.10)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Brain Malf.									
Panel A: Overall, No Dynamics									
(1)	0.05								
	(0.04)								
Panel B: Dynamic Effects, Quadratic									
(2)	0.02	0.04	0.07	0.08	0.09*	0.10*	0.10*	0.10*	0.09*
	(0.07)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Enterocolitis									
Panel A: Overall, No Dynamics									
(1)	0.09								
	(0.09)								
Panel B: Dynamic Effects, Quadratic									
(2)	0.26	0.23*	0.20*	0.18	0.17	0.15	0.15	0.15	0.15
	(0.16)	(0.13)	(0.12)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.10)
Non-Inf. Nerv. Sys.									
Panel A: Overall, No Dynamics									
(1)	0.09*								
	(0.05)								
Panel B: Dynamic Effects, Quadratic									
(2)	0.15*	0.14**	0.14**	0.13**	0.12**	0.11*	0.10*	0.09	0.08
	(0.09)	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)

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. The conception week is obtained by subtracting the pregnancy length to the birth date. *Non-Inf Nerv. Sys.* stands for non-inflammatory nervous system diseases.

Table 8: Estimated impact of price increase on school attendance during 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 will be the ones 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 9: Impact of price increase on school attendance in First- and Second-grade

	1st Grade	2nd Grade
A. Full Sample		
Effect	-0.002	-5.698***
	(0.652)	(0.597)
R-squared	0.001	0.003
Number of births	371,916	
B. Excluding those born in September or October:		
Effect	-5.325***	-13.15***
	(0.608)	(0.595)
R-squared	0.008	0.005
Number of births	285,485	
Mean at baseline$^{(a)}$	879.96	849.41

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. 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 will be the ones 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 10: Impact of price increase on school attendance in services for children with intellectual disabilities (special education) by grade
First two years of formal education

	1^{st} Grade	2^{nd} Grade
A. Full Sample:		
Effect	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:		
Effect	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. 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 will be the ones 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 11: Estimated impact of price increase on Falsifiable Outcomes by Week of Conception

	Week of 2008								
	1	5	9	13	17	21	25	29	33
Arica Births									
Panel A: Overall, No Dynamics									
(1)	-0.87								
	(2.16)								
Panel B: Dynamic Effects, Quadratic									
(2)	-13.27***	-9.06***	-5.49*	-2.55	-0.24	1.43	2.47	2.88	2.65
	(4.41)	(3.51)	(2.97)	(2.76)	(2.75)	(2.81)	(2.84)	(2.79)	(2.69)
Total Births (Fake Cutoff)									
Panel A: Overall, No Dynamics									
(1)	11.80								
	(32.38)								
Panel B: Dynamic Effects, Quadratic									
(2)	-59.59	-44.56	-30.48	-17.36	-5.19	6.03	16.29	25.60	33.96
	(56.22)	(46.15)	(40.42)	(38.25)	(38.15)	(38.59)	(38.59)	(37.71)	(36.04)
Fetal Deaths (Fake Cutoff)									
Panel A: Overall, No Dynamics									
(1)	-6.83***								
	(2.22)								
Panel B: Dynamic Effects, Quadratic									
(2)	-9.14**	-8.10**	-7.12**	-6.21**	-5.35**	-4.57*	-3.84	-3.18	-2.59
	(4.01)	(3.29)	(2.89)	(2.73)	(2.72)	(2.75)	(2.75)	(2.69)	(2.57)
Perinatal (Fake Cutoff)									
Panel A: Overall, No Dynamics									
(1)	-3.06**								
	(1.35)								
Panel B: Dynamic Effects, Quadratic									
(2)	0.30	-1.60	-3.16*	-4.38***	-5.25***	-5.77***	-5.96***	-5.79***	-5.29***
	(2.31)	(1.90)	(1.66)	(1.57)	(1.57)	(1.59)	(1.59)	(1.55)	(1.48)

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. The conception week is obtained by subtracting the pregnancy length to the birth date. *Total Births (Fake Cutoff)*, *Fetal Deaths (Fake Cutoff)* and *Perinatal (Fake Cutoff)* perform the estimations using as cutoff the 31^{st} of December of 2006. The latter corresponds to the effect on the infant mortality rate due to conditions that arose during the perinatal period.

10 Figures

Figure 1: Average contraceptive prices by pharmacy

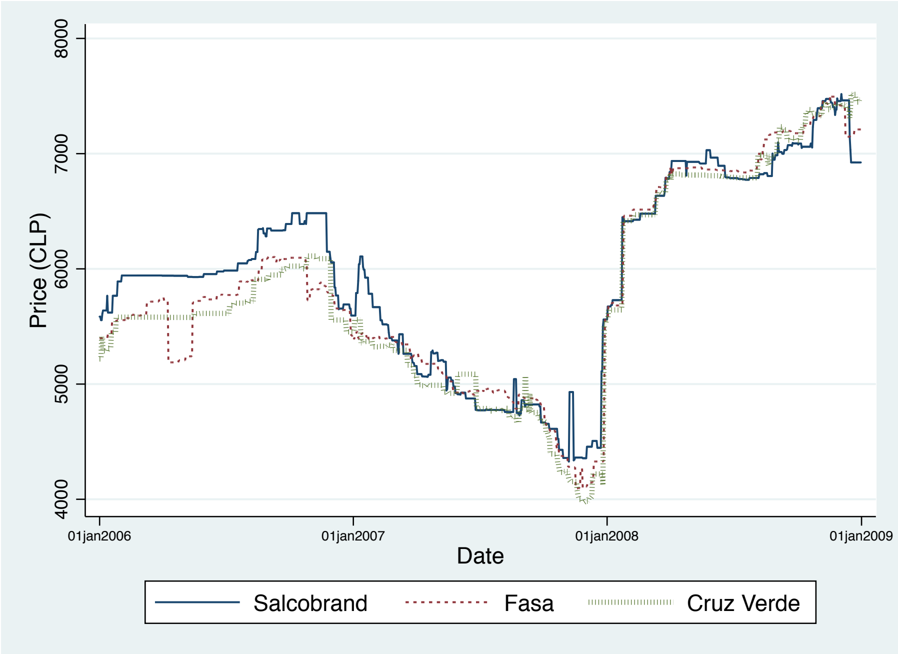


Figure 2: Conceptions per Week

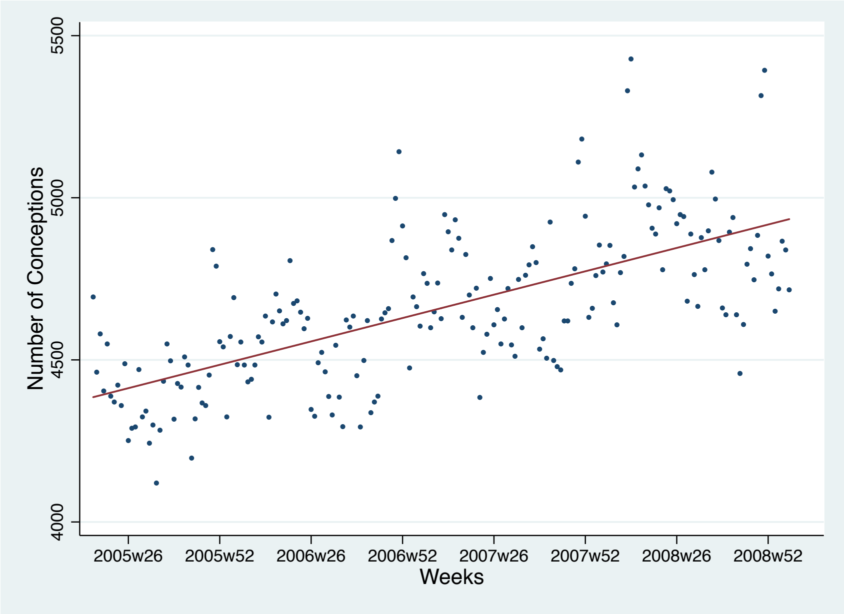


Figure 3: Median Weekly Contraceptive Price by Store: Week 52 of 2007 Vs. Week 2 of 2008

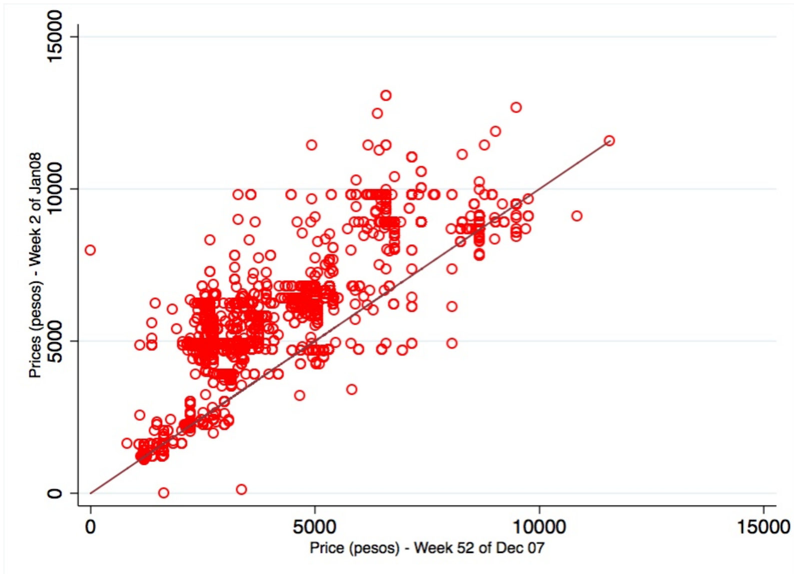
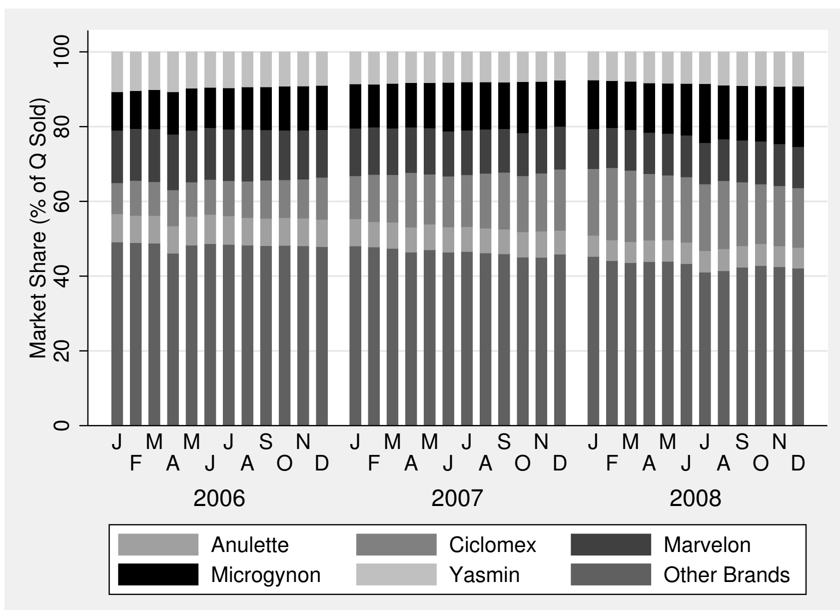
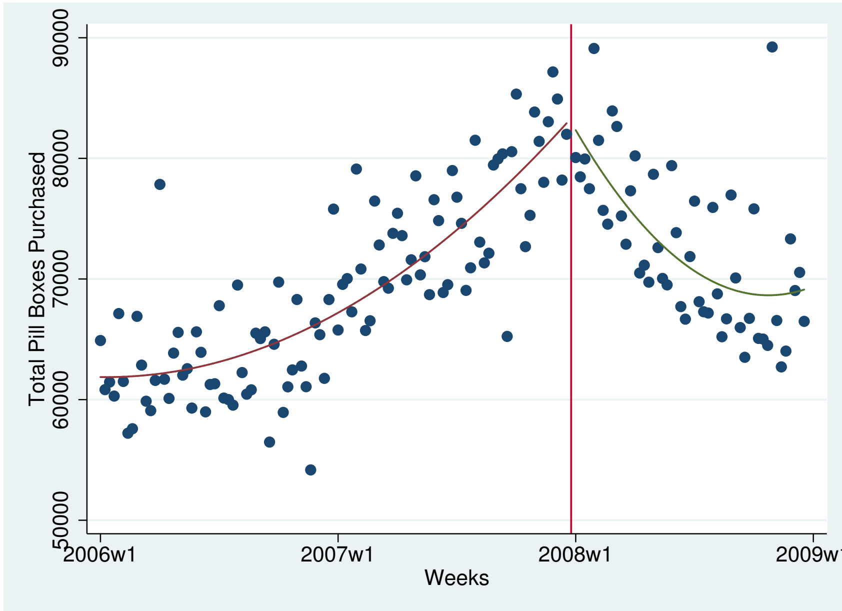


Figure 4: Shares of the contraceptive market by brand



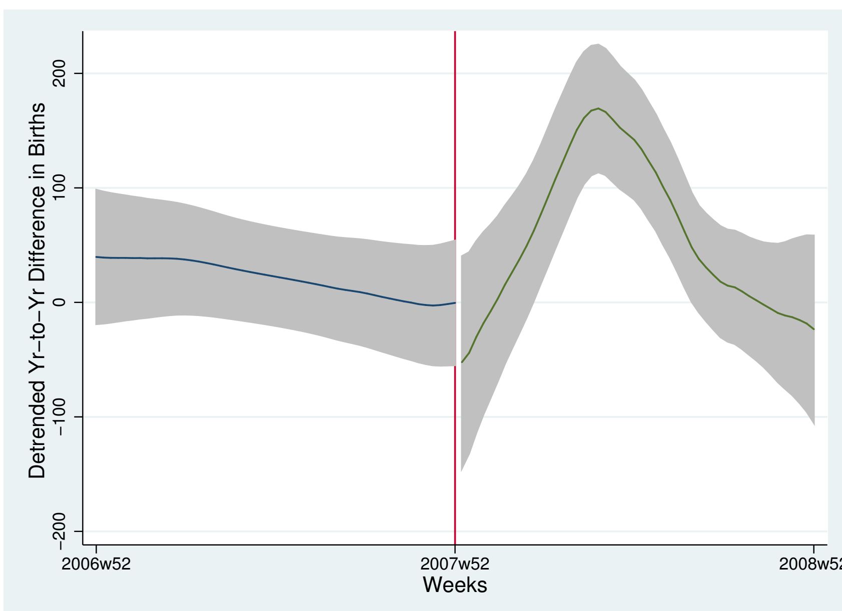
Note: We consider a quantity based on a unit of medicines that will provide contraceptive protection for one feminine cycle.

Figure 5: Weekly Contraceptives Purchases



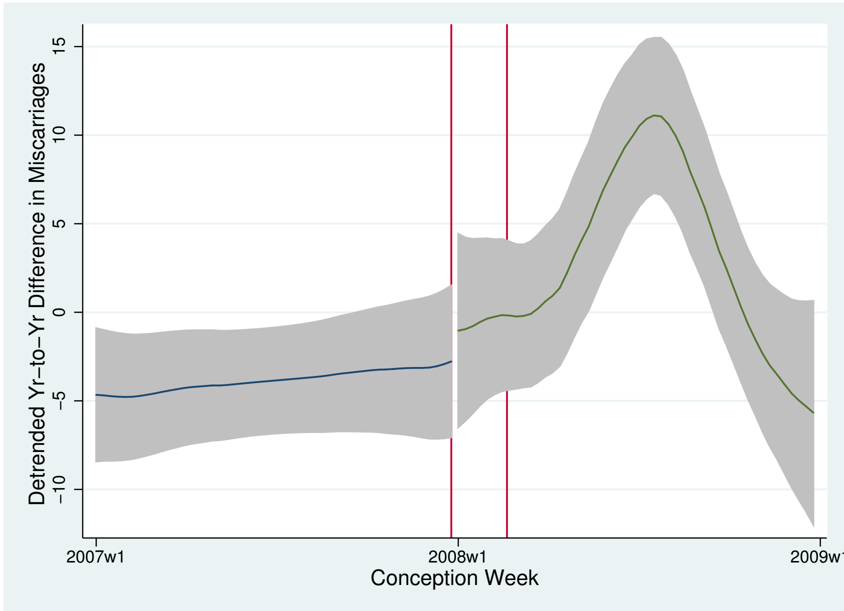
Note: Lines represent quadratic prediction plots.

Figure 6: Year-to-Year difference in Weekly 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.

Figure 7: Year-to-Year difference in Weekly Miscarriages and Stillborns by Conception Week

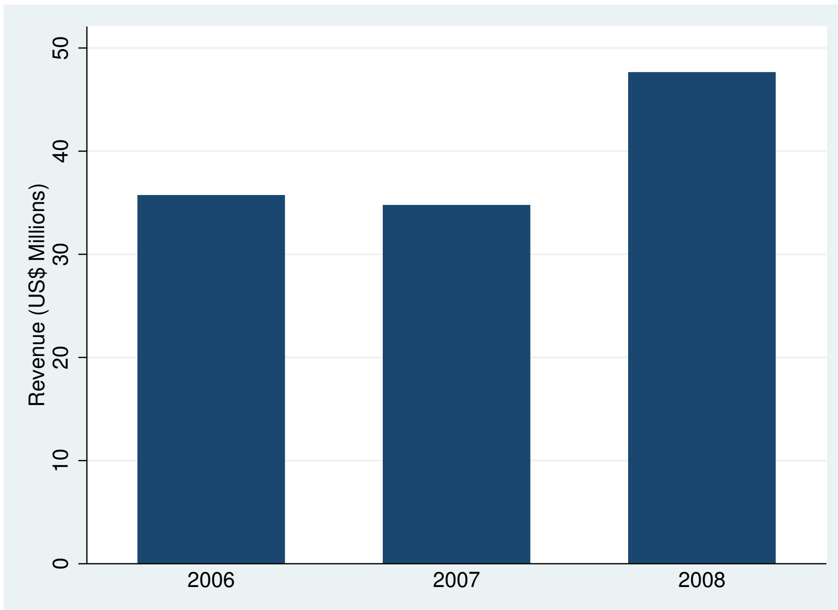


Note: births series are detrended (linear trend) and deseasonalized by standard methods (dummies per week of year). Then, the year-to-year difference in weekly births are computed. The conception week is obtained by subtracting the pregnancy length to the birth date.

Appendix

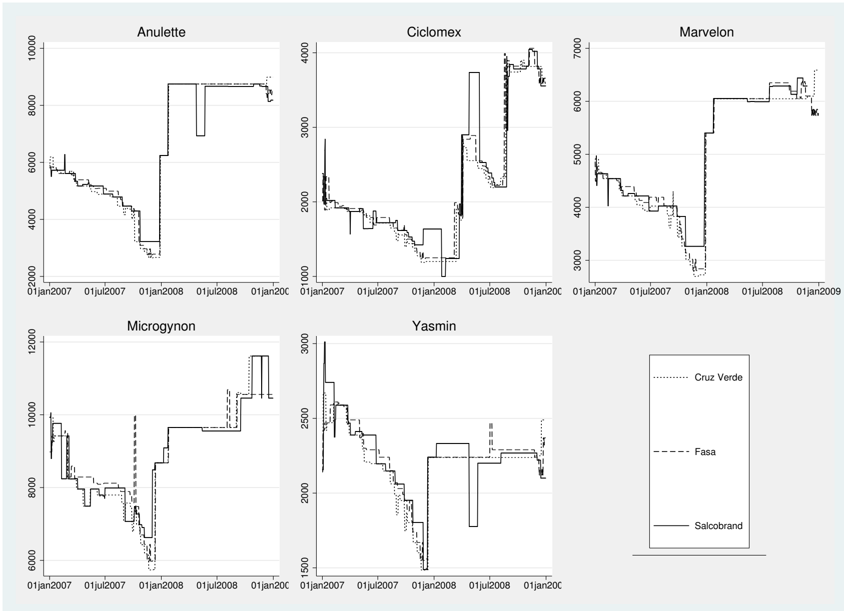
A Additional Evidence on the Collusion

Figure A.1: Revenue from contraceptives by year



Note: Daily revenue is calculated by multiplying the median price for a given contraceptive brand at a given pharmacy at a given day by the number of units sold by that pharmacy of that contraceptive that day. Yearly revenue due to the sale of contraceptives is calculated by aggregating across pharmacies and brands for a given year. The exchange rates used to report these figures in US dollar is the official one provided by the central bank of Chile at http://si3.bcentral.cl/Siete/secure/cuadros/arboles.aspx.

Figure A.2: Average contraceptive prices by pharmacy (Selected brands)

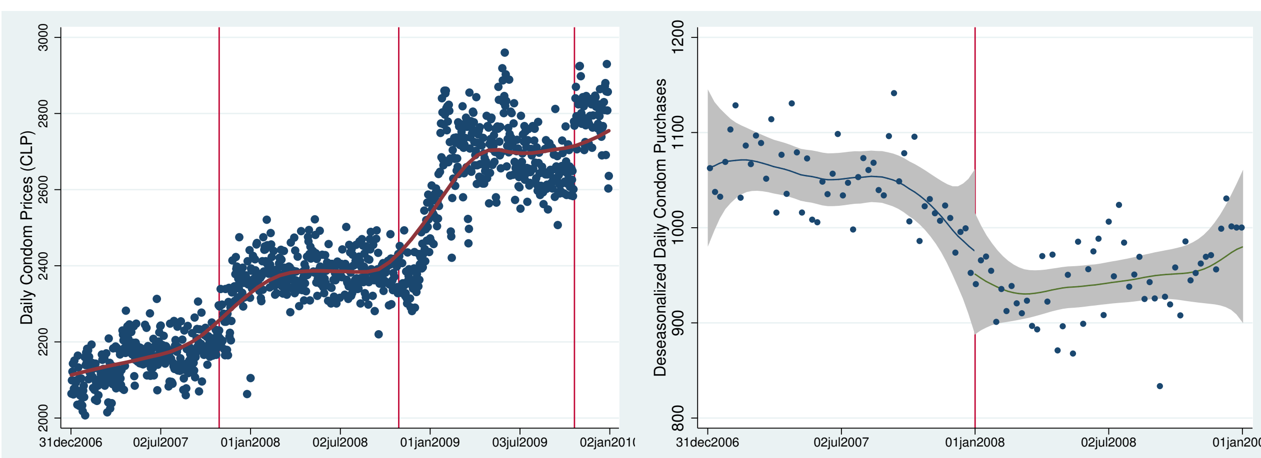


Note: The brands shown are those that maintain a market share of at least 5% each month between 2006 and 2008.

B The Condom as a Substitute

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

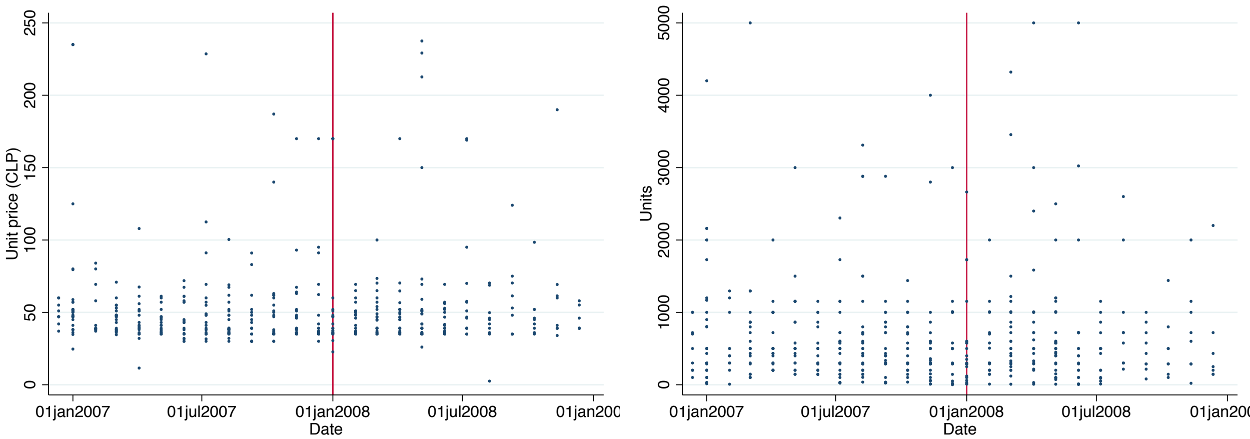
(a) Price of Packet of Condoms (3 Units) **(b)** Units Sold



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 B.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.

C Estimating Contraceptive Demand Elasticities

Table C.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.

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 C.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 Figures 1 and 5.[50]

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

D 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 D.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 D.1: Geographical distributions of Drugstores - Santiago

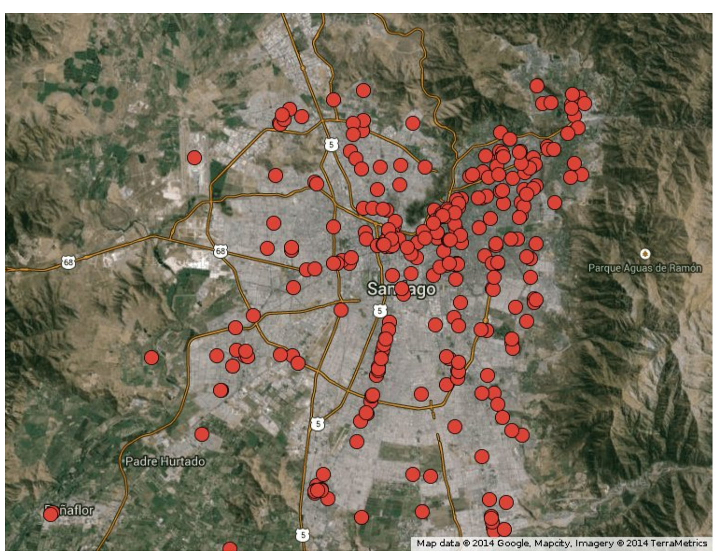


Table D.1: Estimated impact of price increase on births by Week of Conception

	(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)

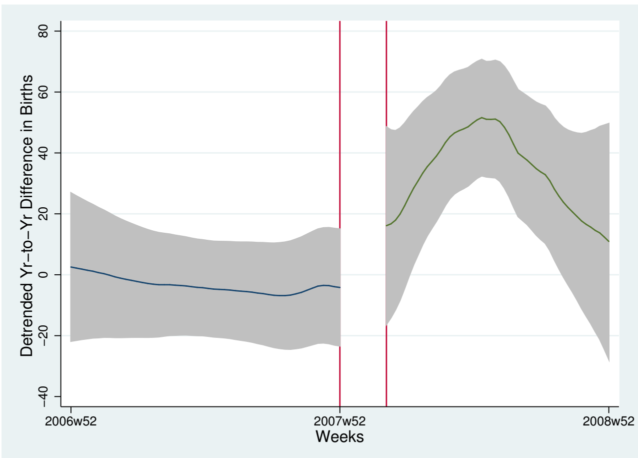
Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. *HD of Pharm* stands for conceptions that took place in Santiago's *comunas* with high density of pharmacies and *LD of Pharm* stands for conceptions that took place in *comunas* with low density of pharmacies. A *comuna* 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.

E Additional Non-Parametric Estimations

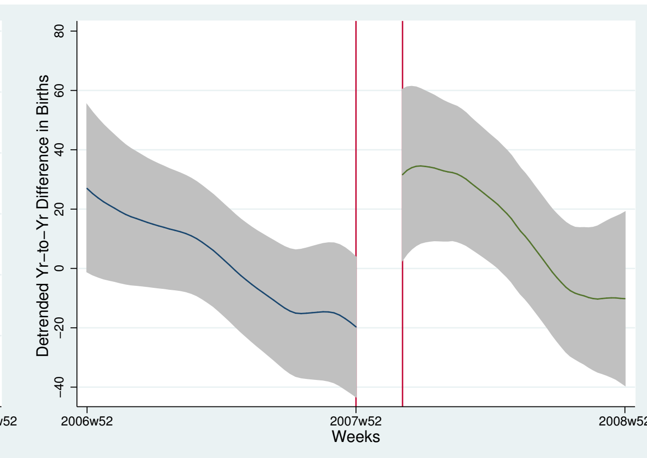
E.1 More Results on Number of Births

Figure E.1: Year-to-Year difference in Weekly Births by Conception Week (By Mother's Age)

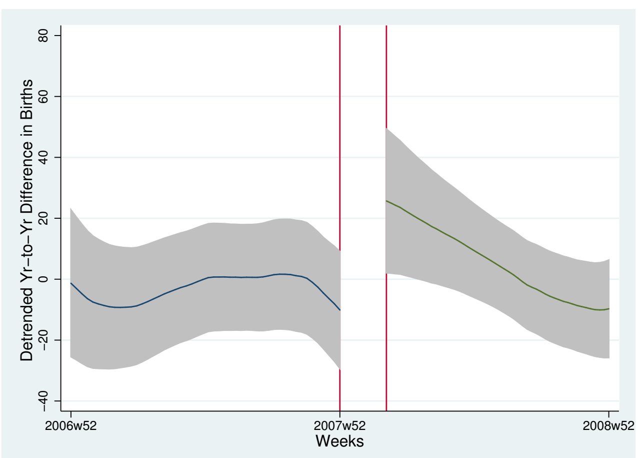
(a) 20-24 year old mothers



(b) 25-29 year old mothers



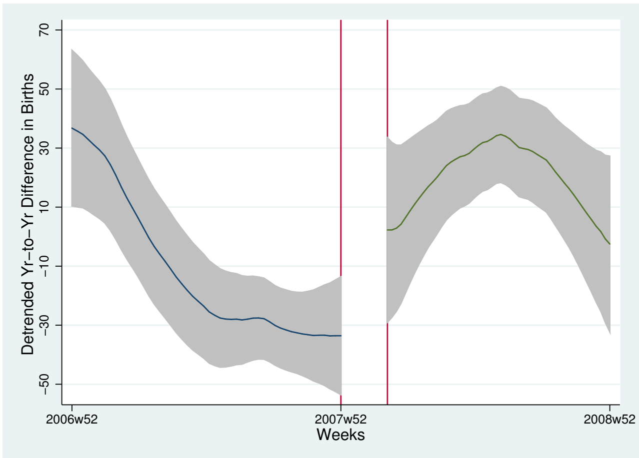
(c) 30-35 year old mothers



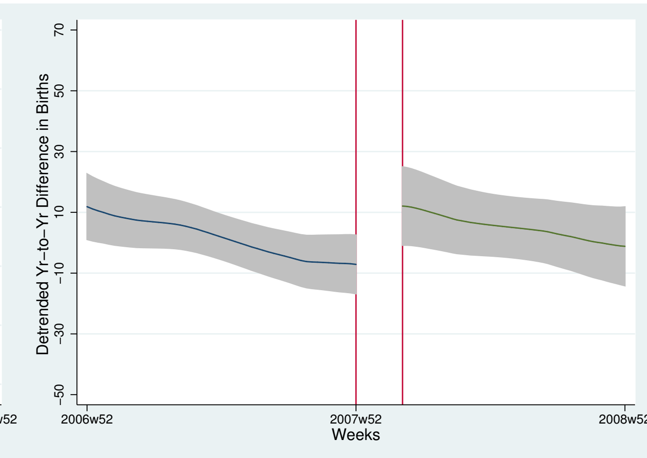
Note: births series are detrended (linear trend) and deseasonalized by standard methods (dummies per week of year). Then, the year-to-year difference in weekly births are computed. The conception week is obtained by subtracting the pregnancy length to the birth date.

Figure E.2: Year-to-Year difference in Weekly First Child Births by Conception Week (By Mother's Age)

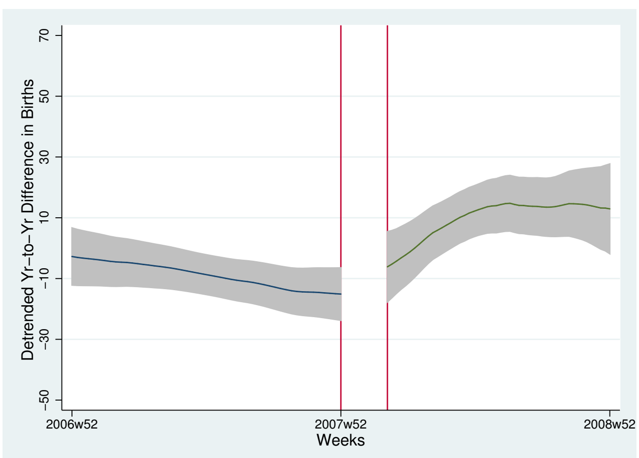
(a) 20-24 year old mothers



(b) 25-29 year old mothers



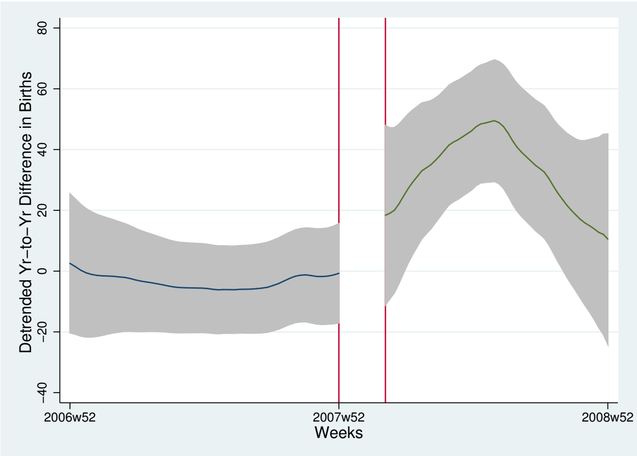
(c) 30-35 year old mothers



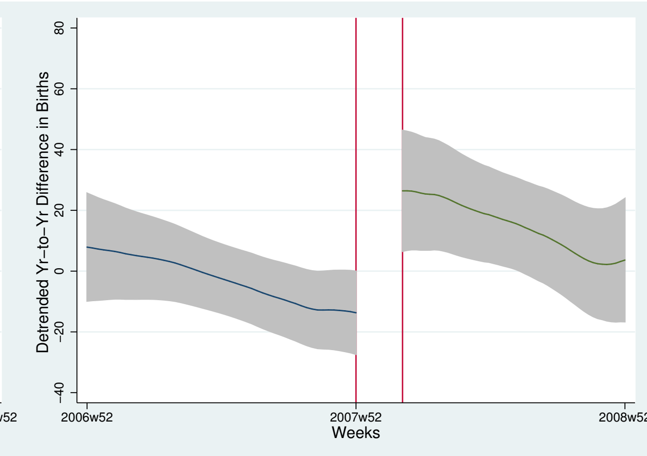
Note: births series are detrended (linear trend) and deseasonalized by standard methods (dummies per week of year). Then, the year-to-year difference in weekly births are computed. The conception week is obtained by subtracting the pregnancy length to the birth date.

Figure E.3: Year-to-Year difference in Weekly Births Out of Wedlock by Conception Week (By Mother's Age)

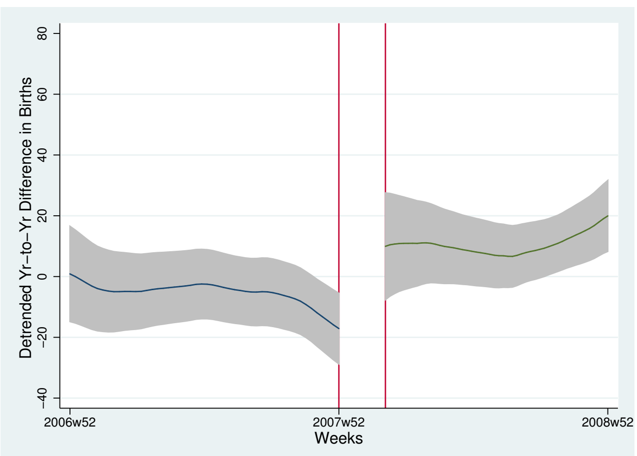
(a) 20-24 year old mothers



(b) 25-29 year old mothers



(c) 30-35 year old mothers

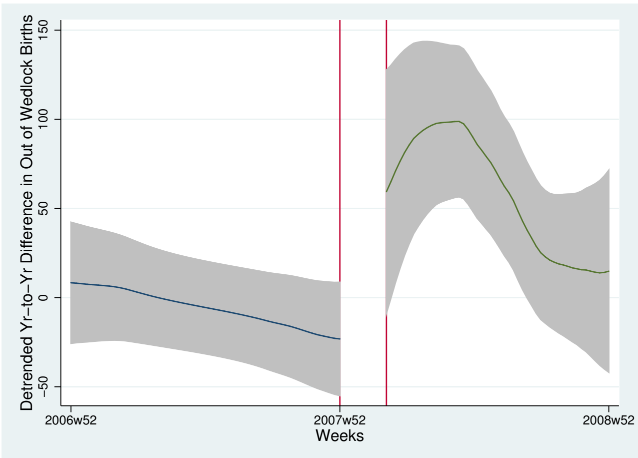


Note: births series are detrended (linear trend) and deseasonalized by standard methods (dummies per week of year). Then, the year-to-year difference in weekly births are computed. The conception week is obtained by subtracting the pregnancy length to the birth date.

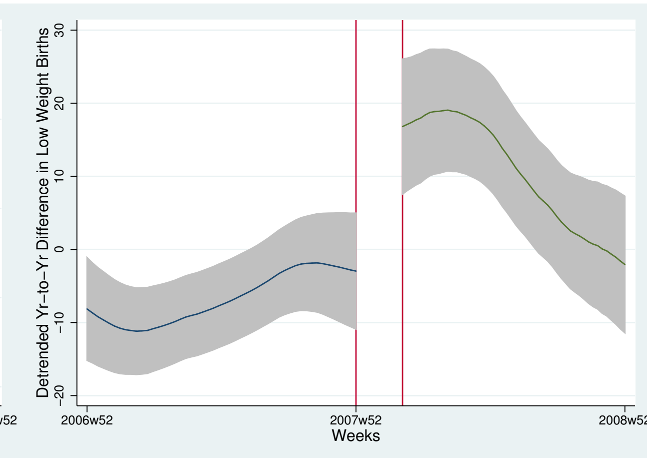
E.2 Non-parametric results on selected subsamples of live births

Figure E.4: Year-to-Year difference in Weekly Births by Conception Week (Subsamples)

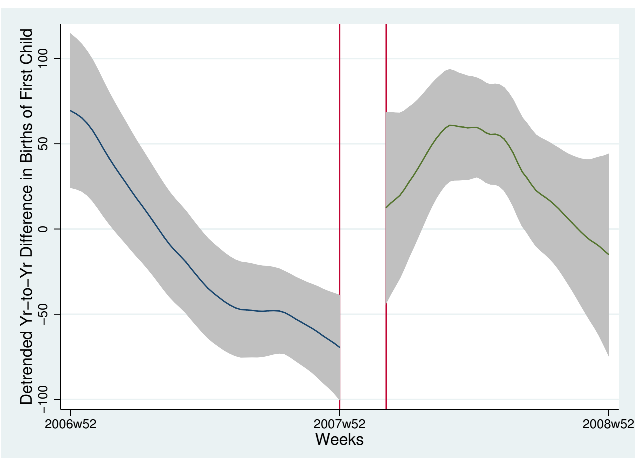
(a) Out of Wedlock



(b) Low Weight Births



(c) First Child



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.

E.3 More Results on Fetal Losses

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

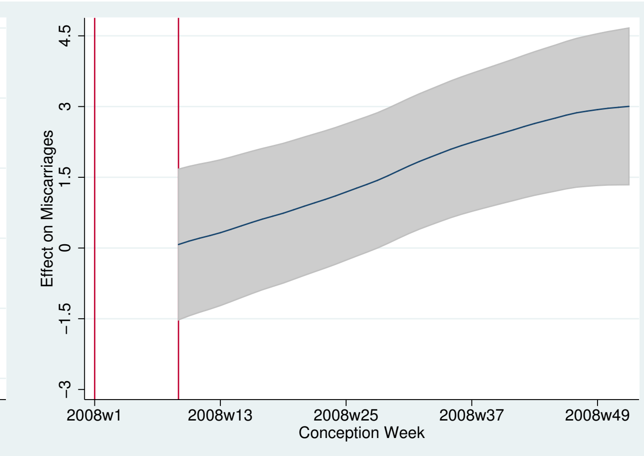
VARIABLES	(1) First Preg.	(2) First Preg.	(3) First Preg.	(4) First Preg.
Treatment	0.7525	7.7520	9.6857	-24.0518
	(29.946)	(30.324)	(29.715)	(490.244)
Mother Age	-0.6722	-0.6807	-0.8763	27.9635
	(0.685)	(0.682)	(0.689)	(24.229)
Mother Age^2				-0.5184
				(0.435)
Dist to Cutoff	0.1670***	0.2144***	0.4546*	0.5286**
	(0.048)	(0.060)	(0.240)	(0.246)
Dist to Cutoff^2			-0.0047	-0.0062
			(0.005)	(0.005)
Treat$\times$Age	-0.0582	-0.2023	-0.3764	1.8791
	(1.068)	(1.070)	(1.066)	(35.129)
Treat$\times$Age^2				-0.0371
				(0.629)
Treat$\times$Dist to Cutoff		-0.1284	0.3578	0.3175
		(0.099)	(0.382)	(0.386)
Treat$\times$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.

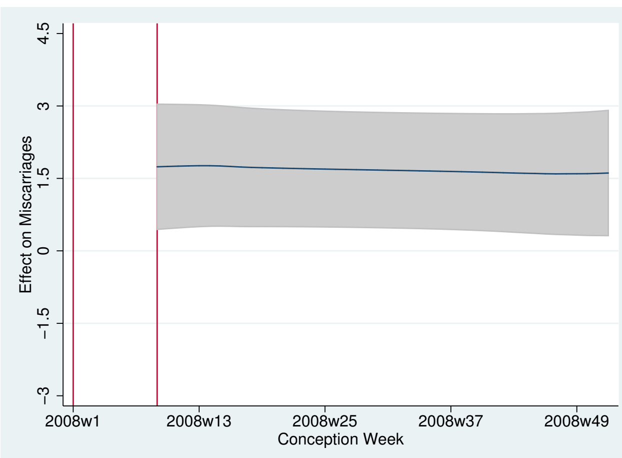
Figure E.5: Effect on Weekly Fetal Losses of Nulliparous Women by Conception Week (By Mother's Age)

(a) 20-24 year old mothers

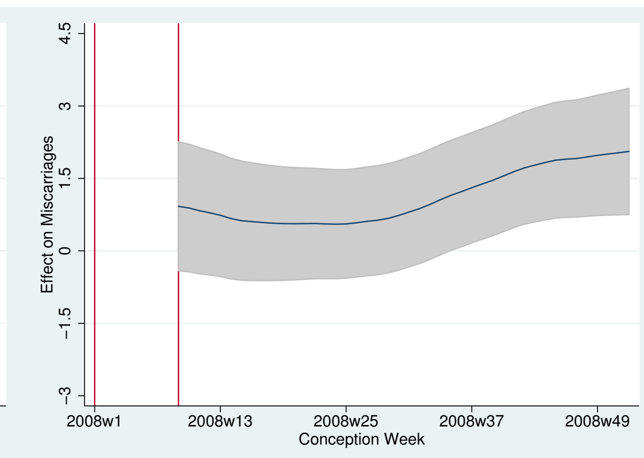
(b) 25-29 year old mothers



(c) 30-35 year old mothers



(d) 36-60 year old mothers



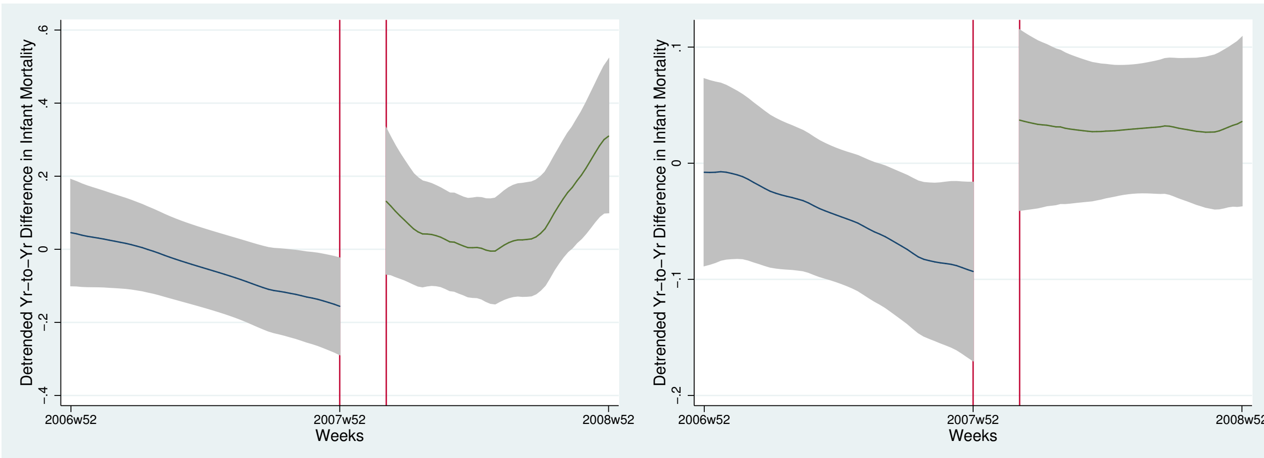
Note: Non-Parametric estimates of the effect of price increase of the weekly fetal deaths. Calonico et al. (2014) bandwidth and the shaded area corresponds to the 95% confidence interval for such effect. Note that these figures do not plot in there vertical axis the levels of the variable of interest.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

E.4 More Non-parametric Results on Infant Deaths

Figure E.6: Year-to-Year difference in Weekly Infant Deaths by Conception Week (By Cause of Death)

(a) Necrotizing Enterocolitis

(b) Non-Inflammatory Diseases of Nervous System



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..

Table E.2: Impact of price increase on school enrollment in special education programs by grade
First two years of formal education

	1^{st} Grade	2^{nd} Grade
A. Full Sample:		
Effect	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:		
Effect	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. 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 will be the ones 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.