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The children of the missed pill[☆]

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ABSTRACT

We assess the impact of exogenous variation in oral contraceptives prices—a year-long decline followed by a sharp increase due to a documented collusion case—on fertility decisions and newborns' outcomes. Our empirical strategy follows an interrupted time-series design, which is implemented using multiple sources of administrative information. As prices skyrocketed (45% within a few weeks), the Pill's consumption plunged, and weekly conceptions increased (3.2% after a few months). We show large effects on the number of children born to unmarried mothers, to mothers in their early twenties, and to primiparae women. The incidence of low birth weight and fetal/infant deaths increased (declined) as the cost of birth control pills rose (fell). In addition, we document a disproportional increase in the weekly miscarriage and stillbirth rates. As children reached school age, we find lower school enrollment rates and higher participation in special education programs. Our evidence suggests these "extra" conceptions were more likely to face adverse conditions during critical periods of development.

1. Introduction

Existing evidence shows that having more control over fertility decisions allows women and families to alter their life choices more freely.[1] In 2019, 76% of women of reproductive age who have their need for family planning satisfied used modern contraceptive methods (United Nations, 2019a), with the contraceptive Pill being the preferred method of choice almost all over the world (United Nations, 2019b). The worldwide efforts to raise awareness for contraception and the absence of cheaper alternatives of re-

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[1] Access to contraceptive methods is associated with lower fertility of teenagers and married and unmarried women, as well as delayed marriage and first births (Bailey, 2006; 2010; Lindo and Packham, 2017). Also, the literature has documented that it increases women's human capital accumulation, labor force participation, and hours worked (Ananat and Hungerman, 2012; Bailey, 2006; Bailey et al., 2012; Goldin and Katz, 2002; Guldi, 2008).

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versible birth control suggest the prevalence of this method will expand (United Nations, 2015). However, to a large extent, access to the Pill is determined by dynamic market forces. Individuals may opt-in or out of this product depending on price fluctuations, which could affect the odds of conception and the conditions under which a potential pregnancy develops.

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

The price war took place during 2007, and it effectively reduced the prices of medicines across the board. In particular, prices of oral contraceptives fell by 24% during that year. By the end of 2007, the three largest pharmacies agreed to end the price war and engaged in a collusion scheme in which they strategically increased the prices of 222 medicines. Oral contraceptives were included in this group, experiencing price increases ranging from 30 to 100% in just a few weeks (45% on average in the first three weeks). We use daily information on prices and quantities sold in the country by the three companies from almost 40 million transactions to determine the date when the price changes for birth control pills took place. Using these data, we implement an interrupted time-series analysis (Bloom, 2003; Cauley and Iksoon, 1988), which takes into account the seasonality of births, the general trends of fertility, as well as dynamics that arise because it takes time for the menstrual cycle to be fully regulated after discontinuing the Pill's intake. We complement the pharmacies' transaction data with administrative information from birth and death certificates collected between 2005 and 2008 and administrative records on school enrollment from 2013 to 2016. Our empirical strategy considers two different treatments: one stemming from a sustained and steady decline in prices (2007) and another one from a massive and sudden increase (first weeks of 2008).

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

Lastly, we analyze the long-term consequences of the unexpected price increase of 2008. In particular, we estimate its impact on kindergarten, first-grade and second-grade enrollment for the years 2013–2016, when the children born in 2008 reached school age. We find that children conceived shortly after the price shock were less likely to enroll relative to those conceived during the 2007 price war. Furthermore, conditional on enrolling, these children were more likely to attend education programs for children with intellectual disabilities.

These findings are consistent with the hypothesis that those conceived during the first weeks of 2008 were relatively more likely to face less favorable conditions during critical periods of development. As an extensive literature has shown, the development of healthy children and adults is greatly affected by economic and environmental deprivation while in-utero (Almond and Currie, 2011b; Black et al., 2007; Eriksson et al., 2009; Kiernan and Huerta, 2008, among others); stress, depression, and emotional hardship during pregnancy (Black et al., 2016; Class et al., 2011; Huttunen and Niskanen, 1978; Kiernan and Huerta, 2008); and maternal behavior (Currie and Moretti, 2003; Jayachandran and Pande, 2017). In addition, previous studies have shown that unintended pregnancies are more likely to suffer from such deprivations. According to public health and medical studies, women with unintended pregnancies are less likely to abandon unhealthy habits (Dott et al., 2009; Hellerstedt et al., 1998), 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).

Two aspects of our setting strengthen the interpretation of our findings. First, we may expect a lack of timely complementary investments in the health of the pregnancy as the numbers of unwanted and/or cryptic—when a woman does not find out she is pregnant until 20 weeks along or later—pregnancies increase.[3] Second, throughout the period of analysis, not only abortion was illegal in Chile but also other medications that could interrupt pregnancy (e.g., Misoprostol) were not sold in the country. As a

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

[3] In cryptic pregnancies, the lack of timely investments may be because mothers are unaware they are pregnant, rather than the unpreparedness or the unwillingness to adopt healthy behaviors. Regardless of the distinction, the fact is that the lack of timely investments affects the development of the fetus. Thus, just like other unintended pregnancies, cryptic ones may be more likely to face deprivation while in-utero than intended pregnancies (Del Giudice, 2007).

consequence, we can isolate the impact of the Pill's price fluctuations on fertility from statutory changes affecting abortion availability and/or access (Lu and Slusky, 2019; Myers, 2017).[4]

This paper extends several branches of the literature. First, it contributes to the limited evidence on the relationship between prices and demand for contraception and its effects on fertility.[5] Second, it provides new evidence linking changes in the Pill's availability to shifts in the average health of the children conceived, which highlights the role of selection into and out of the consumption of the Pill due to changes in its affordability.[6] Third, our analysis pins down a channel by which anticompetitive agreements between firms can cause substantial long-lasting harm even if antitrust enforcement stops them (Baker, 2003; Levenstein and Suslow, 2006). In fact, to the best of our knowledge, we are the first to quantify the short- and long-term impacts of anticompetitive behavior in the pharmaceutical industry on both consumers and their descendants.[7] As a consequence, our findings should serve as a cautionary tale for current cases of market failures in the pharmaceutical sector all around the world, most notably in the US, where cases like the EpiPen, insulin, and Daraprim—to name a few—have caught the public's attention.[8]

The paper is organized as follows. Section 2 describes the price war and the collusion case, the events triggering exogenous variations in prices. Section 3 provides an overview of the Chilean health system and the provision of medicines. Section 4 presents our methodology and identification strategy, while Section 5 describes our data. Section 6 presents the main results as well as falsification tests and robustness checks. Section 7 concludes.

2. The collusion case

Between December 2006 and December 2007, the three largest pharmaceutical retailers in Chile—Farmacias Ahumada (FASA); Cruz Verde (CV); and Salcobrand (SB), who control more than 90 percent of the market—engaged in a price war. As a result, prices of medicines experienced a stable decline during 12 months. Things, however, would drastically change. In January of 2008, the daily average prices of birth control pills increased by about 45 percent within just a few weeks. Fig. 1 displays the evolution of the average daily contraceptive prices by chain between January 2006 and January 2009. The sharp increase at the beginning of 2008 is evident.

This massive and widespread price increase between the last week of 2007 and the first weeks of 2008 was the result of a secret plan to collude and coordinate an "expressive, simultaneous, and uniform" price increases for 222 prescription and over-the-counter (OTC) drugs orchestrated between the three pharmacies (Fiscalía Nacional Económica, 2008).[9] According to lawsuit documents, SB was the company that led the price increases (Tribunal de Defensa de la Libre Competencia, 2012). It was the direct consequence of a change in SB's ownership that took place in April 2007. SB's new owner decided to abandon the existing pricing policy after receiving—in October 2007, just two months before collusion started—reports of a business consultancy firm that advised for a "de-commoditization" of the industry; that is, to end the price war with SB's competitors.[10] Hence, it is unlikely (if not impossible) that individuals could have anticipated the price shifts, much less their timing, rendering them exogenous to the consumers' fertility decisions. Thus, we treat the collusion as an unexpected shock to consumers who had been facing a year-long spell of a steady price decline.

In April 2008, the National Economic Prosecutor (FNE) contacted drug retailers' executives to inquire about the price increases. In December 2008, the FNE filed a lawsuit at the Bureau of Competition (*Tribunal de Defensa de la Libre Competencia*, TDLC) against the pharmaceutical companies for price-fixing (Fiscalía Nacional Económica, 2008). In March 2009, the Competition Court delivered a settlement between the FNE and FASA, in which FNE dropped the charges against the pharmacy. This settlement established a US$1

[4] Between 1989 and 2017, abortion was criminalized without exception in this country. It was again allowed in 2017 but under limited circumstances (if the pregnant woman's life is at risk, if the pregnancy is the result of rape, or if the fetus has severe conditions not compatible with life outside the womb). See Appendix D for the analysis of other medications as emergency contraception methods.

[5] This literature has found that subsidizing contraceptives decreases fertility by about 3 to 6 percent in Indonesia (Molyneaux and Gertler, 2000), and 9 percent for women in a relatively high-income bracket in the US (Kearney and Levine, 2009). However, we are not aware of previous studies analyzing a case in which birth control pills become substantially more expensive while remaining widely available.

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

[7] While some work analyzes the role of pharmaceutical companies' market power in determining drug prices (Howard et al., 2015), we are not aware of previous work linking such market failures to long-lasting effects on consumers.

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

[9] The *Fiscalía Nacional Económica* (National Economic Prosecutor, FNE) estimated the joint market share of the three retailers at 92%. This market concentration had been accompanied by a long tradition of anticompetitive practices in the industry over the last 20 years. In 1995, authorities sanctioned the same drug retailers for price-fixing, and episodes of price wars and unfair competition accusations were not uncommon.

[10] According to the FNE, the fact that the SB's new owner actively recruited executives from FASA and CV during 2007 facilitated coordination between the retailers.

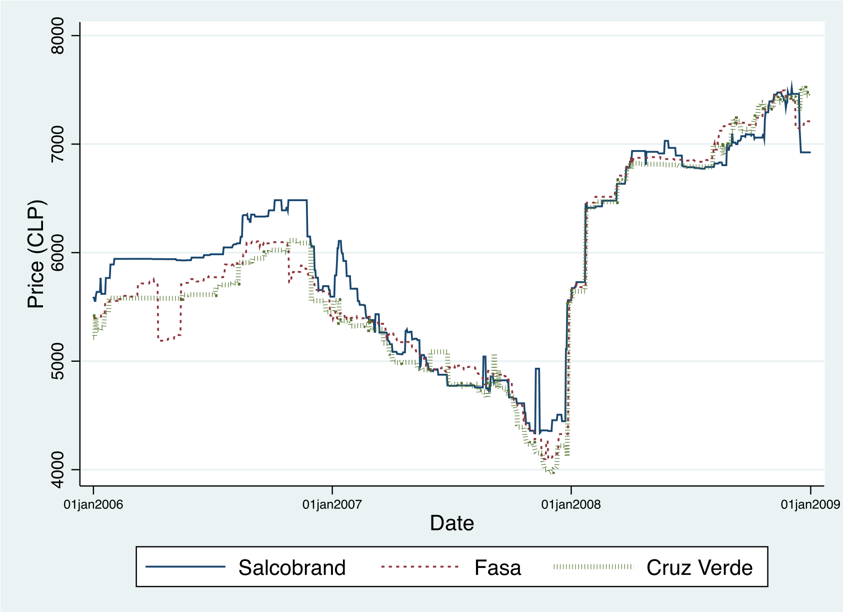

Fig. 1. Average daily contraceptive prices by pharmacy: January 2006–January 2009. *Notes:* Calculations based on transaction-level data from the Chile's Bureau of Competition, (TDLC). We take the average daily price of each contraceptive brand within each pharmacy and then calculate the average across brands. Occasionally in some pharmacies, a given medicine's list price is not the same as the price consumers end up paying, as they often include discounts. We consider only the final price after all discounts.

million fine for FASA, together with a statement that disclosed the coordination mechanisms and exchanges of information that had allowed the concerted price increases. After more than two years of trials, on April 23, 2011, the TDLC unanimously decided that the pharmaceutical companies were "guilty" of price-fixing. It imposed a fine of US$19 million, the largest fine set in the Chilean antitrust history at that time. The Supreme Court ratified this sentence after an appeal process in 2013.[11]

3. The Chilean context: contraceptive provision and limited substitutability

Given that Chile's public health care system provided some contraceptives free of charge during the period we study, what was to stop women facing skyrocketing price increases from substituting to the public system? To answer this question, we need to describe key features of the Chilean health system.

Since the structural reform of 1981, Chile has a dual health care system characterized by the coexistence of public and private health insurance and health care service provision. The public provision goes through a network of decentralized health centers, which operates at the municipal level and is funded to a large extent by the National Health Fund (*Fondo Nacional de Salud* or FONASA). It is structured in four groups defined by income levels: Groups A and B (lower income groups) receive free health care services, while groups C and D must pay between 10 and 20% of the cost of the services. On the other hand, a network of private health care centers managed by insurance companies (*Instituciones de Salud Previsional*, or ISAPREs) constitutes the private alternative.

Public insurance is the default unless individuals opt for the private alternative. Depending upon this decision, by law, all formal workers must contribute 7% of their monthly wages to FONASA or one of the ISAPREs. The private option includes additional fees depending on the health plan selected. That is why low-income families (contributors and dependents) are overrepresented in FONASA. By 2007 (2008), about 71% (73%) of the population was covered by FONASA, while 16.7% (16.6%) voluntarily took out ISAPREs. The remainder fraction either belonged to the Armed Forces health system (around 3%) or had no insurance. Those privately insured could, in theory, access public providers, but in practice, this seldom occurs as they would have to pay full fees for services and secure an appointment in a congested system.

Regarding medicines, for those enrolled in FONASA, access to pharmaceuticals is free of charge only for a list of products (*Arsenal Farmacológico y Terapéutico Básico*) approved every year by the Ministry of Health's regional branches. Notably, these lists included a limited set of pre-intercourse generic contraception drugs (e.g., Levonorgestrel and Noretisterone). On the other hand, for those privately insured, access to medicines involves direct payments with different levels of co-payment depending on the plan. Thus, modern contraceptive methods were not available free of charge for all women through public hospitals or local health centers

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

during the relevant period. Those wanting to access specific brands and those privately insured could not take advantage of the free option. This explains why high levels of out-of-pocket payments have been a constant characteristic of the Chilean health services.[12]

Now, switching from the private to the public system is possible in theory, but it involves a significant amount of time and bureaucracy.[13] The decision should also consider the impact of accessing the statutory social health insurance, characterized by the lack of efficiency among the primary health care centers.[14] Thus, it is not surprising that transitions from the private to the public system are relatively rare, even in the context of skyrocketing prescription drug prices. Using longitudinal survey information for the years 2007 and 2008, we find that less than 15% of women aged 15 to 35 transition from the private system to the public one between the two years (Nov. to Nov.).[15] Moreover, those switching in the opposite direction more than compensated the overall number of these transfers.

The lack of perfect substitutability between the Chilean public and private health systems is further confirmed by the number of women enrolled in FONASA's "Control of Fertility Plan," the national public program established to provide medical advice and access to contraceptive methods. Inspection of the monthly number of women in the program in 2007 and 2008 shows no spike in the program's enrollment during this perod.[16]

4. Empirical strategy

The nature of the treatment examined and the outcomes of interest (e.g., births) entail dynamic considerations. Regarding the former, dynamics come from the fact that conceptions in Chile (and elsewhere) have a secular trend and a marked seasonality. The declining oral contraceptive prices observed throughout 2007 exacerbate this concern (see Fig. 1). Regarding the latter, the medical evidence shows that the probability of conception increases with the time elapsed after the suspension of contraceptives' intake because the contraceptive medication progressively wears off, and the menstrual cycle is gradually regulated (Gnoth et al., 2002).[17] To account for these dynamic elements, we implement an interrupted time-series (ITS) estimator with two structural breaks in the time dimension: the year-long price war and the sudden price increase that followed. In particular, we consider the following model:

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

where $d_{PW} = 1\left[t_1 < t \le t_2\right]$ is a dummy variable for the *price war* period $(t_1 < t \le t_2)$, $d_C = 1\left[t > t_2\right]$ is a dummy variable for the collusion period $(t > t_2)$, $f_{PW}(t) = f\left(\beta_{PW}, t_1 < t \le t_2\right)$ and $f_C(t) = f\left(\beta_C, t > t_2\right)$ are flexible polynomial for the price war and collusion periods, respectively. Lastly, t is a linear trend, and $\sum_{w=1}^{51} \omega_w \times S_w$ accounts for week-of-the-year fixed-effects.[18] In order to allow for flexible dynamic responses of the outcome to the price change, we favor specifications with different parameterizations for $f_{PW}(t)$ and $f_C(t)$ at each side of the discontinuity. The controls for trends and seasonality avoid the confoundedness caused by dynamics in conceptions. Moreover, we highlight the dynamic consequences of the price increase by presenting the estimated impacts at different moments throughout each treatment period. That is, we present the deviation relative to a scenario without the price shocks.

The implementation of the ITS estimator relies on one key assumption: that—barred from structural changes—past realizations are the best predictors of future realizations (Bloom, 2003; Cauley and Iksoon, 1988). Thus, our main identifying argument requires that in the absence of the structural change of interest, the realizations after the break would have been equivalent in expectation to the realizations that would come from extrapolation based on the information available before the break. Fig. 2 shows that conceptions in

[12] For instance, back in 2006, out-of-pocket pharmaceutical spending represented 57.2% (39%) of total health expenditure among households in the first (fifth) quintile of the income distribution (FONASA, 2007). In perspective, Chile has the highest share of out-of-pocket family expenditures on medical care out of all OECD member nations (OECD, 2013), with drugs being the largest component of that spending.

[13] First, the individual must cancel her private insurance policy. This is possible only after a year of contractual benefits. Alternatively, if unemployment triggers the decision, the individual must submit a letter signed by the employer confirming the event to the ISAPRE. Then, to enroll in FONASA, the individual must present a copy of the ID, a certificate of access to social programs indicating the number of dependents, the ISAPRE letter confirming the cancelation of the policy, and a signed form to apply to the health card. If employed, she must also present the latest paystub, her employment contract, and the form to apply to the health card signed by her and the employer.

[14] Using data from 2006 to 2008, Ramirez-Valdivia et al., 2015 report efficiency levels in the range of 54 to 71% in this sector relative to frontiers of best practice.

[15] There are no official statistics documenting such transitions. However, we use data from Panel CASEN 2007–2008. The objective of the CASEN longitudinal study was to provide information from a representative sample of individuals followed over time on different socioeconomic dimensions (the sample size is 13,686 individuals). See more information at http://observatorio.ministeriodesarrollosocial.gob.cl/encuesta-panel-casen-2007.

[16] The Ministry of Health reports this information on a monthly basis in https://deis.minsal.cl/#estadisticas.

[17] In principle, the sharp exogenous increase in prices of birth control pills triggered by the collusion could offer the opportunity to estimate the short-run price elasticity of contraceptives and the causal effect of the Pill's availability on fertility and birth-related outcomes using an RDD. This method, however, would identify the average causal effect of the treatment only *at* the discontinuity point (Card et al., 2017; Lee and Lemieux, 2010). In our setting, the exogenous price increase does not secure the standard required strict exogeneity assumption on the conditioning variable. Hence, the dynamic elements of outcome and treatment variables invalidates the use of conventional discontinuity methods.

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

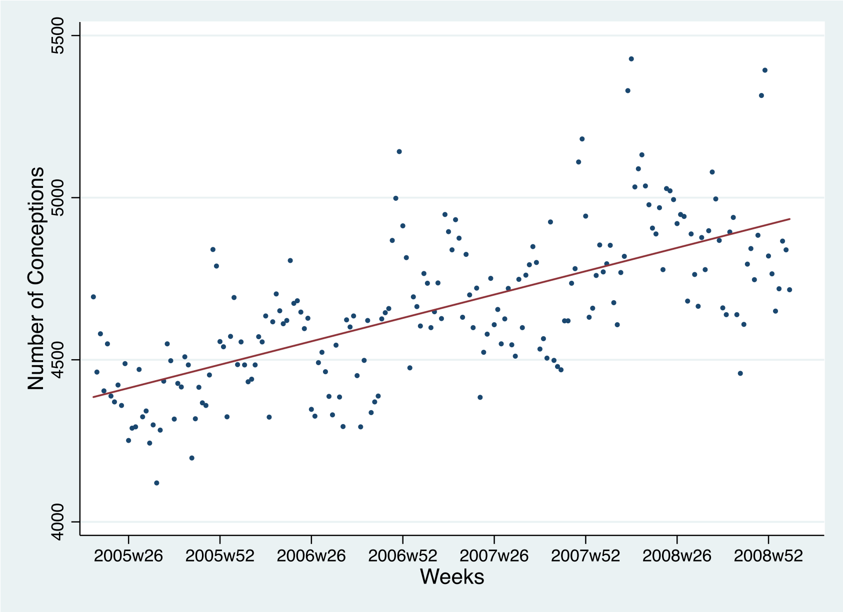

Fig. 2. Total number of conceptions per week: 2005 to 2008. *Notes:* The scatter plot shows Chile's weekly conceptions calculated from birth records collected by the Health Information and Statistics Department (DEIS). We calculate the conception week by subtracting the reported gestation length (in weeks) from the birth date. The solid line represents the fitted values, a linear trend in weekly conceptions in Chile.

Chile follow a *predictable* pattern.[19] The pre-break realizations contain substantial information about possible post-break realizations, if the break had not occurred. In fact, regressing weekly births on a time trend and week-of-the-year fixed-effects yields an R-squared of 0.84. Thus, this suggests the identifying assumption most likely holds in our context.

Threats to identification In the context of our ITS approach, we must assess at least two threats to identification. First, the existence of other structural changes contemporaneous to the one we analyze (Baicker and Svoronos, 2019). Since the ITS estimate captures the gap emerging at a particular moment in time, it cannot disentangle the effect coming from the structural change of interest from other contemporaneous interventions. The second threat to identification is the possible anticipation to the structural change. It implies that the pre-break information used to fit the model in the absence of the break could be deceptive.

Our setting helps to thwart these threats. First, we do not find confounding factors, such as economic changes or other health shocks, that could alter fertility decisions. The unemployment rate is an indicator of economic conditions measured with enough frequency to capture possible changes in economic conditions within our time frame. If Chile had gone through an economic shock, we would notice jumps in the unemployment rate. In Web Appendix I, we show that the female unemployment rate in the country remained stable at or around 6.5% during 2007 and 2008. Furthermore, we inquire about possible environmental or behavioral shifts that could affect the health of Chileans of reproductive age by analyzing detailed administrative records on hospitalizations. In the same Web Appendix, we plot weekly hospitalizations of people aged 15–45 and confirm there were no structural breaks when pharmacies were changing contraceptives' prices, nor can we see breaks indicating any sudden shifts in female reproductive health that could affect fertility choices. All in all, we do not find any indicative source of structural breaks other than the ad-hoc changes of contraceptive prices.

Second, as documented in the court case and described in Section 2, the price shocks came from private and illegal (dis)agreements between pharmacy executives. As such, they were impossible to be anticipated by consumers and unlikely to be accompanied by other contemporaneous shocks affecting conceptions. Nonetheless, candidates of such shocks might include policies altering the availability of contraception or fertility choices; for instance, a reduction in the quantities of contraceptives allocated through the public health system. However, our analysis of government procurement records establishes that this phenomenon did not happen (see Fig. A.1 in Appendix A). With respect to changes in legislation, throughout the period of interest abortion remained illegal, and access to emergency contraception (EC) only changed significantly by mid-2008. Bentancor and Clarke (2017) document that such

[19] They have a clear trend and a marked seasonality. As the fertile-aged population grows, so does the number of conceptions per week. This growth causes a positive trend of about three additional conceptions each week. In addition, fertility patterns also vary seasonally. Conceptions peak during the last three weeks of the year, where summer vacations and end-of-year holidays coincide.

change in EC availability had no effects on avoiding conceptions (we provide further detail on this argument in Section 6.2.2). This result further strengthens our identification strategy.[20]

5. Data

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

Births, mothers' characteristics, fetal and infant deaths The primary source of information for our empirical analysis is Chile's Health Information and Statistics Department (*Departamento de Estadísticas e Información de Salud*, DEIS). The DEIS records information on the date of birth, weight at birth, place of birth, gestation length (in weeks), and characteristics of the mother of every newborn in the country. We carry out our main empirical analysis with data on all births during 2005–2008. The availability of gestation length allows us to calculate the conception date by subtracting the gestation weeks from the week of birth.

In addition to data on live births, the DEIS collects information on all deaths, including those of unborn and newborn children.[21] By studying these cases, we include in our analysis pregnancies that end up in fetal deaths and inquire about the impact of birth control pill price changes on infant mortality (i.e., children under the age of one).[22] Importantly, the census of infant deaths records the dates of birth and death as well as the medical reason for death classified using the International Statistical Classification of Diseases and Related Health Problems (ICD-10) of the World Health Organization.

Chile's official statistics report 739,390 live births in the country between 2007 and 2009, 90.6% of which took place in urban areas and 40.54% took place in the capital city.[23] Our data also indicate that there were 6,582 miscarriages and stillbirths during that period. 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. There was an average of 4,626 births per week, most of them out of wedlock (2,921), 45% are their mother's first child, and about 16% are from teenage mothers. The infant mortality rate is approximately eight deaths per thousand live births. The leading causes of infant deaths are congenital malformations and complications within the perinatal period, accounting for 35% and 44.8% of deaths, respectively.

Contraceptives: consumption and prices We supplement the information on births and fetal and infant deaths with data from Chile's Bureau of Competition (TDLC). This agency examined the evidence of collusion by the pharmaceutical companies, gathering detailed information on around 40 million transactions involving more than 220 medicines from 2006 to 2008. From these data, we can 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. For them, we also have information on the number and location of stores over time.

The TDLC data enable a precise characterization of the market of contraceptives in Chile and its dynamics. Retailing contraceptives in Chile is a sizable business. According the TDLC data, 10,773,126 out of the 39,476,571 transactions (i.e., 27.3%) included contraceptives. In 97% of them, customers purchased the contraceptives and nothing else. On average, 10,542 units of contraceptives were sold each day between 2006 and 2008.[24] Revenues from sales of contraceptives at FASA, CV and SB were US$35.7 million in 2006, US$34.7 million in 2007, and US$47.6 million in 2008.[25]

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[20] The identification assumptions of the ITS are in place to overcome the lack of an explicit control group. We further test the conditions for proper identification of the ITS estimator. Section 6.5 presents these results, providing additional evidence on the fulfillment of the identification assumptions required for the estimator to work.

[21] 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 breathe or show any sign of life such as heartbeat, umbilical cord pulsing, or effective movement of voluntary muscles.

[22] 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 can identify the "product of the conception," regardless of the length of the pregnancy. If 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).

[23] We supplement the birth and death records with information on local characteristics (e.g., income and poverty levels) obtained from different waves of the Socioeconomic Characterization surveys.

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

[25] Data on the prevalence of different contraceptive methods in Chile is scant and dispersed. Nonetheless, using different sources of information one can paint a picture of Chileans' contraceptive use. In 2005, 40.1% (2,379,000) of nonsterilized women above the age of 15 declared that they used a modern contraceptive method (CEPAL and INE, 2005; Ministerio de Salud, 2007). Of them, 869,000 (36.5%) took contraceptive pills. On the other hand, official statistics for the same year indicate that 46.6% (or 1,100,000 women) of the population that used any kind of modern family planning got their contraceptives from private vendors (Ministerio de Salud, 2006). In addition, our data show that during 2007 around 340,000 women purchased oral contraceptives each month from pharmacies. Therefore, 31% of the women who accessed modern methods of contraception through private retailers ended up buying the Pill. This figure—together with the fact that 38% of publicly insured women who use modern contraceptive methods take the Pill (FLACSO-Chile et al., 2008)—matches up the overall prevalence of 36.5% among those who use modern methods of contraception.

Table 1
Weekly live births and deaths in 2007.

	Total number		Mortality rate
	Average	Std. Err	
Live Births			
Total	4626.32	32.14	
Out of Wedlock	2921.29	19.91	
Low Birth Weight	161.92	2.09	
1st Child	2121.83	15.55	
By Mother's Age			
Teen Mom	743.27	6.93	
20–24	1083.36	5.42	
25–29	1084.88	5.93	
30–35	1116.18	4.96	
>35	579.47	2.88	
By Mother's Education			
College	1088.07	137.10	
High School	2709.88	189.07	
< High School	785.52	90.75	
Deaths			
Fetal	42.03	0.71	
Infant	36.6	1.04	7.971
Infant Deaths by Diagnosis			
Perinatal	16.17	0.57	3.495
Extreme Immaturity	4.02	0.29	0.869
Intracraneal Nontrauma. Hemorr.	0.83	0.11	0.179
Cardiac Malformations	0.93	1.05	0.200
Entorocolitis	1.451	1.08	0.314
Malformations	12.92	0.60	2.793
Cardiac Malformations	0.75	0.15	0.162

Note: Total number of live births in 2007: 240,569. Total number of fetal deaths in 2007: 2,165. Total number of infant deaths in 2007: 1,966. *Fetal Deaths* comprise miscarriages and stillborns, while *Infant* stands for children who were born alive but died before turning one year old. Weekly infant mortality rates should be interpreted in terms of 1,000 live births. They were calculated based on the 4,626.32 average weekly live births that took place in 2007. *Intracraneal Nontrauma. Hemorr.* stands for deaths due to intracraneal nontraumatic hemorrhages.

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

School outcomes between 2013 and 2016 Our final source of information comes from public records on school attendance for the years 2013 and 2016. The Ministry of Education of Chile reports individual-level school attendance on a monthly basis for all students attending schools receiving public funding.[26] These records contain students' exact date of birth for the years 2013 and 2014 and month/year of birth for the years 2015 and 2016, grade attended, type of program including those for students with disabilities, school location, and days of school attendance per month. In March of 2013 and 2014 (beginning of the academic year), the files contain data for 3,246,945 and 3,331,326 students, respectively.

6. Main results

6.1. Prices and quantities

We begin by analyzing the responses of contraceptive purchases to the price changes. Fig. 3 suggests that consumers are sensitive to changes in the prices of oral contraceptives. In 2006, when prices were relatively stable—with a very small increasing trend—consumption of contraceptives stayed flat. If anything, they show a nonstatistically different from zero upward trend of 50 extra boxes per week. The 2007 price war ended that stability. As it evolved, lowering prices week after week, consumption of contraceptives raised steadily. The unexpected price increase of January 2008 caused a sizable reduction in the number of contraceptives

[26] 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.

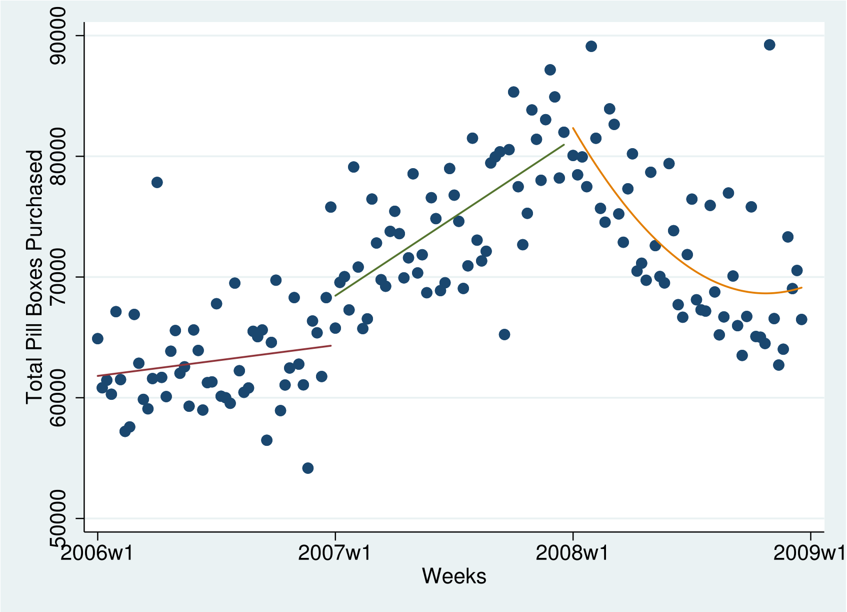

Fig. 3. Weekly contraceptives purchases (units): January 2006–January 2009. *Note:* The scatter plot shows the aggregate raw data on weekly purchases of oral contraception. A unit, in this case, is a box of pills that usually lasts for a menstrual cycle. We aggregate across pharmacies and brands. Lines represent fitted values.

purchased per week.[27] In fact, Fig. 3 suggests that within the first four months, consumption levels were back to those that existed before the price war.

Fig. 4 confirms and quantifies these associations. It depicts the estimated effects (and standard errors) obtained from a regression model in the spirit of Eq. (1) when the outcome variable is the total number of units (boxes) of oral contraceptives sold per week. It shows that by the end of the 2007 price war, pharmacies sold between 18,000 and 21,000 extra units. These represent a relative increase of 28.6% to 33% in contraceptives' sales relative to 2006. However, the price increase of January 2008 was so large and sudden that by the 20th week after the price shock, pharmacies were selling the same number of units they would have sold if the price war had not happened. By mid-2008, weekly sales were 5000 units (8%) below the pre–price war levels. By the end of 2008, weekly sales fell by 11,000 units relative to the amount that would have been sold in the absence of price shocks. That means that by then, the collusion-induced price increase reduced weekly sales of contraceptives by about 30,000 units relative to the price war effect's peak.

We can use the point estimates to construct the demand elasticity. Considering an average price increase of 45 percent during the first weeks after the shock, a Wald estimate of the contraceptives' price elasticity ranges between −0.11 and −0.16. It is thus in the upper end of those found in the literature (between 0 and −0.15). See Appendix B for details.

6.1.1. Interpretation under possible substitutes

We take advantage of the data and revisit the issue of contraceptive consumption and substitutability, which might affect the interpretation of our previous results.

Chileans have several alternatives for oral contraceptives; in fact, pharmacies report selling 24 different brands of contraceptives. Our data show that the choices of contraceptives were remarkably stable over time, even after the spike in prices of January of 2008. Such stability in the market share of each brand was partly due to a general increase across all contraceptive brands. Therefore, as all oral contraceptives became more expensive the scope of the substitutability that might have taken place among them hampered. However, we cannot rule out that some degree of substitution may have occurred. As discussed below, in that case , the substitution would bias our econometric results towards not finding an effect. In that respect, our estimates could be interpreted as lower bounds.

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 was low (only 5.5% according to Ministerio de Salud (2007)). Furthermore, the literature recognizes that very little is known about condom access and fertility, partly because condom availability and use are

[27] It is important to note that the increase of the contraceptives' prices was large, sudden, and unanticipated. Consequently, there are no reasons to believe that people could strategically be stockpiling contraceptives in anticipation of 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 Web Appendix II.1 shows that if there was any stockpiling, it was not as a strategic response to prices but due to pharmacy availability.

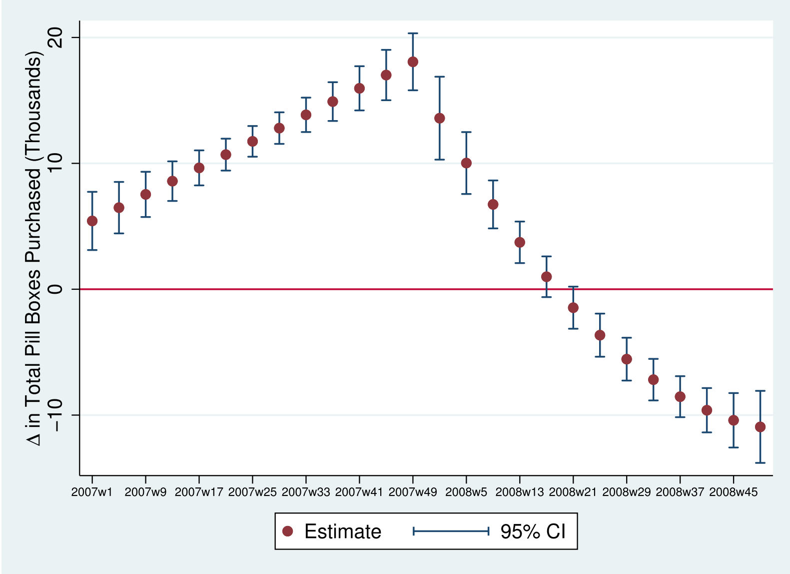

Fig. 4. The effect of the birth control pills' price fluctuations on weekly units of birth control pills sold. *Note:* This figure plots the effects of price changes on boxes of contraceptive pills sold as estimated by Eq. (1), where the dependent variable is the units of contraceptives sold in thousands of boxes per week (i.e., $\tau_{PW} + f_{PW}(t)$ for the price war period and $\tau_C + f_C(t)$ for the post-collusion period). Point estimates and associated standard errors come from Table C.1.

very difficult to observe (Buckles and Hungerman, 2018). Nonetheless, to address this issue, we exploit two sources of information: first, data from one of the pharmacies containing daily condom prices and quantities sold; second, government procurement data on 588 purchase orders for the 2007–2008 period, including condoms for distribution in public hospitals, public health facilities, and municipalities.

The evidence presented in Appendix A shows that pharmacies did not change their condom pricing policy—that entailed raising prices at the beginning of summer and keeping them at that level until next summer—or sell more units after the contraceptives price increase. In fact, Fig. A.2 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. On the other hand, Fig. A.3 shows that public procurement of condoms did not change after the price of contraceptives increased, indicating that there was no reaction from public providers to the contraceptives price shock.

We also find that the quantities of contraceptive pills provided by the public health system did not change when private pharmacies were changing the prices for oral contraceptives. Fig. A.1 shows that there was no increased procurement of contraceptive pills by the public health system. This suggests that not many consumers substituted their purchases of contraceptives in private pharmacies with the publicly provided Pills. In any case, if such substitution had occurred, it would deem our estimates as a lower bound of the true effect.

All in all, 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. With this in mind, we move on to our main results.

6.2. Live births

Fig. 5a presents the weekly estimated impacts on live births of the price war or the collusion (at four-week intervals). Formally, it depicts the point estimates from Eq. (1) when the outcome variable is the total number of live births per week of conception. The slow and steady decline in prices during 2007 and consequent increases in oral contraceptives consumption translated to a small negative slope in the number of live births. This contrasts with the significant increases in births caused by the sudden price increase of January 2008.[28] Just 10 weeks after the price shock, we document an increase of about 54 births per (conception) week; 5 weeks later, there are on average 106 more births. As expected, and in line with medical evidence, the risk of conception increases with time as the effect of past contraceptive medication wears off, and the natural menstrual cycle is progressively restored by gradually extending the length of the luteal phase, which improves the chances of a successful pregnancy (Gnoth et al., 2002). The effect on total conceptions peaks during mid-year at around 146 extra births per week, which represents 3.2% of the births that take place in the average week and yields a price elasticity of 0.066. These findings illustrate the extent to which the massive price increases

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

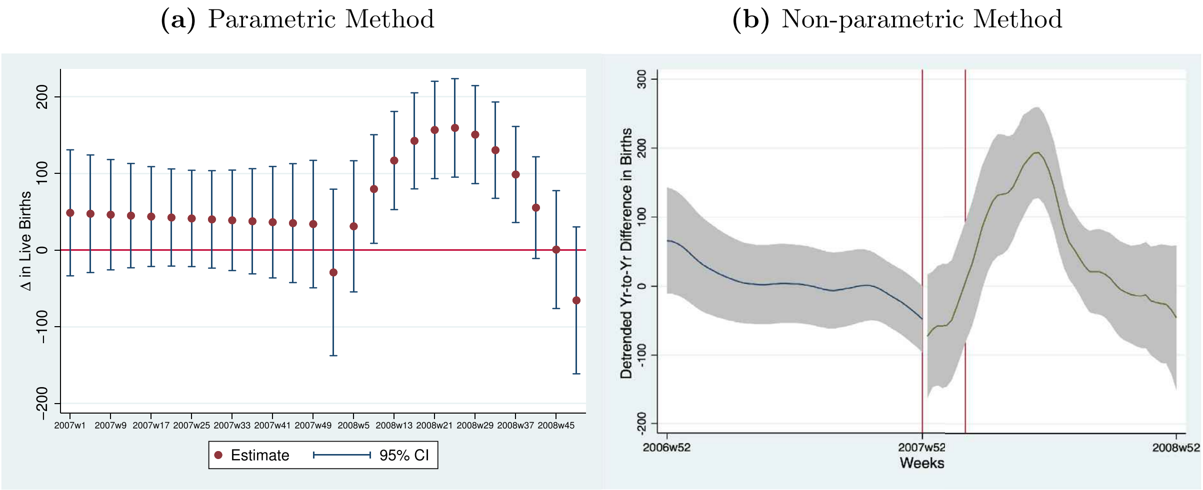

Fig. 5. The effect of the birth control pills' price fluctuations on weekly live births by conception week. *Note:* Panel (a) presents the effects of oral contraceptives' price changes on weekly live births as estimated by Eq. (1) (i.e., $\tau_{PW} + f_{PW}(t)$ for the price war period and $\tau_C + f_C(t)$ for the post-collusion period). Point estimates and associated standard errors come from Table C.1. Panel (b): Births series are first de-trended (linear trend) and de-seasonalized by standard methods (dummies per week of year). Then, year-to-year differences in weekly births are computed. Right-most vertical line indicates the eighth week after the initial price increase. The conception week is obtained by subtracting the pregnancy length to the birth date.

caused a significant change in births. Table C.1 in Appendix C presents the estimated effects and associated standard errors reported in Figs. 4 and 5a.

Our parametric results can be biased if the functional forms assumed are misspecified. To assess this, we also estimate a non-parametric model whose results we present in Fig. 5b.[29] They corroborate our findings in Fig. 5a. First, they show a negative slope in the year-to-year change in conceptions during 2007, reaching a significant decrease of about 48 births by the end of the year when contraceptives prices were the lowest. This is then followed by an increasing effect that peaks during mid-year, reaching a year-to-year increase of 195 conceptions per week, and then returns to pre–price war levels.[30]

The asymmetry in the fertility responses to changes in the price of oral contraceptives is remarkable, and several intertwined reasons could explain it. First, the price changes were not symmetric. As previously discussed, while it took *a whole year* of price declines to increase oral contraceptives' sales by at least 18,000 boxes, it took only *15 weeks* of collusion to bring down the consumption of oral contraceptives by about 15,500 boxes (the difference between 18,180 and 2700—the effect by week 52 of 2007 and by week 15 in 2008, respectively—in Panel A of Table C.1). Such distinct fluctuations should prevent us from expecting symmetric responses at each side of the first week of 2008. Second, existing literature shows it is common for agents in different markets to react differently to price increases than to price decreases (Adeyemi and Hunt, 2014; Bidwell et al., 1995; 1995; Gately and Huntington, 2002; Gerlach et al., 2006; Moosa et al., 2003; Vespignani, 2012). This may be due to psychological features like loss aversion and the role of reference points in determining utility (Tversky and Kahneman, 1991) or the availability of information (Abaluck and Adams, 2017). Current consumers of the good are aware that the price goes up, and they adjust their consumption immediately. Instead, for price decreases to yield an effect on consumption, those who are not the usual consumers of the good need to become aware of the price change, which could take more time.

A third reason comes from the differences in size and speed of the price shocks in 2007 and 2008. They could have led to distinctive effects on consumers with different characteristics. For example, the interaction between price fluctuations and the assessment of the risks across alternatives could have played a role on the intrinsic differences between those selected into the pool of consumers during the price war and those who stopped consuming the Pill during the collusion, and, consequently, triggered asymmetric responses.

[29] 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 flexibility for exploring the treatment dynamics (Fan and Gijbels, 2000). More specifically, we solve:

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

where, K is the kernel function, h is the bandwidth chosen following Calonico et al. (2014) and $\nabla v_t = v_t - v_{t-52}$ is the de-trended and de-seasonalized transformation of the outcome variable, where v_t comes from regressing conceptions at time t, Y_{it}, on a time trend (i.e., $Y_t = \alpha + \gamma t + v_t$). 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 effect at time t is given by $\hat{\tau}_t^{BA} = \hat{\alpha}_{s^*}^r - \hat{\alpha}_t^l$

[30] For results from different parametric specifications, see Web Appendix III. For more non-parametric results, please refer to the Web Appendix IV. The complete set of non-parametric estimates for all the subsamples are available upon request from the authors.

One could consider that—keeping income constant—those women brought into the consumption of the Pill by the slow and steady price declines in 2007 might possess relatively high levels of risk aversion (as they want to avoid a pregnancy at that time). In fact, they might have already been using an alternative method. They might contrast with the marginal consumer shocked out of the consumption of the Pill in 2008. As she faced higher prices, she could have taken the risk and substituted the Pill with other, less successful methods. This should be particularly common among those with relatively *less* risk aversion. Unfortunately, we do not have complete data with all the different methods of contraception or preferences. However, although speculative, this reasoning would explain the small effects on births in 2007 and the large effects in a narrow time window in 2008.

6.2.1. Effects by mother's characteristics

Table 2 presents the estimation results of Eq. (1) across different mother's characteristics. Panel A shows a significant increase in the number of weekly out-of-wedlock births. Fifteen weeks after the 2008 price increase, we find 85.5 extra conceptions from unmarried mothers. This number goes up to around 120 by mid-year. This increase represents about 4.2% of the out-of-wedlock births that take place within the average week. From Panels B and C, we conclude that if we split the total effect on fertility by mother's age, we detect the largest effects among women in their early twenties (20–24). During the peak of the effect, we register 69.45 extra weekly births from mothers in this age group. That represents a 6.5% increase, twice the size of the overall effect on fertility and the effect we find on births from women in the 25–29 age bracket.

Our results also show that the price war drew nulliparae women into the consumption of the Pill. Panel D reports up to 69 fewer weekly conceptions from women without children by the end of 2007, when the price was the lowest. That represents a 3.4% decrease relative to 2006 levels. However, the trend reversed after the January 2008 price increase, and the reductions in first-child conceptions achieved during the price war reversed fast. By week 22 after the price increase, there were 32.7 more first-child conceptions, and by week 29, the effect reached 37.5 additional first-child conceptions relative to the pre–price war period.

Interestingly, we find no effect among teenage mothers (Panel E) or households living in the poorest municipalities (Panel F). Therefore, the results presented in Table 2 confirm our hypothesis that the observed changes in the number of births are the result of a behavioral response to the rise in prices. Poor households not only have a significantly lower rate of Pill use (Ministerio de Salud, 2007), but are also more likely to get contraceptives through the public health system. Use of the Pill is also very low among teenagers. While 19.8% of all women of fertile age use oral contraceptives, only 6.6% of teenagers do.[31]

In addition, we find that the sudden price increase in January 2008 had an effect on fertility among the relatively less-educated women (panel G). While we see no significant effects among college-educated women (G.1), fertility among women with high school degrees (G.2) and among high school dropout (G.2) women increased. We find 64.2 "extra" weekly births (representing a relative increase of 2.3%) at the peak of the response among women with high school degrees. Among high school dropout women, we find 49 "extra" weekly births (6.2% in relative terms)—the latter amounts to almost twice the relative size of the overall effect we detect.

6.2.2. Can changes in EC availability explain the inverse U-shape patterns?

The effect of the price shock persists through September of 2008, after which we no longer observe extra weekly conceptions. The resulting inverse U-shape association suggests that the increased pregnancy risk was later on counteracted either by behavioral changes that took some time to materialize (e.g., substitution to a different birth control mechanism) or by dynamic selection out of the pool of women that could get pregnant—given that those who conceived were no longer able to do so in later weeks. In this context, a *potential* explanation for the inverse U-shape effects is the partial introduction of emergency contraception (i.e., the morning-after pill), which was very restricted until mid-2008.

Emergency contraception found an unexpected route to availability in Chile in April of 2008 after the Supreme Court and the Constitutional Tribunal deemed its prescription illegal across all nationally run health establishments. However, the ruling allowed its prescription by *locally* run health establishments, subject to the authority of the municipality's mayor (Casas Becerra, 2008).

In the literature, there are two opposing views on how this ruling affected EC distribution across municipalities. Bentancor and Clarke (2017) consider it as an expansion in EC access within municipalities with liberal mayors. Nuevo-Chiquero and Pino (2019), on the other hand, treat it as a contraction in EC availability in municipalities with conservative mayors. According to the former, conservative-run municipalities would remain untreated by the changes in EC availability. According to the latter, liberal municipalities would be the untreated group because it would have remained business as usual for them. Thus, if changes to access to EC were an important part of the story, we would observe dramatically different effects in liberal-ran municipalities (where the distribution of EC was allowed) *vis-a-vis* conservative-run ones (where the distribution of emergency contraception was not allowed).

Fig. 6a and b report the non-parametric estimates of the effect of price changes on weekly births in the municipalities with conservative and liberal mayors, respectively. These figures show that the effect of the contraceptives' price increases in January 2008 followed the same inverse U-shape in both types of municipalities. Consequently, regardless of how emergency contraception availability changed due to the Court's ruling, we do not find evidence that this had effects on the behavioral responses that translated to changes in conceptions during the second half of 2008.[32]

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

[32] Another possible channel that could partially explain our findings is the intake of medications intended for other purposes as off-label EC (a method known as Yuzpe). Levonorgestrel, a drug branded under the label of Anullete, is commonly used for this purpose. Importantly, Anullete was

Table 2
The effect of the birth control pills' price fluctuations on the number of weekly births by week of conception and individual characteristics.

	Week of each year							
	1	8	15	22	29	36	44	52
A. Out of Wedlock								
2007	19.18	16.79	14.39	12.00	9.61	7.21	4.48	1.74
	(31.66)	(28.35)	(25.84)	(24.40)	(24.22)	(25.31)	(27.94)	(31.66)
2008	−44.91	32.43	85.54***	114.42***	119.07***	99.49***	47.45*	−36.25
	(39.05)	(26.96)	(23.02)	(23.23)	(23.38)	(22.90)	(25.95)	(39.05)
B. Mom Age 20–24								
2007	12.71	13.43	14.16	14.88	15.61	16.33	17.16	17.99
	(16.86)	(15.09)	(13.76)	(12.99)	(12.89)	(13.47)	(14.87)	(16.86)
2008	−0.87	30.94**	53.26***	66.10***	69.45***	63.31***	44.68***	13.66
	(20.79)	(14.35)	(12.25)	(12.37)	(12.45)	(12.19)	(13.81)	(20.79)
C. Mom Age 25–29								
2007	19.08	14.71	10.33	5.96	1.59	−2.78	−7.77	−12.77
	(19.14)	(17.13)	(15.62)	(14.75)	(14.63)	(15.30)	(16.88)	(19.14)
2008	−49.50**	−10.45	16.35	30.90**	33.21**	23.26*	−3.11	−45.48*
	(23.60)	(16.29)	(13.91)	(14.04)	(14.13)	(13.84)	(15.68)	(23.60)
D. First Child								
2007	5.94	−4.37	−14.67	−24.97	−35.28*	−45.58**	−57.35**	−69.13***
	(25.29)	(22.64)	(20.64)	(19.49)	(19.34)	(20.21)	(22.31)	(25.29)
2008	−92.68***	−32.37	9.44	32.73*	37.51**	23.79	−14.57	−77.10**
	(31.19)	(21.53)	(18.38)	(18.55)	(18.67)	(18.28)	(20.72)	(31.19)
E. Poor 10%								
2007	−2.30	−2.65	−3.00	−3.35	−3.70	−4.05	−4.45	−4.85
	(3.09)	(2.77)	(2.52)	(2.39)	(2.37)	(2.47)	(2.73)	(3.09)
2008	0.84	1.75	2.17	2.11	1.56	0.52	−1.26	−3.68
	(3.82)	(2.64)	(2.25)	(2.27)	(2.29)	(2.24)	(2.54)	(3.82)
F. Teen Mom								
2007	−3.21	−3.50	−3.79	−4.08	−4.37	−4.66	−5.00	−5.33
	(12.71)	(11.38)	(10.37)	(9.79)	(9.72)	(10.16)	(11.21)	(12.71)
2008	−28.14*	−11.43	−1.87	0.53	−4.23	−16.14*	−38.51***	−70.23***
	(15.67)	(10.82)	(9.24)	(9.32)	(9.38)	(9.19)	(10.41)	(15.67)
G. Total Births by Mother's Education								
G.1 College								
2007	5.26	7.00	8.74	10.48	12.22	13.96	15.94	17.93
	(17.63)	(15.78)	(14.39)	(13.59)	(13.48)	(14.09)	(15.55)	(17.63)
2008	−11.41	7.20	18.76	23.28*	20.74	11.15	−8.44	−37.23*
	(21.74)	(15.01)	(12.81)	(12.94)	(13.02)	(12.75)	(14.45)	(21.74)
G.2 High School								
2007	15.39	10.29	5.19	0.09	−5.01	−10.12	−15.95	−21.78
	(30.97)	(27.73)	(25.28)	(23.87)	(23.68)	(24.76)	(27.32)	(30.97)
2008	−75.03**	−7.78	37.85*	61.86***	64.24***	45.00**	−3.46	−80.17**
	(38.20)	(26.37)	(22.51)	(22.73)	(22.87)	(22.39)	(25.38)	(38.20)
G.3 Less Than High School								
2007	−3.51	−1.10	1.31	3.72	6.13	8.54	11.29	14.05
	(14.27)	(12.78)	(11.65)	(11.00)	(10.92)	(11.41)	(12.59)	(14.27)
2008	−11.00	19.00	39.01***	49.03***	49.07***	39.12***	15.53	−21.11
	(17.60)	(12.15)	(10.38)	(10.47)	(10.54)	(10.32)	(11.69)	(17.60)

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

6.2.3. *The impact on weight at birth*

Table 3 presents the results from Eq. (1) when the number of underweight births is the outcome. Panel A shows that conceptions that resulted in underweight newborns were falling year-to-year during the period when contraceptive prices were falling as the

part of the price hike and its consumption responded accordingly (see Fig. D.1 in Appendix D). This suggests both that those individuals priced out of their regular contraceptive did not resort to off-label EC in the form of Yuzpe and that our point estimates should capture the overall impact of all price hikes, including Anullete's. See Appendix D for further details.

(a) Municipalities Where Emergency Contraception Was Not Allowed in 2008

(b) Municipalities Where Emergency Contraception Was Allowed in 2008

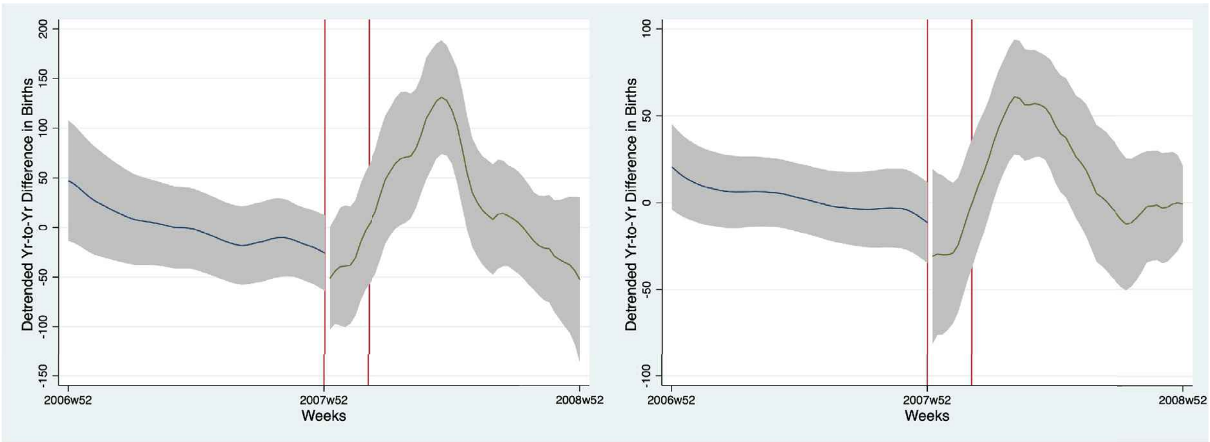

Fig. 6. The effect of the birth control pills' price fluctuations on weekly live births, by conception week (non-parametric specification). *Note:* Births series are first de-trended (linear trend) and de-seasonalized by standard methods (dummies per week of year). Then, year-to-year differences in weekly births are computed. The conception week is obtained by subtracting the pregnancy length to the birth date. Right-most vertical line indicates the eighth week after the initial price increase (left-most line). EC was allowed in municipalities with liberal mayors and not allowed in municipalities with conservative mayors. We take the party of the Mayor from Bentancor and Clarke (2017).

pharmacies engaged in a price war. Then, the price spike that resulted from the pharmacies colluding broke the trend, and weekly underweight births drastically increased (see Figure IV.1 in Web Appendix IV for non-parametric estimates). In particular, it caused a significant increase in the number of underweight births (measured as the proportion of newborns with low birth weight for gestational age, as indicated by Mikolajczyk et al., 2011). At the peak of the effect, there were 15.4 more underweight births per week. This figure represents a 9.5% increase in the weekly average underweight births relative to the pre-treatment period (three times the relative size of the impact from total live births). Despite our empirical difficulties to separate mistimed, unplanned, or unwanted pregnancies from cases in which a woman is unaware she is pregnant during a critical period of prenatal development, such a large effect is in line with medical literature linking unintended pregnancies with the incidence of underweight newborns (Sharma et al., 1994). However, regardless of the circumstances, the result raises concerns as weight at birth represents a good proxy for child endowments (Almond and Currie, 2011a). It is the outcome of genetic background, the extent to which parents were involved in prenatal care, and the mother's previous health and habits (Currie, 2011). As such, it has been shown to be a determinant of cognitive development (Figlio et al., 2014; Torche and Echevarría, 2011), school attainment (Oreopoulos et al., 2008), and even future earnings (Behrman and Rosenzweig, 2004; Black et al., 2007).

The bottom panel (B) of Table 3 shows that underweight babies were born overwhelmingly to high school–educated mothers (B.2). At the peak of the effect, 13.8% of all the "extra" births induced by the price increase in this group were underweight, while in comparison, virtually no underweight babies were born to college-educated women (B.1) and 8.5% of the "extra" births among high school dropouts (B.3) were underweight. These heterogeneous responses across education levels may result from a trade-off between the lower taste for childbearing (i.e., less inclined to adopt healthy behaviors) and wealth effects (i.e., more likely to be willing to pay the costs of adopting healthier behaviors). Our findings are consistent with the idea that college-educated women may comprise a subgroup of the population for which the wealth margin could improve faster the average health quality of the "extra" newborns than the deterioration resulting from the taste-for-children margin (the overall effect depends mainly on the marginal returns to health inputs). Hence, the health of the babies conceived in this subgroup might not deteriorate as it does in others. This further relates to the fact that college-educated women may be more prone to perform remedial investments after cryptic pregnancies are revealed.

In principle, the large increase in the incidence of underweight newborns due to the collusion could be explained by increases in the incidence of unplanned conceptions that receive lower investments as well as changes in the characteristics of women that select into motherhood (relative to higher-educated women, lower-educated women have a higher incidence of underweight births). Appendix E examines this hypothesis. It presents the results of decomposing the overall impact on the number of underweight births across three potential drivers: Selection into motherhood (particularly among low-educated mothers), the mechanical effect of more conceptions (scale effect), and the low investments arising from unexpected conceptions (remainder). We find that the scale effect explains one-third of the additional low-birth-weight newborns, and two-thirds can be linked to lower investments arising from unexpected pregnancies. Thus, based on this exercise, we can conclude that the fraction attributed to selection is negligible.

Table 3
Effect of birth control pills' price fluctuations on the number of weekly underweight births by week of conception and mother's education.

	Week of the year							
	1	8	15	22	29	36	44	52
A. Overall								
2007	−14.37**	−12.14**	−9.91**	−7.68*	−5.45	−3.23	−0.68	1.87
	(5.83)	(5.22)	(4.76)	(4.50)	(4.46)	(4.66)	(5.15)	(5.83)
2008	−8.92	2.99	11.01***	15.15***	15.40***	11.77***	2.86	−11.13
	(7.20)	(4.97)	(4.24)	(4.28)	(4.31)	(4.22)	(4.78)	(7.20)
B. By Mother's Age								
B.1 College								
2007	−3.49	−2.85	−2.21	−1.57	−0.93	−0.28	0.45	1.18
	(2.87)	(2.57)	(2.34)	(2.21)	(2.20)	(2.29)	(2.53)	(2.87)
2008	−1.62	0.01	1.36	2.42	3.19	3.68*	3.88*	3.71
	(3.54)	(2.44)	(2.09)	(2.11)	(2.12)	(2.08)	(2.35)	(3.54)
B.2 High School								
2007	−6.13	−5.26	−4.39	−3.52	−2.65	−1.78	−0.78	0.21
	(4.46)	(3.99)	(3.64)	(3.43)	(3.41)	(3.56)	(3.93)	(4.46)
2008	−4.19	2.38	6.63**	8.54***	8.13**	5.39*	−0.60	−9.62*
	(5.50)	(3.79)	(3.24)	(3.27)	(3.29)	(3.22)	(3.65)	(5.50)
B.3 Less Than High School								
2007	−4.66**	−3.96*	−3.26*	−2.56	−1.87	−1.17	−0.37	0.43
	(2.29)	(2.05)	(1.86)	(1.76)	(1.75)	(1.83)	(2.02)	(2.29)
2008	−3.54	0.33	2.90*	4.18**	4.16**	2.86*	−0.22	−4.98*
	(2.82)	(1.95)	(1.66)	(1.68)	(1.69)	(1.65)	(1.87)	(2.82)

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

6.3. *Fetal deaths*

Just like underweight newborns, miscarriages and stillbirths were consistently falling on a year-to-year basis during the price war period (2007).[33] Table 4 shows that, on average, there were 6.7 fewer fetal deaths per week, which represents a 15.6% decrease relative to 2006. However, when pharmacies colluded and increased contraceptive prices, fetal deaths increased significantly (Panel A). Weekly conceptions that resulted in miscarriages and stillbirths increased by around 5.3 during their peak in mid-year relative to pre-treatment levels. Such an increase represents a 12.3% growth in the average weekly fetal deaths, around four times the relative effect the price shock had on live births. Panel B shows that the effect is largest among 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 (C) might not be able to afford contraceptives in the first place and women in high-income municipalities (D) can afford contraceptives even after the price increase, middle-class women find the price increase binding, ergo the pregnancies. This disproportionate effect on fetal deaths relative to live births may be the product of either a biological response to going off the Pill, or a behavioral response to an unintended pregnancy. Figure IV.2 in Web Appendix IV presents the results from the non-parametric model, which confirms these findings.

Regarding the biological response, medical literature shows that women who take contraceptives for a very long time face an increased probability of miscarriages when they become pregnant (García-Enguídanos et al., 2005). However, we do not find differential effects of the price shock across age groups of nulliparous women. If we assume that older nulliparous women are more likely to have longer exposure to hormonal contraceptives than younger nulliparous women, we would expect more fetal losses among older women. Regressions in Table V.1 in Web Appendix V show no evidence of differential effects of the price shock on fetal losses across the two age groups. Furthermore, non-parametric results presented in Figure V.1 show that even though there are effects for first-time pregnant women in all age groups, these effects are not statistically different across age groups. That is, the confidence intervals of the four estimates always overlap with one another.[34]

[33] 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, where 22 and 28 are the most commonly used (Lawn et al., 2011).

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

Table 4
The effect of the birth control pills' price fluctuations on the number of weekly fetal deaths by week of conception and municipality income level (quartile).

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

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

That leaves us with the behavioral response hypothesis. That is, the possibility that these pregnancies were more likely to be unintended, and women were not prepared to invest in the healthy development of the pregnancy, potentially neglecting or interrupting it. This hypothesis is consistent with research showing that granting access to contraceptives can drastically reduce the incidence of abortion. For instance, Peipert et al. (2012) find that providing at-risk women with free access to long-acting reversible contraceptives in St. Louis reduced the abortion rate by half. Due to a lack of information, and given the unlawfulness of abortion in Chile, we are not able to disentangle fetal deaths due to poor health from intentional abortions—reports estimate there are around 70,000 yearly clandestine abortions in Chile (Casas and Vivaldi, 2013). Furthermore, because of how 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 5, these events are recorded only if the physician or the midwife identifies the "product of the conception." Therefore, fetal deaths at early stages of the pregnancy are less likely to be recorded. In fact, when we analyze stillbirths and miscarriages separately, we find that the effect on fetal deaths is due almost exclusively to a year-to-year increase in stillbirths and not in miscarriages. Such findings suggest that the contraceptives' price increase is associated with an increase in the number of unhealthy fetuses.[35] The following section explores whether this holds for live births as well.

6.4. *Infant deaths*

Having shown that the number of underweight newborns fell due to lower contraceptive prices, and then they dramatically increased due to the price shock after the collusion, we now turn to infant mortality (i.e., the number of children that were born alive and died before they completed their first year of life); an even more stringent margin.

Prior to 2007, there were around 2000 infant deaths per year in Chile (representing an infant mortality rate of about 8 per 1000 live births) due to numerous causes. In fact, DEIS data show that physicians list 589 different diagnoses as causes of infant deaths that range from congenital malformations to infections and trauma. However, almost 80% of the reported infant deaths can be classified in two broad categories: congenital malformations and conditions originating in the perinatal period (i.e., immediately before and after birth). We focus on analyzing deaths that are due to conditions related to the mother's health and habits and prenatal care. 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, we focus on babies born extremely small or immature to sustain life, perinatal complications, brain malformations, and malformations likely caused by exposure to harmful environments.

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

Table 5
The effect of the birth control pills' price fluctuations on infant mortality by week of conception (per 1,000 live births).

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

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

Panel A in Table 5 shows that just like fetal deaths and underweight births (i.e., indications of poor fetal health), infant mortality fell as contraceptives became cheaper and their consumption increased—just to bounce back when prices increased, and consumption retreated in 2008. We estimate that infant deaths fell by about 18% due to the increased consumption of contraceptives during the price war. However, when pharmacies started colluding, those gains were lost, and overall infant mortality stopped declining. In fact, we find almost symmetric increases in infant mortality after the first week of 2008, although not statistically significant.

An even clearer picture of the effect of contraceptives' price changes on newborn health appears when we limit the analysis to conditions related to the unpreparedness of the expectant mother, a lack of healthy habits, or exposure to toxic substances while in-utero. Panel B in Table 5 indicates that the effect of contraceptives' price changes on weekly infant mortality due to conditions arising during the perinatal period account for almost the entirety of the effect we find on total infant mortality. We find that they fell on average by 28.7% due to the steady decline in contraceptives' prices in 2007.[36] However, the sudden price increase in 2008 led

[36] The conditions generated in the perinatal period explored in Table 5 include newborns affected by maternal factors and by complications of pregnancy, labor and delivery, disorders related to the 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.

to significant increases in this type of infant deaths. On average, about 7 "extra" infants died from these causes when contraceptives' consumption fell. It represents a large increase in such deaths of about 40% relative to pre-treatment levels.

When we analyze in more detail the effect on the diagnoses that are bundled together as perinatal conditions, we see that contraceptive consumption shifts affect infant mortality mainly by decreasing or increasing the number of extremely premature or immature babies (Panel C), in line with our findings of low birth weight and fetal deaths. In fact, infant deaths due to extreme immaturity of the baby (i.e., babies with low gestational age or born too small and weak to survive) went up by 50% due to the contraceptives' price increase. Such an uptick in the number of extremely immature newborns is also evident in the increased number of deaths due to intracranial nontraumatic hemorrhage (Panel D), a condition highly prevalent in extremely premature babies. We find that deaths from this condition doubled due to contraceptives becoming more expensive in 2008. In the same way, we find that weekly infant deaths due to necrotizing enterocolitis of the newborn (Panel E)—the second most common cause of death among premature infants, a condition closely related to fetal immaturity that typically develops among premature babies, especially those that are formula fed (Panigrahi, 2006)—decreased by about 75% among babies conceived as contraceptives' prices were declining, especially in the second half of 2007. This is further evidence supporting the idea that the contraceptives' price changes and subsequent consumption shifts led to reductions (in 2007) and increases (in 2008) in the number of babies that were conceived in environments lacking the necessary resources for their adequate development.[37]

Panel F shows no significant effects of the price changes on anatomical malformations. However, when we zoom in to deaths related to cardiac malformations in panel G (i.e., malformations of the cardiac chambers, connections and valves), we find that the increased consumption of oral contraceptives in 2007 caused the weekly infant mortality rate due to these conditions to drop by 80% during the second half of 2007.

Overall, the estimated impacts on infant mortality show that oral contraceptives' take-up affects the average health of the babies born by preventing the conception of children who are less likely to have adequate resources for their development. The nature and causes of the conditions we identify as most likely to be influenced by the Pill's take-up, together with our findings on miscarriages, are suggestive of the relation between oral contraceptives' consumption and the number of unintended/unknown and neglected pregnancies (in congruence with the medical literature on the topic) (Bustan and Coker, 1994). Thus, the skyrocketing prices of contraceptives resulted in a significant increase in weekly births and the arrival of less healthy babies. These findings are consistent with the idea that a sudden interruption of accessibility to contraceptives can increase the number of unintended or cryptic pregnancies, preventing mothers' healthy behaviors and impacting the health of the newborns. We further explore this in Section 6.6.

6.5. Falsification and robustness checks

In this section, we provide additional evidence supporting our results by exploring multiple placebo scenarios and test for the robustness of our findings to different samples. For the sake of brevity, we do not report here the tables and figures associated with these exercises. We do so in our Web Appendix II. The main results are as follows:

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

Dropping 2007 conceptions To assess the robustness of our empirical strategy, we reestimated the model but we now drop the conceptions from 2007. The results are presented in panel A of Table II.2 in our Web Appendix, and they follow closely what was originally reported in Tables C.1 (and III.2). Thus, our findings remain robust to this change.

Modifying the date of the structural break We examine a placebo situation in which we set the structural break in 2006. Fig. 1 shows that, unlike 2007 and 2008, during that year prices did not change dramatically. 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 2006. Panel B of Table II.2 presents these results. They confirm the distinctive patterns in live births documented for 2007 and 2008 and support the hypothesis that these are attributed to exogenous variations in birth control pills prices. Thus, there are no differences in the year-to-year growth of weekly conceptions, providing evidence that our results do come from behavioral responses to the price increase in 2008 and not from mechanical features of the estimation procedure. Web Appendix II.2 presents further results from other exercises, all providing evidence in support of our main hypothesis.

Alternative empirical strategy In principle, every woman in the country experienced the exogenous changes in the Pill's prices during 2007 and 2008. However, some demographic groups are more exposed than others to those price changes because the incidence of oral contraceptive consumption differs greatly. Using data from Chile's National Study of Youth of 2006, we find that the consumption of oral contraceptives among women younger than 20 is significantly lower than that of women older than 19. In Web Appendix II.4 we exploit this variation to implement a difference-in-difference (DID) strategy exploring the behavioral responses of two groups

[37] We implement the empirical strategy outlined in Section 4 to a different data source containing information on mothers and births for the relevant period, The Early Childhood Longitudinal Survey (Encuesta Longitudinal de Primera Infancia or ELPI). In line with findings in the public health and medical literature Indicating that women with unintended pregnancies tend to have a harder time quitting unhealthy behaviors even after knowing they are pregnant (Dott et al., 2009), we find that women who conceived after the price hike were twice as likely to drink alcoholic beverages during pregnancy than those who became pregnant before the contraceptives' price shock. Also, they end breastfeeding one month earlier (Jayachandran, 2014), and the babies conceived were five percentage points more likely to have below-median cranial circumferences.

(proxies for control and treatment) before and after the sudden price increase. The results are presented in Table II.3. The estimated impacts are similar to those obtained using the ITS approach (see Table III.2). They confirm the robustness of our findings to different empirical strategies.

6.6. Long-term outcomes

We now investigate the long-term impact of the price hike of 2008. In particular, we examine differences in school enrollment at least five years after the price increase between two groups: students conceived before and after the 40th week of 2008. In the context of our previous findings, differences in favor of the former group could suggest that those conceived right after the price hike effectively faced more deprived early development (Bailey et al., 2019; Black et al., 2007; Currie and Moretti, 2007). To explore this idea, we use publicly available administrative information on school attendance from academic years 2013 to 2016. The empirical strategy follows a simple DID model where we use the cohort of students conceived at the end of 2006 or the beginning of 2007 (before the price war) as the control group. Formally, the first difference arises from comparing the enrollment of children born in the 2008 cohort with children born one year earlier, and the second difference comes from comparing those conceived during the first weeks of the year (while the Pill's prices skyrocketed if conceived in 2008) with those conceived later in the year. We focus on enrollment in kindergarten, first, and second grades.

Some institutional background is needed before presenting our results. In Chile, first grade enrollment is compulsory for children who turned six years of age by March 31st of a given year. Therefore, this group is expected to be attending a school by March of that year (the academic year goes from the first week of March to the beginning of December). Nevertheless, the Ministry of Education, upon parent's request, might authorize children turning six before June 30th to enroll as well.[38] 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.[39]

We first present the results for kindergarten enrollment (academic years 2013 and 2014).[40] Table 6 displays the DID estimates. Column (1) does so for the sample of children in the treatment (control) group born between weeks 34–48 of 2008 (2007) and column (2) for those born between weeks 27–55 of the respective year. The comparison across columns informs whether the results remain robust as we move away from the 40th week. We find negative and statistically significant effects. The price increase reduced kindergarten enrollment five years later in at least −0.84 per each 1,000 births. At first glance, this might seem a small number. However, this is expected as we capture differences emerging from "extra" births resulting from the price hike.[41]

The rest of Table 6 shows that the reduction in kindergarten enrollment was larger among middle-income municipalities (columns (3) and (4)), and municipalities of the Metropolitan Region with a high density of pharmacy locations (columns (5) and (6)). The point estimates are negative and larger than those reported under columns (1) and (2), providing additional insights on the type of families that were less likely to enroll their children in kindergarten.[42] Nonetheless, since kindergarten enrollment was voluntary, underinvestment in the "extra" post price conceptions might not be the only factor behind these findings. Selection into pre-primary education emerging from, for example, potential changes in family size and its impact on child's education (Black et al., 2005) could also explain the results.

To mitigate concerns arising from voluntary enrollment, we repeat the analysis but now for the academic years 2015 and 2016. It allows us to examine mandatory enrollment in first and second grades. Table 7 presents the results. While we estimate a small and nonsignificant negative effect on first-grade enrollment (column (1)), we find that the 2008 price increase reduced second-grade enrollment by 5.70 students per each 1000 births seven years later (column (2)). The differences across grades suggest higher grade retention in second grade among those conceived after the price hike relative to those conceived before the event.[43]

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[38] Requests to enroll younger children are common in Chile. For example, out of the 242,041 children attending first grade in March 2016, 4.88%, 3.53% and, 2.24% were born in April, May and June of 2010, respectively. Only 177 first graders, equivalent to 0.03%, were born after June 30th of 2010.

[39] The children who 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.

[40] 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.

[41] 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 2013 and 2014, we have some uncertainty on their precise conception week. In consequence, we include specifications that exclude individuals born within few weeks of the 40th week of the year. These mimic a donut-hole specification. Moreover, to examine how sensitive our results are to the downward trend in prices observed during 2007, we present results covering two time periods: all births from weeks 34 to 48 of a given year, and all births from week 27 of a year and week 3 of the following year (we label it "week 55"). As we better capture differences between those conceived before and after the effect of collusion if we exclude the uncertainty surrounding week 40 of 2008, we expect the negative effects to magnify in the donut-hole specifications. These findings are presented in Table VI.1 of our Web Appendix VI. Indeed, excluding weeks before and after the 40th week leads to large effects on kindergarten enrollment.

[42] The full set of results for municipalities from the Metropolitan Region are presented in Table VI.2 in Web Appendix VI.

[43] For the academic years 2015 and 2016, only month and year of birth are reported. This prevents us from estimating specifications that exclude children born between specific weeks of a given year. Instead, in our Web Appendix, we report results excluding those children born between

Table 6
The effect of the birth control pills' price increase on school attendance in kindergarten.

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

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

Table 7
The effect of the birth control pills' price increase on school attendance in first and second grade.

	Overall enrollment		Special education	
	1st Grade (1)	2nd Grade (2)	1st Grade (3)	2nd Grade (4)
Diff-in-Diff	−0.002	−5.698***	0.469***	0.834***
	(0.652)	(0.597)	(0.002)	(0.003)
Mean at Baseline[(a)]	879.96	849.41	2.31	4.19
Number of Children	371,916			

Note: *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parentheses. Sample size (number of births) corresponds to two cohorts (treatment and control groups), i.e., the number of births over two years. We combine individual-level administrative information on daily school attendance and birthdates. For columns (1) and (2) 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 1,000), respectively. For columns (3) and (4) the dependent variable is 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 six 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 six 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 in March 2015. The reference group corresponds to those who turned six between July 2013 and January of 2014, so they could attend first grade starting in 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 one 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.

We further use administrative records to inquire about the effects that the 2008 price hike might have had on enrollment in programs dedicated to supporting the educational needs of students with disabilities in different areas. These include hearing impairment, vision impairment, speech-language impairment, physical impairment, autism spectrum disorder, and intellectual disability. However, most children with educational needs report attending a program for students with intellectual disability, so we focus on those programs.[44] As before, we interpret any attendance during the first quarter of the academic year as a proxy for overall enrollment. Columns (3) and (4) of Table 7 report the point estimates. Among first (3) and second (4) graders, the results disclose increases of 0.47 and 0.83 students with special needs per each 1,000 births, respectively. Our findings reveal a positive association between the price increase of 2008 and enrollment in special programs six and seven years later.[45]

Overall, we interpret these findings as an indication that those conceived during the first weeks of 2008 (post price increase) were more likely to face adverse conditions during critical periods of development, which—in line with economic evidence (Bailey et al., 2019; Behrman and Rosenzweig, 2004; Currie and Hyson, 1999; Currie and Moretti, 2007)—resulted in worse early education outcomes.

September and October. Panel A of Table VI.3 displays the results for the whole sample. The point estimates in Panel B confirm the negative impacts for both grades after excluding individuals born between September and October. In this case, the point estimates are −5.33 and −13.15 for first and second grade, respectively.

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

[45] Table VI.4 in the Web Appendix presents the results aggregating all special education programs. The results confirm our findings. Moreover, Table VI.5 shows that the results in columns (3) and (4) of Table 7 increase in magnitude after excluding students born in September or October (panel B).

7. Conclusions

This paper exploits exogenous price variation in birth control pill prices arising from market failures in the pharmaceutical sector in Chile as a natural experiment to study fertility responses and their consequences on newborns' outcomes. We document sizable and asymmetric short-run responses. While our estimates show small fertility reactions to slow and continuous decreases in contraceptives' prices of 2007 (price war), we find large responses to the sharp and unexpected price increase documented during the first weeks of 2008 (collusion case). In fact, we estimate the price hike produced about 146 additional births per week in Chile during the relevant period. Moreover, we document larger fertility responses among unmarried and primiparae women, and as expected, we do not find significant impacts among poor households and teenage mothers. We provide several falsification tests and robustness checks. Our results are robust to different settings.

Unlike overall fertility, when analyzing newborns' health, we find that poor health indicators improved during 2007, but that trend dramatically changed in 2008. Following the sudden oral contraceptives' price increase agreed by the firms, we document a disproportionate increase in the total number of underweight births, miscarriages, and infant deaths. Overall, the evidence points to the deterioration of average newborns' health (birth weight/mortality) among those conceived in the first months of 2008, suggesting this group was disproportionally more likely to face adverse conditions during critical periods of fetal development. Furthermore, this hypothesis is reinforced by our assessment of the impact of the price hikes on long-term outcomes. As "extra" children reached school age, we document inferior school outcomes: lower kindergarten and second-grade enrollment rates and an increase in the number of children requiring special education several years after the event.

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

Appendix A. Procurement of contraceptives by the public health system and condoms as a substitute and

In this Appendix, we present data on possible substitutes for purchasing contraceptive pills in private pharmacies. First, we present prices and quantities of condoms purchased by the public health system for free distribution. Second, we show prices and quantities of condoms sold in pharmacies. Finally, we show the quantities of oral contraceptives procured by the public health system for free distribution.

All the evidence presented in this Appendix shows that there was no reaction (either by the public, pharmacies—regarding condoms—or the public health system) to the price changes in oral contraceptives in private pharmacies.

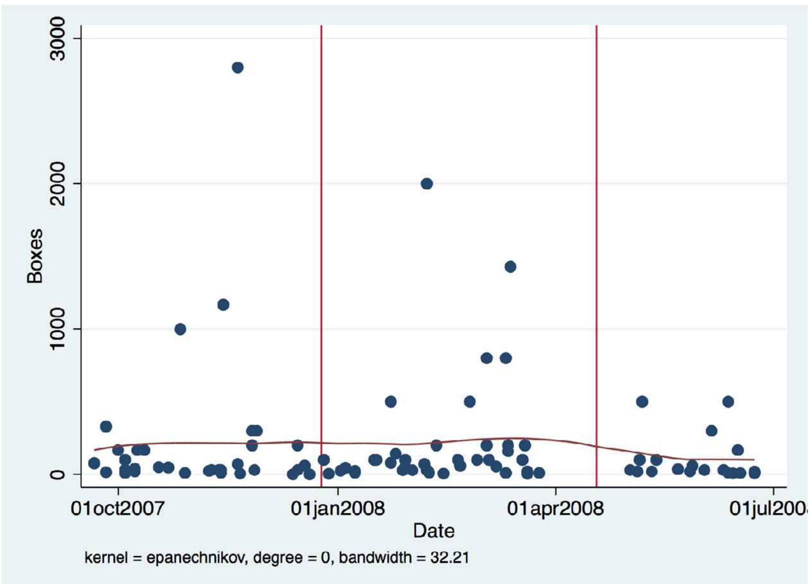

Fig. A.1. Government procurement of contraceptives quantities purchased. *Note:* Quantities obtained from the Chilean government procurement data.

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

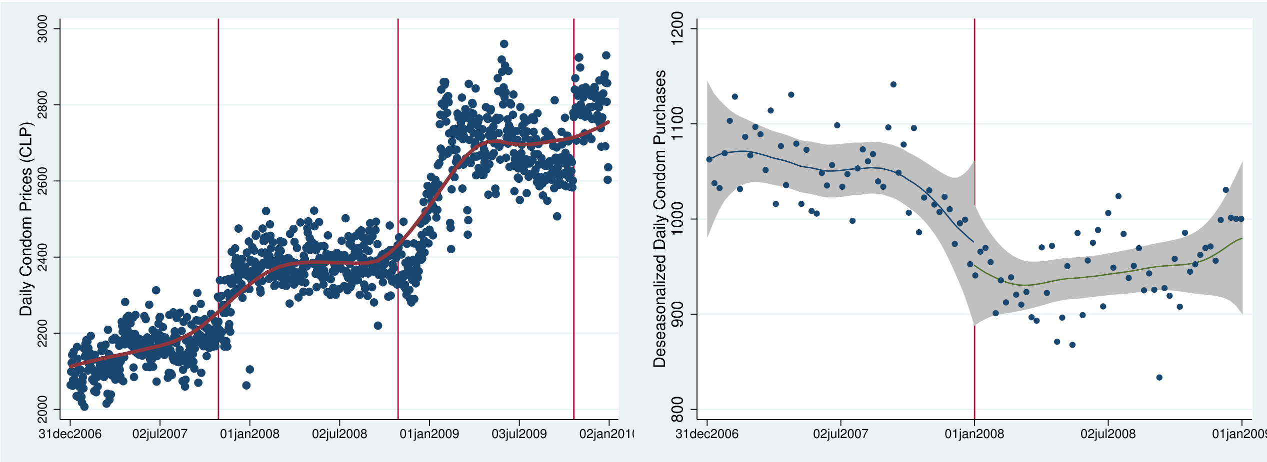

Fig. A.2. Daily prices and quantities sold of condoms in pharmacies. *Note:* Data from Salcobrand. (a) Vertical lines indicate the beginning of Summer. (b) Non-parametric approximations estimated using daily data. Scatter plots weekly averages. The quantities series followed two seasonal patterns. One 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.

(a) Price of Unit of Condoms (b) Quantities Purchased of Units of Condoms

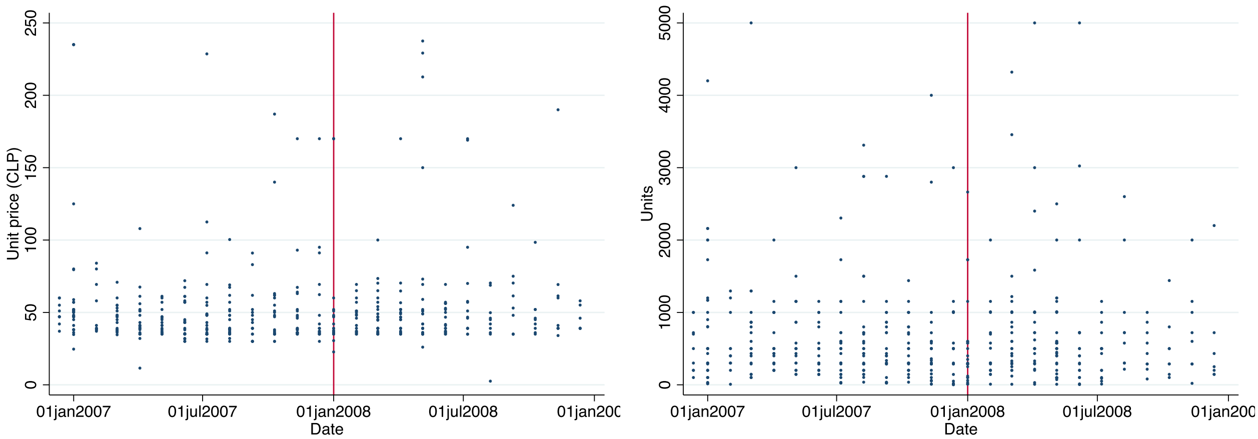

Fig. A.3. Condoms purchases in procurement data. *Note:* Prices obtained from the Chilean government procurement data.

Appendix B. Contraceptive demand elasticities

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

$$\ln(q_t) = \alpha + \beta \ln(p_t) + u_t \tag{2}$$

$$\ln(p_t) = \gamma + \delta 1\left[t > t^*\right] + v_t \tag{3}$$

where p_t and q_t are weekly prices and quantities and $1[t > t^*]$ is dummy variable equal to one after the collusion date t^*. The parameter of interest is β which corresponds to the contraceptive price elasticity. We run four different specifications presented in Table B.1. In

Table B.1
Price elasticity.

	(1) OLS	(2) OLS	(3) IV	(4) IV
β	−0.185***	−0.265***	−0.106**	−0.155**
	(0.037)	(0.041)	(0.044)	(0.071)
Trend	No	Yes	No	Yes
First stage				
δ	–	–	0.37***	0.48***
Cragg–Donald	–	–	377.13	234.51

Note: Standard errors in parentheses.

columns (1) and (2), for comparison purposes, we present ordinary least squares (OLS) estimates for Eq. (2) that yields a parameter of −0.185 and −0.265 when a log-linear trend is added to the model. Columns (3) and (4) present the IV results. Column (3) presents the results of the triangular model described by Eqs. (2) and (3) and column (4) adds a log-linear trend in both equations. 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 Fig. 1.[46]

Appendix C. Point estimates from preferred specification

Table C.1
The effect of the birth control pills' price war and collusion on units of birth control pills sold (in thousands) and weekly births, by week of conception.

	Week of the year							
	1	8	15	22	29	36	44	52
A. Number of Units of Birth Controls Pills Sold								
2007	6.03***	7.69***	9.36***	11.03***	12.70***	14.36***	16.27***	18.18***
	(1.10)	(0.88)	(0.70)	(0.58)	(0.57)	(0.66)	(0.86)	(1.10)
2008	13.71***	7.81***	2.70***	−1.60**	−5.11***	−7.82***	−9.93***	−11.00***
	(1.50)	(0.93)	(0.74)	(0.76)	(0.77)	(0.73)	(0.88)	(1.50)
B. Total Births								
2007	20.67	19.71	18.76	17.81	16.85	15.90	14.81	13.72
	(44.95)	(40.24)	(36.68)	(34.64)	(34.37)	(35.93)	(39.65)	(44.95)
2008	−90.84	27.41	106.32***	145.88***	146.09***	106.96***	14.05	−130.25**
	(55.44)	(38.27)	(32.67)	(32.98)	(33.19)	(32.50)	(36.83)	(55.44)

Note: We fit a different polynomial $f(\beta, |t - t^*|)$ to each side of the cutoffs. The results come from a linear before 2008 and quadratic polynomial afterwards specification. See Tables III.1 and III.2 in Web Appendix for sets of results across alternative specifications. *** $p < 0.001$, ** $p < 0.05$, * $p < 0.1$. Standard errors in parenthesis. All estimations include a linear trend. For results across different specifications see Section III in our Web Appendix.

Appendix D. Pregnancy interruption and off-label emergency contraception methods

During the period of study, abortion was illegal and the morning-after pill largely unavailable in Chile. Nonetheless, different drugs could have been used to interrupt a pregnancy or as off-label emergency contraception methods. Among the former we identify Misoprotsol, which is a prostaglandin that induces uterine contractions that will cause the expulsion of an implanted embryo or fetus. Among the latter we identify Levonorgestrel, which is a progestin used in various methods of hormonal contraception. Price changes in medications containing these drugs could have produced changes in fertility, potentially jeopardizing the interpretation of our main findings. However, as we argue next, our conclusions are robust to this possibility. First, if those medications were unaffected by the price shifts affecting contraceptives, our estimates would become —like with other possible substitutes, such as condoms—lower bounds of the true effect. On the contrary, if those medications were part of the price hikes, our point estimates would capture them.

To further examine each case, we took advantage of the documents from the lawsuit and the rich pharmacies' transactions data recorded during the relevant period. When it comes to pregnancy interruption drugs, according to the National Economic Prosecutor (FNE), Misotrol and Cytotec—the commercial brands of Misoprostol—were not part of the 222 drugs for which prices skyrocketed.[47]

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

[47] For more details, see https://www.fne.gob.cl/wp-content/uploads/2011/03/requ_0009_2008.pdf.

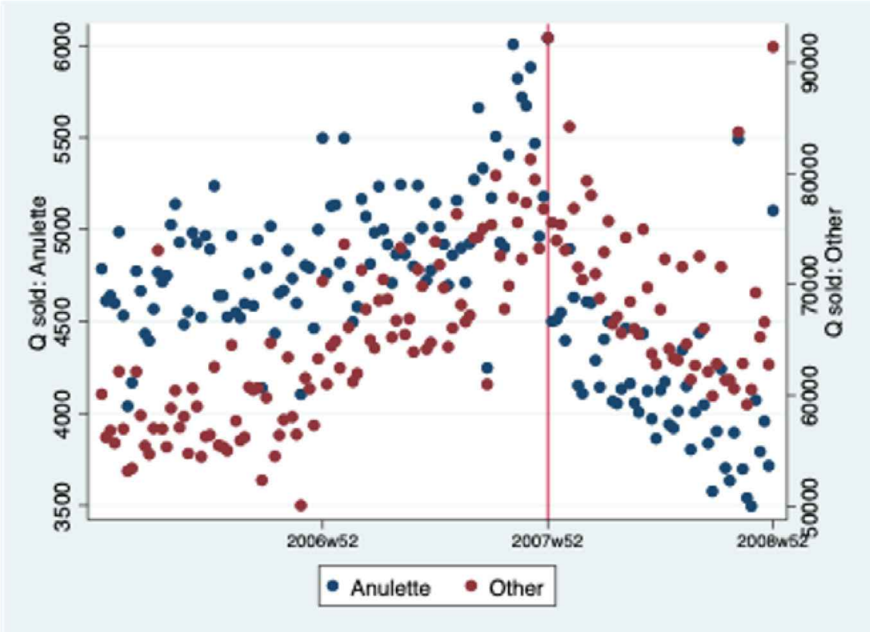

Fig. D.1. Weekly contraceptives purchases of anullete and other contraceptives: January 2006–January 2009

Moreover, a search for “Misotrol” or “Cytotec” across more than 40 million transactions delivered no records of sales. This is consistent with the fact that Pfizer—the maker of Cytotec—denies the distribution of the medication in the Chilean market. Of course, that does not rule out the possibility that Cytotec could have been sold in the black market, probably under the brand Oxaprost, which was sold in neighboring Argentina. But in this case, Misoprostol’s price would have not been directly manipulated by pharmacies as they did with contraceptives, turning our estimates into lower bounds of the true effect. Consequently, our conclusions are robust to changes in Misoprostol-induced abortion patterns.

With respect to Levonorgestrel, the difference between using it as a regular contraceptive pill and on off-label emergency contraception relies on the dosage. Emergency contraception requires Levonorgestrel taken in two pills of 0.75 mg, whereas the regular contraception use of this progestin requires pills of 0.15 mg. Given that in the relevant period the morning-after pill was not available but contraceptives containing Levonorgestrel were (commercialized under the name Anulette), Chileans could have resorted to using high doses of Anulette (two takes of five pills each) as off-label emergency contraception in a method known as “Yuzpe.” Fig. D.1 plots the weekly sales of Anulette and compares them with the sales of other contraceptives. It shows that, like the other contraceptives, consumption of Anulette was increasing during the price war but dropped rapidly after the price increase in January 2008. If anything, the decline in the consumption of Anulette was steeper than that observed for the consumption of other contraceptives. Therefore, if people who were priced out of their regular contraceptive resorted to off-label EC in the form of “Yuzpe,” we would not see such a drop in the sales of Anulette during 2008. As a result, the price shock most likely did not increase the use of “Yuzpe”; instead it made it more costly. Either way, the increase in this method or the lack thereof is part of the treatment we analyze as it is driven by the pharmacies tinkering with the contraceptives’ prices. As such, it would be again captured by our estimates.

Appendix E. Assessing potential channels

In principle, our effects on the incidence of low birth weight presented in Table 3 could be explained both by changes in who selects into motherhood and increases in the incidence of unplanned births. In what follows we put our results in context and bound the effect of the change in the composition of mothers on children’s health. We implement the following exercise:

1. We calculate the effect the collusion had on the distribution of new mothers according to their education attainment level. These results are reported in Table E.1.
   We do find a significant increase in the number of mothers without a high school degree in 2008. However, relative to the weekly number of mothers without a high school degree in 2006 (pre–price war and collusion), its magnitude is small (0.069 percentage points at most). Such a small change in the pool of mothers puts the size of the impact in context.
2. By combining the distribution of births by mother’s education level in 2006, the proportion of underweight births by mother’s education also in 2006, and the estimated effect of collusion on overall births, we predict the number of extra-conceptions and low birth weight newborns had the mother’s education distributions of 2008 been that of 2006. This quantifies the scale effect: keeping sorting into motherhood constant, how many additional underweight babies would have been born to high/low-educated mothers just because the number of overall births increased.

Table E.1
Change in the fraction of births to mother with different education levels by week of conception.

Week of 2008							
1	8	15	22	29	36	44	52
% HS Dropout							
0.22	0.45**	0.60***	0.68***	0.69***	0.61***	0.44**	0.16
(0.32)	(0.22)	(0.19)	(0.19)	(0.19)	(0.19)	(0.21)	(0.32)
% HS Graduate							
−0.48	−0.55**	−0.58**	−0.56**	−0.51**	−0.42*	−0.27	−0.07
(0.40)	(0.27)	(0.23)	(0.24)	(0.24)	(0.23)	(0.26)	(0.40)
% College							
0.12	−0.09	−0.26	−0.37*	−0.43*	−0.44**	−0.39	−0.28
(0.37)	(0.25)	(0.22)	(0.22)	(0.22)	(0.21)	(0.24)	(0.37)

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

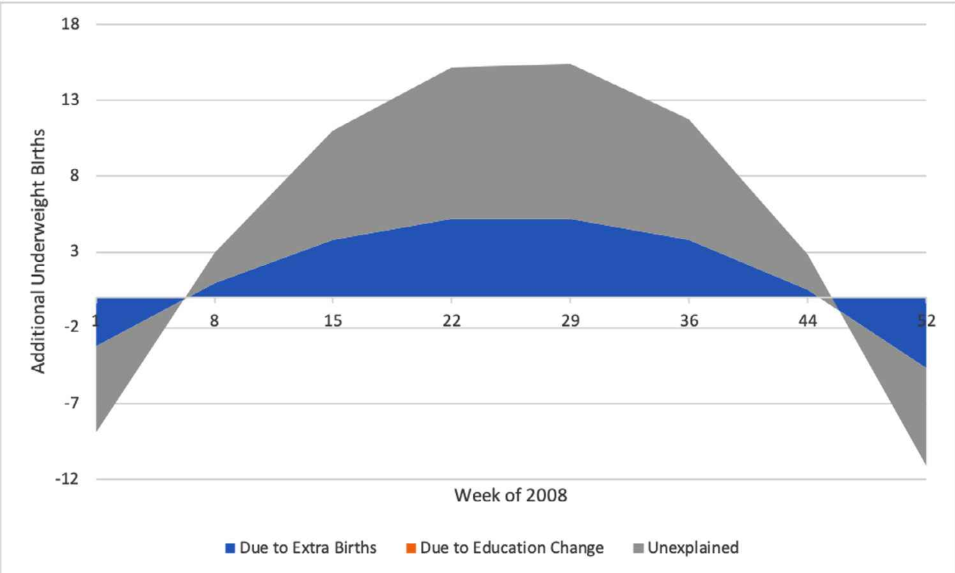

Fig. E.1. Decomposing the drivers of the changes in the number of low-birth-weight newborns *Note:* The decomposition comes from comparing the results with and without changing the education distribution of the mothers as indicated in the four steps described above.

3. We implement the same simulation (step 2), but now using the post-collusion distribution (2008) of new mother's schooling level (i.e., increase in the fraction of low educated mothers by 0.06pp). In this way, we factor in the potential impact of the collusion on the sorting into motherhood by schooling level.
4. We compute the differences between the two resulting simulated outcomes, which we interpret as the impact of selection into motherhood triggered by collusion.

Based on this exercise, we can distinguish two potential channels: Selection into motherhood (particularly among low-educated mothers) and the mechanical effect of more conceptions. The unexplained fraction (remainder) could be attributed to low investments arising from unexpected pregnancies. Fig. E.1 displays the results.

The figure indicates that one-third of the additional number of low-birth-weight newborns can be linked to and increase in the number of conceptions (scale effect), and two third to what we interpret as lower investments arising from unexpected pregnancies. The fraction attributed to selection is negligible.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jhealeco.2021.102496

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