Price Shocks and Child Mortality: Evidence from Anti-Drug Policies in Peru*

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Abstract

How do aggregate income shocks affect child mortality in developing countries? In the developing world, child health may be vulnerable to aggregate income shocks as credit constraints and other market imperfections may prevent households from fully smoothing consumption and health investments. Empirical studies, however, have found positive, negative, and null relationships. This paper brings new evidence by exploiting quasi-exogenous variation in the price of coca leaves—the main input for cocaine production—induced by an anti-drug policy to study how sharp decreases in coca revenues affect child mortality in producing sites. The identification strategy relies on an abrupt decline in prices to compare survival rates across cohorts and areas with different levels of baseline coca cultivation. I document important increases in mortality: for the average coca district, the 50 percent price drop caused by the policy is associated with an effect equivalent to a 6-11 percent increase in under 5 mortality. Moreover, I establish that deaths occur both in-utero and during the first years of life. To do this, I use direct mortality records and a "missing children" approach that infers survival rates by comparing relative cohort sizes in census and survey data. Using data from before and after the shock, I find that households increase their labor supply to cope with the price drop; however, reductions in health investments make health vulnerable to income losses. The results are robust to several alternative explanations. This paper contributes to a literature on aggregate income shocks and health and a growing body of research on illegal markets and law enforcement. Furthermore, this paper suggests that anti-drug policies can impose important costs to the weakest links involved in drug trade.

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1 Introduction

Children are healthier in wealthier nations. In rich countries, deaths under age five account for less than one percent of all deaths; in poor countries, around one out of three deaths is of a child under age five (Cutler et al., 2006). Motivated by disparities in wealth and health across countries, prominent studies have documented a negative association between fluctuations in aggregate income and child mortality (e.g. Pritchett and Summers, 1996).

Despite considerable scientific progress, the causal effect of aggregate income shocks on child and infant mortality in the developing world is still elusive. Most empirical evidence comes from studies using economic recessions or economic cycles as sources of variation for aggregate income. This avenue of research has generated mixed results, yielding positive, negative, and null relationships (for a review, see Ferreira and Schady, 2009).[1] Although these studies are useful from a macroeconomic perspective, two characteristics of this literature prevent us from understating how mortality is affected when households face changes in economic conditions. In addition to the potential endogeneity of some crises and economic cycles, there are two challenges. First, economic cycles and crises may affect the quantity and quality of the supply of healthcare and not only the demand for health inputs. Second, it is difficult to separate the effects of income on mortality from behavioral responses affecting the composition of births, such as selective fertility.

This paper exploits a natural experiment to study how a negative price shock in the Peruvian coca industry affected mortality rates among children in coca-producing cites. Coca is the primary input for cocaine production, and its cultivation only takes place in the eastern side of the Andean region due to agronomic conditions (FAO, 2007). As such, coca production is the objective of various anti-drug policy interventions that aim to curb the supply of cocaine. This paper exploits variation in the price of coca induced by a program promoted by the U.S. that sought to cut the supply chain of cocaine by enacting a shoot-down policy against narco-airplanes transporting exports of coca from Peru to Colombia. The policy caused an abrupt 50% drop in the price of coca leaves in Peru. By analyzing this context, this paper aims to contribute to a literature on aggregate income shocks and child mortality, as well as a strand of research on illegal markets and law enforcement.

Child mortality, or health more generally, can be affected by two main mechanisms when coca prices

[1] In addition, Pérez-Moreno et al. (2016) and Van den Berg et al. (2017) provide recent and useful overviews of the literature.

fall; the overall effect is theoretically ambiguous. The production function of health is assumed to depend on two arguments: consumption of health-promoting goods (e.g. nutritious food and medicines) and time allocated to health-promoting activities (e.g. taking children to preventive health visits, collect clean water, or caregiving more broadly). When coca prices drop, households may reduce consumption of health-promoting goods, negatively affecting child health (income effect). However, if the price drop is translated into a lower opportunity cost of time, caregivers may increase their allocation of time to health-promoting activities (price effect). In the end, the total effect of the coca price drop on health and mortality is theoretically ambiguous.[2] [3]

Using variation of prices over time and of baseline levels of coca cultivation across districts, I employ a difference-in-difference design that aims to avoid key threats to identification. The empirical specification compares cohorts of children born in years of high prices with cohorts born in years of low prices and across areas with different levels of baseline coca cultivation. A salient challenge is being able to isolate the effect of the price shock on mortality from changes in the composition of cohorts, for instance, driven by selective fertility. I take advantage of the abrupt drop in coca prices to credibly isolate the effects from changes in selective fertility and other responses. The analysis compares the first cohort exposed to the shock—and conceived the year before the shock—with adjacent, older cohorts based on information on year and district of birth.

I overcome the challenge of not observing reliable vital records in developing countries by using census data to infer mortality rates from relative cohort sizes (i.e., "missing" individuals). I use the full 2007 Peruvian Population Census to construct population counts by district and year of birth 13 years after the implementation of the policy. The measure of (cumulative) mortality I use has several advantages, including capturing fetal deaths and delayed mortality (Jayachandran, 2009; Miller and Urdinola, 2010).

I find that the 50 percent price cut associated with the policy caused a reduction in cohort size of 0.43 to 0.53 percent for the average coca district, with the effects being particularly strong for males. In terms of under 5 mortality, this difference in cohort size represents an increase of 6 to 11 percent.[4] The effect is particularly large for boys: 0.81 to 0.89 percent decrease in cohort size for the average coca district. The

[2] In addition, maternal exposure to stressful events may affect fetal health through the release of hormones that can be harmful in high concentrations (Beydoun and Saftlas, 2008; Navara, 2010).

[3] Note that aggregate income shocks, or shocks defined more broadly, may affect health through additional causal pathways. For instance, natural disasters may affect health and mortality directly; weather shocks in developing settings may compromise the availability of food; economic crises may affect the quantity and quality of the supply of public health care services; etc.

[4] Mortality under age five is a sound benchmark as most deaths are happening before that age. This is shown in later sections of the paper.

pattern of higher mortality among boys is consistent with the survival disadvantage of males relative to females both in-utero and in early life (Drevenstedt et al., 2008; Navara, 2010).

Moreover, I determine that deaths take place both in-utero and during the first years of life. Using the Peruvian Demographic and Health Surveys (DHS), I find that the shock is first associated with an increase in miscarriages, especially of baby boys, as suggested by a "missing babies" approach. Although the weakest fetuses were lost before birth, newborns affected by the price drop also tend to be smaller, a marker of impoverished health. After birth, infants face higher mortality rates as well.

An analysis of the mechanisms at play suggests that both income and price effects explain the increase in mortality, contrary to recent evidence suggesting that price effects undo income effects (e.g. Miller and Urdinola, 2010). Using general household surveys collected before and after the shock, I conclude that the downturn is associated with a reduction in overall household expenditure, including food and health. In addition, to cope with the decrease in income, increases in labor supply are in place. Individuals in school age (6 to 17 years old) and adult women supply more labor, which may crowd out time-intensive health investments. This is consistent with poor individuals supplying more labor when wages are low to compensate for losses in income (Jayachandran, 2006).

The main results are unlikely to be driven by factors other than the price shock. First, the main analysis exploits and abrupt change in coca prices and defines exposure based on district and year of birth. Thus, ex-post migration is unlikely to play any role. Moreover, I limit the analysis to cohorts 0-3 and 0-2 years of age at the time of the shock. This is, the exposed cohort was born during the price shock and (mostly) conceived the year before. This limits the influence of selective fertility in the analysis. The comparison with adjacent, older cohorts ensures that both treatment and control would have very similar life cycle experiences had it not been because of the price shock. Second, the results are consistent across a number of specifications and robustness checks. These include controlling for coca-specific time trends and a large set of baseline covariates interacted with year fixed effects, arbitrarily changing the cohorts used as the control, careful examination of the timing of the effects, among others.

The main contribution of this paper is twofold. First, it contributes to a literature on aggregate income shocks and child mortality by addressing endogeneity concerns using plausibly exogenous variation in the price of coca (Ferreira and Schady, 2009). Moreover, contrary to papers studying economic crises, this study pins down the effects separately from changes in the public sector provision of health services by using a price shock to a non-taxable commodity. Also, the abruptness of the price change is used to carry the main analysis over a narrow window of time and prevent fertility and other behavioral responses from contaminating the results. Using a missing children approach, this paper documents that mortality takes place both in-utero and after birth. The main results suggest that the income effect and price effect have the same sign. This is consistent with poor, developing settings and arguably different from quasi-experimental evidence from more developed areas where price effects may compensate for income losses (Miller and Urdinola, 2010). More broadly, this study relates to a literature on early life shocks (Almond and Currie, 2011; Almond et al., 2017; Currie, 2009; Currie and Almond, 2011; Prinz et al., 2018) and the more general, two-way relationship between economic development and health (Deaton, 2007; Strauss and Thomas, 1998, 2007). This paper also relates to a literature on natural resources. Although natural resources are salient in developing countries, where health is more vulnerable, this literature has paid limited attention to how resource booms and busts affect health of vulnerable populations (for a review, see Aragón et al., 2015)

Second, this paper establishes that economic downturns in drug industries may have negative effects on the wellbeing of vulnerable populations. This adds to a growing body of work documenting the also negative consequences of drug booms. Angrist and Kugler (2008) document that a growth in opportunities in the coca sector in Colombia is related to a moderate increase in earnings among Colombian households accompanied by an increase in violence levels in the Colombian armed conflict. Sviatschi (2018) documents that increases in prices of coca in Peru are associated with moderate income gains, higher school dropouts, and criminal human capital accumulation. Dammert (2008) analyzes the same setting than this paper and establishes that households respond with increased child labor during the economic downturns of the coca industry. Putting this evidence together, these studies call for careful analysis of the welfare implications of anti-drug policies and drug trade, as well as new policy designs.

The paper is organized as follows. Section 2 describes the setting and coca production in Peru. Section 3 describes the natural experiment and motivates the use of exogenous sources of variation. Section 4 presents the main datasets and the empirical strategy. Results and robustness checks are presented in Section 5 and 6, respectively. Section 7 concludes.

2 Coca Cultivation in Peru

Coca (*Erythroxylum coca*) requires specific environmental conditions for cultivation. As such, virtually the entire supply of cocaine in the world can be traced back to only three countries: Bolivia, Colombia, and Peru (UNODC, 2007). The plant is native to South America, where the tropical and sub-tropical climates of the eastern side of the Andes Mountains provide suitable conditions for its growth. These conditions include annual rainfall between 1,000 to 2,100 millimeters a year, temperatures between 17 to 23 degrees Celsius, very bright sunlight intensity, and altitudes between 600 and 2,000 meters above the sea level with slopes of around 20 degrees (FAO, 2007; UNODC, 2003).

Peru was the largest producer of coca in the world in the 1980s and early 1990s, but there was substantial variation in coca cultivation across areas in the country (see World Drug Reports, e.g. 2002). Figure 1 shows districts by levels of coca cultivation as of 1994 grouped in quintiles.[5] Out of 1777 districts, 193 produce coca.[6] It is easy to notice that most coca districts are located to the right of an almost perfect diagonal line: coca districts are located in the eastern side of Andes Mountains, where the climate is suitable for cultivation. Sviatschi (2018) establishes that cultivation levels are positively and strongly related to climatic conditions favorable for coca farming. Bolivia and Colombia also exhibit spatial concentration of coca cultivation in particular geographies (see Illicit Crop Monitoring Reports for each country, e.g. 2002; 2005a). As I explain later, I will use variation in cultivation levels in the identification strategy.

Coca-producing sites are generally undeveloped, and estimates suggest that they depend heavily on coca as an economic activity. Over 200,000 households had an economy based on coca farming or related activities in 1995, the year in which the anti-drug policy studied in this paper was enacted (DEVIDA, 2004). Studies on the most important coca-producing regions estimate that around half of households' income comes from coca farming (Bedoya, 2003; DEVIDA, 2013). It has also been suggested that coca crowds out other sources of income (Pedroni and Yepes, 2011).[7] In addition, coca districts are worse off. According to the 1993 Population Census, coca districts have smaller populations, with over 70 percent of them residing in rural areas, and are less educated. Moreover, child mortality is high, reaching 6.7 percent in rural areas,

[5]Districts are the smallest administrative unit in Peru. The average district has a population of around 12.4 thousand individuals according to the 1993 Population Census.

[6]The actual number of total districts in Peru is slightly higher than 1777. The difference arises because I collapsed less than five percent of all districts into time-consistent geographical units for the analysis. This is needed as I use various data sources collected at different points in time, and some district boundaries change. Details are provided in the data section below.

[7]Note that these are conservative estimates as they were calculated with data collected after 1995, when production of coca in Peru had sharply decreased, and the expected revenues from coca activities were lower than before 1995.

and access to public health services is limited (INEI, 2001).

Coca cultivation and harvest are the first steps in the supply chain of cocaine production.[8] Coca has been produced in Peru since pre-Inka times for ceremonial and religious purposes. People living in the highlands use it to fight fatigue and altitude sickness. Since the boom of cocaine in the 1970s, however, the legal market for coca became very small relative to its illicit counterpart. Studies from the early 2000s estimated that 90% of the total amount produced of coca is sold to the illicit market (UNODC, 2003).

Small-scale farmers produce coca as a source of monetary income. Coca plots tend to be under one hectare. The first harvest takes place after one year. Then the bushes can be regularly harvested 3-5 times a year for about 20 years. The harvest consists of picking the leaves carefully without damaging the plant's bulbs. This process is very labor intensive and needs to be done in a short period of time (about two weeks) while the leaves are "ripe". After harvest, the leaves are dried under the sun and sold to intermediaries for cocaine production. One hectare can produce about 2.2 tons of sun-dried coca leaves a year (World Drug Report 2010). In terms of 2009 prices, one hectare produces 6600 dollars in revenues (over two times the minimum wage). Importantly, coca is not part of the nutritional intake of households. In fact, studies show that coca lacks nutritional content (Castro de la Mata and Zavaleta Martinez Vargas, 2009; Zavaleta Martinez Vargas, 2012; Zavaleta Martínez Vargas et al., 2016).

Sun-dried coca leaves are then transformed into coca paste and exported. Farmers sell their coca leaves to middlemen who higher local "cooks" to transform the leaves into coca paste or cocaine base, intermediate goods before the final product. This step requires "micro labs" or maceration pits, and some additional inputs, such as kerosene.[9] The process drastically reduces the volume and weight of coca leaves: estimates for the Peruvian coca industry suggest that around 450 Kg of sun-dried coca leaves are needed to produce a single kilogram of cocaine base (World Drug Report 2010).[10] This process is done locally. Then coca paste, with a much smaller volume is smuggled through roads and rivers to centers, such as the Uchiza district, where small narco airplanes would ferry coca paste from Peru into Colombia (IDL, 2012).

[8]This and the following paragraphs in this section draw heavily from (DOJ, 1991; García Díaz and Stöckli, 2014; UNODC, 2005b)

[9]Farmers own some of these, but it is not clear how many have vertically integrated this step.

[10]This was estimated in 1994, which is relevant for the period of analysis of this paper. Later estimates suggest increases in productivity in the 2000s after Peru became also a cocaine producer and direct exporter to final consumer markets.

3 Price Shocks in the Peruvian Coca Industry: A Natural Experiment

The so-called "air-bridge denial" policy changed the structure of cocaine markets in South America in 1995, and I use it as a source of quasi-exogenous variation in this paper (Angrist and Kugler, 2008; Dammert, 2008).

Up to 1994, Peru specialized in producing coca. Coca would be ferried to Colombia in the form of coca paste, usually using small narco-airplanes taking off from improvised runways in the Amazon jungle. Once in Colombia, it would be transformed into cocaine and exported to final consumer markets. Peru was the largest producer of coca in the world, cultivating three times more coca than Colombia. Coca production in Peru reached levels of around 120 thousand hectares in the 80s and early 90s. Colombia and Bolivia produced about 40 thousand hectares each (see figure 2).

In 1995, an aggressive policy that aimed to cut the supply chain of cocaine in the Andean region represented an abrupt and negative demand shock to the Peruvian coca industry. Among other efforts, the Peruvian and Colombian Airforce implemented a shoot-down policy of aircrafts suspicious of ferrying coca and that did not follow commands of forced landing. The operation was conducted in cooperation with the U.S. government, which provided funding, aircrafts, and ground radars. US cooperation was authorized on December 8th, 1994, by President Bill Clinton's Presidential Determination and its Memorandum of Justification (CIA, 2008). In general, this strategy followed from the shift in US interdiction and seizure efforts from Central America to the source countries and the overall militarization of the war on drugs (Zirnite, 1998). Although similar strategies existed in the 80s and 90s, none had the intensity of efforts carried out in 1995 and later years.

For Peru, this policy generated an abrupt collapse in the price of sun-dried coca leaves. Figure 3 shows the evolution of the price in real terms over time. In a single year, from 1994 to 1995, the price dropped about 50 percent. The price remained low until 1999 when an airplane of the Peruvian Airforce suffered an accident in February of the year. This raised concerns about how the safety protocols where being implemented and the policy weakened. In 2001, the program is suspended after a civil airplane with a US missionary and her daughter was shoot down by mistake. The policy was associated with a sizable reduction in coca production in Peru and an increase in Colombia, leaving the total amount produced between 1994 and 2001 virtually unchanged (see figure 2).

4 Data and Empirical Strategy

I exploit variation in the price of coca leaves generated by a counter-narcotic policy to measure its impact on child survival and health. The policy significantly reduced farm gate prices of coca leaves during 1995-1999, with an unprecedented initial drop of 50% from 1994 to 1995. This period is known as the "crisis of coca". I compare cohorts born in years of high prices with cohorts born in years of low prices across districts with different levels of coca suitability. The identification strategy focuses on the effects of price shocks during the first year of life. Life is fragile and mortality high both in-utero and during the first year of life. In additional specifications I allow for effects at different ages.[11]

4.1 Data

The measure of coca revenues combines prices of sun-dried coca leaves in Peru and cultivation levels of coca crops by districts measured *before* the price collapse. Yearly prices are collected by the United Nations Office on Drugs and Crime and reported in U.S. dollars.[12] I transform this price sequence to 2009 real Nuevos Soles for the analysis.[13] Coca cultivation is obtained from the 1994 Agricultural Census. I construct cultivation levels by district measured as the total area covered with coca crops in thousands of hectares. This is the measure of coca intensity of this paper. Note that coca intensity is measured in 1994, while the price drop took place in 1995.

I use the full 2007 Peruvian Population Census to construct cohort sizes by year-and-district-of-birth cells. Cohort sizes will be used as an indirect measure of cumulative survival (mortality) 13 years after the price shock (Jayachandran, 2009; Miller and Urdinola, 2010). The empirical specification below establishes the conditions under which cohort size can be interpreted as cumulative survival.

In addition, I use Peru's DHS to draw detailed data on births and direct measures of child mortality. To have wider coverage over coca and non-coca districts, I pool information from the two earliest waves after the price drop: 1996 and 2000. DHS records information on birth histories, child mortality, maternal characteristics, and women and children's health for nationally representative samples of women ages 15-49. I use these surveys to infer how the shock is associated with mortality in-utero and after birth.

[11]Variations of the empirical specification that allow for effects before and after the first year of life corroborate that shocks at other ages have little to null effects.Econometrically, we have more confidence in the assignment of the treatment at the time of birth using district-of-birth and year-of-birth than years after birth when exact locations are not known.

[12]Price data are available at the month-valley level since 1998. Unfortunately, I cannot use these data because the identification strategy is based on the quasi-exogenous drop of prices in 1995. Prices across regions are highly correlated.

[13]Exchange rate and price index data are from the Central Bank of Peru.

Finally, I use the 1994 and 1997 Peruvian Living Standards Measurement Surveys (LSMS)—which cover precisely the period before and after the price shock—to characterize how households expenditure and labor decisions were affected.

District boundaries were mostly constant over the period of analysis. However, to ensure a correct match across data sources, all districts were transformed to the 1993 administrative division using information from National Decrees published in the official newspaper El Peruano. Less than 5 percent of districts suffered boundary changes in the period under analysis.

4.2 Empirical Strategy

4.2.1 Cohort Size

I first analyze how cohort size relates to district and year of birth variation in coca revenues generated by the policy. Using the full 2007 Population Census, I construct population counts by cohort (year) and district of birth, which are linked to a measure of revenue that combines yearly prices of sun-dried coca leaves and coca cultivation intensity by district.

In particular, to investigate this relationship, I estimate equation 1

$$ln(CohortSize_{dt}) = \beta(P_t \times Coca_d) + X'_{dt}\pi + \alpha_d + \gamma_t + \delta_r \times t + \varepsilon_{dt} \quad (1)$$

Where $CohortSize_{dt}$ is the number of individuals (that survived until 2007) born in district d and year t, and the measure of coca revenue is the product of the year-of-birth price of sun-dried coca leaves in real soles per kg, P_t, and the district-of-birth measure of coca intensity, $Coca_d$. I use the 1994 Agricultural Census, which precedes the 1995 price shock, to calculate the total area of cultivated land with coca crops in thousands of hectares as a measure of coca intensity.

The estimation is conditioned on a set of covariates. District and year of birth fixed effects are represented by α_d and γ_t, respectively. The specification also includes state-level linear trends, $\delta_r \times t$. There are 25 states in Peru, and many policies are executed at this level.[14] These trends partial out factors (linearly) changing over time in each state. The vector X'_{dt} represents year-of-birth effects interacted with a set of agricultural controls including district-level intensity of cacao and coffee measured in thousands of hectares

[14] Peru has three layers of administrative division: 25 states (including Callao Constitutional Province) which are divided into approximately 180 provinces, and these are subdivided into over 1800 districts.

in the 1994 Agricultural Census. This controls for volatility of other commodities beyond variation in international prices, such as pests. In addition, to control for other changing conditions in the agricultural sector, I control for the total area of cultivated land as of the 1994 Agricultural Census in each district interacted with year fixed-effects.

The coefficient of interest is β. Holding all other independent variables constant, increases in the price of coca imply pro-cyclical (larger) cohort size if $\beta > 0$. Alternatively, cohort size is counter-cyclical if $\beta < 0$.

To interpret changes in cohort size as excess cumulative mortality, the regression analysis focuses on the 1995 abrupt price decline and compares the 1995 cohort to immediately preceding cohorts. The 1995 cohort is the first cohort exposed to the shock and was (mostly) conceived in 1994, before the shock. Thus, it is unlikely that changes in the demand for children drive the main results unless the price drop was anticipated. Further analysis on shows that this is unlikely. Including later cohorts or exploiting other sources of price variation that are not as abrupt, however, could complicate the interpretation of the results as these could include a combination of mortality and compositional effects across cohorts, such as selective fertility or selective migration as a response to the change in coca revenues. In addition, the analysis uses district-of-birth and not district-of-residence at the time of the census; therefore, cohort counts are more likely to be correctly assigned to exposure to treatment.

To compare cohorts that would have shared very similar life-cycle events in the absence of the price shock during the year of birth, I perform the analysis over the 1993-1995 and 1994-1995 cohorts. This is a sample of individuals ages 0-3 and 0-2 at the time of the shock.[15] When analyzing the 1993-1995 sample, I show results with and without a coca-specific linear trend of the form $\delta \times 1\left[Coca_d > 0\right]$. Conditional on this trend, changes in coca revenues are identified as deviations from the average evolution of coca districts over time. As robustness checks, I show results for alternative definitions of the control cohorts. The Peruvian economic crisis of the late 1980s and the abrupt economic reforms of the early 1990s, however, prevent me from analyzing much longer time intervals.

Working with census data and inferring cumulative survival (mortality) rates from relative cohort sizes

[15] The 2007 Population Census does not record date of birth directly but age in years as of Census day (October 21st, 2007). Thus, individuals who report being X years of age are assigned to the Oct/21/(2007-X-1) to Oct/20/(2007-X) year of birth. By assigning calendar year prices to census birth years, the oldest individual in a given census cohort (born in October 21st) is matched to prices during months 2 to 14 of age. On the other hand, the youngest individual of the census cohort (born in October 20) is match to prices during months -9 to 2 of age. I do not find evidence of age stacking. The cohorts under analysis are 12-14 years of age at the time of the census.

has two advantages over using Vital Statistics. First, cumulative survival captures fetal mortality and not only mortality after birth (Jayachandran, 2009; Miller and Urdinola, 2010) Second, the quality of Vital Statistics in developing countries may not be adequate. In Peru, mortality and birth certificates data suffer from underreporting in rural districts. The census program cover all districts. A drawback of the census is that it does not have detailed information on each individual and household, and although survival rates can be inferred, little information is collected on households and mothers that experience the death of child. Moreover, the timing of deaths (in-utero and after birth) cannot be distinguished. As I explain below, I also use survey data to overcome these challenges.

Two main identifying assumptions are key to interpret β causally. First, exposed and unexposed cohorts should be statistically exchangeable. This is, there should be no differences in the composition of cohorts based on the measure of exposure. Second, there are no other factors varying over time in a way that is proportional to coca intensity and affecting the outcomes of interest.

I provide tests for these assumptions and evidence on further robustness checks in later sections. Importantly, to address cohort composition, I study maternal characteristics of the cohorts exposed to the shock and show results after controlling for these. Although newborns and children could have been severely affected by the shock, pre-determined characteristics of older cohorts should have not. To study if the effects are driven by other confounding factors correlated with coca revenues, I show results after controlling for a range of time-varying observables. Finally, I also consider robustness to alternative explanations, such as the Peruvian civil conflict, the expansion of health services to non-coca areas, and alternative anti-drug enforcement efforts.

Note that the identification strategy using the 1993-1995 cohorts relies on price variation preceding the shock and not only on the 1994-1995 price drop. This could be problematic if the 1993-1994 variation is a result of factors driving both prices and the mortality of children and is a relevant source of variation. Thus, in addition to conducting close comparisons between the 1994 and 1995 cohorts, I also replicate the 1993-1995 analysis with a difference-in-difference estimation that drops the price sequence and uses a post-treatment variable for 1995. I also use these dummy as an instrument for prices.

4.2.2 Miscarriages, Mortality, and Birth Outcomes

I test if the exposed and unexposed cohorts are statistically exchangeable in terms of maternal characteristics using microdata from the 1996 and 2000 DHS. Mothers with different Socio-Economic Status (SES) may select into having children if they are able to anticipate the shock.[16] At the same time, worsened nutritional conditions and stress can be associated with unexpected termination of births and selection in-utero (Beydoun and Saftlas, 2008; Navara, 2010). Unfortunately, reliable data on miscarriages since the time of conceptions is rarely available.[17] Thus, I use the histories of live births for all women in the sample and then contrast this analysis with predictions on the probability of birth that relate to miscarriages.

$$y_{imdt} \ = \ \beta(P_t \times Coca_d) \ + X'_{dt}\pi \ + \alpha_d \ + \gamma_t \ + \delta_r \times t \ + \varepsilon_{imdt} \tag{2}$$

I estimate equation 2, where y_{imdt} is a maternal outcome for birth i in year t to mother m and district d (for example, years of education, number of preceding births, among others). The regressions include a survey-wave fixed effect, and all other variables are defined as before. The estimation is done over the 1993-1995 and 1994-1995 cohorts.

The test of interest is if $\beta = 0$, or the lack of ability of the measure of exposure to predict pre-determined characteristics of the mothers of children exposed to the shock. Note that both in-utero mortality or changes in the demand for children could cause this test to reject the null.

Then I test if exposure is related to changes in the probability of giving birth, and if these changes are associated with miscarriages rather than changes in the demand for children. To do this, I create a yearly panel for all women in the DHS sample and estimate equation 3

$$birth_{mdt} \ = \ \beta(P_t \times Coca_d) \ + X'_{mdt}\pi \ + \alpha_d \ + \gamma_t \ + \delta_r \times t \ + \varepsilon_{mdt} \tag{3}$$

where $birth_{mdt}$ indicates if woman m in district d gave birth in year t. This sample includes childless women that had not given birth at the time they were interviewed. Moreover, in addition to the controls specified in equation 1 and survey-wave fixed effects, I will show results with and without a set of maternal characteristics. The notation of X'_{mdt} is now changed to allow for this. Maternal characteristics include sets of fixed effects for the following variables: mother's year of birth, preceding number of births, marital status at birth, mother's years of schooling, and mother's ethnicity (proxied by language).

If miscarriages—and not some behavioral effect in anticipation of the shock—are the main driver of missing births, they should be affecting more male fetuses than female fetuses, as the former are much more

[16] DHS records histories of all live births, including those not alive at the time of the survey, for all women ages 15-49.

[17] Even if available, data on self-reported conceptions and miscarriages may be of poor quality. Miscarriages are more likely to happen early in pregnancy. See Orzack et al. (2015) for recent efforts on studying in-utero mortality.

likely to be miscarried overall (Navara, 2010). To test for this, I replace the outcome of equation 3 for an indicator variable for gender-specific births and test for the difference in magnitude of these estimates.[18] Another implication of miscarriages being the main driver of missing births is that the observed male-to-female ratio should be changing within the shock year. Male fetuses are more likely to be miscarried during the first trimesters of pregnancy, while female fetuses are more likely to be miscarried during the last trimester of pregnancy. Thus, the male-to-female ratio should be lower at end of 1995—when both male and female babies are missing—than at the beginning of 1995—when mostly baby girls are missing—, and relative to the control cohorts. I also implement this test.

Finally, I study if exposure to the shock also affected mortality probabilities and other birth outcomes for those children that survived to in-utero exposure. To do this, I estimate equation 4 at the individual (child) level,

$$o_{imdt} = \beta(P_t \times Coca_d) + X'_{imdt}\pi + \alpha_d + \gamma_t + \delta_r \times t + \varepsilon_{imdt} \tag{4}$$

where o_{imdt} is the outcome of interest for individual i in district d born in year t and to mother m, such as health markers at the time of birth or survival status up to a particular age. As before, the controls included here are those of equation 1, survey wave effects, gender and multiple birth dummies, mother's age at first birth, and the set of maternal characteristics described in equation 3.

Note that DHS do no record district of birth. However, these surveys record the number of years mothers have been living in the current place of residence. Using this information, I show evidence that migration is not a problem for identification with DHS data by (i) predicting migrant status with the treatment variable and (ii) re-doing the analysis with the non-migrant sample.

4.2.3 Mechanisms: Income Effects and the Value of Time

I turn to study the mechanisms at play using LSMS data. In particular, I analyze how the price shock affected households' expenditures and labor force allocation. The LSMS survey waves of 1994 and 1997 cover the periods exactly before and after the price drop. LSMS have information on migration for individuals 15 years of age or more at the province level. Thus, I relocate individuals to their pre-shock province of residence. For individuals 14 years of age or younger, I assume they were located in the same province

[18] The dependent variable takes value 1 if the newborn is born alive *and* is of a particular gender. For instance, for females, live births of female babies takes value 1 and zero if there was no birth or the new born was male.

than the household head. Since provinces are larger administrative divisions than districts, I aggregate all district-level variables to the province level. [19]

For an individual or household i in pre-shock location d and year t, I estimate equation 5

$$h_{idt} = \beta(P_t \times Coca_d) + X'_{idt}\pi + \alpha_d + \gamma_t + \delta_r \times t + \varepsilon_{idt} \tag{5}$$

Where h_{idt} is an outcome of interest, and all other variables are defined as in equation 1. I will estimate effects on labor force participation by gender and age group (in school age, 6-17 years, and adults, 18-59 years).[20] In addition, for individual-level regressions, I include age, age squared, and ethnicity fixed effects (proxied by language). In general, I do not include years of education as a control because this is an endogenous variable for individuals in school-age (Dammert, 2008).[21]. Moreover, I do not include any additional controls to those of equation 1 in household-level regressions as household composition may be endogenous.

5 Results

5.1 Missing Children: Mortality In-Utero and After Birth

Table 1 presents results of estimating the relationship between coca revenues and cohort size through equation 1. The dependent variable is the natural logarithm of the population count born in a district-year cell. Thus, the coefficients of interest, β, can be interpreted roughly in percentage terms. To ease interpretation further, I show implied effects for the average coca district and a *price drop* of 5.56 real Peruvian Soles—the price collapse between 1994 and 1995. The first two columns show results for individuals born between 1993 and 1995 (ages 0-3 at the time of the shock) with and without coca-specific time trends. The third column focuses on the adjacent cohorts 1994 and 1995 (ages 0-2 at the time of the shock) and does not include a coca-specific time trend. Results are stratified by gender in each panel: Panel A for all individuals, Panel B for males only, and Panel C for females only. The bottom of the table presents model specifications.

Panel A of Table 1 shows evidence of pro-cyclical cohort size across specifications: the 1995 price collapse is associated with a 0.3 to 0.5 percent smaller cohort for the average coca district. The estimates for

[19] On average, a province encompasses 9 districts

[20] Defining age groups differently do not affect the results significantly.

[21] The results do not change when I include education as a control for individuals in the age group 18-59

the 1993-1995 cohorts with or without a coca-specif trend (columns 1 and 2) are positive and statistically significant. A similar result emerges from the adjacent-cohort comparison (column 3).

The magnitudes are large. Attributing this reduction in cohort size to under 5 mortality, the effects would imply and increase of approximately 6 to 11 percent.[22] Mortality under age 5 is a good benchmark given that, as shown in later sections in an analysis using DHS data, most deaths are taking place in-utero and during the first years of life. As a benchmark, the Peruvian economic crisis of the late 80s, which was associated with a 30% drop in per capita GDP, increased infant mortality in 2.5% (Paxson and Schady, 2005). In addition, note that this estimates are likely a lower bound: not every individual in treatment areas is actually treated, and cohort sizes in the main analysis are constructed using the entire population in each district-year cell. Untabulated results document that the effects are twice as large for districts with above median levels of coca cultivation. A well executed paper by Miller and Urdinola (2010) estimates that coffee price shocks in Colombia are associated with cohort sizes through a *negative* elasticity between -0.01 and -0.04. These effects are large according to Ferreira and Schady (2009) review of the literature. The implied elasticity of the coca price shocks is *positive* and around 0.01. In later sections I come back to discussing the difference in signs between this study and (Miller and Urdinola, 2010).

Table 2 presents the results of estimating equation 2 and studies how *pre-determined* maternal characteristics of the exposed cohorts relate to the price shock using DHS data. The analysis is performed for the 93-95 cohorts (columns 1-3) and 94-95 cohorts (columns 4-5). The table shows estimated coefficients, implied effects, and dependent variables means, for a range of maternal outcomes. Importantly, this is a sample of live births, regardless of their survival status at the time of the survey, linked to maternal outcomes.

The evidence suggests that, conditional on observing a live birth, exposure to the shock is associated with higher maternal SES, if anything. In other words, there are missing live births to lower SES women. Table 2 shows that, out of nine outcomes proxying for demographic and economic characteristics, two are statistically significant: exposed women who gave birth to a child at the time of the shock were more educated and literate. Out of the other seven outcomes, five of them show point estimates that would suggest a similar intuition, but these are not statistically significant: women are older, and older at the time of first birth, with fewer preceding births, and less likely to be the first birth (first births usually have higher probability of complications), and these women are less likely to be born in rural areas).

[22] This calculation is based on a child mortality rate of 47 per thousand individuals under age 5 (Peruvian Demographic and Health Survey, 2000). Then $0.003\times1000/47= 0.063$ and $0.005\times1000/47=0.106$. In terms of the rural child mortality rate of 64 per thousand, these effects are 0.047 and 0.078, respectively.

Panel A of Table 3 shows the results of estimating equation 3 and further confirms that exposure to the shock is associated with missing live births. The estimates are statistically significant across all specifications, including corrections for maternal controls. The implied effect for the average coca district is a *decrease* of 0.56-0.60 points in the probability of birth in a given year, or 4.7% to 5.1% with respect to the mean.

I then establish that the effects are consistent with the survival disadvantage of males both in-utero and in early life documented in biological studies (Drevenstedt et al., 2008; Navara, 2010). First, male fetuses are more likely to be miscarried than female fetuses (Navara, 2010). One could argue that the positive selection in maternal characteristics I find is a product of an anticipated fertility response over those women for which the shock is more relevant: low SES women. On the other hand, if babies are being miscarried, and this is the main factor driven the results, then it is likely that we would observe a gender difference in this pattern.

Separately estimating equation 1 by gender shows strong and statistically significant effects for males but not for females. This is consistent across all specifications. Panel B of table 1 shows the results for the cohorts of males. The implied effect is a reduction of approximately 0.8 to 0.9 percent in cohort size. Panel C shows the results for females; all estimates are statically indistinguishable from zero. The difference between males and females is statistically different.

The evidence on missing live births is also consistent with a gender-specific pattern. Panels A and B of Table 3 show estimates of the price shock stratifying equation 3 by gender. Again, the effects are strong and significant for males and smaller and not statistically different from zero for females. The effects for males and females are statistically different.

Moreover, an additional pattern consistent with miscarriages is that the male-to-female ratio of life births should have decreased over time within the shock year, 1995, relative to control years. This is because male fetuses are more likely to be miscarried at the beginning of pregnancy, while female fetuses are more likely to be miscarried at the end of pregnancy (Orzack et al., 2015). Thus, more male live births should be missing at the end of 1995 because they were miscarried earlier in 1995, while this pattern should be less pronounced for females. I test this hypothesis by fully interacting equation 2 with an indicator variable that takes value 1 for the second half of the year and using male births as an outcome. I focus on the coefficient of the triple interaction between prices, coca intensity, and the indicator for the second half of the year.

The results in Table 4 are consistent with the hypothesis that the likeness of observing male newborns relative to female newborns is *lower* in coca areas when prices are low and during the second half of the year. This is consistent across specifications. This effects are unlikely to be driven by selective abortion. First, abortion is illegal in Peru. Second, even if available informally, it would require a technology that allows mothers in rural Peru to screen the gender of the fetus and then act upon this information. This seems unlikely given the lack of health infrastructure in the country. For instance, only 60 percent of babies born in 1995 in all Peru (including urban districts and the capital region, Lima, which holds one third of the population) were weighed at birth. Third, it seems unlikely that, even if the above conditions are met, (baby boys) and not baby girls would have been the subject of selective abortions. Finally, as I show later, this gender-specif pattern of deaths also takes place after birth, which is consistent with the hypothesis that the price shock affects the weakest (baby boys both in-utero and after birth) more.

Next I analyze if, conditional on in-utero survival, exposure during the year of birth increased mortality rates of children. Live births were, if anything, higher SES according to maternal characteristics. Relative to unexposed newborns and conditional on in-utero survival status, exposed newborns are probably stronger that unexposed newborns had they not been affected by the price drop. Thus, to investigate the effects of exposure to the price drop on child mortality and birth outcomes, I will show results with and without controlling for maternal characteristics.

Table 5 shows that exposure to the price drop also increased mortality after birth. As expected, the impacts are slightly stronger after controlling for maternal characteristics—once sample selection is partially addressed. This finding is consistent with newborns being smaller at birth as reported by their mothers (Table 6).[23]

Overall, the evidence suggests that the price drop is associated with more missing children, particularly boys. The effect is consistent with increased mortality in-utero affecting more males than female fetuses. This male-to-female pattern is grounded in research in biology and reproductive science, as well as in the characteristics of the setting. First, it has been documented that the ratio of males to females at birth decreases under stressful events and adverse conditions both in mammals in general and humans (Navara, 2010). What is more, males have a survival disadvantage both in-utero and in early life relative to females; in general, perinatal conditions, neonatal care, and infectious diseases affect more males than females (see Drevenstedt et al., 2008, and citatios therin). Second, the effect on females may not be large enough to be statistically significant given sample sizes because the male-to-female ratio in rural Peru was already low.

[23]Size at birth is self-reported by mothers as a subjective assessment of the size of their children at the time of birth relative to other children. Weight at birth is not available for over 40% of the sample. Size at birth is only available in the 1996 wave. Thus, a more limited number of districts are covered in the sample

Regular male to females ratios are around 1.05, but rural Peru exhibited 1.02 in 1994 and 1.01 in 1993-1994, the control years in the analysis. This may suggest that the female babies born in the control years were already pretty resilient. In addition, there is little evidence of differential investments in health by gender in Peru (Attanasio et al., 2017), which contrasts with evidence from other countries such as India and China.

5.2 Expenditure and Labor Markets

Table 7 documents that household real expenditure decreased as a result of the price shock, in particular, expenditures in health. Panel A shows results for real expenditure in logs, while Panel B shows results for real expenditure per capita in logs. Across both panels, the evidence suggests that households decreased total expenditure in 5.7 and 7.9 percent. Expenditures in food also decreased, although by a smaller fraction. Changes in food expenditures may affect the nutrition of household members. As a fraction of the total budget, however, food is not decreasing, indicating that households are reallocating their budgets to this category, or that food is part of the home production function and less likely to present overall reductions if there are no changes in inputs. Health expenditures only represent about 5 percent of the total household budget; however, expenditures in this category could be of special importance for vulnerable populations, such as young children, pregnant women, and the elderly. Expenditures in this category suffer an important reduction of 30 to 20 percent.

Table 8 examines the impacts of the shock on labor force participation and total hours worked in columns 1 and 2. Columns 3 and 4 report results for participation in household chores. Although the surveys do not break down this category further, some activities may relate to caregiving and health directly or indirectly (e.g., cooking, cleaning, taking care of young children). The results reported below are consistent with this view.

Individuals in school-age increase their labor supply probably as a mechanism to compensate against the economic shock; the effects are mixed for adults. The strongest impacts are concentrated among females in school age. More young females supply labor, and there is also an increase in the number of hours worked. Note that this is the group with the lowest levels at baseline. Interestingly, they do no decrease their participation in household chores (columns 3 and 4). Previous research in Peru suggests that young females play an important role in the caregiving of younger siblings (Levison, 1998). There is no evidence of more young males supplying labor; however, those already participating in the labor force increase the number of hours worked. This group also show reductions in household chores.

The effects are mixed for adults. More adult females supply their labor as a result of the price shock, but this is not changing significantly the total number of hours supplied. The increase in labor supply is probably related to the observed reduction in hours allocated to household chores. Finally, adult males do not significantly change their labor supply. Most adult males were already working at baseline. If anything, adult males may be working fewer hours, but this is not statistically significant. This group also seems to participate less from household chores because of the shock, but this does not affect the average number of hours allocated into this category.

6 Conclusion

This paper studies how price shocks in the Peruvian cocaine industry affect the health of children in areas that produce coca—the main input for cocaine production. The empirical strategy is based on a difference-in-difference design that exploits variation across baseline levels of coca cultivation across areas and abrupt drops in the price of coca leaves over time due to an anti-drug policy. I find that price drops increase mortality rates among children. A combination of data sets and approaches allows me to establish that survival is compromised, especially for boys, both in-utero and after birth. The increase in mortality is consistent with the behavioral responses of the household. When coca prices drop, household income decreases. To smooth the income shock, individuals in school age and adult females increase their labor supply. Despite this coping strategy, household cannot fully smooth consumption: expenditures in food fall, and expenditures on health drop sharply. In addition to reduced health investments, the increase in labor supply and the reduction of time allocated to household chores is consistent with a setting in which the decrease in the opportunity cost of time is not translated to increases in time-intensive health investments.

The empirical strategy used here addresses important threats to identification. First, to overcome potential endogeneity issues between coca revenues and child health, I use an antidrug policy as a quasi-exogenous source of variation for coca prices over time. Importantly, this policy focused on the interception and shoot-down of narco-airplanes ferrying coca from Peru to Colombia and caused an abrupt decline in coca prices in Peru. Although this policy was later followed by law enforcement efforts on the ground, during the period I analyze, market forces as a result of the negative demand shock for Peruvian coca are the only factors at play. Second, the abrupt fall in coca prices allows me to confidently identify the effects of the price on mortality isolated from confounding factors, such as selective fertility. In particular, I compare cohorts of children that were conceived the year before the shock with older, adjacent cohorts. In addition, the price shock does not predict pre-determined characteristics of mothers, which supports the hypothesis that the policy and subsequently drop in prices was not anticipated.

In addition, the setting and source of shocks allow me to focus on the behavioral responses of the household as the main mechanisms at play. Previous studies using aggregate income shocks, such as financial crises or deviations from the economic cycle, may affect child health through additional causal pathways including the provision of public goods (Ferreira and Schady, 2009). In addition, other sources of shocks commonly used in the literature, such as weather shocks and natural disasters, may affect health directly (Almond and Currie, 2011; Almond et al., 2017; Prinz et al., 2018). This paper isolates the effects of market forces on health through household response.

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Figures

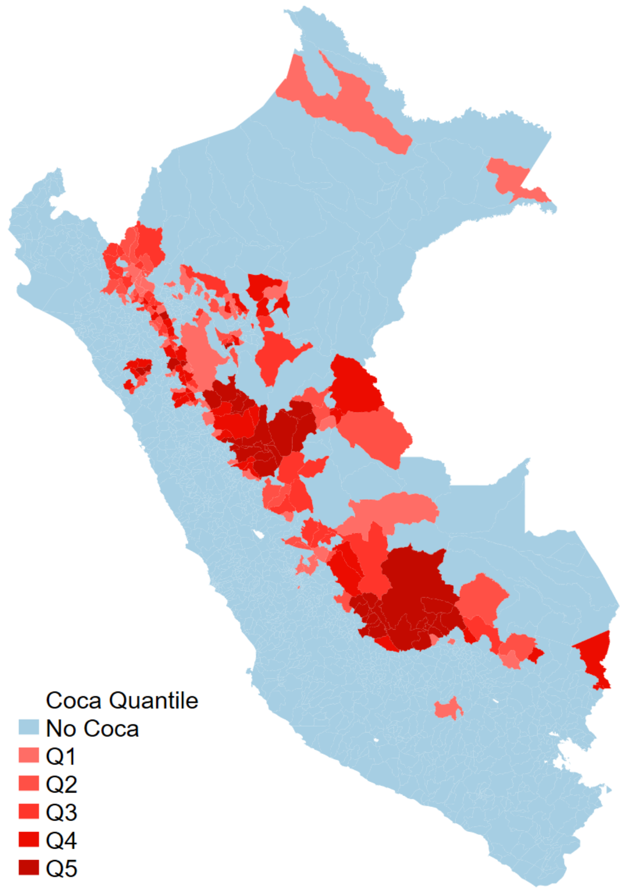

Figure 1: Coca cultivation in 1994

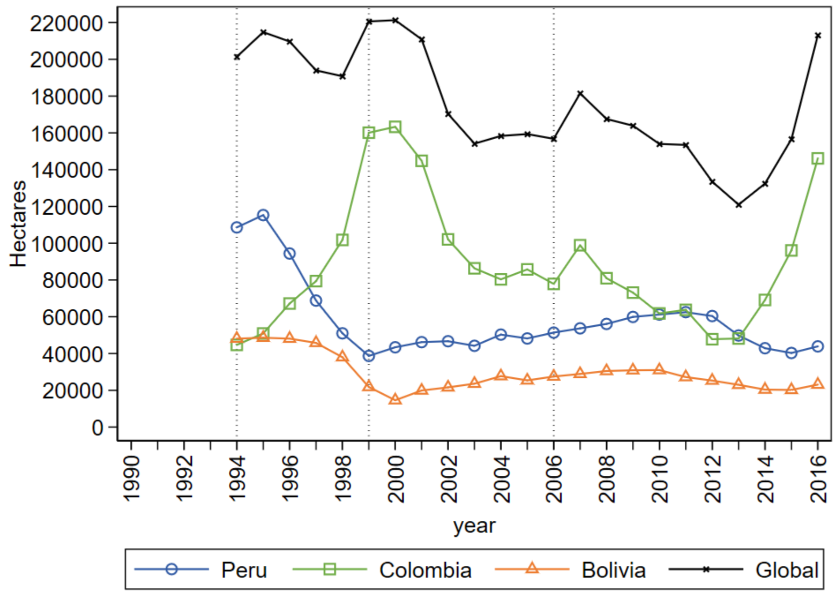

Figure 2: Coca Production by Country

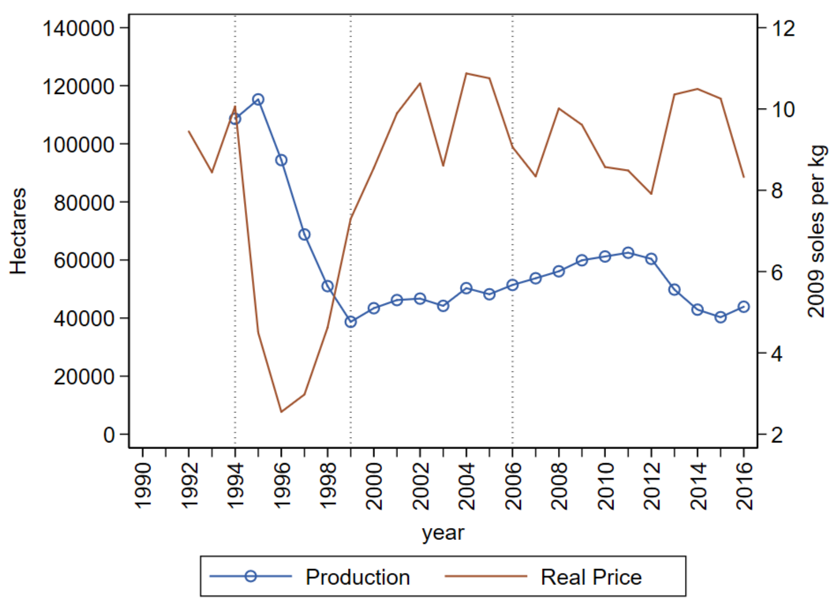

Figure 3: Coca production and real price in Peru

Tables

Table 1: Effects of Coca Price Shocks on Cohort Size

Dependent Variable:	Log cohort size		
Cohorts:	93-95		94-95
Model:	(1)	(2)	(3)
	Panel A: All individuals		
Price at t $\times$ Coca Intensity	0.0065*	0.0079**	0.0044*
	(0.0034)	(0.0036)	(0.0026)
Implied Effect (%)	-0.43	-0.53	-0.29
Districts	1777	1777	1777
Observations	5331	5331	3554
	Panel B: Males		
Price at t $\times$ Coca Intensity	0.0121***	0.0134***	0.0116***
	(0.0040)	(0.0043)	(0.0038)
Implied Effect (%)	-0.81	-0.89	-0.77
Districts	1777	1777	1777
Observations	5331	5331	3554
	Panel C: Females		
Price at t $\times$ Coca Intensity	0.0001	0.0017	-0.0037
	(0.0049)	(0.0050)	(0.0049)
Implied Effect (%)	-0.01	-0.12	0.25
Districts	1777	1777	1777
Observations	5331	5331	3554
Model Specifications:			
District FE	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes
Coca Trend	No	Yes	No

*** $p<0.01$, ** $p<0.05$, and * $p<0.10$. Standard errors clustered at the district level are in parenthesis. Implied effects are calculated for the average coca district (119.95 hectares) and the 1994-1995 price drop (-5.56 real Nuevos Soles).

Table 2: Exposure to Coca Price Shocks and Maternal Characteristics

Treatment:	Price at t × Coca Intensity					
Cohorts:	93-95			94-95		
	(1) Estimate (S.E.)	(2) Imp. Effect	(3) Var. Mean	(4) Estimate (S.E.)	(5) Imp. Effect	(6) Var. Mean
Depedent Variable:						
Mother's age in years at time of birth	-0.027	0.018	27.278	-0.004	0.003	27.272
	(0.199)			(0.188)		
Mother was married at time of birth (=1)	0.035	-0.024	0.661	0.050	-0.033	0.654
	(0.027)			(0.034)		
Mother's age in years at first birth	-0.011	0.007	20.299	-0.013	0.009	20.264
	(0.075)			(0.070)		
Mother's number of preceding births	0.019	-0.013	2.566	0.028	-0.019	2.594
	(0.043)			(0.044)		
Mother's first birth (=1)	0.008	-0.005	0.244	0.008	-0.005	0.243
	(0.005)			(0.005)		
Mother is illiterate (=1)	0.017**	-0.011	0.194	0.013**	-0.009	0.205
	(0.007)			(0.007)		
Mother's years of education	-0.141***	0.094	5.394	-0.100**	0.067	5.361
	(0.046)			(0.043)		
Mother was born in a rural area (=1)	0.011	-0.008	0.399	0.008	-0.005	0.407
	(0.015)			(0.015)		
Mother is a Quechua speaker (=1)	-0.000	0.000	0.174	-0.002	0.001	0.187
	(0.002)			(0.003)		
Model Specifications:						
District FE	Yes			Yes		
Year of Birth FE	Yes			Yes		
Agro Controls	Yes			Yes		
Region Trend	Yes			Yes		
Coca Trend	Yes			No		

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the district level are in parenthesis. Implied effects are calculated for the average coca district (119.95 hectares) and the 1994-1995 price drop (-5.56 real Nuevos Soles).

Table 3: Effects of Coca Price Shocks on Probability of Birth

Dependent Variable:	Live birth (=1)					
Maternal controls:	No Maternal Controls			With Maternal Controls		
Cohorts:	93-95		94-95	93-95		94-95
Model:	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Any birth					
Price at t × Coca Intensity	0.0084***	0.0090***	0.0088***	0.0084***	0.0090***	0.0087***
	(0.0025)	(0.0026)	(0.0024)	(0.0026)	(0.0027)	(0.0024)
Implied Effect	-0.0056	-0.0060	-0.0059	-0.0056	-0.0060	-0.0058
Dep. Var. Mean	0.1187	0.1187	0.1180	0.1187	0.1187	0.1180
Districts	847	847	847	847	847	847
Observations	164835	164835	109890	164835	164835	109890
	Panel B: Male births					
Price at t × Coca Intensity	0.0061***	0.0066***	0.0073***	0.0061***	0.0066***	0.0072***
	(0.0015)	(0.0014)	(0.0015)	(0.0014)	(0.0013)	(0.0015)
Implied Effect	-0.0041	-0.0044	-0.0048	-0.0041	-0.0044	-0.0048
Dep. Var. Mean	0.0595	0.0595	0.0602	0.0595	0.0595	0.0602
Districts	847	847	847	847	847	847
Observations	164835	164835	109890	164835	164835	109890
	Panel C: Female births					
Price at t × Coca Intensity	0.0023	0.0024	0.0015	0.0023	0.0024	0.0015
	(0.0023)	(0.0023)	(0.0024)	(0.0023)	(0.0023)	(0.0024)
Implied Effect	-0.0015	-0.0016	-0.0010	-0.0015	-0.0016	-0.0010
Dep. Var. Mean	0.0592	0.0592	0.0578	0.0592	0.0592	0.0578
Districts	847	847	847	847	847	847
Observations	164835	164835	109890	164835	164835	109890
Model Specifications:						
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes	Yes	Yes	Yes
Maternal Controls	No	No	No	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes	Yes	Yes	Yes
Coca Trend	No	Yes	No	No	Yes	No

*** $p<0.01$, ** $p<0.05$, and * $p<0.10$. Standard errors clustered at the district level are in parenthesis. Implied effects are calculated for the average coca district (119.95 hectares) and the 1994-1995 price drop (-5.56 real Nuevos Soles).

Table 4: Effects of Coca Price Shocks on Male to Female Ratio and Miscarriages

Dependent Variable:	Newborn is male (=1)		
Maternal controls:	With Maternal Controls		
Cohorts:	93-95		94-95
Model:	(1)	(2)	(3)
Price at t $\times$ Coca Intensity $\times$ Second Half of Year	0.0302*	0.0299*	0.0309**
	(0.0158)	(0.0158)	(0.0156)
Implied Effect	-0.0201	-0.0200	-0.0206
Dep. Var. Mean	0.5014	0.5014	0.5109
Districts	836	836	823
Observations	19294	19294	12689
Model Specifications:			
District FE	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes
Maternal Controls	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes
Coca Trend	No	Yes	No

*** $p<0.01$, ** $p<0.05$, and * $p<0.10$. Standard errors clustered at the district level are in parenthesis. Implied effects are calculated for the average coca district (119.95 hectares) and the 1994-1995 price drop (-5.56 real Nuevos Soles).

Table 5: Effects of Coca Price Shocks on Mortality

Dependent Variable:	Child not alive (=1)					
Maternal controls:	No Maternal Controls			With Maternal Controls		
Cohorts:	93-95		94-95	93-95		94-95
Model:	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Males and Females					
Price at t $\times$ Coca Intensity	-0.0063**	-0.0058**	-0.0103***	-0.0080***	-0.0075***	-0.0117***
	(0.0026)	(0.0027)	(0.0034)	(0.0022)	(0.0023)	(0.0034)
Implied Effect	0.0042	0.0039	0.0069	0.0053	0.0050	0.0078
Dep. Var. Mean	0.0680	0.0680	0.0695	0.0680	0.0680	0.0695
Districts	836	836	823	836	836	823
Observations	19294	19294	12689	19294	19294	12689
	Panel B: Males					
Price at t $\times$ Coca Intensity	-0.0071	-0.0063	-0.0123**	-0.0088*	-0.0081*	-0.0144**
	(0.0045)	(0.0047)	(0.0061)	(0.0045)	(0.0046)	(0.0062)
Implied Effect	0.0047	0.0042	0.0082	0.0059	0.0054	0.0096
Dep. Var. Mean	0.0753	0.0753	0.0770	0.0753	0.0753	0.0770
Districts	812	812	783	812	812	783
Observations	9779	9779	6529	9779	9779	6529
	Panel C: Females					
Price at t $\times$ Coca Intensity	-0.0047	-0.0045	0.0026	-0.0090	-0.0087	-0.0001
	(0.0100)	(0.0100)	(0.0089)	(0.0111)	(0.0111)	(0.0121)
Implied Effect	0.0031	0.0030	-0.0018	0.0060	0.0058	0.0001
Dep. Var. Mean	0.0608	0.0608	0.0617	0.0608	0.0608	0.0617
Districts	813	813	766	813	813	766
Observations	9515	9515	6160	9515	9515	6160
Model Specifications:						
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes	Yes	Yes	Yes
Maternal Controls	No	No	No	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes	Yes	Yes	Yes
Coca Trend	No	Yes	No	No	Yes	No

*** $p<0.01$, ** $p<0.05$, and * $p<0.10$. Standard errors clustered at the district level are in parenthesis. Implied effects are calculated for the average coca district (119.95 hectares) and the 1994-1995 price drop (-5.56 real Nuevos Soles).

Table 6: Effects of Coca Price Shocks on Size at Birth

Dependent Variable:	Small at Birth (=1)					
Maternal controls:	No Maternal Controls			With Maternal Controls		
Cohorts:	93-95		94-95	93-95		94-95
Model:	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Males and Females					
Price at t × Coca Intensity	-0.0233***	-0.0233***	-0.0204***	-0.0240***	-0.0241***	-0.0188**
	(0.0048)	(0.0047)	(0.0070)	(0.0050)	(0.0049)	(0.0076)
Implied Effect	0.0155	0.0156	0.0136	0.0160	0.0161	0.0125
Dep. Var. Mean	0.2231	0.2231	0.2215	0.2231	0.2231	0.2215
Districts	476	476	472	476	476	472
Observations	10246	10246	6756	10246	10246	6756
	Panel B: Males					
Price at t × Coca Intensity	-0.0280***	-0.0283***	-0.0238***	-0.0314***	-0.0318***	-0.0238***
	(0.0104)	(0.0106)	(0.0054)	(0.0118)	(0.0119)	(0.0069)
Implied Effect	0.0187	0.0189	0.0159	0.0210	0.0212	0.0159
Dep. Var. Mean	0.2038	0.2038	0.1991	0.2038	0.2038	0.1991
Districts	460	460	446	460	460	446
Observations	5192	5192	3506	5192	5192	3506
	Panel C: Females					
Price at t × Coca Intensity	-0.0115	-0.0117	-0.0009	-0.0134	-0.0134	0.0082
	(0.0157)	(0.0157)	(0.0308)	(0.0166)	(0.0168)	(0.0306)
Implied Effect	0.0077	0.0078	0.0006	0.0089	0.0089	-0.0054
Dep. Var. Mean	0.2425	0.2425	0.2459	0.2425	0.2425	0.2459
Districts	467	467	447	467	467	447
Observations	5054	5054	3250	5054	5054	3250
Model Specifications:						
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year of Birth FE	Yes	Yes	Yes	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes	Yes	Yes	Yes
Maternal Controls	No	No	No	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes	Yes	Yes	Yes
Coca Trend	No	Yes	No	No	Yes	No

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the district level are in parenthesis. Implied effects are calculated for the average coca district (119.95 hectares) and the 1994-1995 price drop (−5.56 real Nuevos Soles).

Table 7: Effects of Coca Price Shocks on Household Real Expenditure

	Real Expenditure		
Expenditure Category:	Total (1)	Food (2)	Health (3)
	Panel A: Log Expenditure		
Price at t × Coca Intensity	0.0343**	0.0177**	0.1917***
	(0.0159)	(0.0087)	(0.0666)
Implied Effect (%)	-5.72	-2.95	-31.98
Dep. Var. Mean	17937.40	7980.28	945.48
Provinces	139	139	139
Observations	7453	7453	7453
	Panel B: Log Expenditure Per Capita		
Price at t × Coca Intensity	0.0475**	0.0309**	0.1278**
	(0.0223)	(0.0125)	(0.0534)
Implied Effect (%)	-7.92	-5.16	-21.32
Dep. Var. Mean	4259.49	1798.36	221.56
Provinces	139	139	139
Observations	7453	7453	7453
	Panel C: Budget Shares		
Price at t × Coca Intensity	. . .	-0.0073	0.0037***
	. . .	(0.0051)	(0.0012)
Implied Effect	. . .	0.0122	-0.0062
Dep. Var. Mean	. . .	0.5305	0.0471
Provinces	. . .	139	139
Observations	. . .	7453	7453
Model Specifications:			
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the province level are in parenthesis. Implied effects are calculated for the average coca province (300.75 hectares) and the 1994-1995 price drop (-5.56 real Nuevos Soles).

Table 8: Effects of Coca Price Shocks on Labor Margins by Age and Gender

Type of Work:	Market Work		Household Work	
Dependent Variable:	Any work (=1)	Total Hours	Any work (=1)	Total Hours
	(1)	(2)	(3)	(4)
	Panel A: Adult Males (18-59 years)			
Price at t × Coca Intensity	-0.0064	0.5969	0.0208***	0.0818
	(0.0054)	(0.4037)	(0.0071)	(0.1052)
Implied Effect	0.0107	-0.9956	-0.0347	-0.1364
Dep. Var. Mean	0.8423	42.6093	0.6569	7.3331
Provinces	139	139	139	139
Observations	8887	8887	8887	8887
	Panel B: Adult Females (18-59 years)			
Price at t × Coca Intensity	-0.0174*	-0.5655	0.0012	1.2903*
	(0.0102)	(0.3688)	(0.0016)	(0.6998)
Implied Effect	0.0290	0.9432	-0.0021	-2.1522
Dep. Var. Mean	0.5391	20.7156	0.9567	34.0830
Provinces	141	141	141	141
Observations	9639	9639	9639	9637
	Panel C: Males in school age (6-17 years)			
Price at t × Coca Intensity	-0.0047	-0.6187***	0.0322***	0.3309**
	(0.0134)	(0.2362)	(0.0044)	(0.1501)
Implied Effect	0.0078	1.0319	-0.0537	-0.5520
Dep. Var. Mean	0.2620	5.9355	0.7018	7.7000
Provinces	132	132	132	132
Observations	5524	5523	5524	5524
	Panel C: Females in school age (6-17 years)			
Price at t × Coca Intensity	-0.0347***	-1.3053***	-0.0000	-0.1733
	(0.0092)	(0.1664)	(0.0081)	(0.2064)
Implied Effect	0.0580	2.1772	0.0001	0.2891
Dep. Var. Mean	0.1761	3.6901	0.8268	12.9886
Provinces	130	130	130	130
Observations	5416	5416	5416	5415
Model Specifications:				
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Ind Controls	Yes	Yes	Yes	Yes
Agro Controls	Yes	Yes	Yes	Yes
Region Trend	Yes	Yes	Yes	Yes
Coca Trend	No	No	No	No

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the province level are in parenthesis. Implied effects are calculated for the average coca province (300.75 hectares) and the 1994-1995 price drop (-5.56 real Nuevos Soles).

Table 9: Effects of Coca Price Shock on Cohort Size – Robustness to 1993 controls

Dependent Variable:	Log cohort size					
Cohorts:	93-95					
Sample:	All Individuals		Males Only		Females Only	
	(1)	(2)	(3)	(4)	(5)	(6)
	PANEL A: BASELINE SPECIFICATION					
Price at t × Coca Intensity	0.0065*	0.0079**	0.0121***	0.0134***	0.0001	0.0017
	(0.0034)	(0.0036)	(0.0040)	(0.0043)	(0.0049)	(0.0050)
Districts	1777	1777	1777	1777	1777	1777
Observations	5331	5331	5331	5331	5331	5331
	PANEL B: ADDING 1993 CENSUS CONTROLS					
Price at t × Coca Intensity	0.0060*	0.0072*	0.0119***	0.0130***	-0.0013	0.0000
	(0.0035)	(0.0037)	(0.0044)	(0.0046)	(0.0050)	(0.0051)
Districts	1777	1777	1777	1777	1777	1777
Observations	5331	5331	5331	5331	5331	5331

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors clustered at the district level are in parenthesis. Columns 1 to 6 from Panel A replicate Table 1. Panel B shows results for each column after adding the following 1993 census controls interacted with year fixed effects: log population, male to female ratio, share rural, share Quechua, shares by age groups (0-14, 15-49, 50+), average years of education (for population 21+), and share unemployed (for population 6+).