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

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Abstract

This paper documents a causal relationship between variation in agricultural commodity prices and child mortality and sheds light on causal pathways. We exploit a natural experiment generated by an aggressive anti-coca policy in the mid-1990s in Peru, which substantially cut demand channels and resulted in a sudden collapse of the price of coca. Inferring excess mortality from "missing children" in the 2007 Population Census and exploiting variation in coca cultivation across districts, we find that the abrupt 50% price drop increased child mortality by 6%-11%. Using supplementary survey data with detailed birth records, we find that deaths occurred both in utero and during early life. We also use household surveys collected before and after the shock to establish two potential mechanisms: lower consumption of health-promoting goods and a reduction in time-intensive health investments as mothers allocate more time to work to compensate for income losses. We show evidence that health investments are reduced during pregnancy and in the first years of life. Interestingly, the effects are different in low-poverty areas, in which mothers decrease their labor supply due to the negative price shock, and child survival is unaffected by income loss. Our findings suggest different predictions for the effects of commodity price shocks on child mortality for developed and developing settings.

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1 Introduction

Developing countries exhibit greater income volatility and more severe aggregate income shocks (Koren and Tenreyro, 2007). In particular, price volatility of agricultural commodities can affect the livelihoods of millions of low-income households in producing sites: 65% of poor working adults in the developing world depend on agriculture (Castaneda et al., 2016).[1] Designing effective policies to protect households from commodity price volatility requires understanding the various dimensions of welfare at risk.

This paper aims to rigorously understand the impacts of commodity price shocks on child mortality. Influential studies have documented a negative association between fluctuations in aggregate income and child mortality in developing countries (e.g. Baird et al., 2011; Pritchett and Summers, 1996). Despite considerable progress, the causal effect of aggregate income shocks on child and infant mortality in the developing world remains an open question. Most empirical evidence comes from studies that use economic recessions or economic cycles as sources of variation for aggregate income. This avenue of research has generated mixed results, with positive, negative, and null relationships (Ferreira and Schady, 2009; Pérez-Moreno et al., 2016; Van den Berg et al., 2017, 2020). In particular, these studies face two challenges. First, it is difficult to credibly separate the effects of income on mortality from anticipated behavioral responses that affect the composition of births, such as selective fertility. Second, economic cycles and crises may affect a multiplicity of causal pathways, such as the quantity and quality of the supply of health care as well as the demand for health inputs.[2]

We exploit a natural experiment to study how a negative price shock in the Peruvian coca industry affected children's mortality rates in coca-producing areas. Coca leaves are the primary input for cocaine production. Since coca cultivation only takes place in the eastern side of the Andean region due to specific agronomic conditions, it is the objective of various anti-drug policy interventions that aim to curb the supply of cocaine (FAO, 2007). We exploit variation in the price of coca leaves induced by a program promoted by the U.S. that sought to cut the demand channels of this commodity.[3] The program included a shoot-down policy against narco-airplanes transporting coca exports from Peru to Colombia and resulted in an abrupt 50% drop in the price of coca leaves in Peru in a single year.

Fluctuations in the price of coca leaves can affect child health and mortality through two causal

[1] Agricultural commodity price volatility has increased over time. (FAO et al., 2011).

[2] Other aggregate shocks used in the literature may affect health through additional causal pathways. For instance, natural disasters may affect health and mortality directly, shocks to the agricultural sector may compromise the availability of food for rural households, and droughts and floods may have implications for the overall disease environment.

[3] Coca leaves are harvested and then sun dried before being sold. Throughout the text, we refer to the price of sun-dried coca leaves, the price of coca leaves, or the price of coca, interchangeably.

behavioral responses. The overall effect is theoretically ambiguous. The production function of child health depends on two arguments: health-promoting goods (e.g., nutritious food and medicines) and time allocated to health-promoting activities (e.g., taking children to preventive health visits, collecting clean water, and caregiving more broadly). In the presence of market imperfections that do not allow households to fully smooth consumption, households in coca-producing sites may reduce their consumption of health-promoting goods when coca prices fall. This would negatively affect child health. However, if the price decrease is translated into lower agricultural wages, caregivers may supply less labor and increase their time allocation to health-promoting activities. This would positively affect child health. In the end, the total effect of the reduction in the price of coca child health and mortality is theoretically ambiguous.[4]

Using the abrupt price decline generated by the policy and variation in baseline coca cultivation across districts in Peru, we employ a difference-in-differences design that allows us 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 isolating the effect of the price shock on mortality from changes in the composition of cohorts; for instance, those driven by selective fertility. We take advantage of the abrupt drop in coca prices to credibly isolate the effects from changes in selective fertility and other confounding factors. The analysis compares the first cohort exposed to the shock—and conceived the year before the shock—with adjacent, older cohorts using information on year and district of birth.

We use census data to infer mortality rates from relative cohort sizes (Jayachandran, 2009; Miller and Urdinola, 2010). In particular, we use the full 2007 Peruvian Population Census to construct population counts by district and year of birth 13 years after implementation of the policy. With these counts, we infer deaths from "missing" individuals at the time of the census. Our measure of (cumulative) mortality has several advantages over survey data, including the ability to capture fetal deaths and delayed mortality. Moreover, this strategy allows us to measure mortality with wide geographic coverage when reliable vital records are not available in a developing country setting.

This paper finds that the 50% price cut associated with the policy caused a reduction in cohort size of 0.43% to 0.53% for the average coca district. This difference in cohort size is equivalent to a 6% to 11% increase in mortality under five years of age. Moreover, we shed light on the timing of deaths by exploiting detailed information on birth histories and mortality records from the Peruvian Demographic and Health Surveys (DHS). The study finds that deaths occur both in utero and during the first years of life. The shock is first associated with an increase in miscarriages,

[4] 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).

as suggested by an extension of our cohort-size approach in which we compare the probabilities of women giving birth in a given district and year. That is, we infer deaths in utero by studying "missing" live births. Moreover, our analysis also finds that infants affected by the price collapse are more likely to die during the first years of life. .

Our main results are unlikely to be driven by factors other than the price shock. First, we adapt our main empirical specification to study pre-trends through an event study framework and find no evidence of behavioral changes in anticipation of the price shock. Second, ex post migration is unlikely to play any role. The main analysis exploits an abrupt change in coca prices and defines exposure based on district and year of birth (and not district of residence at the time of the survey). Third, our analysis limits the influence of selective fertility. Our identification strategy is restricted to cohorts 0-3 and 0-2 years of age at the time of the shock. Thus, the exposed cohort was born during the price shock and (mostly) conceived the year before. By not including cohorts conceived after the shock, the analysis limits the influence of selective fertility. Fourth, we find that the effects are larger for male babies, which is consistent with the survival disadvantage of males relative to females both in utero and in early life (Drevenstedt et al., 2008; Navara, 2010). It is unlikely that a confounding factor could explain this gender-specific pattern, especially given the limited abortion and gender screening technology at the time. Fifth, the results are consistent across several specifications and robustness checks, including controlling for coca-specific time trends and a large set of baseline covariates interacted with year fixed effects.

We find that deaths increase because households reduce their consumption of health-promoting goods, and mothers reallocate time from caregiving to paid work. We shed light on the main causal pathways by exploiting household survey data collected right before and after the collapse of the price of coca leaves. As a result of the price drop, households reduce expenditures, including food and health. Moreover, women are more likely to supply labor when prices are low, consistent with a framework in which female labor is used as a consumption-smoothing mechanism (Jayachandran, 2006; Beck et al., 2019). The increase in working hours reduces the time allocated to household chores and child health. In low-income settings, an increase in maternal employment can deteriorate infant health and increase child mortality (Chari et al., 2019; Debela et al., 2021). Consistent with these two channels, we find evidence of reductions in health investments both during pregnancy (e.g., prenatal appointments) and the early years of the child's life (e.g., taking a child to the doctor when sick).

However, the negative price shock does not result in excess mortality in wealthier areas. Additional analysis that interacts our treatment variable with a baseline poverty measure at the local level suggests that the price shock generates different behavioral responses depending on local economic development: Women in poor locations increase their labor supply as response to the reduction in prices. In contrast, women in wealthier locations decrease their labor supply as response to the reduction in the opportunity cost of time. Mortality does not increase in locations that are better off, likely as a result of enhanced consumption smoothing and hand-in-hand increases in the availability of maternal time (Miller and Urdinola, 2010; Van den Berg et al., 2017, 2020).

This paper contributes to the literature on aggregate income shocks and child mortality by addressing endogeneity concerns, establishing the timing of deaths, and shedding light on key causal pathways (Ferreira and Schady, 2009). Our paper builds on studies that analyze the effects of aggregate economic fluctuations by identifying the effects of income shocks independently from changes in the public sector provision of health services and behavioral responses, such as migration and selective fertility. In addition, we offer two explanations consistent with the increases in child mortality: Households reduce their consumption of health-promoting goods and mothers increase their labor supply, which reduces the time available to allocate to the production of child health. This is consistent with a developing economic context, in which credit constraints and other market imperfections render child health vulnerable to income losses (Bhalotra, 2010). Moreover, our results for richer areas in our setting are aligned with evidence from more developed countries (Miller and Urdinola, 2010).

In particular, our paper contributes to the literature that investigates the impact of commodity price fluctuations on welfare in producer developing countries. Price fluctuations not only shape the economic growth of commodity-dependent countries (Benjamin and Deaton, 1993; Addison et al., 2016) but also impact household welfare. While the influence of acute price variations on consumption patterns and labor choices has been widely documented (Deaton, 1999), less is known about how they shape other dimensions of welfare with long-lasting effects. Price fluctuations influence school choices (Kruger, 2007; Cogneau and Jedwab, 2012) as well as children's physical and mental health (Adhvaryu et al., 2019; Mekasha et al., 2022). Our paper not only documents the impact of commodity price cuts on child mortality, but more importantly, we show how this effect is consistent with the observed changes in labor and consumption patterns.

This paper also relates to the literature on natural resources. Although natural resources are salient in developing countries, in which health is more vulnerable, studies on the relationship between resource booms and busts and child mortality are scarce (for a review, see Aragón et al., 2015). More broadly, this study relates to the literature on early-life shocks (Almond and Currie, 2011; Almond et al., 2017; Currie, 2009; Currie and Almond, 2011; Prinz et al., 2018)[5] and the more general two-way relationship between economic development and health (Deaton, 2007; Strauss and Thomas, 1998, 2007).[6]

[5] In particular, previous literature has documented the relationship between rainfall shocks and investment in children with long-lasting effects (Jensen, 2000; Hoddinott and Kinsey, 2001; Maccini and Yang, 2009). Unlike this literature, changes in the provision of public health services is not a concern when exploring mechanisms.

[6] This paper also contributes to a growing number of studies on the consequences of booms and bursts of illegal economies. We show that economic downturns in drug industries may have negative effects on the well-being of

The paper is organized as follows. Section 2 describes the natural experiment and coca production in Peru. Section 3 presents the main datasets and the empirical strategy. We report our main results in Section 4, including robustness checks. Mechanisms and heterogeneity analysis are presented in Sections 5 and 6, respectively and Section 7 concludes.

2 Peru's anti-coca policy: a natural experiment

2.1 The collapse of the farm-gate coca prices

This study exploits a sudden and pronounced commodity price shock that affected Peruvian communities whose economies depend on coca cultivation. In 1995, the Peruvian government implemented a counter-narcotic policy that significantly reduced the farm-gate prices of coca and caused an unprecedented initial drop of 50% from 1994 to 1995. The policy was in effect from 1995 to 1999. This period is referred to as the "crisis of coca".

Peru was the largest producer and exporter of coca in the 1980s and early 1990s. Due to agronomic conditions, coca is cultivated in only three countries in the world: Bolivia, Colombia, and Peru (UNODC, 2007). By the mid-1990s, Peru grew three times more coca than Colombia and Bolivia, reaching levels of around 120 thousand hectares a year (Figure 1). During this period, Peruvian coca would be ferried to Colombia in the form of coca paste, usually using small narco-airplanes and improvised runways in the Amazon jungle. Once in Colombia, it would be transformed into cocaine and exported to final consumer markets.

In 1995, an aggressive policy that aimed to cut the supply chain for cocaine in the Andean region caused an abrupt and negative demand shock to the Peruvian coca industry (Angrist and Kugler, 2008; Dammert, 2008). Among other efforts, the Peruvian and Colombian Air Forces implemented the "air-bridge denial" policy, in which aircraft suspected of ferrying coca that did not follow force landing commands were shot down. The operation was conducted in cooperation with the U.S. government, which provided funding, aircraft, and ground radars.[7]

For Peru, this policy generated an abrupt collapse in the price of coca. Figure 2 shows the evolution of the price over time in real terms. In a single year, from 1994 to 1995, the price dropped about 50% and remained low until 1999, when a Peruvian Air Force plane was involved

vulnerable populations. This adds to a growing body of work documenting other negative consequences of drug booms (Angrist and Kugler, 2008; Sviatschi, 2022). Dammert (2008) analyzes the same policy intervention we study in this paper, and establishes that households respond with increased child labor during economic downturns in the coca industry.

[7]U.S. cooperation was authorized on December 8, 1994, by President Bill Clinton's Presidential Determination and its Memorandum of Justification (CIA, 2008). In general, this strategy followed the shift in U.S. interdiction and seizure efforts from Central America to source countries in South America and the overall militarization of the war on drugs (Zirnite, 1998). Although similar strategies existed in the 1980s and 1990s, none had the intensity of efforts carried out in 1995 and later years.

in an accident. The accident raised concerns about the operation's safety protocols, and the policy became less stringent. In 2001, the program was suspended after a civil airplane with a U.S. missionary and her daughter was shot down by mistake. Overall, the policy was associated with a sizable reduction in coca production in Peru and an increase in Colombia, which left the total amount produced between 1994 and 2001 virtually unchanged (Figure 1). [8]

2.2 The Peruvian coca industry

Coca (*Erythroxylum coca*) requires specific environmental conditions for cultivation. 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 and 2,100 millimeters a year, temperatures between 17 and 23 degrees Celsius, very bright sunlight intensity, and altitudes between 600 and 2,000 meters above sea level with slopes of around 20 degrees (FAO, 2007; UNODC, 2003).

Because of climatic suitability, there is substantial variation in coca cultivation levels across localities in Peru. Figure 3 shows districts —Peru's smallest administrative units— by level of coca cultivation as of the 1994 Agricultural Census. Of over 1,700 districts, 193 (11%) of them cultivated coca .[9] It is easy to notice that most coca-producing districts are located to the right of an almost perfect diagonal line: coca districts are located on the eastern side of the Andes Mountains, where the climate is suitable for cultivation. Sviatschi (2022) establishes that cultivation levels are positively and strongly related to climatic conditions favorable for coca farming.

Coca-producing districts tend to be less developed than districts where coca is not produced. Table 1 presents the main characteristics of coca districts vis-à-vis non-producing districts in 1993-1994, before the price shock.[10] Although on average the two sets of districts have similar population sizes and poverty levels, coca districts show some signs of underdevelopment.[11] Coca districts are more rural and depend more heavily on agriculture. A larger share of land in coca districts is allocated to cash crops (42.7% vs. 29.9%). Moreover, working-age males are more likely to participate in the labor force in coca districts, where agriculture employs 75% of working males relative

[8] Coca production is strongly determined by local climate conditions. Typically, national-level policies to reduce coca production do not generate cultivation spill-overs across areas within the same country but across neighboring countries. For example, Rozo (2014) finds that eradication efforts in Colombia did not move production to other areas within the country. Similarly, Mejia and Restrepo (2016) predict that Plan Colombia, a nationwide strategy to control coca production in Colombia, generated spillovers only across countries.

[9] Bolivia and Colombia also exhibit spatial concentration of coca cultivation in particular areas (see Illicit Crop Monitoring Reports for each country, e.g. 2002; 2005a).

[10] It is important to note that district boundaries were mostly constant over the period of analysis. However, to ensure a correct match across data sources, all districts were transformed into the 1993 administrative division. To this end, we use information from National Decrees published in the official newspaper *El Peruano*. Less than 5% of districts underwent boundary changes in the period under analysis.

[11] Poverty headcount is only measured at the province level for 1994 (Escobal and Ponce, 2016).

to 64% in non-coca districts. The same is not true for females, whose labor force participation rate and employment in agriculture percentage is similar across coca and non-coca districts. The population in coca producing district is also less educated, which is the case for both men and women.

Small-scale farmers produce coca as a source of monetary income.[12] [13] It is estimated that over 200,000 households had an economy based on coca farming or related activities in 1995, the year the anti-drug policy studied in this paper was enacted (DEVIDA, 2004). Coca plots tend to be under 1 hectare. The plant is ready for harvest after one year, with regular harvests occurring 3-5 times a year for about 20 years. The harvest consists of manually picking the leaves. After harvest, the leaves are dried under the sun and sold to intermediaries for coca paste production. This process is done locally to reduce the product's volume and weight.[14] Coca paste would then be smuggled via roads and rivers and ferried to Colombia, often by small narco-airplanes (IDL, 2012).[15] Studies on the most important coca-producing regions estimate that around half of the households' income was from coca farming (Bedoya, 2003; DEVIDA, 2013).

3 Data and empirical strategy

We use the sudden collapse of the price of coca leaves and variation in cultivation levels across districts in Peru as a source of quasi-exogenous variation to study how agricultural commodity price shocks affect child mortality.

3.1 Data

This paper uses three main sources of information: (i) agricultural census data and coca prices, to construct a measure of coca revenues at the district level; (ii) population census data, to measure cohort sizes by district and year of birth (our main measure of mortality); and (iii) household

[12] 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, but 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). 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 about 90% of the total amount produced of coca is sold to the illicit market (UNODC, 2003).

[13] This paragraph draws heavily from DOJ (1991), García Díaz and Stöckli (2014), and UNODC (2005b).

[14] This step requires some investments: "micro labs" (or maceration pits) and some additional inputs such as kerosene.

[15] One hectare of coca can produce about 2.2 tons of sun-dried coca leaves a year (World Drug Report 2010). In terms of 2009 prices, one hectare generates U.S. 6,600 dollars in revenues for the farmer (about two times the annual minimum wage). 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 paste (World Drug Report 2010).

surveys, to get direct measures of children's health, health investments, time allocation choices, labor market outcomes, and consumption patterns.

To construct an estimate of potential coca revenues at the district level, we combine baseline cultivation levels of coca by district with yearly prices on sun-dried coca leaves in Peru. Baseline coca cultivation is obtained from the 1994 Agricultural Census. We construct cultivation levels by district, measured as the total area covered by coca crops in thousands of hectares. The more hectares of coca cultivated in a district, the greater its exposure to the commodity price shock. Since cultivation is measured before the shock, our exposure measure is not endogenous to the commodity price shock. Yearly farm gate coca prices are collected by the United Nations Office on Drugs and Crime and reported in U.S. dollars.[16] We transform this price sequence to 2009 real Nuevos Soles for the analysis.[17]

We then use population census data to construct cohort sizes by year and district of birth cells. Cohort sizes will be used as an indirect measure of cumulative survival (mortality) (Jayachandran, 2009; Miller and Urdinola, 2010). We use the full 2007 Peruvian Population Census to construct these cohorts. Later, in the empirical specification section, we establish the conditions under which cohort sizes can be interpreted as cumulative survival.

In addition, we use household survey data to get additional measures of child mortality, health investments, and household behavioral responses. First, we use Peru's 1996 and 2000 DHS to draw detailed data on births and direct measures of child mortality and health investments. The DHS records information on birth histories, child mortality, maternal characteristics, and women's and children's health investments for nationally representative samples of women ages 15-49. We use these surveys to infer how the shock is associated with mortality in utero and after birth. Second, we 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 household expenditure and labor decisions were affected by the shock.

3.2 Empirical strategy

3.2.1 Cohort size

We first analyze how variation in coca prices generated by the policy impacts cohort size. Using the full 2007 Population Census, we construct population counts by year and district of birth (i.e. cohort sizes), which we link to our measure of coca revenues that combines yearly prices of sun-dried coca leaves and baseline coca cultivation intensity by district.

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

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

In particular, we estimate equation 1,

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

where $CohortSize_{dt}$ is the number of individuals born in district d and year t from the 2007 Population Census. The measure of coca revenue, $P_t \times Coca_d$, is the product of the year-of-birth price of sun-dried coca leaves in real soles per kg, P_t, and a district-of-birth measure of coca intensity, $Coca_d$, which is defined as the total area of cultivated land with coca crops in thousands of hectares. To construct this measure, we use the 1994 Agricultural Census, which precedes the 1995 price shock (Sviatschi, 2022).[18]

The estimation is conditioned on a set of covariates. District-of-birth 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 (*departamentos*) in Peru, and many policies are executed at this level.[19] These trends partial out factors that linearly change over time in each state. The vector X'_{dt} represents year-of-birth effects interacted with a set of agricultural controls, including the district-level intensity of cacao and coffee which are measured in thousands of hectares in the 1994 Agricultural Census. Cacao, Coffee, and Coca are grown under similar climatic conditions. The interactions control for the volatility of these commodities beyond variation in international prices, such as the variation coming from pests and other unobserved variables. In addition, to control for other changing conditions in the agricultural sector, we control for the total area of cultivated land as of the 1994 Agricultural Census in each district interacted with year-of-birth fixed effects.

The coefficient of interest is β, which can be interpreted as the percentage change in cohort size or excess cumulative mortality for an additional unit of coca revenues. With $CohortSize_{dt}$ defined as the number of individuals who survived until the 2007 Population Census, *decreases* in the price of coca imply smaller cohort sizes (excess cumulative mortality) if $\beta > 0$. Alternatively, $\beta < 0$ implies that price drops lead to larger cohorts (increased cumulative survival). This empirical strategy is a difference-in-differences model that compares cohorts of individuals born in years of high prices with cohorts of individuals born in years of low prices across districts with different levels of coca intensity.

To correctly interpret β as the impact of coca revenues on cohort size, we restrict the analysis to a narrow time window: 1993 to 1995 cohorts. Thus, in the analysis we use the population of individuals ages 0-3 at the time of the shock and who survived until the 2007 Population Census.[20]

[18] Sviatschi (2022) uses the same measure of coca intensity to show that increases in coca revenues result in the accumulation of human capital specific to illicit activities.

[19] 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 1,800 districts.

[20] The 2007 Population Census does not record date of birth directly but rather age in years as of Census day (October

We exploit the 1995 abrupt price decline and compare the 1995 cohort with immediately preceding cohorts for two reasons. First, the 1995 cohort was the first cohort exposed to the shock and was (mostly) conceived in 1994, before the shock. Including later cohorts could complicate interpretation of the results, as changes in cohort size after the abrupt price drop could include a combination of mortality and compositional effects, such as selective fertility or selective migration. Second, we use 1993-1994 cohorts as a comparison group, since these individuals and those born in 1995 are likely to have had very similar life-cycle events in the absence of the shock. As an additional check, we restrict the analysis to the population born in 1994-1995 (ages 0-2 at the time of the shock).[21] When performing analysis on the 1993-1995 cohorts, we show results with and without a coca-specific linear trend of the form $\delta \times 1\left[Coca_d > 0\right]$ for robustness. Conditional on this trend, changes in coca revenues are identified as deviations from the average evolution of coca districts over time.

Moreover, we find evidence that the shock was not anticipated. Anticipation of the shock could lead, for instance, to selective fertility, generating a spurious comparison across cohorts. Thus, we first adopt an event study framework that adapts equation 1 to estimate the impacts of coca revenues interacted with years of birth to show that there are no impacts on cohort sizes in anticipation of the 1995 price drop. Second, we draw from studies on the survival disadvantage of male infants, both in utero and in early life, to predict differential mortality rates between males and females by estimating equation 1 stratified by gender (Drevenstedt et al., 2008; Navara, 2010). If the main force that explains excess mortality in the data is a behavioral response that results in the avoidance of pregnancy or a live birth, this would affect male and female fetuses equally.[22]

3.2.2 Mortality in utero and after birth

We then explore the timing of deaths by studying whether additional mortality occurs in utero, after birth, or both. To this end, we exploit microdata from the 1996 and 2000 DHS. This survey records the histories of all live births (and survival status) to all women ages 15-49 at the time of the survey.

21, 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 on October 21) is matched to prices during months 2 to 14 of age. On the other hand, the youngest individual of the census cohort (born on October 20) is matched to prices during months -9 to 2 of age. We do not find evidence of age stacking. The cohorts under analysis are 12-14 years of age at the time of the census.

[21] We also tested for alternative definitions of control cohorts with no important changes in the results.

[22] Abortion is not legal in Peru, and even if available illegally 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 during this time period. For instance, only 60% of babies born in 1995 in Peru were weighed at birth. This estimate includes urban districts and the capital region, Lima, which holds approximately one-third of the population. Moreover, it seems unlikely that even if gender-specific abortions were taking place in a systematic matter in anticipation of the price drop, female and not male babies would have been preferred.

First, we test whether exposure to the price collapse results in an increase in fetal mortality. For instance, worsened nutritional conditions and stress can be associated with unexpected termination of pregnancy (Beydoun and Saftlas, 2008; Navara, 2010). Unfortunately, reliable data on miscarriages since the time of conception are rarely available.[23] Thus, we focus on changes in the probability of observing live births to infer deaths in utero. To this end, we create a yearly panel of all women in the DHS sample and estimate equation 2 using OLS:

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

where $birth_{mdt}$ indicates whether woman m located in district d gave birth in year t. This is an adaptation of our "missing children" approach presented in equation 1. Moreover, in addition to the controls specified in equation 1 and survey-wave fixed effects, we include maternal characteristics' controls to increase precision. These controls are include in the term X'_{mdt}. Maternal characteristics include sets of fixed effects for the following variables: year of birth, preceding number of births, marital status, years of schooling, and ethnicity proxied by language. As before, we focus on the sample of years 1993-1995 and 1994-1995 for the analysis.[24]

Second, we study if exposure to the shock increases after-birth mortality by estimating equation 3,

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

where $death_{imdt}$ is an indicator that takes value one whether child i born in year t to mother m residing in district d had died by the time of the survey. This is a child-level equation. The controls included here are those of equation 2.

To show additional evidence that our results are not driven by some spurious relationship, we take two further steps. First, we show that the price shock does not predict pre determined maternal characteristics. Second, as with the cohort-size strategy, we test for differences in mortality by gender (Drevenstedt et al., 2008; Navara, 2010).

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

[24] Note that DHS records the district of residence at the time of the survey but does not record information on the district of birth. Using information on the number of years mothers have been living in the current place of residence, we test if the price shock predicts migration. Untabulated results show evidence that the price shock fails to predict maternal migration.

4 Main results

4.1 Cohort size

We first estimate the impact of the price shock on cohort size using equation 1 (columns 1-3 in Table 2). 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, we show implied effects for the average coca district given the *price drop* of 5.56 real Peruvian Soles (PEN) per kilogram—the price shock generated by the policy in 1995. The first two columns show results for individuals born between 1993 and 1995 (ages 0-3 at the time of the shock). For robustness, column 2 shows results controlling for a coca-specific trend, and column 3 shows results for the 1994 and 1995 cohorts (individuals ages 0-2 at the time of the shock).

We show that when coca prices drop, cohort sizes are reduced, which implies excess mortality. This pattern is documented across specifications in columns 1 to 3 of Table 2. The implied effect of a 5.56 real Nuevos Soles price drop in the average coca district is a 0.45% reduction in cohort size (column 1). Adding a coca-specific trend to the specification does not change the results (column 2). Moreover, the adjacent-cohort comparison in column 3 also shows a statistically significant effect on cohort size, although the point estimate is slightly smaller. The smaller effect in column 3 might be the result of some mortality in the 1994 cohort due to the shock, given that these individuals were in a still vulnerable age (1 to 2) in 1995.

The effects are sizeable. The implied elasticity between coca price shocks and cohort size is *positive* and around 0.01.[25] The impact on cohort size implies an increase in child mortality rate of approximately 6% to 11%.[26] In addition, the methodology we follow provides estimates of a lower bound of the effect, since not every individual in a treatment area is actually treated. [27] Untabulated results document that the effects are twice as large for districts with levels of coca cultivation above the median.

4.2 Mortality in utero and after birth

Next, we examine if deaths occur in the prenatal period or after birth. We find that excess mortality takes place both in utero and during early life, when life is more vulnerable.

[25] See Ferreira and Schady (2009) for a review of the literature on aggregate income shocks, infant and child mortality, and effect sizes.

[26] This calculation is based on a rate of child mortality 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.

[27] Note that cohorts in our analysis are constructed using the entire population in each district-year cell.

The results in columns 4 to 6 in Table 2 confirm that exposure to the shock resulted in prenatal deaths. We estimate equation 2 with an outcome variable takes value 1 if we observe a live birth in the data. As before, the first two columns focus on 1993-1995, and the third column on 1994-1995. The estimates are statistically significant across all specifications, with an implied *decrease* of 0.56 to 0.60 percentage points in the probability of birth in a given year for the average coca district, or 4.7% to 5.1% fewer live births with respect to the outcome mean. This is consistent with the vulnerability of life in utero (Navara, 2010).

In addition, we find that deaths occured after birth. Conditional on live birth, columns 7 to 9 in Table 2 show the results of estimating equation 3, where the outcome variable is the probability of the child's having died by the time of the survey. Both the specifications that focus on the 1993-1995 cohorts (columns 7 and 8) and the one that use the 1994-1995 sample (column 9) indicate that the price drop increased mortality after birth. The implied effects are between an additional 0.50 to 0.78 percentage point for the average coca district.

4.3 Robustness

We bring several pieces of evidence that the results are not driven by a spurious relationship between the price collapse and mortality. To begin, our main results in Table 2 are consistent across specifications. Our baseline regression for each specification (columns 1, 4, and 7) already includes a set of controls. Adding a coca-specific trend (columns 2, 5, and 8) or analyzing the 1994-1995 sample (columns 3, 6, and 9) did not change the results.

To obtain further evidence that the relationship between the price drop and mortality is causal, we use an event-study framework to show that the effects occur when prices drop and not before. We focus on the cohort size specification as cohort sizes capture both in utero and after birth mortality.[28] Figures 4 and 5 show graphic evidence of no pre existing trends. The estimated coefficients for 1992 and 1993 are not statistically different from the omitted category, 1994. The effects only appear in 1995, which is the treatment year. We take this as supporting evidence that there is no anticipation of the shock that led to changes in cohort size before 1995.

We then show that price shock is uncorrelated with maternal characteristics. A potential concern is that women with different socioeconomic backgrounds might have self-selected into having

[28] To do this, we estimate equation 4

$$ln(CohortSize_{dt}) = \sum_{s \neq 1994} \beta_s (Coca_d \times year_s) + X'_{dt}\pi + \alpha_d + \gamma_t + \delta_r \times t + \varepsilon_{dt} \quad (4)$$

which is a slight modification of the main equation of this paper (equation 1). We drop the price variable and interact the measure of coca cultivation with a year-specific dummy, $year_s$. This specification is an event-study design with 1994 as the omitted year, and in which β_s recovers the difference in the dependent variable across districts experiencing different intensities of coca cultivation relative to 1994, the pre-shock year. All other variables are defined as before.

children, potentially leading to a spurious comparison across cohorts. We test for selection on maternal characteristics using DHS data. DHS records histories of all live births, including those not alive at the time of the survey, for all women ages 15-49.[29] Table A.1 in the appendix shows little evidence of selection on maternal characteristics. The treatment variable cannot predict seven out of eight outcomes. If anything, the analysis suggests that women with less education might have been less likely to have children, which would be consistent with our findings on in utero losses as women with lower socioeconomic status are expected to be less likely to anticipate and weather the shock.

Finally, we find evidence in support of the survival disadvantage of males both in utero and during early life (Drevenstedt et al., 2008; Navara, 2010). The first two columns of Table 3 show the results of estimating equation 1 stratified by gender. Panel A (panel B) shows results for cohorts constructed counting males (females). We show estimates for our main specifications using the 1993-1995 cohorts, with the second column including a coca trend. We find strong and statistically significant effects for males but not for females. This pattern is consistent across all specifications. The effects for females have the expected sign but are smaller and not statistically significant. We replicate the analysis using the DHS data for the probability of birth, in columns 3 and 4; and mortality after birth, in columns 5 and 6. Again, we find evidence of higher mortality rates for males than for females across specifications, both in utero and during early life: the estimates for males (panel A) are statistically significant. In contrast, the results for females (panel B) have the same sign but are not statistically different from zero.

This male-to-female pattern is grounded in research in biology and reproductive science. Males have a survival disadvantage both in utero and 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 citations therin). It has been documented that the ratio of males to females at birth decreases under adverse conditions in mammals and humans (Navara, 2010). The male-to-female ratio in rural Peru was already low. Regular male to females ratios are around 1.05, but rural Peru exhibited 1.01 in 1993-1994, the control years in the analysis. This suggests that the female babies who were born in the control years were 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.

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[29] We test if the exposed and unexposed cohorts are statistically exchangeable in terms of maternal characteristics using microdata from the 1996 and 2000 DHS.

$$y_{imdt} = \beta(P_t \times Coca_d) + X'_{dt}\pi + \alpha_d + \gamma_t + \delta_r \times t + \varepsilon_{imdt} \quad (5)$$

We estimate equation 5, where y_{imdt} is a maternal outcome for birth i in year t to mother m and district d (for example, years of education and number of preceding births, among others). The regression includes survey-wave fixed effects. All other variables are defined as in equation 1. The estimation is done using the 1993-1995 and 1994-1995 cohorts.

5 Mechanisms

5.1 Empirical specifications

This section studies the mechanisms at play: changes in household consumption of health-promoting goods and the allocation of time between labor and time-intensive health investments. After the negative price shock, the demand for health-promoting goods, such as nutritious foods and medicines, might decrease because of income losses in the coca sector. In addition, the coca price shock might also affect time-intensive health investments, such as time allocated to caring for children, practicing hygiene, and traveling long distances for prenatal visits and vaccinations. This is because lower wages in the coca sector change the opportunity cost of time. In response to lower wages, households might reduce their labor supply and increase their time at home, resulting in more time available to care for children. Contrarily, households might increase their labor supply to compensate for income losses in the coca sector, resulting in less time for caring for children.

We use LSMS data to analyze how the price shock affected household expenditures, household labor supply, and the time allocated to take care of children. In particular, we use the LSMS waves of 1994 and 1997. These nationally representative surveys cover the periods precisely before and after the shock. In addition to detailed records on labor supply and expenditures, the LSMS collects information on migration for individuals 15 years and older at province level. This allows us to relocate individuals of the 1997 survey wave to their province of residence in 1994 (i.e., pre-shock) for estimation.[30] We aggregate all district-level variables to province level for the analysis since provinces are larger administrative divisions than districts. [31]

We estimate equation 6, where h_{idt} is the outcome of interest for individual or household i, such as maternal labor supply or expenditure on health services, in pre-shock location d and year t. All other variables are defined as in equation 1.[32]

$$h_{idt} \;=\; \beta(P_t \times Coca_d) \;+ X'_{idt}\pi \;+ \alpha_d \;+ \gamma_t \;+ \delta_r \times t \;+ \varepsilon_{idt} \tag{6}$$

Finally, we use DHS data and adapt equation 3 to study how time-intensive health investments respond to coca price fluctuations. We exploit the information available for children born between 1991 and 2000 on prenatal care and characteristics of the delivery (e.g., whether the delivery occurred in a medical facility). This information is only available for children born in 1991 or after

[30]For regressions with individuals 14 years or younger, we assume they were located in the same province as the household head.

[31]On average, a province encompasses nine districts.

[32]For individual-level regressions, we include age, age squared, and ethnicity fixed effects (proxied by language). We do not include years of education as a control because this is an endogenous variable for individuals who had not finished their education by the time of the shock (Dammert, 2008). The results do not change when we include education as a control for individuals in older cohorts whose education was finished by the time of the shock.

in the 1996 survey and children born in 1995 or after in the 2000 survey. Relative to previous models, this implies a smaller number of districts for estimation if we restrict the sample to 1993-1995 cohorts. Thus, we use all information available in the survey and estimate the model for the 1991-2000 cohorts.

5.2 Results

We find that households reduced their expenditures due to the price drop. Panel A of Table 4 shows the results of estimating equation 6 with log real expenditure per capita on the left-hand side. We show point estimates and the implied effect for the average province and a 5.56 real Nuevos Soles price drop. We find that total per capita expenditure decreased by 7.9% (column 1).

We then focus on two important categories for child health: food expenditures and expenditures on health. We find especially large decreases in expenditures on health-promoting goods. Whereas food represented 42% of total expenditures for the average household in our sample, health expenditures accounted for 5%. Our analysis in Table 4 shows that the price drop results in a 5.2% reduction in per capita expenditure on food (column 2). In addition, column 3 shows that health expenditures suffer a sizable reduction: The price drop is associated with a 21.3% decrease. Although health expenditures represent a small share of the overall household budget, reductions in this category could be significant for vulnerable populations, such as young children, pregnant women, and the elderly. Overall, this evidence suggests that the coca price drop led to reduced food and health-related expenditures.

In addition, households supplied more labor in response to the price drop and reduced the time available for time-intensive health investments in children. Table 5 show estimates of equation 6 with labor market outcomes on the left-hand side for three samples: household head and spouse, females ages 15-59, and males ages 15-59. Columns 1 to 3 are estimated at household level. We observe that the price drop increased the likelihood that households have both the household head and spouse employed in the labor market by 4 percentage points, a sizeable increase of 10% with respect to the baseline mean (column 1). In addition, households increased the average weekly hours worked by the household head and spouse by about 1 hour (column 2), although this effect is not statistically significant. Column 3 shows that the coca price drop induced a reallocation of time from household chores to labor responsibilities: The price drop generated a reduction of about 2 hours in the average total weekly hours spent by household heads and spouses on household chores, such as cooking, cleaning, and caring for children.[33]

We find that changes in time allocation are driven by adult women, who are also the primary caregivers for children in this setting. Columns 4 to 6 of Table 5 show that more adult females

[33] The surveys do not have information on the time allocated to specific household chores. The questionnaire only asks for the total number of hours dedicated to household chores.

(ages 15-59) supplied labor and allocated less time to household chores. This evidence suggests a decrease in the amount of time allocated to the production function of child health. For the average province, the price drop increased the probability of observing females ages 15 to 59 supplying labor by 3.2 percentage points (column 4). This is a sizable effect: The increase in labor supply represents about 6% of the baseline value. Women allocated more time to work (column 5) and decreased the number of weekly hours allocated to household chores (column 6). Appendix Table A.2 shows that the effects are not different for mothers of children under 5 and other women, which suggests that primary caregivers did in fact reallocate time from rearing child to market work.[34] Interestingly, adult males ages 15-59 did not significantly change their labor supply. This might be because most adult males were already working at baseline (80%). Our results are consistent with previous studies that show that low-income households respond to aggregate income shocks with an increase in wage labor among adults, especially women (Beck et al., 2019).

Finally, we find evidence of reductions in health investments both during pregnancy and the early years of life of the child. Panel A of Table 6 focuses on health investments after birth. We estimate equation 6 for children under 5 at the time of the LSMS survey waves of 1994 and 1997 with health markers as outcome variables. Each row shows the implied effect of the price drop on a given dependent variable. We also report the standard error of the implied effect and the sample mean of each dependent variable for ease of interpretation. We do not find evidence that the price drop increased the likelihood of sickness during the four weeks preceding the survey (first row). But we see a sizeable decrease in the likelihood of the child going to the doctor if sick (second row). The price drop reduced the likelihood of going to the doctor when sick in 5.9 percentage points for the average coca province. This is a considerable decline equivalent to about 15% of the baseline measure.

Panel B in Table 6 focuses on health investments during pregnancy. As before, in each row, we report the implied effect of the price shock on a different dependent variable. We find that pregnant women had fewer prenatal appointments (first row). Importantly, we also find effects on the extensive margin: Women were less likely to have at least one prenatal appointment (second row). In addition, deliveries were more likely to occur at home and not in a medical facility (third row). Relative to births in a medical facility, deliveries at home are usually associated with more complications at birth and higher mortality rates in developing settings. Consistently, the price drop reduced the likelihood of a medical professional's assisting with the delivery (fourth row).

The findings of reduced health investments during pregnancy and after birth are consistent with the evidence of households' having much tighter budgets and less time available to care for children due to the price shock. In addition, the reduction in health investments during pregnancy

[34] We prefer the results in Table 5, as they are not conditional on motherhood status. This is because we only observe motherhood in the LSMS conditional on the child's being alive at the time of the survey.

is consistent with our findings on fetal mortality, while our findings on decreased investments after birth align with the evidence on mortality in the early years of life.

6 Heterogeneity

This section explores heterogeneous effects of the coca price drop by poverty levels at baseline. Poor households tend to have fewer mechanisms to smooth consumption (e.g., savings, access to credit) and are more likely to supply the labor of available household members to smooth out shocks. Mortality should be higher among poorer areas if poor households are less likely to smooth consumption and more likely to supply maternal labor as a coping mechanism.

We define high-poverty districts as those with baseline poverty above the upper quartile. Since poverty at baseline (in 1993) is only available at the province level, we first rank each district in our sample by the level of poverty in its province and create an indicator variable $1[\text{High poverty} = 1]_d$.[35] To estimate heterogeneous effects, we decompose the term of interest $\beta(P_t \times Coca_d)$ into $\beta_1(P_t \times Coca_d) + \beta_2(P_t \times Coca_d) \times 1[\text{High poverty} = 1]_d$. In addition, we control for the interactions ($P_t \times 1[\text{High poverty} = 1]_d$) and ($Coca_d \times 1[\text{High poverty} = 1]_d$). Thus, while β_1 captures the impact of coca revenues on the variable of interest for low-poverty districts, the sum of $\beta_1 + \beta_2$ captures it for high-poverty districts.

Table 7 presents the results of the heterogeneous analysis. We show the implied effect of the coca price drop over three sets of outcomes for high-poverty and low-poverty districts. Columns 1 to 3 focus on females ages 15-59 and present results for the probability of doing any paid work, the number of hours worked, and the number of hours allocated to household chores. Columns 4 and 5 focus on per capita total expenditure and health expenditure, respectively. Finally, column 6 focuses on child mortality for the 1993-1995 cohorts as in the main specification.

We find different behavioral responses triggered by the price drop in low-poverty and high-poverty areas. First, females are more likely to supply labor in high-poverty areas. The implied effect is a 4.2 percentage point increase in the likelihood of doing any paid work (column 1). This result is consistent with households using the time of household members with the ability to supply more labor as a consumption smoothing mechanism. Households in poor areas are sought to obtain some extra income to alleviate the impact of the shock. On the other hand, females in low-poverty areas are less likely to supply labor as a response to the price drop, consistent with a decrease in the opportunity cost of time and the ability of households to smooth consumption in ways other than supplying extra labor.[36] We also find evidence that households in high-poverty areas cannot

[35] Poverty rate information at provinces is from Escobal and Ponce (2016).

[36] The impact of the coca price drop on male labor supply is not different from zero, independently of whether households are located in low-poverty or high-poverty areas. These results are consistent with the fact that groups

fully smooth their consumption. As we can observe in columns 4 and 5, among households in high poverty areas, the coca price drop decreased expenditure per capita by 8.3 percentage points with a substantial decrease in the spending on health (23.1%). In the case of households in low-poverty areas, the impact on expenditure is also negative but not statistically different from zero.

As a result, we find that mortality increases in high-poverty districts, but it does not in low-poverty ones. The implied effect of the price shock in high-poverty districts is a 0.61% reduction in cohort size (column 6). However, in low-poverty districts, the effect is not statistically significant. The latter result suggests that survival rates are unaffected in wealthier areas. If anything, the estimated coefficient has a positive sign, indicating that survival rates improve when coca prices drop. This is consistent with evidence from developed countries where negative income shocks reduce labor supply among mothers, translating into more time allocated to children's health and thus less child mortality (Miller and Urdinola, 2010; Van den Berg et al., 2017, 2020).

7 Concluding Remarks

In order to design effective policies that aim to protect households from income volatility, it is important to understand both: the dimensions of welfare at risk and the mechanisms that reinforce or lessen the impacts of income shocks. This study aims to make progress on these two fronts by exploiting a unique natural experiment that generated an abrupt decrease in the price of coca leaves in Peru. In 1995, the Peruvian government implemented an aggressive coca interdiction policy that cut the demand for coca leaves. The policy generated an unprecedented initial drop of 50% in the price of coca.

The first goal of our study is to rigorously assess the impact of the abrupt price shock on children's mortality. The empirical strategy is a difference-in-differences design that exploits variation in baseline levels of coca cultivation across Peruvian districts and compares cohorts of children born in years of high and low prices. Our main result is that the negative price shock increased child mortality by 6% to 11% in the average coca-growing district. A combination of data sets and approaches allows us to establish that survival is compromised both in utero and after birth.

The empirical strategy addresses critical threats to identification. The estimated impact is isolated from confounding factors, such as selective fertility or anticipated migration. We use information on year and district of birth to exploit the abruptness of the price drop and compare cohorts of children born the year right before the shock with those born the year of the shock. We show that our results are robust to several tests. We use an event study framework to show that there is no evidence of anticipation of the shock. Moreover, we show that exposure to the shock did not

with a lower labor force participation at baseline are more likely to increase their labor supply as a response to a negative commodity price shock (Beck et al., 2019).

lead to selective fertility. Finally, we overcome the challenge of not observing reliable vital records in developing countries by using a Population Census to infer mortality rates from relative cohort sizes (i.e., "missing" individuals) 13 years after the shock.

The second goal of this paper is to understand the mechanisms in play. In particular, we examine the impact of the shock on household expenditure patterns and caregiver's time allocation between wage labor and time-intensive health investments. To this end, we use two nationally representative household surveys: the LSMS to study expenditure and labor choices and the DHS to assess impacts on health investments. Overall, we find that households could not fully smooth consumption, especially of health-related goods, and reallocate time from household chores and caring for children to paid labor to compensate for income losses. Consistent with previous studies, commodity price shocks increase labor supply among those groups with lower ex ante participation. In our setting, we observe time reallocation among women.

To further understand the connection between household behavioral responses and the impact of the price shock on child mortality, we explore heterogeneous effects by the level of poverty at baseline. Results confirm the intuition that the inability to smooth consumption and the pressure to supply more wage labor were greater in high-poverty areas. Thus, the price shock only increased child mortality in poorer districts. In contrast, in low-poverty districts, mortality did not increase. Interestingly, in wealthier areas, female labor supply decreased as a response to the price drop, and child mortality did not increase in these areas. These findings can accommodate alternative predictions of the effects of aggregate income shocks on child mortality in more developed settings (Miller and Urdinola, 2010; Van den Berg et al., 2017, 2020).

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Figures

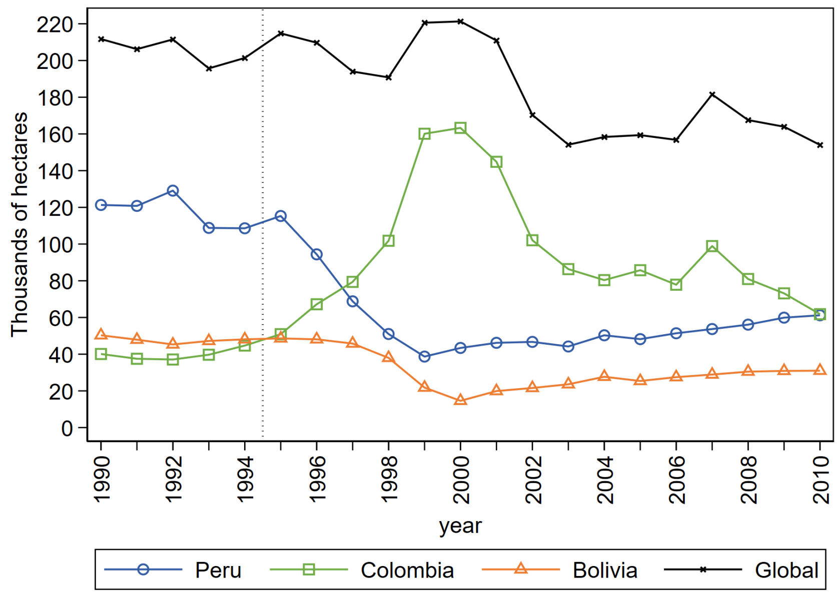

Figure 1: Coca Production by Country

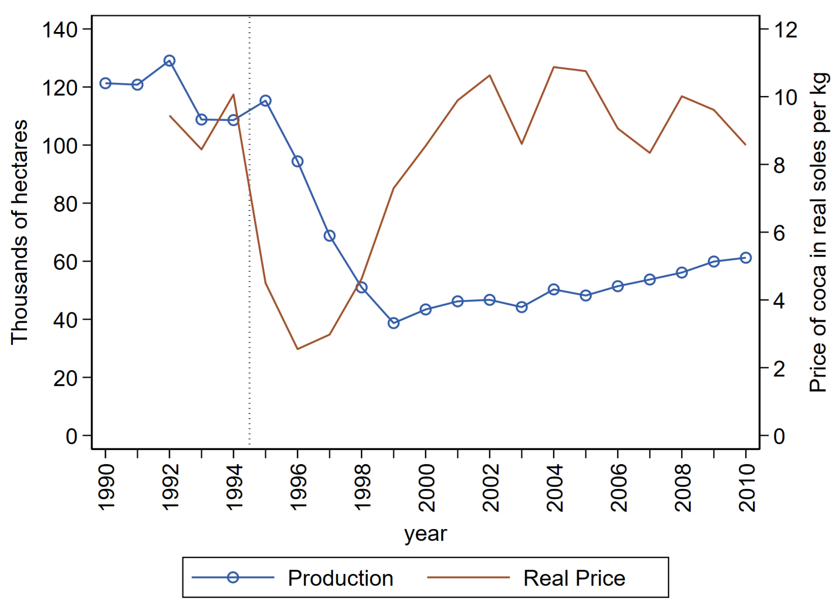

Figure 2: Coca production and real price in Peru

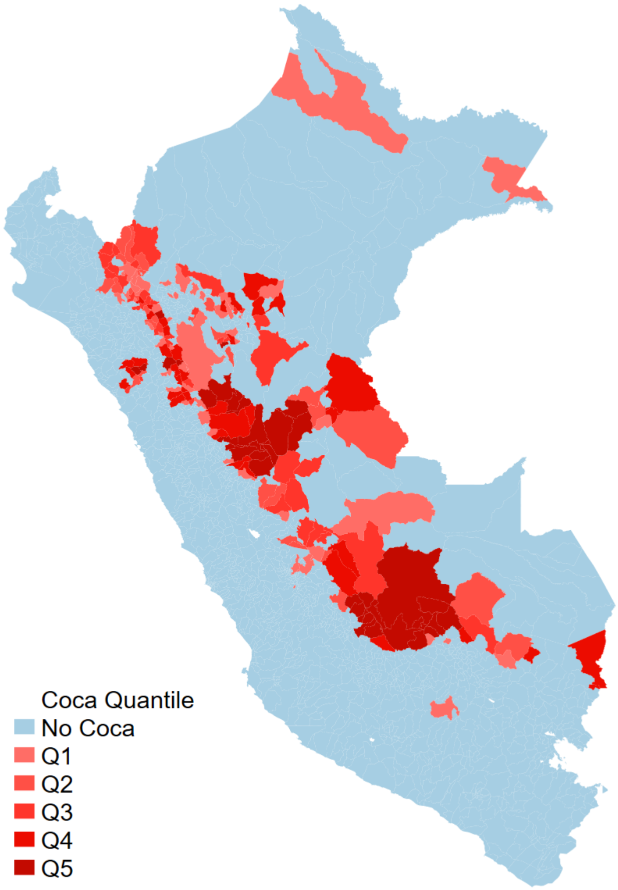

Figure 3: Coca cultivation in 1994

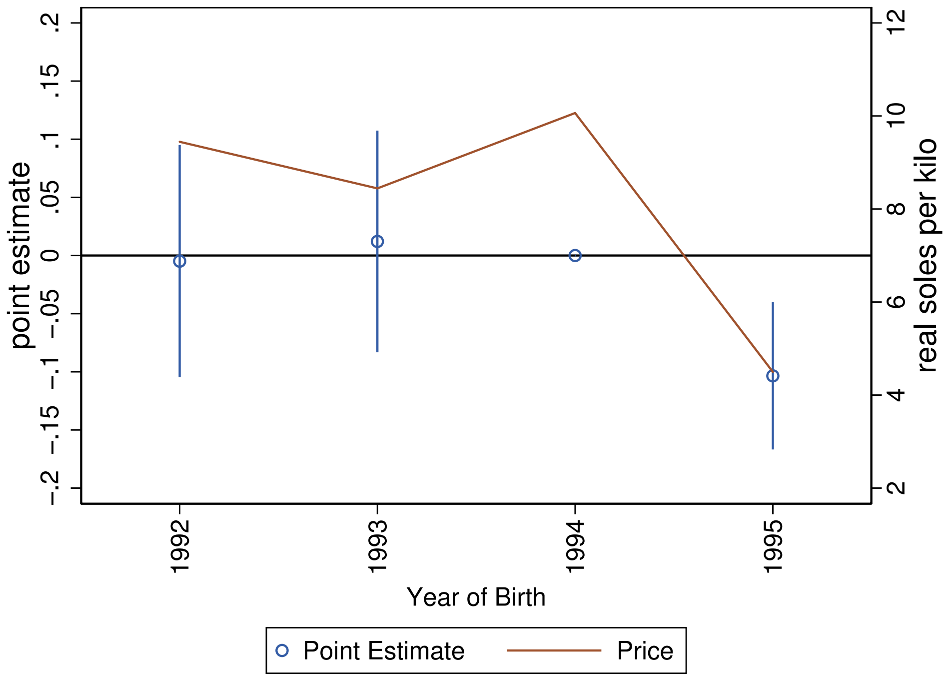

Figure 4: Event-study framework: Effect of price shock on male cohort size

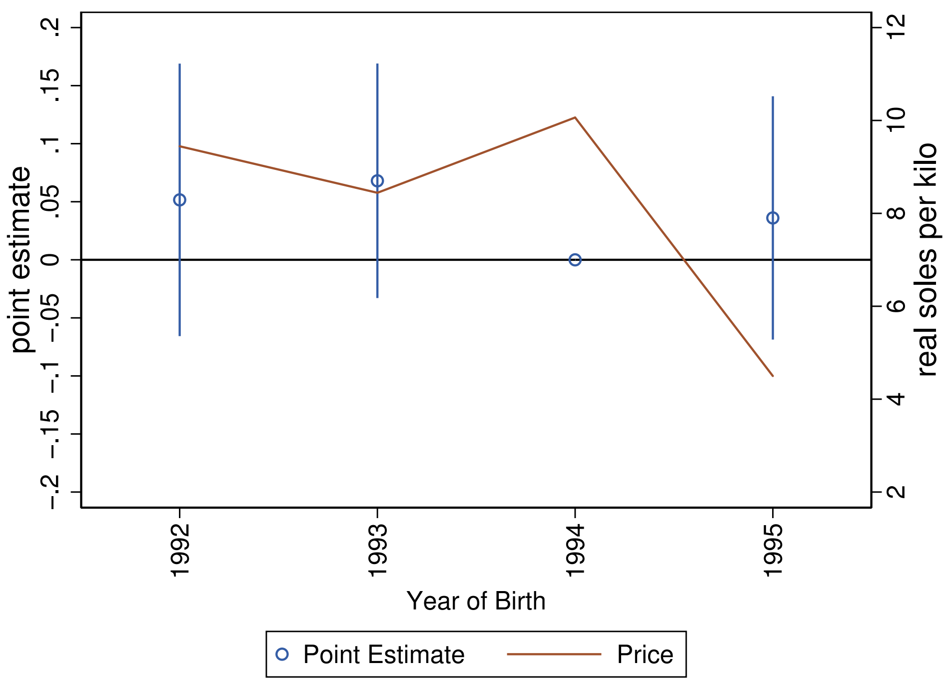

Figure 5: Event-study framework: Effect of price shock on female cohort size

Tables

Table 1: Districts' Summary Statistics conditional on Coca Production

	Sample Average		
	Coca Districts (1)	Other Districts (2)	Difference (1)-(2)
POPULATION CENSUS - DISTRICTS 1993			
Population (thousands)	10.9	12.47	-1.562
Share rural	73.87	55.72	18.151***
Share female	47.5	49.86	-2.363***
Years of education, males	5.66	6.31	-0.656***
Years of education, females	5.01	5.47	-0.464***
Labor force participation, males (ages 15+)	83.96	76.34	7.624***
Labor force participation, females (ages 15+)	25.44	25.47	-0.026
Share of male employment in agriculture (ages 15+)	75.12	63.86	11.253***
Share of female employment in agriculture	33.71	34.95	-1.242
N (All 1793)	193	1600	
AGRICULTURAL CENSUS - DISTRICTS 1994			
Hectares of coca (thousands)	0.12	0	-
Percent of cultivated land with crops for sale	42.7	29.9	12.8***
N (All 1721)	193	1528	
POVERTY - PROVINCES 1993			
Poverty headcount (%)	67.1	65.4	1.6
N (All 188)	63	125	

Note: Poverty at the province level in 1993 has been estimated by Escobal and Ponce (2016).

Table 2: Effects of Coca Price Shocks on Cohort Size

Dependent Variable:	Log Cohort Size			Probability of Birth			Mortality		
Cohort:	93-95		94-95	93-95		94-95	93-95		94-95
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Price at t × Coca Intensity	0.0065*	0.0079**	0.0044*	0.0084***	0.0090***	0.0087***	-0.0080***	-0.0075***	-0.0117***
	(0.0034)	(0.0036)	(0.0026)	(0.0026)	(0.0027)	(0.0024)	(0.0022)	(0.0023)	(0.0034)
Implied Effect (in percentage)	-0.43*	-0.53**	-0.29*	-0.56***	-0.60***	-0.58***	0.53***	0.50***	0.78***
Districts	1777	1777	1777	847	847	847	836	836	823
Observations	5331	5331	5331	164835	164835	109890	19294	19294	12689
Coca Trend	No	Yes	No	No	Yes	No	No	Yes	No

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors are clustered at the district level and are in parenthesis. Implied effects at the average coca district (119 hectares) and for a -5.56 real Nuevos Soles price change. All specifications control for district fixed effects, year of birth fixed effects, agricultural controls as in equation 1, and regional trends.

Table 3: Effects of Coca Price Shocks on Cohort Size by Gender

Dependent Variable:	Log Cohort Size		Probability of Birth		Mortality	
Cohort:	93-95		93-95		93-95	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Males			
Price at t × Coca Intensity	0.0121*** (0.0040)	0.0134*** (0.0043)	0.0061*** (0.0014)	0.0066*** (0.0013)	-0.0088* (0.0045)	-0.0081* (0.0046)
Implied Effect (in percentage)	-0.81***	-0.89***	-0.41***	-0.44***	0.59*	0.54*
Districts	1777	1777	847	847	812	812
Observations	5331	5331	164835	164835	9779	9779
			Panel B: Females			
Price at t × Coca Intensity	0.0001 (0.0049)	0.0017 (0.0050)	0.0023 (0.0023)	0.0024 (0.0023)	-0.0090 (0.0111)	-0.0087 (0.0111)
Implied Effect (in percentage)	-0.01	-0.12	-0.15	-0.16	0.60	0.58
Districts	1777	1777	847	847	813	813
Observations	5331	5331	164835	164835	9515	9515
Coca Trend	No	Yes	No	Yes	No	Yes

*** $p<0.01$, ** $p<0.05$, and * $p<0.10$. Standard errors are clustered at the district level and are in parenthesis. Implied effects at the average coca district (119 hectares) and for a -5.56 real Nuevos Soles price change. All specifications control for district fixed effects, year of birth fixed effects, agricultural controls as in equation 1, and regional trends.

Table 4: Effects of Coca Price Shocks on Household Real Expenditure

	Log real expenditure per capita		
Expenditure Category:	Total (1)	Food (2)	Health (3)
Price at t × Coca Intensity	0.0475** (0.0223)	0.0309** (0.0125)	0.1278** (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

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors are clustered at the province level and are in parenthesis. Implied effects at the average coca province (300 hectares) and for a -5.56 real Nuevos Soles price change. All specifications control for district fixed effects, year of birth fixed effects, agricultural controls as in equation 1, and regional trends.

Table 5: Effects of Coca Price Shocks on Labor Margins at the Household

Sample:	Household Head & Spouse			Females (15-59)			Males (15-59)		
Dependent Variable:	Both Work (=1)	Work Hours	Chore Hours	Any Work (=1)	Work Hours	Chore Hours	Any Work (=1)	Work Hours	Chore Hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Price at $t \times$ Coca Intensity	-0.0237**	-0.6258	1.1615*	-0.0194*	-0.7725**	1.2572*	-0.0099	0.1972	0.0876
	(0.012)	(0.672)	(0.675)	(0.011)	(0.350)	(0.666)	(0.006)	(0.414)	(0.093)
Implied Effect	0.0396**	1.0438	-1.9374*	0.0324*	1.2885**	-2.0970*	0.0165	-0.3289	-0.1461
Dep. Var. Mean	0.39	57.43	43.34	0.54	19.96	32.40	0.80	39.23	7.52
Districts	139	139	139	141	141	141	141	141	141
Observations	7453	7453	7453	10874	10874	10874	10125	10125	10125

*** p<0.01, ** p<0.05, and * p<0.10. Standard errors are clustered at the province level and are in parenthesis. Implied effects at the average coca province (300 hectares) and for a -5.56 real Nuevos Soles price change. All specifications control for district fixed effects, year of birth fixed effects, agricultural controls as in equation 1, and regional trends. Work and chore hours refer to the number of hours a person works during a week.

Table 6: Changes in Coca Prices and Health Investments

	Implied Effect (1)	S.E. (2)	Dep. Var. Mean (3)
Panel A: Children ages 0-5 in LSMS waves 1994 and 1997			
Sick in the last 4 weeks (==1)	0.0021	(0.01)	0.4931
Went to doctor, if sick (==1)	-0.0592*	(0.02)	0.4056
Panel B: Live births between 1991-2000 in DHS waves 1996 and 2000			
Number of prenatal appointments	-0.0627**	(0.0261)	3.4229
At least one prenatal appointment (==1)	-0.0135*	(0.0078)	0.6222
Delivery at home (==1)	0.0089**	(0.0038)	0.4437
Birth assisted by medical professional (==1)	-0.0050**	(0.0024)	0.2002

*** $p<0.01$, ** $p<0.05$, and * $p<0.10$. Implied effects at the average coca district (DHS) or province (LSMS) and for a -5.56 real Nuevos Soles price change.

Table 7: Effect of the Coca Price Shock by Province Poverty Level in 1993

Dependent Variable:	Female Labor Outcomes (15-59)			Log Real Expenditure Per Capita		Log Size - Cohorts 93-95
	Any Work (=1)	Work (hours)	Chores (hours)	All	Health	All
	(1)	(2)	(3)	(4)	(5)	(6)
HIGH POVERTY PROV. 93						
Implied Effect of Price Drop (%)	0.0423*	1.5558**	-2.4748*	-8.32**	-23.15**	-0.61*
	(0.024)	(0.660)	(1.308)	(4.11)	(10.21)	(0.32)
Dep. Var. Mean	0.62	21.09	33.25	2,599	125	195
LOW POVERTY PROV. 93						
Implied Effect of Price Drop (%)	-0.0574***	-1.0757	1.0913	-3.63	-3.6	0.32
	(0.014)	(0.771)	(0.815)	(4.88)	(13.18)	(1.84)
Dep. Var. Mean	0.51	19.56	32.10	5,019	246	537
Clusters (Districts or Provinces)	141	141	141	139	139	1,777
Observations	10,874	10,874	10,874	7,453	7,453	5,331
Controls	Yes	Yes	Yes	Yes	Yes	Yes

*** $p<0.01$, ** $p<0.05$, and * $p<0.10$. Implied effects at the average coca province at each subgroup and for a -5.56 real Nuevos Soles price change. Note that the dependent variable mean in column (1), (2) and (3) are in levels.

A Appendix

Table A.1: 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) Dep. Var. Mean	(4) Estimate (S.E.)	(5) Imp. Effect	(6) Dep. Var. Mean
Depedent Variable:						
Mother's age in years at time of birth	-0.027 (0.199)	0.018	27.278	-0.004 (0.188)	0.003	27.272
Mother was married at time of birth (=1)	0.035 (0.027)	-0.024	0.661	0.050 (0.034)	-0.033	0.654
Mother's age in years at first birth	-0.011 (0.075)	0.007	20.299	-0.013 (0.070)	0.009	20.264
Mother's number of preceding births	0.019 (0.043)	-0.013	2.566	0.028 (0.044)	-0.019	2.594
Mother's first birth (=1)	0.008 (0.005)	-0.005	0.244	0.008 (0.005)	-0.005	0.243
Mother's years of education	-0.141*** (0.046)	0.094	5.394	-0.100** (0.043)	0.067	5.361
Mother was born in a rural area (=1)	0.011 (0.015)	-0.008	0.399	0.008 (0.015)	-0.005	0.407
Mother is a Quechua speaker (=1)	-0.000 (0.002)	0.000	0.174	-0.002 (0.003)	0.001	0.187
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 at the average coca district (119.95 hectares) and for a −5.56 real Nuevos Soles price change.

Table A.2: Effect of the Coca Price Shock on Mothers

Sample:	Female 15-59		
Outcomes:	Any Work (=1) (1)	Work (hours) (2)	Chore (hours) (3)
MOTHER IN HH WITH CHILDREN (0-5y) (=1)			
Implied Effect of Price Drop	0.0338*	1.3560**	-2.1532*
	(0.019)	(0.606)	(1.153)
Dep. Var. Mean	0.56	19.84	40.39
OTHER WOMEN			
Implied Effect of Price Drop	0.0310*	1.2051**	-2.0599*
	(0.018)	(0.588)	(1.116)
Dep. Var. Mean	0.52	20.02	28.15
Provinces	141	141	141
Observations	10,874	10,874	10,874
Controls	Yes	Yes	Yes

*** p<0.01, ** p<0.05, and * p<0.10. Implied effects at the average coca province (300 hectares) and for a -5.56 real Nuevos Soles price change.