The Value of Time and Skill Acquisition in the Long Run: Evidence from Coffee Booms and Busts

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October 23, 2018

Abstract

This paper examines the long-run impacts of local income shocks on completed human capital. I exploit geographic variation in coffee cultivation patterns in Colombia and real-world coffee prices at the time cohorts were of school-going age in a differences-in-differences framework. Using five decades of data, I show that cohorts who faced sharp rises in the return to coffee-related work when they were of school-going age have lower educational attainment and are in lower-paid occupations. These findings suggest that transitory changes in the opportunity cost of schooling can cause permanent changes in human capital investments and not just a mere delay.

JEL codes: J24; O12; O13.

Keywords: coffee price shocks; transitory income shocks; human capital accumulation.

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1 Introduction

How aggregate income shocks affect human capital is a question of central importance to both policymakers and economists. Improvements in economic conditions can facilitate the accumulation of human capital by making education more affordable, especially in the context of imperfect capital markets and limited access to formal safety net programs.[1] Counterbalancing this, however, is the fact that higher wages can significantly raise the opportunity cost of schooling and thus discourage educational investments, potentially compromising economic growth and development in the long run. A large body of work provides important evidence that economic booms are generally associated with poorer contemporaneous school outcomes, including enrollment, dropout and grade attained by the end of a specific year (Shah and Steinberg, 2017; Ferreira and Schady, 2004; Kruger, 2007; Rosenzweig and Evenson, 1977; Soares et al., 2012).[2] To date, however, there is very little evidence establishing the extent to which these shocks translate into long-run differences in total human capital, and much less evidence evaluating possible differences in subsequent labor market trajectories.

There are multiple reasons why short-run and long-run impacts of income shocks on human capital formation may not be the same. While existing literature generally documents that child enrollment declines during booms, this will not affect completed human capital unless individuals continue to make different educational choices even after these episodes. Children interrupting school and taking up employment opportunities during temporary booms may simply return to school once these shocks have dissipated.[3] This will alter the timing of schooling without any consequence on completed education.[4] Furthermore, many youths may delay schooling and enter the workforce during booms to accumulate savings and finance subsequent education that otherwise would has been unaffordable (Lochner and Monge-Naranjo, 2012; Johnson, 2013). As a result, the total accumulation of human capital could be unaffected or even increase over the long run. Alternatively, delaying or interrupting schooling may discourage later educational investments if schooling at critical ages raises the productivity of investments at subsequent stages (Cunha and Heckman, 2007). This

[1] In addition, if economic expansions lead to increased returns to schooling, then they could further encourage educational investments (Ferreira and Schady, 2009).

[2] These studies do not always find countercyclical patterns. For example, Thomas et al. (2004) show that the 1998 Indonesian crisis was associated with significant reductions in school enrollment. A comprehensive review of this literature can be found in Ferreira and Schady (2009).

[3] For example, Light (1995) reports that a significant fraction of individuals who leave school end up returning to school later in the United States. Similarly, Annan et al. (2011) find high returning rates among youths who were recruited temporarily by the Army in Uganda.

[4] Some studies, however, document that delaying schooling is associated with lower earnings, suggesting that the timing of schooling may be important for the labor market (Light, 1995).

dynamic complementarity may lead to larger long-run impacts of income shocks on human capital.

This paper examines this question by exploring the long-term effects of plausibly exogenous income shocks in Colombia generated by changes in real-world coffee prices. Colombia is a major exporter of washed arabica coffee, and its price in international markets is an important determinant of household incomes in coffee-growing areas. From the mid-1950s to the mid-1990s, several events caused sudden and dramatic fluctuations in coffee prices. These include several climatic shocks that decimated Brazilian supply, the collapse of the international coffee price agreement, and the unprecedented expansion in Vietnam's coffee industry. Following these episodes, the real price of coffee changed by 55 to 130 percent of the baseline and previous studies have documented that it significantly affected local employment and wages across coffee-growing areas (Miller and Urdinola, 2010; Dube and Vargas, 2013). Since all these events were originated outside of Colombia, they created shocks to income virtually independent of local schooling decisions.

To examine the long-run impacts of these shocks, I combine historical data on predetermined coffee cultivation patterns across municipalities with the timing of coffee price shocks in a differences-in-differences strategy. Specifically, I compare educational attainment of individuals in areas with varying coffee cultivation intensities and who were exposed to different world coffee market conditions in childhood. Because the role of coffee to local economy varies substantially across geographic areas and coffee price shocks were large and of varying duration, different cohorts in different areas faced quite different economic conditions when they were of school-going age. Since migration in Colombia was relatively low among youths during the analysis period, I can approximate the market where they grew up using information about the municipality of birth. This strategy identifies an intention-to-treat effect under the assumption that trends in outcomes would have been similar in areas with varying coffee cultivation patterns in the absence of coffee price shocks. While this assumption is untestable by definition, I provide a number of pieces of evidence supporting its plausibility.

The research design offers two key features that enable me to make stronger inferences about the long-run impacts of income shocks and potential mechanisms than previous studies. First, coffee is not a health input and represents an insignificant component of the total household budget, so it is unlikely to have a direct effect on outcomes. This is in sharp contrast with previous work focusing on staple agricultural crops (Beegle et al., 2009) and rainfall shocks (Shah and Steinberg, 2017).[5] Second, the Colombian government does not tax coffee production and thus coffee market conditions are not linked to the financing of local public spending.[6] This provides an unusual opportunity to more directly evaluate the role of household income and the opportunity cost of schooling.[7] Since both mechanisms are of the opposite sign, reduced-form estimates offer a test about their relative importance in the production function of human capital.

The estimates suggest that a rise in international coffee prices reduces educational attainment disproportionally in municipalities cultivating more coffee. The increase in coffee prices from cohorts born in 1954 to those born in 1970 led to a decline in completed schooling that is 0.09 years larger in areas with one standard deviation additional coffee cultivation. This estimate is very precise and of comparable magnitude to that of well-documented interventions targeting education in developing countries. For instance, Angrist et al. (2002) document that the Colombian PACES program, which randomly assigned private school vouchers, increased educational attainment by about 0.1 years of schooling.

I show that these results are robust with respect to an array of different specifications. For example, allowing for flexible and differential trends, parameterized as function of a number of baseline characteristics (i.e., the incidence of specific diseases, early internal conflict, and measures of general economic development), has no discernible impact on the baseline estimate. A model that uses Brazilian coffee production levels as an instrument for international coffee prices gives comparable estimates to the baseline model. This is consistent with the notion that the major coffee market shocks were largely originated from Brazil and thus suggests that the results are not an artifact of Colombia's prominence on world markets. Finally, there are no differential changes in schooling across municipalities with different coffee cultivation intensities for cohorts who were exposed to relatively stable coffee prices in childhood.

These findings suggest the primacy of the opportunity cost of schooling over income in determining long-run human capital. Several pieces of evidence support this interpretation. Further specifications that consider exposure at different ages reveal that the timing of the effects coincides with that of schooling decisions. Data on child outcomes show that

[5] In particular, while heavy rainfall and droughts can influence agricultural income, they can also affect environmental sanitary conditions and the prevalence of mosquito vectors that transmit a number of diseases, all of which are likely to have an independent effect on human capital formation (Maccini and Yang, 2009; Rocha and Soares, 2015)

[6] Miller and Urdinola (2010) provide evidence consistent with this view by showing that coffee price shocks are not associated with changes in local tax revenue and public spending.

[7] Some previous studies have investigated the impacts of oil production on schooling in the United States (Emery et al., 2012). However, this commodity is generally heavily taxed and linked to the financed of local public spending on education and health (Acemoglu et al., 2013), so the role of household income is not clear.

children are more likely to work and less likely to stay in school during coffee booms. Additionally, the results are significantly larger and more precisely estimated for men than for women. The correspondence in the child labor and educational patterns across gender suggests that girls were less responsive to the same changes in the return to coffee-related work and hence female school attainment declined less than male. Finally, I am able to rule out several prominent alternative mechanisms, including changes in the supply of teachers, local violence, and household work decisions.

In the last part of the paper, I examine potential changes in subsequent labor market earning profiles. Work at earlier ages may provide some benefits, including acquisition of specific skills, increased social capital and general work experience, that may be rewarded later in the labor market. If these potential rewards are large relative to income losses from leaving school "too soon", then one could observe positive overall impacts on subsequent labor market prospects. I find that individuals who faced coffee booms when they were of school-going age are in lower-paid occupations. The main estimate suggests that the increase in coffee prices from cohorts born in 1954 to those born in 1970 resulted in a 0.8-percent larger reduction in income in areas with one standard deviation more coffee cultivation. This suggests that the negative impacts of reduced schooling appear to dominate any positive impacts.

Existing research suggests that these patterns are not obvious. In a simple model where education is entirely viewed as a financial investment, forward-looking students anticipating that the potential rewards from early work are small relative to gains from extra schooling should not have incentives to leave school (Eckstein and Wolpin, 1999). My findings suggesting that students drop out of school to take up work opportunities with relatively low subsequent returns are difficult to reconcile with this view. Rather, the results are consistent with the possibility that youths ignore or heavily discount the future. This behavior may be driven by constrained youths rationally trading off between immediate income gains and future returns to extra schooling, with no overall impact on lifetime welfare. Alternatively, it may be that youths are not well informed on the expected returns to schooling (Dominitz and Manski, 1997; Rouse, 2004; Jensen, 2010) and therefore make sub-optimal choices when faced with immediate economic opportunities (Frederick et al., 2002; Laibson, 1997; O'Donoghue and Rabin, 1999).[8] The possibility that students might be making sub-optimal choices is consistent with previous studies showing that compulsory schooling laws improved a number of lifetime outcomes (Lochner and Moretti, 2004; Lleras-Muney,

[8] For example, Jensen (2010) finds that the perceived returns to secondary school in terms of earnings are extremely low despite the high measured returns in Dominican Republic. He convincingly shows that providing information to students on the higher measured economic returns to education leads to an increase in completed schooling of 0.20-0.25 years.

2005; Oreopoulos, 2007), and previous studies in neurology and psychology suggesting that youths are especially susceptible to myopic behaviors (Spear, 2000; Graber and Petersen, 1991).

The results of this paper provide novel evidence that transitory income shocks can cause permanent changes in human capital. They build on and relate to several previous studies. Atkin (2016) documents that the arrival of formal jobs during years of substantial expansions in export-manufacturing industries in Mexico led to reduced school attendance and lower educational attainment, although it had no overall impacts on subsequent labor market income. An important distinction is that Atkin (2016) focuses on large formal firms, which provide valuable on-the-job-training opportunities and skill accumulation that may offset income losses from reduced formal schooling. This paper contributes by providing evidence that different labor market shocks are likely to have different long-run implications. Charles et al. (Forthcoming) show that the housing boom during the 2000s reduced college attendance among young adults and this decline was not completely reversed years later, providing suggestive evidence that these shocks may have permanently affected college education in the United States. However, it is unclear the extent to which findings in industrialized settings generalize to human capital formation in less developed countries. In these contexts, poor enforcement of child labor laws and the absence of well-functioning credit markets are likely to play a prominent role in early educational choices of youths when faced with improvements in economic conditions.

There are also a few studies that focus on the long-run effects of macroeconomic crises or big one-off shocks, although the evidence is much more mixed (Stuart, 2017; Ferreira and Schady, 2004; Funkhouser, 1999). This is perhaps unsurprising given that macroeconomic crises are a multifaceted treatment —for example they are generally associated with a collapse in public spending (including that on education). Furthermore, severe recessions in fragile states are generally preceded by political chaos, disruptions in institutions, and civil conflict, so before-after-like comparisons are difficult to interpret.[9] In contrast, the specific features of the natural experiment employed in this paper provide a cleaner identification of long-run impacts of income shocks and at the same time allow me to focus on more specific mechanisms. In particular, the variation I exploit helps to more directly test the relative importance of income versus time in the production of human capital.

This paper is also connected to a broad literature on the determinants of human cap-

[9] For example, the Peruvian macroeconomic crisis between 1988 and 1990, the focus of Ferreira and Schady (2004), was preceded by a period of major political instability, institutional collapse, and rapid expanding civil war. The expansion of guerrilla campaigns resulted in the control of most rural areas, leading to a state of emergency that caused massive rural-urban migration, shut down markets and disrupted infrastructure (Crabtree, 2016).

ital formation. Several studies examine the effects of persistent variation in school supply (Duflo, 2001), school quality (Chetty et al., 2011, 2014), conditional-cash transfer interventions (Behrman et al., 2009, 2011), tuition fees policies (Angrist et al., 2002; Hübner, 2012), neighborhood characteristics (Chetty and Hendren, 2018), and school-based health interventions (Baird et al., 2016). The present study adds to this literature by providing evidence that transitory income shocks can have comparable effects in magnitude on completed education. Another related work by Sviatschi (2017) documents that increased return to illegal labor markets during adolescence is associated with reduced school attendance and higher incarceration rates as adults, suggesting that youths substitute formal schooling for criminal careers.

Finally, the findings of this study are also connected to the "resource curse" literature, which suggests that mineral and agricultural resource abundance may be bad for development (Sachs and Warner, 1995). This literature has generally emphasized the effects of commodity booms on institutions (Sala-i-Martin and Subramanian, 2003; Hausmann and Rigobon, 2003), and recently on civil conflicts (Angrist and Kugler, 2008; Dube and Vargas, 2013). The results from this paper suggest that reduced long-run human capital is a less explored channel by which agricultural resource abundance may hamper development and economic growth.

The rest of the paper is organized as follows. Section 2 provides background information about coffee and the boom and bust episodes analyzed in this study. Section 3 describes data sources. Section 4 presents the empirical strategy. Section 5 shows the main results of the effect of coffee price shocks on long-run human capital and discusses alternative explanations for the main results and provides tests about their relevance. Section 6 investigates the long-run effects of coffee price shocks on income. Finally, Section 7 concludes. Further results and robustness checks are presented in the online Appendix.

2 Background

2.1 Coffee Cultivation in Colombia

Colombia has traditionally grown coffee since at least 1835.[10] Coffee cultivation requires quite specific geographic and climatic conditions. The most suitable areas for growing coffee in Colombia are those located on hillsides, with a steep slope and intense rain. Ideal climatic conditions include temperatures between 15 and 24 Celsius degrees, annual rainfall ranging from 1500 to 2000 millimeters, and relative humidity between 70 and 90 percent (Clifford and Wilson, 1985; Baron, 2010). Areas satisfying these cultivation conditions are

[10] Miller and Urdinola (2010) provide detailed description about coffee production in Colombia. Here, I capitalize and summarize their excellent review.

found particularly in the departments of Antioquia, Caldas, Quindío, Valle del Cauca, and Risaralda and as a result they concentrate the highest levels of coffee production (see Figure 1). Since rainy regimes vary across regions, Colombia is a particular case where coffee is harvested in two different periods each year. In most of the coffee growing regions, there is a main harvest period that goes from October to December, and another one that goes from April to May.

Coffee farming is a time- and labor-intensive process in Colombia. Harvest is performed by hand given that coffee is a tree crop grown on rough and steep terrain, which does not allow mechanical picking of the coffee beans. Pickers must distinct whether a cherry is fully ripe or not, and then select those in the right state of ripening, which can influence significantly the quality of the beans. Upon harvesting, cherries must be processed quickly to prevent spoilage. In Colombia, it is generally used the wet method to process coffee cherries (Giovannucci et al., 2002). Under this method, the cherries initially are separated from other byproducts (such as pulp and skin). The remaining beans are separated by size and then placed in a water tank for one or two days to remove any layer of mucilage and make the beans rougher. Next, the beans are spreading out on a surface to dry in the sun, which may take several weeks depending on weather conditions. Finally, the processed beans are sorted by size/weight and then collected in sacks of 60 to 70 kilograms to control the moisture level and store the beans up to one year. Distributors buy the processed beans and then export and sale to roasters.

Most coffee farms are small, occupying an average of fewer than six hectares of land, and therefore much of the nonharvest processes are performed by their families. Conversely, large farms can require day laborers for the purposes of nonharvest maintenance. Since selecting cherries in the right state of ripening is a time-demanding manual activity, both small and large farmers generally require hiring significant additional labor during harvest seasons. Pickers are generally men, and qualitative studies suggest that many farmers hire informally children to participate in this process (Bacca, 2015).

Coffee was Colombia's chief export product throughout much of the twentieth century. In the 1970s, the entire industry, including processing and transporting, accounted for as much as 30 percent of total agricultural GDP (Giovannucci et al., 2002). The prominence of coffee in the Colombian economy led to the creation of the National Federation of Coffee Growers (NFCG) in 1927, an industrial organization that seeks to advance the interests of coffee farmers. Following its creation, Law 76/1927 gave to the NFCG the authority to administer and manage all revenue generated from coffee exports. The organization directly charges an "export tax" to coffee producers and the national government transfer to the NFCG any other revenue generated by coffee production. These resources are used primarily to support and provide adequate service to the coffee growers by facilitating and financing the production, harvesting, processing, transport and exporting of coffee. These resources are also used to partially shield farmers against external shocks through an internal price system. In this system, the NFCG sets an internal price paid to farmers as a function of international prices and guarantees the purchase of all coffee that met quality requirements at this given price. It reduces partially volatility because farmers receive higher prices than they would otherwise receive during bust periods, and lower prices during booms. The internal price paid to coffee-growers is the same across all regions and net of the coffee export tax, and transportation and other markup costs incurred by exporters. Figure 2 shows the dynamics of the internal and international coffee prices.

2.2 Coffee Booms and Busts

Between 1950 and 2000, the world coffee market witnessed a series of contractions and expansions. During this period, Brazilian frosts and droughts were a major source of fluctuations in the international price of coffee. Harsh frosts can kill entire coffee trees and affect the following harvests, and since new plants take 3-4 years before the trees begin to bear fruit, it can have short- and mid-terms consequences on the supply of coffee. Brazil is the only major producer vulnerable to frosts due to its unique geographic and climatic conditions. The most severe frost occurred in 1976, which hit 55 to 70 percent of the coffee crops and generated world shortages of the crop product (Caviedes, 1981).This coincided with a rise in the international price of coffee of about 130 percent, the major boom in the recent history of the coffee market. In 1953 and 1994, other episodes of intense frosts destroyed crops and increased coffee prices by 15 to 70 percent relative to the previous year. Coffee prices also rose sharply between 1985 and 1986 because of a severe drought that decimated Brazilian coffee crops. While this shock was less severe compared to the 1976 frost, it coincided with a 94-percent increase in coffee prices in 1986.

Besides the frosts and droughts, there were other factors that also affected coffee prices. The coffee shortage caused by the 1985 drought was completely reversed by the supply expansion following the collapse of the International World Coffee Organization in 1989 (which regulated world coffee prices through a quota system).[11] In the years following the collapse, coffee prices fell to historically low levels. While prices rose in 1994 because of the Brazilian frost, they fell abruptly to new unprecedented levels from 1998 to 2002 due to a rapid and sharp expansion in the supply of coffee from Brazil and Vietnam. The

[11] The organization included both exporting and importing countries. In 1989, consumers in member countries were demanding increased quality of coffee and the end of selling coffee to non-member importing countries at reduced prices. The disagreement on a way to control exports to non-member countries, combined with Brazil's lack of interest in preserving the export quota system because of the domestic pressure, led to the suspension of the export quotas that regulated the market (Akiyama et al., 2001).

rapid expansion in Brazilian coffee production was caused by a government policy which promoted massive planting in areas affected by the 1994 frost and a series of reforms that devalued exchange rates and boosted exports (Akiyama et al., 2001). At the same time, the Vietnam's coffee industry was rapidly expanding during that years as result of the restoration of bilateral trade relations with the United States, and aggressive government export policies (Nguyen and Grote, 2004). Consequently, Vietnam had become the world's leading producer of Robusta beans by the late 1990s, second only to Brazil in total coffee production.

Figure 2 documents in detail the timing of these events. In sum, the major coffee booms and busts were originated outside of Colombia and are therefore plausibly exogenous to individual schooling decisions. As a result of these shocks, different cohorts faced different coffee prices, and thus returns to coffee-related work, at the time they were of school-going age. As we shall, this led to differential changes in completed schooling in areas with different coffee cultivation patterns.

3 Data

3.1 Coffee Cultivation and Price data

This paper uses data on average annual world coffee prices from the National Federation of Coffee Growers. Using Colombian consumer price index and exchange rate data, I convert the coffee price series to real 1998 Colombian pesos. Because the consumer price index is available only from 1954 onwards, my analysis focuses on the years 1954-2003. As discussed above, this period includes the major boom and bust episodes in the recent history of the coffee market. Although individual human capital investments are unlikely to affect the internal price paid to a coffee grower, I use international coffee prices in the main analysis. Supplementary analyses instrument the international price of coffee with the data on Brazilian coffee production, available from the Brazilian Institute for Applied Economic Research (IPEA).

To measure local coffee intensity, I draw on data from the NFCG's 1932 coffee census, the first nation-wide enumeration of coffee growers conducted in Colombia. Using these data, coffee intensity of municipality j is measured as the total hectares of land used for cultivating coffee in that municipality in 1932. I scale this variable by the total land area, given that some municipalities in Colombia vary significantly in size.[12] Since the intensity of coffee cultivation is measured before the major coffee price shocks, endogenous production responses to variation coffee prices is not a concern. Still, this measure is likely to capture

[12] Appendix Table B.1 shows that the results are qualitatively and quantitively similar if I use coffee cultivation in levels rather than normalized by total land area.

accurately the relative importance of the coffee to the local economy during the entire period of analysis. As discussed above, climatic and geographic differences within Colombia greatly determine suitability and thus the distribution of coffee cultivation (de Graaff, 1986). This is reflected in a high persistence of the geographic distribution of coffee cultivation patterns over time. As an illustration, Figure 3 plots the 1970 and 1932 coffee cultivation intensity ranks. It shows that municipalities with relatively high coffee cultivation intensities in 1932 tend to be the same ones with relatively high coffee cultivation intensities in 1970, with a correlation coefficient of roughly 0.87. The estimated slope of this relationship is 0.90: a 10 percentile increase in 1932 coffee cultivation intensity is associated with an 9 percentile increase in 1970 coffee cultivation intensity. Additional results in Appendix B show that the conclusions are essentially the same if coffee cultivation intensity is measured using the 1970 census.

There is substantial variation in the intensity of coffee cultivation across municipalities. For example, about 45 percent of municipalities in the sample are not classified as coffee producers. Conditional on being coffee producing, the standard deviation in the hectares of land used for cultivating coffee is 4.56 hectares per square kilometer (relative to a mean of 2.8 hectares per square kilometer). This variation in the role of coffee to the local economy, combined with the timing of coffee price shocks, forms the basis of my identification strategy.

3.2 Micro-level data and definitions

My long-run analysis relies on data from the 1973, 1993 and 2005 Colombian censuses, available through the Integrated Public Use Micro Sample (IPUMS).[13] Another available census include that conducted in 1985. I do not use these data in my long-run analysis because they do not contain any information about an individual's place of birth, information that is important to identify exposure to coffee market conditions in childhood (as described in detail below). The IPUMS provides information on 10 percent of individuals randomly drawn from the original census, along with expansion factors to preserve national representation. It includes basic demographic and socioeconomic information, including education, age, municipality-of-birth, and labor force participation, as well as industry and status in employment (class of worker) for individuals employed at the time of the census. I limit the sample to cohorts born between 1949 and 1983, who are 22-56 years old at the time they are observed in the census and thus have likely completed their schooling decisions.[14]

To estimate the extent to which individuals were exposed to coffee market conditions

[13]The IPUMS data are publicly available at https://international.ipums.org/international/.

[14]Specifically, the 1973 census includes cohorts born between 1949 and 1951, or individuals who are 22-24 years of age; the 1993 census includes cohorts born between 1949 and 1971, or individuals aged 22-44; and the 2005 census include cohorts born between 1949 and 1983, or individuals aged 22-56.

when they were of school-going age, I assume that the municipality where they were born is the same as the one where they grew up.[15] The data suggest that this assignment is plausible. Approximately 75 percent of children aged 5-16 were residing in their place of birth at the time of the 1964, 1973, 1985 and 1993 censuses.[16] Moreover, the vast majority of children residing in their place of birth did not move to a different municipality in the previous five years (about 96 percent), suggesting that migration was infrequent. Since children aged 5-16 in these censuses are virtually the same ones in the long-run analysis when they are adults, these statistics are very informative that the assignment is likely to be highly accurate for most of the sample.[17] Among the remaining 25 percent who reside in a different municipality at census time, about 50 percent lived in their place of birth five years earlier and this is true even among older children (ages 11-16). This suggests that the municipality of birth will still contain some information about school-age coffee market conditions for this group.

I match the individual census data with municipality-level coffee cultivation and price data by using information on the municipality and year of birth.[18] School-age exposure to coffee market conditions of the individual i is then measured as the average coffee price observed when that individual was age 5-16, interacted with the intensity of coffee cultivation in her place of birth.[19] I also present results from extended specifications that break down this age group and consider exposure at other ages (from before birth to age 22).

The primary outcome of interest is total years of education attained as defined in the census. In the original data, this variable was top coded by applying a cap at 18 years in the 1973 and 2005 census data and at 12 years in the 1993 census. Despite these differences, the results are very similar when excluding the 1993 census or when I impose a uniform top-coding. I also estimate potential changes in individual income. Since the Colombian census

[15]During the study period, compulsory schooling was 6. Since I use age at census time to infer individuals' year of birth, I am not able to identify accurately the year they turned 6. Consequently, I assume that the school-age period begins when an individual is $t+5$ years old. The results are essentially the same if I use instead $t+6$.

[16]These waves of the census collect data on whether the current municipality is the same one where an individual was born. In the 1964 and 1985 censuses, the municipality of birth is not collected for individuals who are residing in a different municipality at the time of the census.

[17]For example, I observe the 1978 cohort at ages 7 and 15 in the 1985 and 1993 censuses, but this cohort enters into the long-run analysis only through the 2005 census sample.

[18]The number of municipalities in Colombia is about 1120. However, the IPUMS combines neighboring municipalities to create geographical units with population greater than 20000, yielding approximately 500 time-consistent geographical units or simply municipalities. Therefore, I collapse the coffee census data into this broader definition of municipality to then match the observations to the individuals in the IPUMS sample.

[19]As an illustration, consider an individual born in 1949. The average international price of the coffee over the 1954-1965 period, interacted with coffee cultivation levels in her municipality, represents her childhood exposure to coffee market conditions.

does not collect any information about income, I follow Bleakley (2010) and calibrate an income score based on industry and class of worker using data from other Latin American censuses with available information on income.[20,21] I first estimate a regression of log total income on a full set of year × country fixed effects to remove census-country specific effects. The residuals from this regression are then collapsed into industry (15) × class of worker (3) × sex (2) cells and matched to the Colombian sample. Given this two-stage procedure, the resulting income score is measured in natural logs.

The expanded sample consists of approximately 33 million observations. To ease the computational burden, I collapse these data into municipality-of-birth × year-of-birth × census-year × sex cells. The resulting means are used as dependent variables in the regressions below, which are weighted by square root of the cell size to adjust for precision with which the cell means are estimated.[22] Descriptive statistics of these data are shown in Panel A of Table 1.

3.3 Other data

Other data sources are also used for supplementary analyses. To examine the relationship between coffee price shocks and school enrollment, I use published statistics about education from the Colombian *Anuario General de Estadistica* for the period 1954-1977.[23] It reports the total number of students enrolled in public and private schools at the department level. Breakdowns of these data at finer geographical levels are not available. Moreover, information on secondary-school enrollment is not systematically reported in these books, so I can examine only changes in primary-school enrollment. These records also contain information on the number of teachers, so I can also explore the potential role of teacher supply responses to coffee market conditions. Primary-school enrollment and teacher rates are calculated using data on student enrollment and teachers in the numerator. For the denominator, I linearly extrapolate population aged 5-11 using census data.

Finally, I have obtained data on a number of time-invariant municipality characteristics. These include local violence, incidence of specific diseases, manufacturing employment, level of development, and transport infrastructure, all of which are measured around 1950. I control for differential trends associated with these characteristics to assess the robustness

[20] In particular, I use IPUMS data from Brazil (1960, 1970, 1990, 2000, and 2010), Dominican Republic (1981, 2002), Mexico (1970, 1995, and 2000), Panama (1980, 1990, and 2010), Puerto Rico (1970, 1980, 1990, 2000, 2005, and 2010), and Trinidad and Tobago (1970, and 2000). I use the variable "INCTOT", which corresponds to total personal income from all sources in the previous month.

[21] The 1973 Colombian census does provide information on total income, but it covers a too limited set of cohorts in my analysis.

[22] Estimates based on this type of group-means data are asymptotically equivalent to the ones derived from the micro-data counterpart (Donald and Lang, 2007)

[23] After 1977, education statistics were not systematically collected and reported in these books.

of the main results. I also use data on conflict intensity from Dube and Vargas (2013) to examine the potential role of violence in explaining the main results. Appendix Table F.5 describes in more detail the source and definition of these variables.

4 Empirical Strategy

The empirical strategy exploits geographic variation in coffee cultivation patterns, together with time variation in international coffee prices. This approach is basically a differences-in-differences strategy. The first difference is over time across cohorts, because different cohorts faced different world coffee market conditions when they were of school-going age. The second difference is across geographic areas, as the predetermined intensity of coffee cultivation varies substantially across municipalities. The major difference between this approach and the standard two-group/two-period differences-in-differences is that I use two continuous measures of "treatment" intensities and thus exploit greater variation in the data. Since I use information on coffee cultivation intensity in an individual's municipality of birth, this approach is an intention-to-treat (ITT) design. In particular, I employ the following specification:

$$S_{jgtc} = \beta \left(\mathbb{P}_t \times \mathbb{I}_j\right) + \kappa \, \mathbf{T}_{jt} + \lambda_j + \gamma_g + \mu_{ct} + \xi_{jgtc} \tag{1}$$

where S is average years of completed schooling for individuals in municipality j, gender g, birth cohort t and census-year c. The key independent variable is given by the interaction of childhood coffee prices, $\mathbb{P}_t$, and the (time-invariant) measure of land used for cultivating coffee in the municipality in 1932 (scaled by municipality area), $\mathbb{I}_j$.[24] Childhood coffee prices is measured as the (log) average coffee prices observed between the years $t + 5$ and $t + 16$. In all specifications, I include municipality fixed effects and birth cohort $\times$ census-year fixed effects, λ_j and μ_{ct}, respectively, which capture any time-invariant differences across municipalities and common changes in each cohort. The baseline specification also allows for gender-specific effects by including γ_g. I also include municipality-specific time trends, $\mathbf{T}_{jt}$, in order to account for possible long-run dynamics in socioeconomic and other characteristics across areas.

The key parameter of interest is β, which summarizes the magnitude of the impact of coffee price shocks on schooling. If coffee price shocks lead to reduced educational attainment, then one would expect to see negative coefficients on β. To ensure that the results are not driven by serial correlation in schooling across cohorts, I use standard errors that are

[24] An alternative approach would be to use the intensity of coffee cultivation in 1932 as an instrumental variable for coffee cultivation intensity in 1970. This would eliminate any possible bias induced by endogenous production response to coffee prices, while using a more recent measure of coffee cultivation intensity. The results are quantitively and qualitatively similar when following this approach (results available upon request). Hence, one can interpret equation (1) as a reduced-form expression.

clustered at the municipality level throughout the analysis (Bertrand et al., 2004). A possible disadvantage of these standard errors is that they do not account for possible correlation across space, which might lead to misleading inference if there is significant spatial correlation in coffee suitability across municipalities. Appendix Table D.1 shows that statistical significance is largely unaffected under different assumptions about the covariance-variance matrix that address spatial correlation in error terms.

Identification requires the counterfactual assumption that absent any change in coffee prices, long-run outcomes of individuals in municipalities that produce coffee more and less intensively would have followed the same trends. This identifying assumption is plausible insofar both global coffee prices and geography of coffee cultivation are not affected by changes in an area's human capital investments. Although municipalities with varying coffee cultivation intensities may differ in ways that could affect human capital investments, any unobserved differences that are time-invariant will be stripped out by the inclusion of municipality fixed effects. Identification would be threatened only if there were omitted determinants of long-run individual human capital varying both over time in the same way as international coffee prices and disproportionally over space across municipalities cultivating more coffee. In principle, it is hard to think of any such a story given that many of the factors known to influence world coffee prices during the period analysis were originated outside of Colombia, and the timing of such shocks was plausibly unanticipated by individuals in prior years. Moreover, since the potential coffee market of an individual is given by her municipality of birth, it is not endogenous to future erratic changes in coffee price shocks.

The preferred specification includes a robust set of basic fixed effects and municipality-specific linear time trends, but results are almost unaffected if a number of additional controls are included (see Appendix Table B.1). Since treatment intensity varies across municipalities and cohorts, the ITT involves a fuzziness in the treatment and thus could mask the magnitude of the true treatment effect (De Chaisemartin and D'HaultfŒuille, 2017). Appendix Table F.1 present results from specifications that consider different definitions of the treatment variables. In particular, I consider different binary variables to classify areas into high and low cultivation intensities groups as well as periods into low and high coffee price categories. The implied magnitudes and significance from these different treatment definitions are strikingly similar to the baseline. After presenting the basic long-run results below, I present further results that provide support to the main interpretation of the findings and assess some possible alternative explanations.

5 Results

5.1 Coffee Price Shocks and Schooling

5.1.A Visual Evidence

I begin by examining graphically the relationship between coffee prices and educational attainment. Figure 4 plots the average coffee price faced by cohorts when they were of school-going age and the difference in years of schooling between high and low cultivation intensity municipalities. High and low coffee cultivation areas are defined as those municipalities above and below the 75th percentile of the coffee intensity distribution, respectively.[25] A clear-cut pattern emerges from this figure. It reveals that coffee booms are associated with fewer years of completed schooling in municipalities cultivating more coffee.

Figure 5 provides pictures of the association between coffee cultivation intensity and changes in cohort schooling for different time periods. Panel (a) shows that the changes in cohort schooling from 1949 to 1955 are statistically unrelated to the intensity of coffee cultivation, with a slope of 0.0004 (S.E. = 0.01, p-value=0.97).[26] Given these cohorts were exposed to relatively stable and similar coffee prices in childhood, this lack of association provides reassuring evidence that there were no pre-existing differential trends in schooling across municipalities with varying coffee cultivation intensity. Panel (b) compares schooling of cohorts born from 1956 to 1977, who were exposed to a big coffee boom in childhood, relative to that of cohorts born between 1949 and 1955. It shows clearly that the increase in cohort schooling was less pronounced in municipalities with greater coffee cultivation (slope=-0.03, S.E = 0.005, p-value=0.00). The pattern is completely reversed when one compares changes in schooling from the 1956-77 to the 1978-83 cohorts (panel c), where the latter group was exposed to relatively low coffee prices in childhood compared with the former group. Indeed, the increase in the completed years of education was significantly more pronounced in municipalities with greater coffee cultivation (slope=0.013, S.E = 0.005, p-value=0.00).

To examine in more detail the trends in schooling over time across municipalities with different coffee cultivation patterns, I estimate the following equation:

$$S_{jtc} = \sum_{t=1949}^{1983} \beta^t \left(\mathbf{1}(t=\tau) \times \mathbb{I}_j\right) + \lambda_j + \mu_{ct} + \xi_{jtc} \tag{2}$$

where $\mathbf{1}(\cdot)$ are dummy variables indicating the birth year of cohort t. The omitted group is $t = 1955$. This semi-parametric specification compares the trends in schooling over time

[25] The patterns are very similar if I define high and low cultivation areas using instead the median coffee cultivation.

[26] The conclusion is the same if the period 1949-1955 is divided into two approximately equal size groups.

in municipalities with different coffee cultivation intensities. It allows for cohort $\times$ census fixed effects, μ_{ct}, and municipality fixed effects, λ_j. Standard errors are clustered at the municipality level to account for serial correlation in error terms, ξ_{jtc}.

Figure 6 shows the results from estimating equation (2). It plots the coefficients β^t and the respective 95 percent confidence intervals. There are no differential trends in schooling among cohorts who were born between 1949-1955 across municipalities cultivating coffee more and less intensively. For the boom cohorts, those born between 1956 and 1977, there is a statistically significant decline in schooling in municipalities cultivating disproportionally more coffee. The pattern is reversed for the cohorts between 1978 and 1983, who faced lower coffee prices in childhood. Overall, these patterns in schooling mirror the trends in coffee prices. Taken as a whole, these pictures provide informal evidence that increases in the return to coffee-related work in childhood are negatively associated with long-run schooling.

5.1.B Formal Regression Results

Table 2 reports formal estimates of the effect of coffee price shocks on educational attainment based on equation (1). Column (1) presents results from a specification with no covariates besides municipality, cohort, census-year and gender fixed effects. Confirming the visual evidence, I find a significant effect of coffee price shocks on schooling, with a coefficient of -0.047 (standard error =0.013). It implies that higher coffee prices in school age lead to fewer years of education in areas with greater intensity of coffee cultivation. Columns (2)-(4) add various other controls sequentially to this specification. The addition of municipality-specific linear time trends in column (2) reduces marginally the magnitude of the estimated coefficient, which is now -0.040 (standard error =0.009). Point estimate is virtually identical when census-year $\times$ cohort fixed effects are included (column 3). In addition, controlling for municipality $\times$ census-year fixed effects in column (4) hardly change the results (-0.04 versus -0.038). Column (5) drop observations from the 1993 census to determine the extent to differences in the coding of schooling years across censuses affect the results. While this sample restriction drops 35 percent of observations in the expanded sample, the magnitude and standard error of the estimated relationship remain unchanged. Finally, column (6) imposes a uniform top-coding by applying a cap at 12 years across all census data. While this reduces somewhat the coefficient, it remains quite precise and highly significant. Overall, neither set of alternative estimates are statistically distinguishable from my preferred baseline specification (column 3).

Because coffee prices are likely serially correlated, a concern with these results is that they might be picking up the effects of different periods of coffee price shocks. To explore this issue, I evaluate how the effects of coffee price shocks vary with children's exposure age. In doing so, I estimate an extended version of equation (1):

$$S_{jgtc} = \sum \beta^a \left(\mathbb{P}_t^a \times \mathbb{I}_j\right) + \kappa\, \mathbf{T}_{jt} + \lambda_j + \gamma_g + \mu_{ct} + \xi_{jgtc} \tag{3}$$

where $\mathbb{P}_t^a$ denotes now the (log) average coffee price observed at age a for cohort t. I group exposure ages into four-year age bins to increase precision with which β^a is estimated. This specification is more flexible than the baseline and provides a more detailed picture of the relationship between coffee prices and completed schooling. It also provides an opportunity to directly evaluate the plausibility of the identifying assumption. If the research design is valid, then the magnitude of the coefficients should decline to zero for ages for which individuals already completed schooling decisions. Large and significant effects would suggest the presence of pre-existing differential trends in schooling driven by other factors.

Figure 7 shows the results from estimating the extended model (3). It plots estimates of β^a and respective 95 percent confidence intervals. Consistent with the identifying assumption, the effects of exposure to coffee price shocks after age 16 are small and statistically indistinguishable from zero. This is unsurprising given that the vast majority of individuals completed about 12 years of schooling (about 90 percent) and thus finalized schooling decisions at age 17. The largest negative and significant effects are observed for exposure at ages ranging from 5 to 16, the timing of schooling decisions. The effects of exposure to coffee prices before age 5 are smaller and generally statistically significant.[27] The timing of the effects is in line with the baseline specification and consistent with the interpretation that coffee price shocks induce an opportunity cost of schooling effect that dominates any income effect

Overall, the results indicate that coffee booms in childhood lead to reduced educational attainment. To gauge the magnitude of this effect, consider the change in the average price of the coffee from cohorts born in 1954 to those born in 1970. The former cohort was exposed to coffee prices relatively low when they were of school-going age, while the latter faced the major booms caused by the Brazilian frosts and droughts. This resulted in a difference of 0.5 log points in the average coffee price these cohorts faced when they were of normal schooling ages. The preferred estimated coefficient of -0.040 implies that, given the 50 percent change in the international price of coffee, the decline in education is 0.09 years larger in areas with one standard deviation more coffee cultivation (0.5 $\times$ -0.04 $\times$ 4.5=-0.09). This represents about a 1.2-percent reduction in schooling relative to the

[27]The fetal origins literature suggests that income shocks in early infancy should have long-run repercussions on schooling. However, since income shocks are accompanied by substitution and income effects in the production function of infant health ((Miller and Urdinola, 2010)), one possibility is that both effects are of similar magnitude in terms of long-run outcomes in this setting.

sample mean of 7.5 years.

5.1.C Brazilian Coffee Production as a Source of Variation

A potential concern is that Colombia is a major coffee exporting nation and it may bias the estimates above. In particular, it might be that education changes reflect simply unobservable shocks that affect negatively both human capital investments and coffee production levels in large coffee production regions —which in turn causes the international prices to increase —rather than a causal relationship. I argue that this is unlikely to be the case given that the major booms and busts in the period of analysis have been shown to be originated outside of Colombia (Talbot, 1997). In this section, I provide explicit evidence supporting this claim by exploiting variation in coffee production volume of Brazil. Brazil is a world's leading producer of coffee, and the major booms and busts in the study period were the result of changes in its supply. Thus, variations in Brazilian coffee production levels provide a shock in international coffee prices virtually independent of Colombian market conditions.[28]

I begin by presenting reduced-form estimates of the relationship between Brazilian coffee production levels and educational attainment. Specifically, I rererun the baseline specification (1), but replace coffee prices by Brazilian coffee production. These results are shown in column (2) of Table 3. I find that reductions in Brazilian coffee production levels are associated with fewer years of education, a relationship that is statistically distinguishable from zero at the conventional levels of significance. This finding makes sense and is in line with the baseline results given that large contractions in coffee production in Brazil translate into increased coffee prices.

Next, I present results from estimating two-stage least squares (2SLS) regressions where Brazilian coffee production levels is used as a source of exogenous variation in the international price of coffee. Specifically, I use the interaction between coffee cultivation intensity and Brazilian coffee production as an instrument for the interaction between coffee prices and cultivation intensity. This means that the model will be identified solely by the variation induced by coffee production shocks in Brazil, but the magnitudes can be interpreted as the marginal effect of international coffee prices. Table 3, column (3) reveals a coefficient of -0.034 (standard error =0.009), which is statistically significant at the 1 percent level of significance. The magnitude of the estimate coefficient is somewhat smaller than the baseline reported in Table 2, but I cannot reject that both coefficients are the same. Thus, it

[28] In the data, the correlation between international coffee prices and Brazilian coffee production is about -0.60. This high correlation is very unlikely to be the result of an immediate production response to changes in coffee prices, since the biological time required for a new coffee plant produce their first harvest is about 3-4 years. Rather, this high correlation is likely to be the result of changes in available supply in Brazil affecting coffee prices.

seems unlikely that my baseline results reflect unobservable shocks affecting both individual human capital investments and international coffee price dynamics.

5.1.D Are Children Really Attending School Less and Working More during Coffee Booms?

If higher coffee prices raise the opportunity cost of staying in school, then one would expect to see students attending school less regularly and working more during coffee booms. I now examine this question by estimating how school attendance and child labor change with changes in current year coffee prices. To do so, I aggregate census microdata to the department/census-year/cohort/gender level and estimate the following model:

$$Y_{jcgt} = \beta \left(\mathbb{P}_t \times \mathbb{I}_j\right) + \kappa \, \mathbf{T}_{jt} + \lambda_j + \gamma_g + \mu_{ct} + \xi_{jcgt} \tag{4}$$

where Y is either the proportion of children who are currently attending school or working in the department j cohort aged c and gender g at the time of the year t census. The interaction term measures, therefore, prevailing coffee market conditions at the census-year t. The model controls for department-specific time trends, and department, gender, census-year and cohort-census fixed effects. For this analysis, I use data from the 1973, 1985 and 1993 censuses.[29]To improve precision, I limit the sample to children in rural areas, since coffee price shocks is likely to have only limited impacts in large urban areas.[30] The unit of observation is at the department rather than municipality level for two reasons.[31] First, municipality of birth is not available in the 1985 census. Second, matching individuals with coffee cultivation rates of the municipality where they are observed at the time of census is problematic because of selective migration. Since the vast majority of migration occurs within departments, aggregating the data to the department level largely reduces concerns about selective migration. Indeed, only 5 percent of children moved to a municipality in a different department in the previous five years before the census year. Summary statistics for this sample are displayed in Panel B of Table 1

Table 4 shows the results from estimating model (4). For inference, I estimate standard errors clustered at the department level. Because these standard errors may be biased due to the small number of clusters, I also calculate two-tailed p-values using the wild cluster bootstrap-T method (Cameron et al., 2008). Column (1) documents that increases in real-world coffee prices are associated with reduced school attendance. The estimated coefficient is precisely estimated and thus highly significant at the conventional levels of

[29]I do not use information from the 1964 census because data on school attendance and child labor are not comparable with respect to those of the other censuses.

[30]While including children in urban areas leads to reduced precision, the results and conclusions are basically the same.

[31]Appendix Table A.1 documents that the long-run results discussed above are similar if the data are aggregated at the department (rather than municipality) level.

significance. It implies that for the coffee price change from 1985 to 1993 (a reduction of 0.82 log points), the increase in school attendance is about 2.3 percentage points larger in areas with one standard deviation larger amount of coffee cultivation ($-0.011 \times 0.82 \times 2.6 = -0.023$). Columns (2)-(3) show that the magnitude and significance is similar when the regressions are run separately for children aged 5-11 and 12-16.

I supplement these results by examining school enrollment rates using official statistics about education at the department-by-year level over the 1954-1977 period. An important strength of these data is that they are from administrative records and likely less subject to measurement error than self-reported school attendance. Column (4) shows the results from estimating a variant of equation (4) that uses a department-level panel of primary-school enrollment rates. Consistently with the census results, I find that increases in international coffee prices are associated with reduced school enrollment rates, a relationship that is statistically significant at the conventional levels of significance. The sharp rise in the price of coffee from 1970 to 1976 (a difference of 0.78 log points) implied a reduction in school enrollment that is approximately 10 percentage points larger in municipalities with one standard deviation additional coffee cultivation. Since the average school enrollment rate in the sample is 72 percentage points, this is a relatively large effect.

Column (5) shows the results of the effect of coffee price shocks on child labor. I find a positive effect of international coffee prices on this outcome, with an estimate coefficient of 0.003 (standard error =0.0012) which is statistically distinguishable from zero at the conventional levels of significance. The estimate implies that the fall in the price of coffee between 1985 and 1993 led to a decline in the proportion of child work that is 0.7 percentage points larger in municipalities with one standard deviation more coffee cultivation. This effect represents a 13-percent reduction relative to the sample mean.

Summarizing, the results of this section suggest that coffee booms lead to reduced school attendance and increased child labor. This finding is consistent with the view that during coffee booms, the opportunity cost of schooling rises significantly and consequently some youths at the margin respond by supplying more labor and reducing educational investments.

5.1.E Gender Heterogeneities

Although women's labor supply was rapidly increasing during the 1970s and 1980s, it was generally much lower compared to that of men, with differences in employment rates of more than 40 percentage points. These differences are similarly striking when considering only children under 16 years of age: while 18 percent of boys were employed in 1985, this figure was only 9 percent for girls. This suggests that many of the factors affecting the decision of supplying labor were relatively less important for girls than for boys. If this applies to changes in the opportunity cost of studying, one would expect girls to be less responsive to the same coffee-related work opportunities, and hence female school attainment would decline less than male. Confirming this prediction represents a test that coffee price shocks affect schooling primarily through changes in the opportunity cost of schooling.

In Table 5, column (2) estimates the effects of coffee price shocks on child labor using an extended version of equation (4) that includes an interaction between a dummy for gender and coffee market conditions. Given this three-way interaction term, I also include the interaction terms $female_{jgtc} \times \mathbb{P}_t$ and $female_{jgtc} \times \mathbb{I}_j$. In addition, the model allows for gender-region-specific time trends. I find that male employment rates increase significantly more than female during coffee booms in areas with greater intensity of coffee cultivation. This finding is consistent with the notion that girls respond less pronouncedly to the same changes in the return to coffee-related work.

Column (4) examines gender differences in the effects of childhood coffee market conditions using an analogous extended version of equation (1). Consistent with the finding above, the coefficient on the key interaction term is statistically significant and suggests that coffee price shocks have a more pronounced impact on male schooling than female. Boys respond more to changes in the return to coffee-related work, and it translates into larger changes in their completed human capital relative to girls.

5.1.F Interpretation of Magnitude

Th results above indicate that for an increase in coffee prices of 0.5 log points, the decline in educational attainment is 0.09 years larger in areas with one standard deviation additional coffee cultivation. To get a better sense of the magnitude of this effect, I can compare it with well-documented interventions targeting education in developing countries. Perhaps the best-known examples are the Sekolah Dasar INPRES program in Indonesia (Duflo, 2001) and the PROGRESA experiment in Mexico (Behrman et al., 2009, 2011). The INPRES program resulted in the construction of more than 60,000 new primary schools within a short timeframe, increasing enrollment rates from 69 to 83 percent. Under the PROGRESA experiment, mothers were randomized to receive bimonthly cash payments that are conditional on their children's regular school attendance and healthcare center visits. The INPRES program raised educational attainment by 0.15 years, while PROGRESA led to an average increase of 0.19 years of education. Other well-documented intervention that is particularly relevant to my setting is the Colombian PACES program, which randomly assigned private school vouchers among 125,000 students (Angrist et al., 2002). In this program, treated children completed an additional 0.1 years of schooling. Therefore, the estimated effects of coffee price shocks are almost identical to those produced by the PACES program and approximately half of those associated to the INPRES and PROGRESA.

5.2 Robustness Checks

I have conducted a number of specification checks to investigate the robustness of the main findings, all of which are described in detail in the Online Appendix. Appendix A shows that the conclusions are essentially the same when excluding "movers" or aggregating the data to the department level. This suggests that mobility during the school-age period is unlikely to play a major role. Appendix B documents the robustness of the results to a variety of alternative specifications: excluding non-growing areas, measuring coffee cultivation intensity using the 1970 coffee census, and controlling for interactions of time fixed effects with a number of baseline characteristics. In Appendix C, I investigate the role of selective attrition and conclude that it is not large enough to significantly affect the estimates. Appendix D considers alternative assumptions about the covariance-variance matrix (using the Conley's spatial covariance matrix, clustering errors either at the department level or both at the municipality and year-of-birth level) and shows that inference is largely unaffected by the choice between different assumptions. Finally, Appendix E conducts a non-parametric permutation test and documents that the conclusions remain unchanged.

5.3 Alternative Explanations

While I have argued that the opportunity cost of studying is an important mechanism linking childhood coffee price shocks to long-run human capital, there are other possible explanations for the results. These include supply of teachers, household work decisions, and local violence. This section discusses these hypotheses and provides tests about their relevance.

5.3.A Supply of Teachers

One might argue that increased return to coffee-work related activities may not only raise the opportunity cost of time for students, but also for teachers. Teachers may leave school during coffee boom years to participate in coffee-related labor activities, including weeding and harvesting, which could negatively affect student outcomes. It may explain the negative relationship between coffee prices and educational attainment. I argue this is unlikely given that teachers are highly educated workers with salaries significantly large relative to wages in the agricultural sector in general. According to the 1973 census, the median wage in the educational sector is more than 300 percent higher than that in the agricultural sector. The differences are substantial even if one compares the median income in the educational sector with the 90th percentile income in the agricultural sector, a differential of the order of 98 percent.

Published data about teachers and school enrollment allow me to evaluate directly the teacher supply hypothesis. I reestimate the effect of coffee price shocks on student enrollment rates, but include teacher rates as a control. If the results were driven by changes in the supply of teachers, the coffee price shocks should lose all its association with student enrollment rates once one controls for teacher supply. While the inclusion of teacher rates is a strong predictor of student enrollment rates, it hardly changes the coefficient on the log coffee price × coffee cultivation interaction (Appendix Table F.2, column (2)). I also estimate a model where teacher rates is used as a dependent variable and find no statistically meaningful effect of coffee price shocks on this outcome (Appendix Table F.2, column (3)).

5.3.B Household Work Decisions

Coffee price shocks may increase the probability that adult family members enter the workforce, including mothers and older siblings. Hence, a possibility is that the patterns of schooling I find are not very determined by the opportunity cost of schooling but by adults making a child stay home (rather than in school) when they enter the workforce. This could explain $\beta < 0$ in the estimation of equation (1). However, the results in Section 5.1.D showing that children enter the labor force (and hence do not stay home) during coffee booms provide some evidence against this alternative interpretation. Moreover, this alternative hypothesis seems harder to reconcile with the patterns showing larger impacts on schooling for males than females. For this to arise, adults would have to be more likely to make males stay home during booms than females. It is not completely clear in principle why this would be the case.

To investigate this potential mechanism in more detail, I reexamine the relationship between coffee price shocks and school attendance described in detail in Section 5.1.D. In particular, I estimate a variant of the model (4) that includes as a control the fraction of household members over 16 years of age who are employed at census time. The results are shown in Appendix Table F.3. If coffee price shocks affect schooling through changes in household work decisions, then including this control should significantly reduce the magnitude of the effect of coffee price shocks on school attendance. However, I find that the key coefficient of interest remains unchanged and highly significant after controlling for adult employment rate. In column (3), I present results from a specification that allows the effects of coffee price shocks to differ depending on adult employment rate. If the household work hypothesis is an important mechanism, one would expect to see larger effects for children from households with greater adults in the labor market. I find no evidence of differences in the effect of coffee prices shocks with respect to adult employment.

5.3.C Local Violence

Colombia faced an intense war between governments, paramilitary groups, and left-wing guerillas that began in the mid-1960s and affected some regions. A prominent body of work, conducted in a variety of countries, have documented that income shocks can affect the intensity of conflicts (Dube and Vargas, 2013; Angrist and Kugler, 2008). In turn, intense violence episodes may negatively affect school's physical and human resources, and hence have disruptive effects on human capital formation (Monteiro and Rocha, 2017). To explain the reduction in educational attainment I find, increases in the price of coffee should significantly increase violence disproportionately in municipalities cultivating more coffee. The evidence indicates that the exact opposite is true. Dube and Vargas (2013) convincingly show that increased coffee prices lead to less guerrilla attacks, less paramilitary attacks, and less clashes in areas where coffee cultivation is more salient. The extent to which changes in violence matter, this mechanism would imply positive effects of coffee price shocks on schooling, which is inconsistent with my results.

Using data on conflict, I can empirically assess the possibility that differences in violence intensity during coffee booms explain my results. In Appendix Table F.4, I reestimate the long-run regression including interactions of coffee prices $\times$ coffee cultivation intensity with different measures of conflict intensity (attacks, clashes, massacres, and political kidnappings). If my results were entirely driven by differences in conflict intensity, one should observe significant estimates on these interactions and an insignificant coefficient on the log coffee price $\times$ coffee cultivation intensity interaction. None of the additional interactions is statistically significant. Moreover, the magnitude and standard error of the key coefficient of interest is very similar to the baseline. Hence, it is very unlikely that coffee price shocks act primarily through changes in conflict intensity.

6 Coffee Price Shocks and Labor Market Prospects

6.1 Basic Results

Although the schooling results are of central interest, a natural question is whether these changes in schooling decisions can alter subsequent labor market prospects. Work at earlier ages may provide some benefits, including acquisition of specific skills, increased social capital and general work experience, that may be rewarded later in the labor market. If these potential rewards are large relative to income losses from reduced schooling, then this would imply positive overall impacts on subsequent labor market prospects.

To examine this question, I rerun the baseline model (1), but use the income score described in Section 3.2 as dependent variable. The results are shown in Table 6. I find that a rise in coffee prices in childhood leads to a statistically significant reduction in adult income. The estimated coefficient implies that the coffee price change between the cohorts born in 1949 and 1970 is associated with 0.8 percent larger decline in income in areas with one standard deviation larger amount of coffee cultivation.

Columns (2)-(4) separate the effects of exposure at the primary-school and secondary-school ages. I find that coffee price shocks the largest effects come from exposure at the normal secondary-school-going ages. I also estimate the extended model (3) which considers separately the effects of exposure at different ages (Figure 8). Consistent with the results above, coffee price shocks affect long-run income primarily from exposure at ages 12-15. These patterns suggest that the mechanisms underlying the income effects vary depending on the age of exposure. It may be, for example, that the returns to secondary schooling are higher than the returns to primary schooling (Jensen, 2010). If schooling is the only mechanism driving the relationship between coffee market conditions in childhood and subsequent labor market income, then this the differences in returns to schooling may explain these patterns.

In sum, the results of this section suggest that cohorts who faced sharp rises in the return to coffee-related work when they were of school-going age are in lower-paid occupations. This suggests that the negative impacts of coffee price shocks on human capital appear to dominate any positive impacts.

6.2 The Role of Education

In this section, I evaluate empirically the extent to which changes in completed schooling can account for the effects of coffee market conditions in childhood on labor market income. I begin by reestimating the baseline model, but including years of schooling as an additional control. Table 7, column (2) shows that there is a strong, positive and significant association between education and income, with an estimated coefficient of 0.042 (standard error=0.001). After controlling for this association, the effect of coffee price shocks on income falls by about 50 percent and now is marginally significant only at the 10 percent level. This suggests that the changes education can account for about half of the observed changes in income induced by coffee price shocks.

A shortcoming of this exercise is that education is likely to be endogenous. In particular, if education is subject to measurement error, then my analysis is likely to underestimate the importance of education in explaining the changes in income.[32] Given the absence of

[32]In addition, the variation in schooling may be more limited in the collapsed data, weakening the relationship between education and income. Consistent with this possibility, OLS regressions of income on education using data at the individual level suggest returns to education that are significantly larger than that derived from the collapsed data.

an instrument for educational attainment in my setting, I perform a bounding analysis where the effect of education is restricted to values that are consistent with estimates in quasi-experimental studies.[33] This literature suggests that the causal economic return to schooling is generally higher than the estimate that emerges from OLS regressions. For example, studies exploiting changes in compulsory schooling laws find returns to education that are from 20 to 40 percent higher than the respective OLS estimates (Card, 1999; Oreopoulos, 2006).[34]. Therefore, I conservatively bound the return to schooling to values that are 40 percent below and above the OLS estimate found in column (2).

The results of this bounding analysis are presented in columns (3)-(6). I find estimates that are significantly smaller than the baseline across all the different education effects considered. If one assumes that the causal return to education is 40 percent above its OLS estimate, I find an effect of coffee price shocks on income that is 64 percent smaller than the baseline and statistically indistinguishable from zero. Alternatively, if one assumes a return to education that is 40 percent below the OLS, which is inconsistent with the magnitude of the OLS bias suggested by the literature, I find even an estimate of β that is substantially smaller: about 30 percent smaller than the benchmark.

As an alternative approach, I directly apply the causal effect of education found in previous literature. These studies estimate returns to education that range from 6 to 12 percent (Card, 1999; Acemoglu and Angrist, 2000; Duflo, 2001). Assuming that these estimates are close to the causal return to education in my setting, columns (10)-(16) show estimates of β from this exercise. All estimates are small and statistically insignificant. For example, assuming economic returns to education ranging from 6 to 10 percent —as suggested by Duflo (2001) for a developing country —the effect of coffee price shocks on income falls by 65 to 90 percent.

Overall, I find that once the economic return to education is restricted to values consistent with existing literature, the point estimate of the effect of coffee price shocks on earning becomes substantially smaller than the baseline and statistically insignificant. This suggests that the direct economic returns to coffee-related work must be, if anything, small relative to leaving school "too soon".

6.3 Implied Returns to Schooling

Given the evidence above, I can combine the schooling and income results to get a "back-of-the-envelope" estimate of the marginal effect of schooling on income. The estimates

[33] This strategy is similar in spirit to that of Becker and Woessmann (2009), who estimate the causal effect of Protestantism on economic prosperity.

[34] Exceptions to this pattern find a causal effect of education on earning that is about 10 percent below the OLS estimate (Leigh and Ryan, 2008)

suggest that coffee prices reduced educational attainment by 0.09 years and income by 08 percent. Together, these estimates imply that the marginal effect of an extra year of schooling on income is about 10 percent (0.099=0.008/0.09). This estimate is of reasonable magnitude and virtually identical to the local average treatment effect (LATE) obtained from an instrumental variable framework where the interaction of coffee prices with the intensity of coffee cultivation is used as an instrument for schooling. Compared to well-identified studies in the literature, this implied return to schooling is well within the range of existing estimates ranging from 6 to 12 percent (Card, 1999; Acemoglu and Angrist, 2000; Duflo, 2001).

7 Conclusion

This paper has provided new evidence on the long-term consequences of transitory income shocks on human capital. Previous studies examining this question has focused on short-run school outcomes and it is unclear whether these shocks reflect persistent long-run differences in completed human capital or simply temporary interruptions to schooling. To advance our understanding of this important question, this study exploits variation in local economic conditions in Colombia generated by dramatic fluctuations in the international price of coffee. The results indicate that a rise in coffee prices in school age reduces educational attainment differentially in areas cultivating more coffee. The main estimate suggests a reduction of 0.09 years of schooling, which is similar to the impacts of well-documented interventions targeting education in developing countries. These findings are consistent with the notion that transitory changes in the relative opportunity cost of schooling can cause permanent changes in human capital investments and not just a mere delay. Importantly, I show that other alternative mechanisms are unlikely to play a major role in explaining these patterns, including changes in teacher supply, household work decisions, and local violence.

This paper also documents that cohorts who faced sharp rises in the return to coffee-related work when they were of school-going age are in lower-paid occupations as adults. This suggests that the negative impacts of increased return to coffee-related work on human capital appear to dominate any positive impacts. An important implication of these findings is that youths may be ignoring the future in response to transitory improvements in local labor market conditions. This behavior may be driven by inpatient and constrained youths rationally trading off between immediate income gains and future returns to extra schooling, with no consequences in terms of welfare. Alternatively, youths may heavily focus on the present and make suboptimal choices when faced with immediate economic opportunities if they are not well informed on the expected returns from staying in school (Frederick et al., 2002; Laibson, 1997; O'Donoghue and Rabin, 1999; Jensen, 2010). This is especially true if adolescents are risk-averse, and higher expected returns from extra schooling are offset by greater income uncertainty (Levhari and Weiss, 1974). In this case, providing extra incentives to stay in school during economic booms may lead to improvements in welfare. Differentiating between both stories is beyond the scope of this study, but these results may motivate future studies in this area given the very different welfare implications of both mechanisms. Further research using hypothetical questions or experimental games may provide important insights regarding this question.

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Figure 1: —Coffee Cultivation (in hectares per square kilometer)

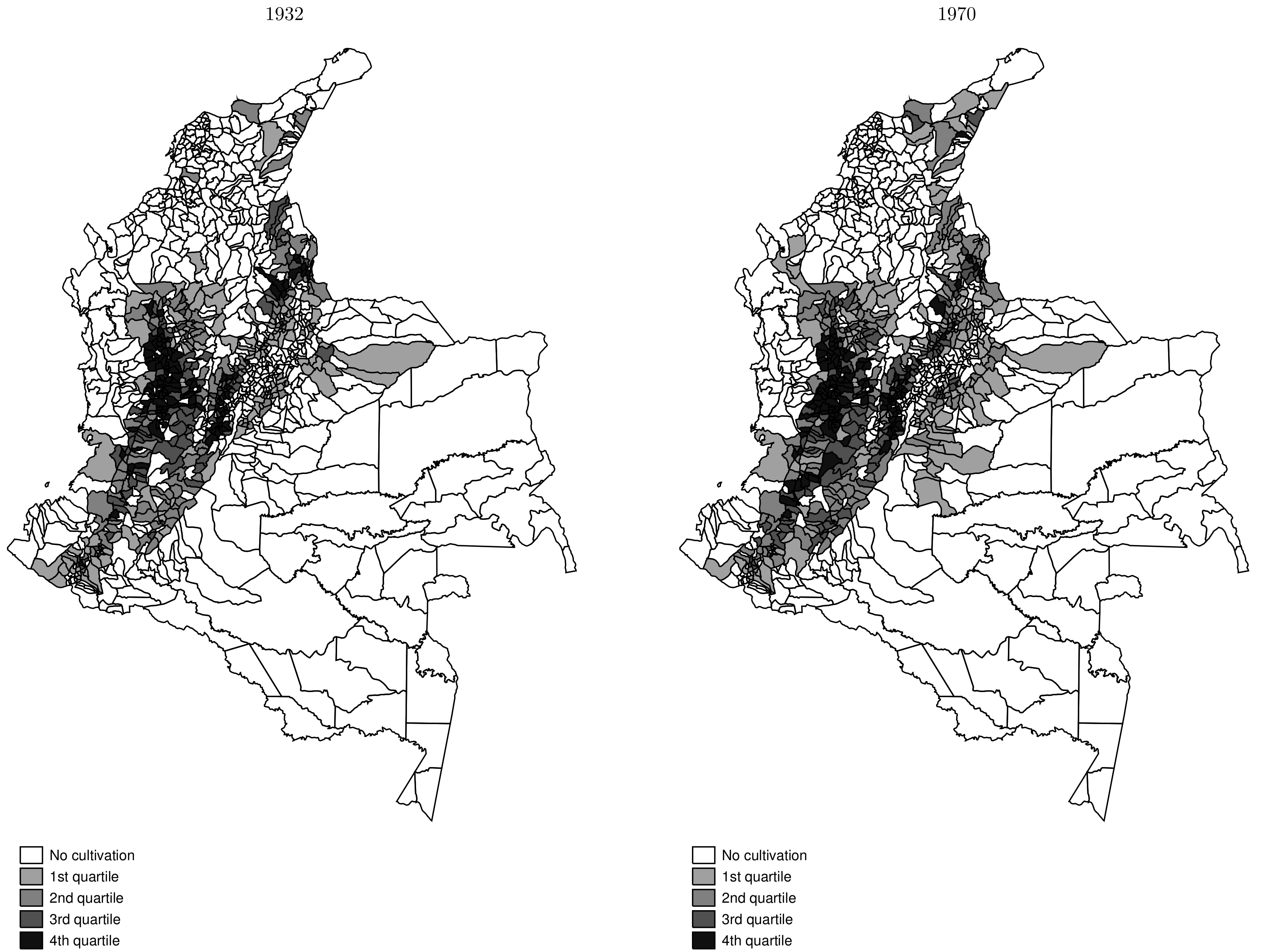


Figure 2: —Real International and Internal Coffee Prices

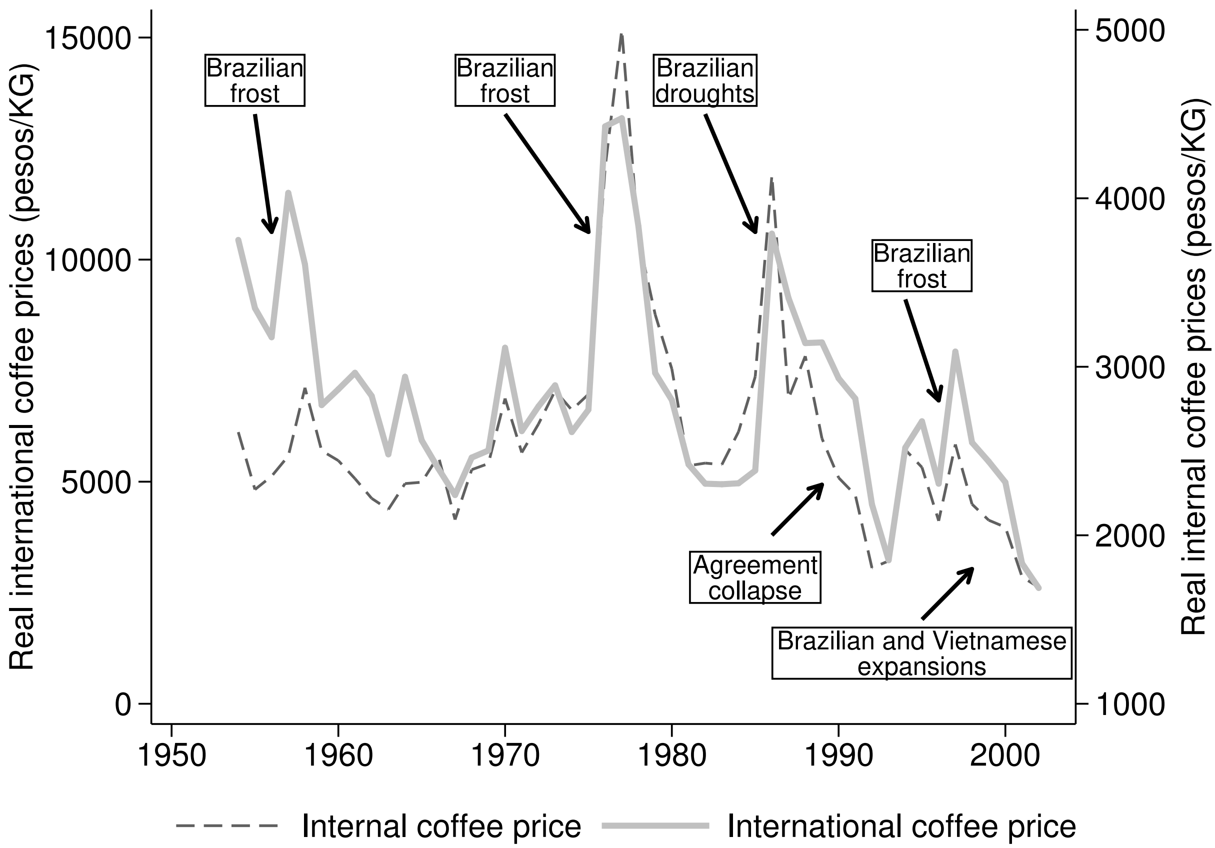
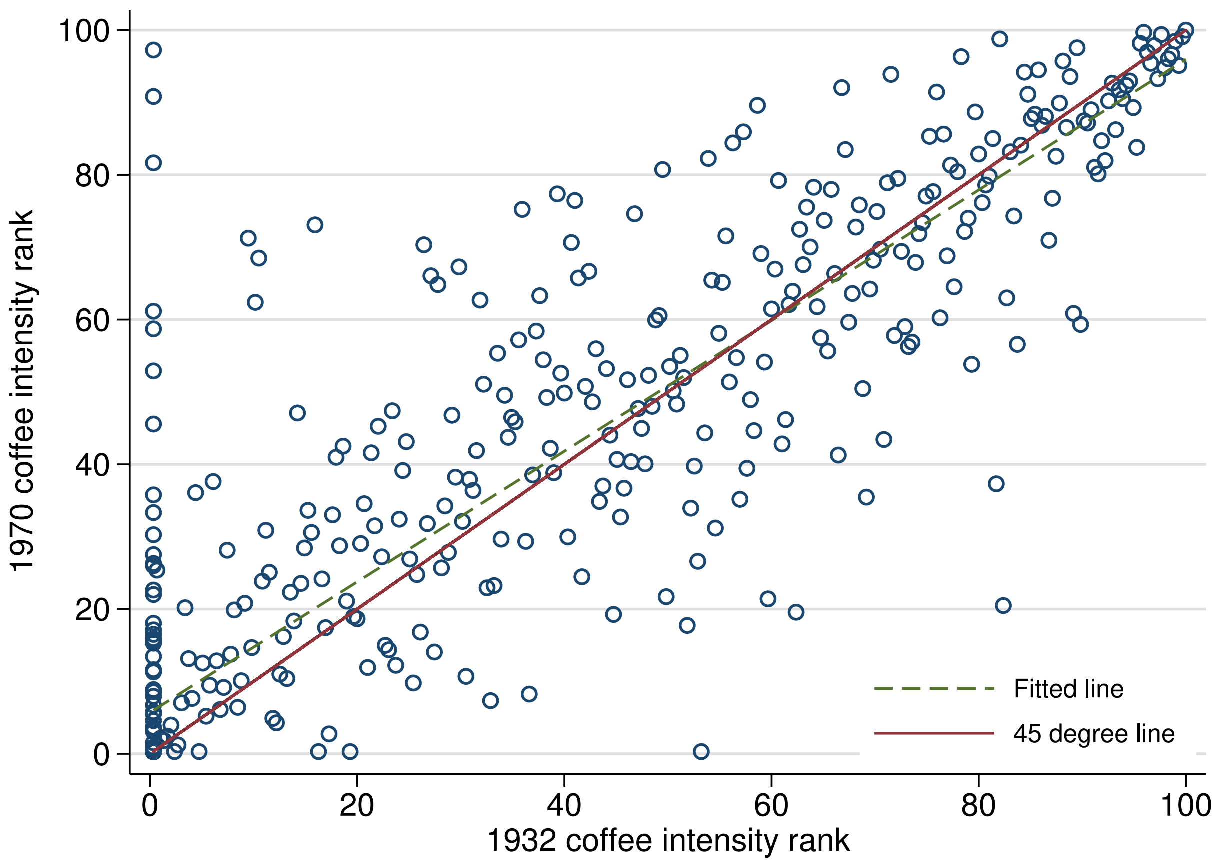
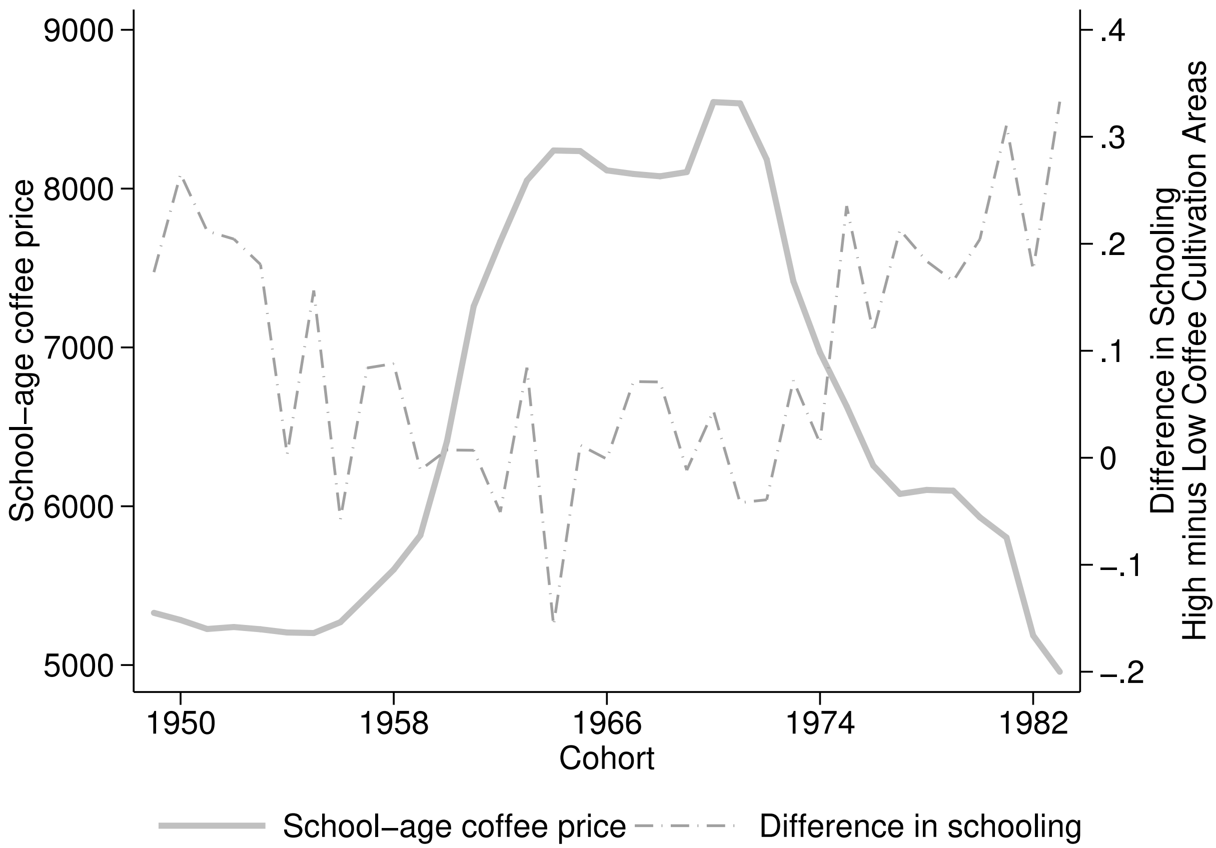


Figure 3: —Distribution of Coffee Cultivation Intensities in 1932 and 1970



Notes. This figure plots the 1970 coffee intensity percentile rank against the percentile rank of coffee intensity in 1932. The solid line correspond to the 45° line. The dashed line is from a regression of these variables, where 1980 coffee intensity rank is the dependent variable. The estimated slope is 0.86 (SE=0.02).

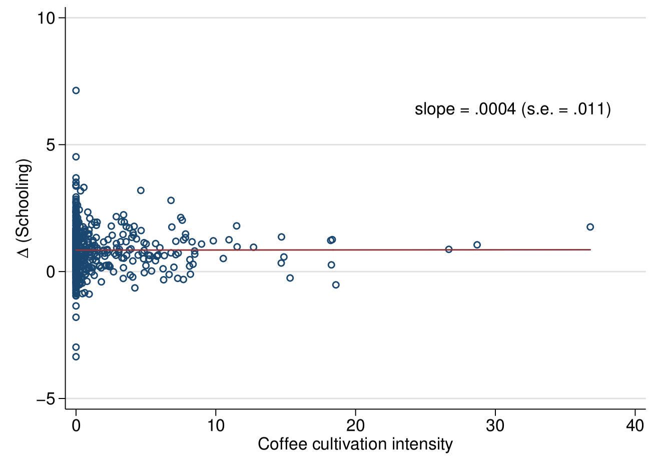
Figure 4: —School-Age Coffee Prices and Completed Schooling



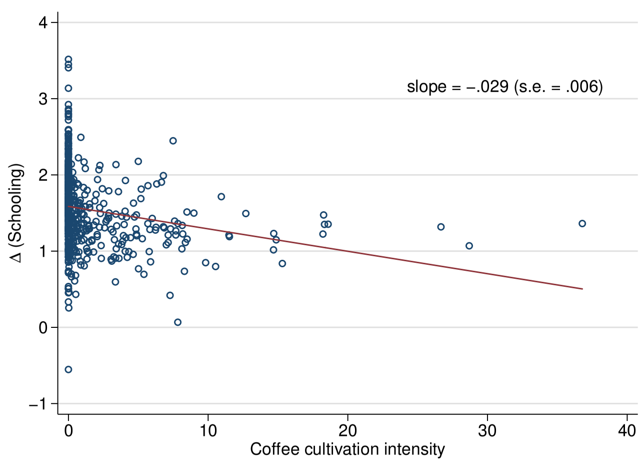
Notes. High coffee cultivation areas correspond to those cohorts born in municipalities above the 75th percentile of the coffee cultivation distribution. Analogously, low coffee cultivation areas correspond to those in municipalities below the 75th percentile of the coffee cultivation distribution.The school-age coffee price of the cohort born in year t is the average coffee price observed between the years $t + 5$ and $t + 16$.

Figure 5: Coffee Cultivation and Changes in Cohort Schooling

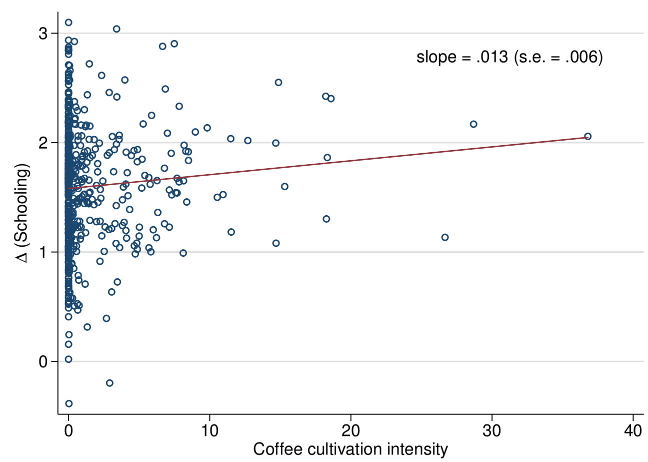
(a) 1955 vs 1949 cohorts
(exposed to stable coffee prices)



(b) 1956-77 vs 1949-55 cohorts
(Exposed to boom vs stable coffee prices)

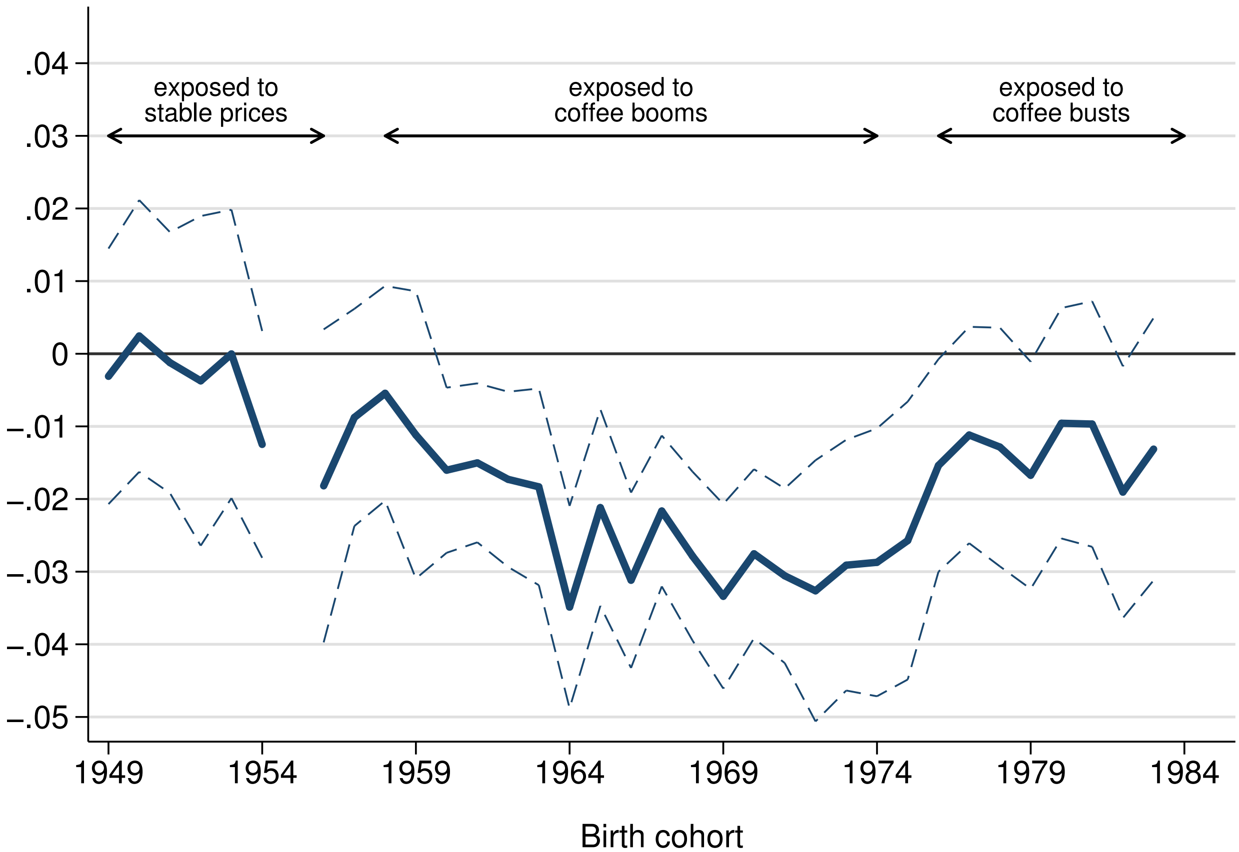


(c) 1978-83 vs 1956-77
(Exposed to bust vs boom)



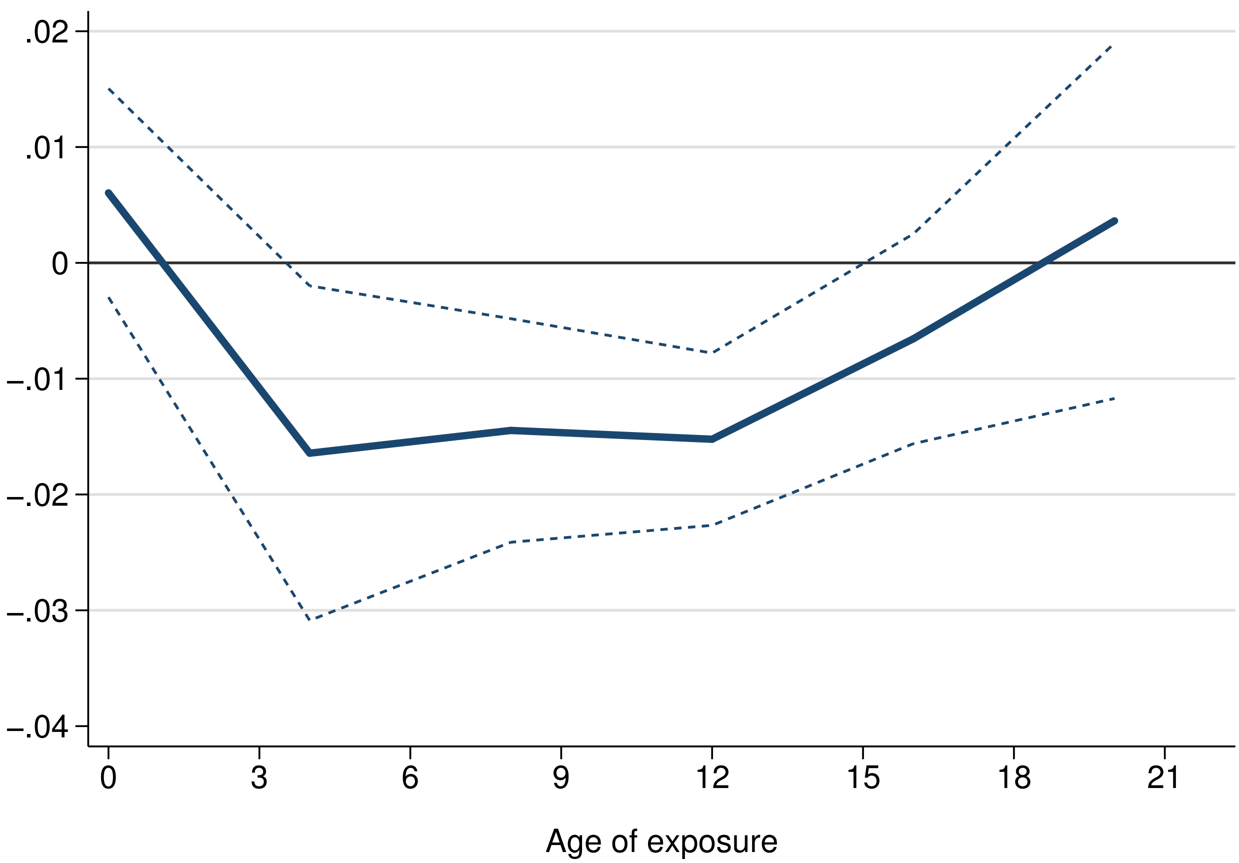
Notes. This figure plots the changes in schooling across cohorts over time and coffee cultivation intensity in each municipality. Panel (a) plots the changes in schooling from the 1949 cohort to the 1955 cohort. Panel (b) plots the changes in schooling from the 1949-55 cohorts to the 1956-77 cohorts. Panel (c) plots the changes in schooling from the 1956-77 cohorts to the 1978-83 cohorts. Sample includes individuals who are 22-56 years old at census time (1973, 1993, 2005). The solid line corresponds to OLS estimates of the relationship between changes in cohort schooling and coffee cultivation intensity, where the latter is used as independent variable.

Figure 6: —Cohort Schooling and Coffee Cultivation



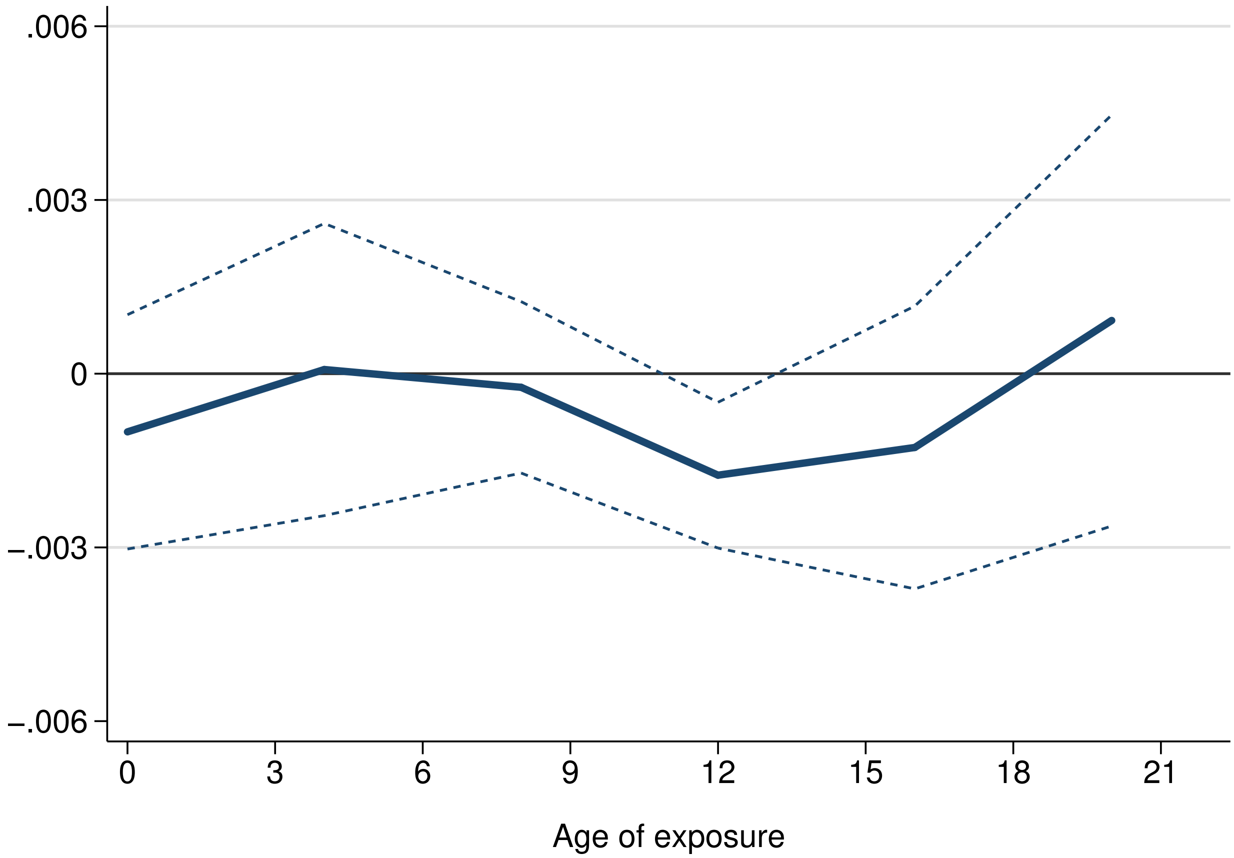
Notes. This figure presents estimates of β^t from $S_{jtc} = \sum_{t=1949}^{1983} \beta^t \left(\mathbf{1}(t = \tau) \times \mathbb{I}_j\right) + \lambda_j + \mu_{ct} + \xi_{jtc}$. The omitted group is the 1955 birth cohort. Dependent variable is average years of schooling for cohort t born in municipality j observed in census year c. Coffee cultivation intensity is given by $\mathbb{I}_j$. The specification includes municipality fixed effects, λ_j, and cohort $\times$ census-year fixed effects, μ_{ct}. Sample includes individuals who are 22-56 years old at census time. Microdata are collapsed into municipality-cohort-census cells and regressions are weighted by the square root of cell size. Robust standard errors are clustered at the municipality level. Dashed lines plots 95 percent confidence intervals for estimates of β^t.

Figure 7: —Effects of Coffee Price Shocks on Completed Schooling



Notes. This figure plots estimates of the effects of coffee price shocks at different ages of exposure on years of education. Each coefficient is from the same regression. The regression includes controls for municipality-of-birth, year-of-birth-by-census-year and gender fixed effects as well as municipality-specific linear time trends. The dashed lines represent the respective 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. I group ages to increase precision: 0-1, 2-5, 6-9, 10-13, 14-17, 18-21. Sample includes individuals who are 22-56 years old at census time. Microdata are collapsed into municipality-cohort-census cells and regressions are weighted by the square root of cell size.

Figure 8: —Effects of Coffee Price Shocks on Industrial score Income



Notes. This figure plots estimates of the effects of coffee price shocks at different ages of exposure on industrial income score. Each coefficient is from the same regression. The regression includes controls for municipality-of-birth, year-of-birth-by-census-year and gender fixed effects as well as municipality-specific linear time trends. The dashed lines represent the respective 95 percent confidence intervals, where robust standard errors are clustered at the municipality-level. I group ages to increase precision: 0-1, 2-5, 6-9, 10-13, 14-17, 18-21. Sample includes individuals who are 22-56 years old at census time. Microdata are collapsed into municipality-cohort-census cells and regressions are weighted by the square root of cell size.

Table 1: Summary Statistics

	Mean	Standard Deviation	observations
	(1)	(2)	(3)
Panel A: Adults aged 22-56			
Total years of education	7.50	2.29	64482
Income score	-0.05	0.25	61693
Age	34.36	9.05	64505
Sex: female=1	0.52	0.50	64505
Log School-age coffee price (ages 5 to 16)	8.80	0.19	64505
Coffee cultivation (hectares per km2)	1.96	4.50	64257
Panel B: Children aged 5-16			
School attendance (=1)	0.80	0.17	3035
Child labor (=1)	0.05	0.07	2102
Age	10.46	3.45	3038
Sex: female=1	0.50	0.50	3038

Notes. Panel A contains summary statistics using 1973, 1993, and 2005 censuses aggregated at the municipality-of-birth, year-of-birth, sex and census-year level. The observations are weighted by the number of observations in each cell. The sample contains cohorts born between 1949 and 1983, who are 22-56 years old at the time they are observed in the census. Panel B contains summary statistics using 1973, 1985, and 1993 aggregated at the department, census-year, age, and sex level. Child labor is only available for children over 10 years old.

Table 2: Coffee Price Shocks and Completed Schooling

	Dependent variable: years of education attained			
	No controls	Add municipality × linear trends	Add census × cohort fixed effects	Add census × municipality fixed effects
	(1)	(2)	(3)	(4)
log school-age coffee price ×	-0.047	-0.0404	-0.04	-0.0382
coffee cultivation intensity	[0.0132]***	[0.0094]***	[0.0093]***	[0.0097]***
Observations	64234	64234	64234	64234
R^2	0.7157	0.7291	0.7319	0.7482
	Drop 1993 census observations	Cap at 12 years		
	(5)	(6)		
log school-age coffee price ×	-0.0424	-0.034		
coffee cultivation intensity	[0.0126]***	[0.0093]***		
Observations	40084	64234		
R^2	0.725	0.7455		

Notes. Dependent variable is total years of education attained. School-age coffee price of the cohort born in year t is the average real-world coffee price observed between years $t + 5$ and $t + 16$. Coffee cultivation intensity is measured as the total hectares of land used for cultivating coffee in each municipality in 1932 scaled by the total land area. Sample restricted to 1973, 1993 and 2005 census data on individuals born between 1949 and 1983, who are 22-56 years old at the time they are observed in the census. The data are collapsed into municipality-of-birth, year-of-birth, sex and census-year cells, and the observations are weighted by the square root of the cell sizes. All regressions include controls for municipality-of-birth, year-of-birth, census-year and gender fixed effects. Robust standard errors (in brackets) are clustered at the municipality level.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 3: Coffee Price Shocks and Completed Schooling
(Reduced-form and Instrumental Variable Estimates)

	Dependent variable: years of education attained		
	Baseline	Reduced-form estimates	Instrumental variable estimates
	(1)	(2)	(3)
log school-age coffee price × coffee cultivation intensity	-0.04		-0.0345
	[0.0093]***		[0.0094]***
log school-age Brazilian coffee production × coffee cultivation intensity		0.0446	
		[0.0122]***	
Observations	64234	64234	64234
R^2	0.7319	0.7318	0.7319

Notes. Column (1) reports the baseline estimate based on equation (1). Column (2) repeats equation (1), but uses Brazilian coffee production rather coffee prices. Column (3) reports two-stage least squares, where (log) school-age Brazilian coffee production × coffee cultivation intensity is used as an instrument for (log) school-age coffee prices × coffee cultivation intensity. Coffee cultivation intensity is measured as the total hectares of land used for cultivating coffee in each municipality in 1932 scaled by the total land area. Sample restricted to 1973, 1993 and 2005 census data on individuals born between 1949 and 1983, who are 22-56 years old at the time they are observed in the census. The data are collapsed into municipality-of-birth, year-of-birth, sex and census-year cells, and the observations are weighted by the square root of the cell sizes. All regressions include controls for municipality-of-birth, year-of-birth-by-census-year and gender fixed effects as well as municipality-specific linear time trends. Robust standard errors (in brackets) are clustered at the municipality level.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 4: Coffee Price Shocks and School Attendance, Enrollment, and Child Labor

	Dependent variable:				
	School attendance			Enrollment rates	Child labor
	ages: 5-16	ages: 5-11	ages: 12-16	period: 1954-1977	ages: 10-16
	(1)	(2)	(3)	(4)	(5)
log coffee price × coffee cultivation intensity	-0.0112	-0.0112	-0.011	-0.0909	0.0033
	[0.0027]***	[0.0026]***	[0.0032]***	[0.0320]***	[0.0012]***
	(0.000)	(0.000)	(0.000)	-	(0.000)
Observations	2203	1283	920	486	1287
R^2	0.9396	0.959	0.8717	0.8508	0.8275

Notes. School attendance and child labor results are based on 1973, 1985 and 1993 census data on children in rural areas aggregated at the the department/census-year/cohort/gender level, and the observations are weighted by the square root of the cell sizes. The regressions include controls for department-specific time trends, and department, gender, census-year and cohort-census fixed effects. Enrollment rates represent children in primary-schools divided by 5-11 children. This variable is at the department/year level. Column (4) includes controls for department and year fixed effects as well as department-specific linear time trends, and weights the observations by the square root of the number of 5-11 children. Robust standard errors (in brackets) are clustered at the department level. Two-tailed p-values based on the wild cluster bootstrap-T method in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 5: Coffee Price Shocks and Child Labor, and Schooling
(Gender Heterogeneities)

	Dependent variable:			
	Child labor ages: 10-16		Educational attainment ages: 22-56	
	(1)	(2)	(3)	(4)
log coffee price × coffee cultivation intensity	0.0033 [0.0012]*** (0.000)	0.0054 [0.0015]*** (0.000)		
log coffee price × coffee cultivation intensity × female		-0.0042 [0.0014]*** (0.002)		
log school-age coffee price × coffee cultivation intensity			-0.04 [0.0093]***	-0.0531 [0.0095]***
log school-age coffee price × coffee cultivation intensity × female				0.0238 [0.0063]***
Observations	1287	1287	64234	64234
R^2	0.8275	0.8567	0.7319	0.7309

Notes. Columns (1)-(2) are from a sample restricted to 1973, 1985 and 1993 census data on children in rural areas aggregated at the the department/census-year/cohort/gender level, and the observations are weighted by the square root of the cell sizes. Columns (3)-(4) are from a sample restricted to 1973, 1993 and 2005 census data aggregated at the municipality-of-birth/cohort/gender/census-year level, and the observations are weighted by the square root of the cell sizes. Regressions in columns (1)-(2) control for department-specific time trends, and department, gender, census-year and cohort-census fixed effects. Column (2) in addition controls for female × log coffee price, and female × coffee cultivation intensity. Columns (3)-(4) include controls for municipality-of-birth, year-of-birth-by-census-year and gender fixed effects as well as municipality-specific linear time trends. Column (4) in addition controls for female × log school-age coffee price, and female × coffee cultivation intensity. Robust standard errors (in brackets) are clustered at the department level in columns (1)-(2) and at the municipality level in columns (3)-(4). Two-tailed p-values based on the wild cluster bootstrap-T method in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 6: Coffee Price Shocks and Adult Income

	Dependent variable: industrial income score			
	(1)	(2)	(3)	(4)
log school-age coffee price ×	-0.0037			
coffee cultivation intensity	[0.0013]***			
log coffee price (age: 5-11 yrs. old) ×		-0.0001		0.0003
coffee cultivation intensity		[0.0007]		[0.0007]
log coffee price (age: 12-16 yrs. old) ×			-0.0032	-0.0032
coffee cultivation intensity			[0.0010]***	[0.0010]***
Observations	61472	61472	61472	61472
R^2	0.4177	0.4176	0.4178	0.4178

Notes. School-age coffee price of the cohort born in year t is the average real-world coffee price observed between years $t + 5$ and $t + 16$. Coffee price (ages 5-11) of the cohort born in year t is the average real-world coffee price observed between years $t + 5$ and $t + 11$. Coffee price (ages 12-16) of the cohort born in year t is the average real-world coffee price observed between years $t + 12$ and $t + 16$. Coffee cultivation intensity is measured as the total hectares of land used for cultivating coffee in each municipality in 1932 scaled by the total land area. Sample restricted to 1973, 1993 and 2005 census data on individuals born between 1949 and 1983, who are 22-56 years old at the time they are observed in the census. The data are collapsed into municipality-of-birth, year-of-birth, sex and census-year cells, and the observations are weighted by the square root of the cell sizes. All regressions include controls for municipality-of-birth, year-of-birth-by-census-year and gender fixed effects as well as municipality-specific linear time trends. Robust standard errors (in brackets) are clustered at the municipality level.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 7: Coffee Price Shocks and Adult Income
(Adjusting for Education)

	Dependent variable: industrial income score						
	(1)	(2)					
log school-age coffee price ×	-0.0037	-0.002					
coffee cultivation intensity	[0.0013]***	[0.0011]*					
years of education attained		0.0424					
		[0.0011]***					
Observations	61472	61471					
R^2	0.418	0.447					
	Bounding analysis						
	40% below OLS estimate	20% below OLS estimate	10% below OLS estimate	10% above OLS estimate	20% above OLS estimate	40% above OLS estimate	
	(3)	(4)	(5)	(7)	(8)	(9)	
log school-age coffee price ×	-0.0027	-0.0023	-0.0021	-0.0018	-0.0016	-0.0013	
coffee cultivation intensity	[0.0012]**	[0.0011]**	[0.0011]*	[0.0011]*	[0.0011]	[0.0010]	
Observations	61471	61471	61471	61471	61471	61471	
R^2	0.378	0.37	0.368	0.365	0.365	0.368	
	Return to one year of schooling:						
	6%	7%	8%	9%	10%	11%	12%
	(10)	(11)	(12)	(13)	(14)	(15)	(16)
log school-age coffee price ×	-0.0013	-0.0009	-0.0004	0.0000	0.0004	0.0008	0.0012
coffee cultivation intensity	[0.0010]	[0.0010]	[0.0010]	[0.0010]	[0.0010]	[0.0010]	[0.0010]
Observations	61471	61471	61471	61471	61471	61471	61471
R^2	0.368	0.377	0.389	0.405	0.423	0.442	0.461

Notes. School-age coffee price of the cohort born in year t is the average real-world coffee price observed between years $t+5$ and $t+16$. Coffee cultivation intensity is measured as the total hectares of land used for cultivating coffee in each municipality in 1932 scaled by the total land area. Sample restricted to 1973, 1993 and 2005 census data on individuals born between 1949 and 1983, who are 22-56 years old at the time they are observed in the census. The data are collapsed into municipality-of-birth, year-of-birth, sex and census-year cells, and the observations are weighted by the square root of the cell sizes. All regressions include controls for municipality-of-birth, year-of-birth-by-census-year and gender fixed effects as well as municipality-specific linear time trends. Bounding analysis: the return to schooling stems from the OLS estimate on years of education attained (as reported in column (2)) multiplied by the adjustment factor reported in columns (3)-(9). Each result in columns (10)-(11) refer to a different assumption on the return to schooling. Robust standard errors (in brackets) are clustered at the municipality level.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Online Appendix to "The Value of Time and Skill Acquisition in the Long Run: Evidence from Coffee Booms and Busts"

Bladimir Carrillo

October 23, 2018

A The Role of Mobility

The baseline results assume that the place of birth is the same as the one where an individual grew up. The data indicate that this assignment is likely to be correct for the most of individuals. As shown above, approximately 75 percent of individuals were living in their place of birth during childhood. For the remaining limited set of individuals, childhood coffee market conditions will be measured noisily, but if such measurement error it is unrelated to coffee market conditions, then it is likely to introduce only a downward bias.[1] As a robustness check, I estimate the main specification only for those individuals who currently reside in their municipality of birth, a subsample that certainly includes a greater fraction of individuals who are more likely to have grown up in their place of birth. Although this restriction drops about 50 percent of individuals, I find a coefficient estimate that is very similar and statistically indistinguishable from the baseline (Appendix Table A.1, column (2)).

As an additional check, I aggregate the data to the department-of-birth level and run the baseline specification given by equation (1). I sum the total hectares of land used for cultivating coffee within a department and then scale it by the total land area as in the municipality level analysis, so the resulting coefficient is comparable to the baseline. As can be seen in Appendix Table A.1, column (3), the estimated coefficient is -0.10 (standard error=0.035), which is larger (in absolute value) than the baseline estimate of -0.04. Since migration across departments is very low among individuals, the larger effect in the department-level analysis is consistent with the presence of some attenuation in the baseline model. Additionally, these differences in effect size may also be the result of spillovers that are not captured in the municipality-level analysis. For instance, youths in non-producing areas may leave school and commute or move temporarily to adjacent areas where new cultivation is taking place during booms. Since most spillover effects that occur across neighboring areas are built into the estimates produced by the department-level analysis, the difference in estimates suggest that the resulting bias is likely to be downward and hence the baseline results should be taken to be lower bounds.

[1] More of a concern is whether the municipality of birth is endogenous to future price shocks, particularly if parents anticipate future erratic changes in international coffee prices. But this seems intuitively implausible.

Table A.1: Coffee Price Shocks and Completed Schooling
(Stayers Subsample and Department-level Analysis)

	Dependent variable: years of education attained		
	Baseline	Stayer subsample	Department-level analysis
	(1)	(2)	(3)
log school-age coffee price × coffee cultivation intensity	-0.040 [0.0093]***	-0.035 [0.0085]***	-0.100 [0.0358]** (0.000)
Observations	64234	63645	2781
R^2	0.7319	0.7979	0.9292

Notes. Column (1) replicates the baseline results (Table 2, column (3)). Column (2) repeats the baseline specification, but limits the sample to adults who were living in their place of birth at census time. Column (3) repeats the baseline specification, but aggregates the data by department of birth (rather than municipality of birth). In addition, column (3) sums the total hectares of land used for cultivating coffee within a department and then scale it by the total land area as in the municipality level analysis. Robust standard errors (in brackets) are clustered at the place of birth level. Two-tailed p-values based on the wild cluster bootstrap-T method in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

B Alternative specifications

In the main analysis, I control for a full set of municipality and time fixed effects along with municipality-specific time trends. Table B.1 considers a variety of additional factors, all of which interacted with time fixed effects, that may be correlated with coffee cultivation intensity and determinants of human capital investments. For ease of comparison, column (1) replicates the baseline results. Column (2) includes controls for the intensity of 1950s civil war in Colombia, an episode of intense conflict known as *La Violencia* that varied heterogeneously across regions. Column (3) adds controls for manufacturing employment in 1945, an index variable measuring proximity to the major markets in 1960, and a measure of economic development in 1960. These controls address any possible convergent process in the level of development that might be correlated with changes in schooling across municipalities with varying coffee cultivation intensities. Column (4) controls for differential trends correlated with the incidence of leishmaniasis, hookworm, non-hookworm helminth and malaria diseases. Particularly relevant is controlling for malaria incidence because it was salient in some regions and intense eradication campaigns in Colombia began in the mid-1950s.[2] If the identifying assumption is valid, the inclusion of these controls should not have any discernible impact on the estimate coefficient of interest. Consistent with the validity of the research design, the magnitude of the estimate coefficient is very similar and remains highly significant across these alternative specifications.

In the baseline model, I measure coffee cultivation intensity using information from the 1932 coffee census, which was conducted about two decades before any cohort was born. As a robustness check, I rerun the baseline specification but measure coffee cultivation intensity using the 1970 coffee census, the middle of the sample period. While 1932 and 1970 coffee cultivation intensities are strongly correlated, the estimated coefficients are not necessarily comparable because the levels of production in 1970 increased substantially relative to 1932. With this caveat in mind, column (5) shows the results from using the alternative measure of coffee intensity. I find an estimate coefficient of -0.016 (standard error=0.003), which is statistically significant at the 1 percent level of significance. In terms of magnitude, this coefficient implies that a 50-percent increase in the price of coffee in childhood reduces educational attainment by 0.08 years in areas with a 1 standard deviation higher in the intensity of coffee cultivation. The magnitude of this result is strikingly similar to the baseline, but it is not my preferred specification because 1970 coffee production levels might

[2] A number of studies, conducted in Colombia, India, Paraguay, Sri Lanka, and the United States, have documented that malaria eradication campaigns translated into long-term improvements in human capital among cohorts exposed in early-life (Bleakley, 2010; Cutler et al., 2010; Lucas, 2010).

reflect endogenous response to past periods of high or low coffee prices. However, the results from using the 1970 coffee census represent a useful check and suggest that such potential source of bias is unlikely to play a major in practice.

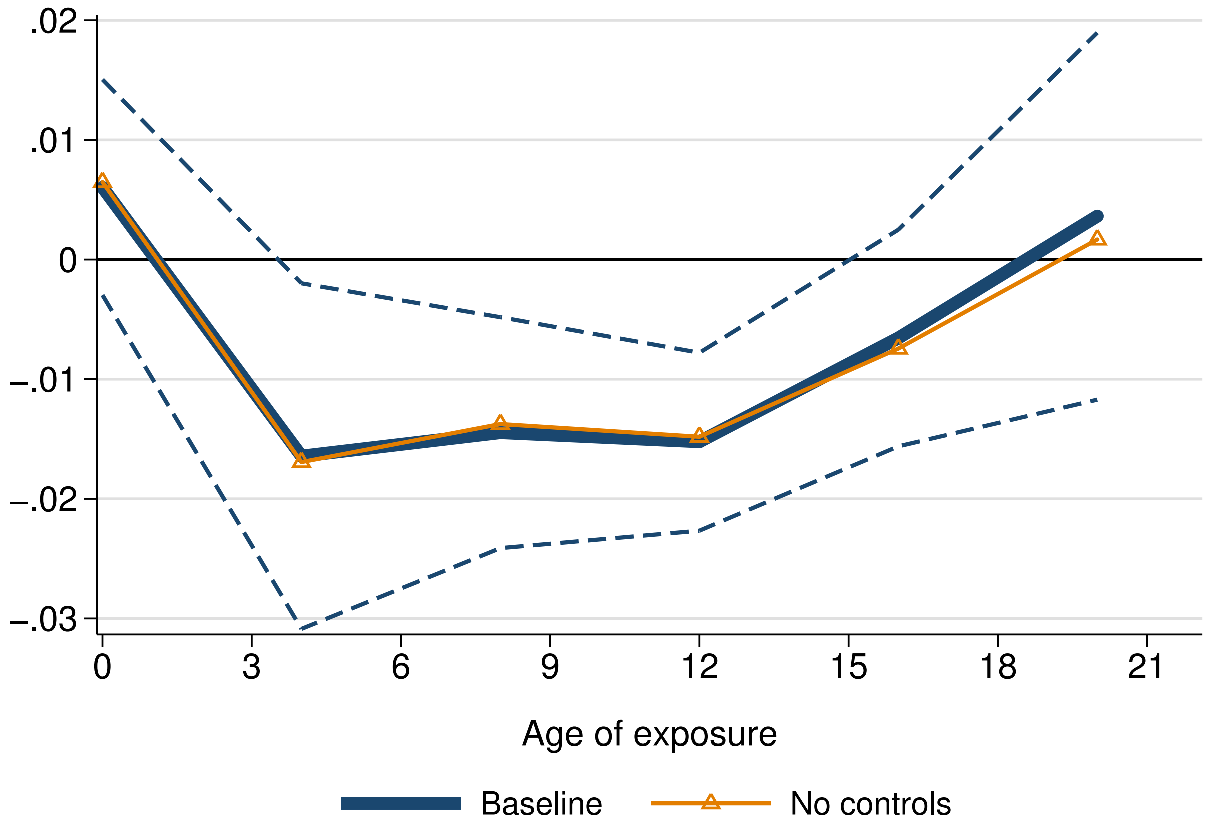
Column (6) excludes municipalities with no coffee cultivation. An advantage of limiting the sample only to coffee-growing areas is that it increases the comparability of the municipalities and thus reduces the risk of differential trends in schooling driven by other factors. While this reduces sample size by about 50 percent, the magnitude and significance levels of the relationship remain extremely similar to the baseline. Finally, column (7) shows that the results are qualitatively and quantitatively similar if I use coffee cultivation in levels rather than normalized by total land area.

Table B.1: Coffee Price Shocks and Completed Schooling
(Specification Checks)

	Dependent variable: years of education attained				
	Baseline	Add Internal conflict *(La Violencia)*	Add Economic activity	Add Diseases incidence	1970 coffee cultivation intensity
	(1)	(2)	(3)	(4)	(5)
log school-age coffee price × coffee cultivation intensity	-0.04	-0.0417	-0.0391	-0.0351	-0.0156
	[0.0093]***	[0.0105]***	[0.0095]***	[0.0099]***	[0.0034]***
Observations	64234	63746	63625	63625	64234
R^2	0.7319	0.7324	0.7332	0.7339	0.7319
	Excludes non-growing areas	Coffee cultivation in levels			
	(6)	(7)			
log school-age coffee price × coffee cultivation intensity	-0.0368	-0.0988			
	[0.0095]***	[0.0201]***			
Observations	35865	64234			
R^2	0.7223	0.7319			

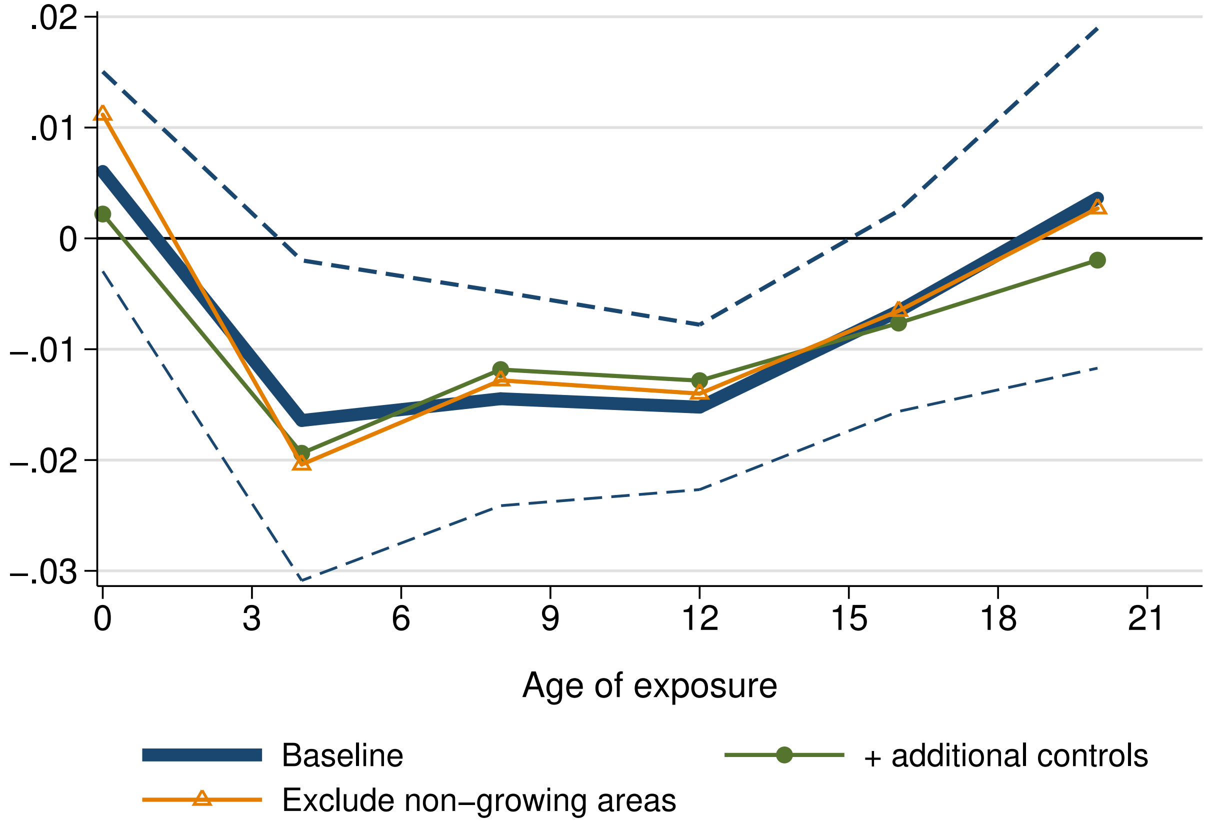
Notes. Column (1) replicates the baseline results (Table 2, column (3)). Columns (2)-(4) include sequentially time-invariant factors interacted with year-of-birth fixed effects as additioanl controls. *La Violencia* refers to two variables measuring the intensity of the Colombian civil war in the 1950s. Economic activity refers to manufacturing employment per capita in 1945, a market access index in 1960, and a measure of general economic development in 1960. Diseases refers to variables measuring the risk of Hookworm, non-hookworm Helminth, and malaria diseases. Column (5) repeats the baseline specification, but uses the 1970 coffee census to measure coffee cultivation intensity. Column (6) reruns the baseline specification, but excludes individuals who were born in non-growing areas. Robust standard errors (in brackets) are clustered at the municipality level. Column (7) uses coffee cultivation in levels rather than normalized by total land area.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Figure B.1: —Effects of Coffee Price Shocks on Completed Schooling



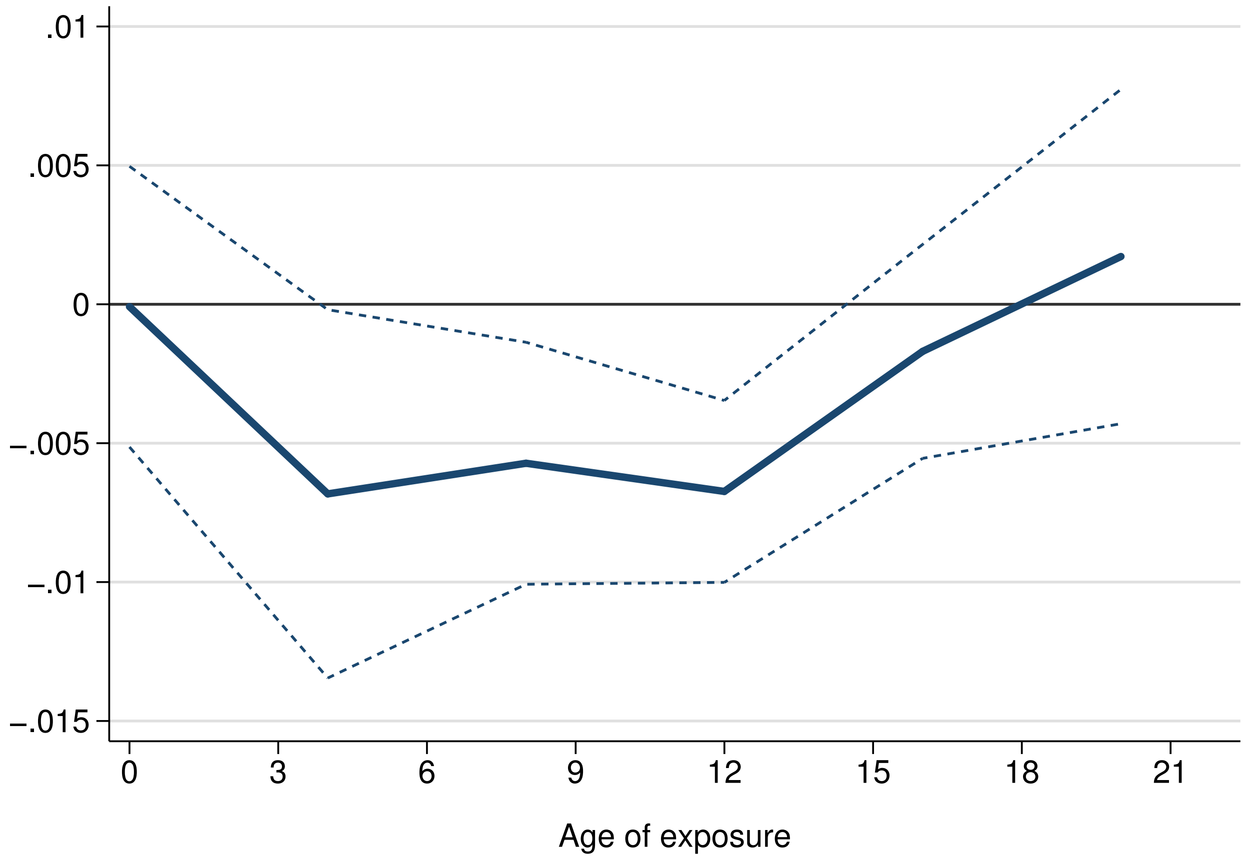
Notes. This figure plots estimates of the effects of coffee price shocks at different ages of exposure on years of education. The thick line corresponds to the baseline specification. The line with triangles corresponds to a specification that includes only municipality-of-birth fixed effects, year-of-birth fixed effects, census-year fixed effects and gender fixed effects. The dashed line represents the respective 95 percent confidence intervals of the baseline estimates, where robust standard errors are clustered at the municipality-level. I group ages to increase precision: 0-1, 2-5, 6-9, 10-13, 14-17, 18-21. Sample includes individuals who are 22-56 years old at census time. Microdata are collapsed into municipality-of-birth, year-of-birth, census-year and gender cells and regressions are weighted by the square root of cell size.

Figure B.2: —Effects of Coffee Price Shocks on Completed Schooling



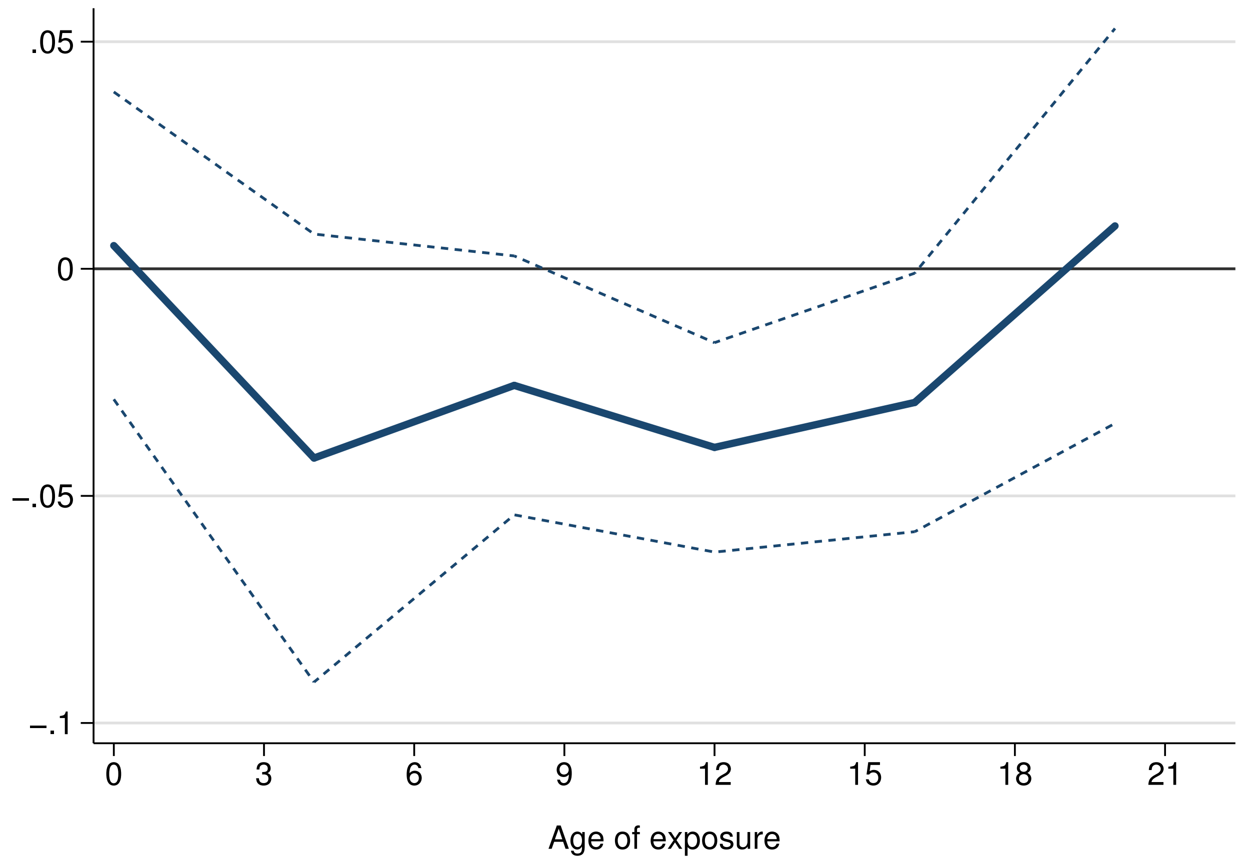
Notes. This figure plots estimates of the effects of coffee price shocks at different ages of exposure on years of education. The thick line corresponds to the baseline specification. The line with circles corresponds to a specification that includes the baseline controls and additional time-invariant characteristics interacted with year-of-birth fixed effects. Additional time-invariant characteristics include: two variables measuring the intensity of the Colombian civil war in the 1950s; manufacturing employment per capita in 1945: a market access index in 1960; a measure of general economic development in 1960; incidence of Hookworm; incidence of non-hookworm Helminth; and incidence of malaria disease. The line with triangles repeats the baseline specification, but excludes municipalities with no coffee cultivation in 1932. The dashed line represents the respective 95 percent confidence intervals of the baseline estimates, where robust standard errors are clustered at the municipality-level. I group ages to increase precision: 0-1, 2-5, 6-9, 10-13, 14-17, 18-21. Sample includes individuals who are 22-56 years old at census time. Microdata are collapsed into municipality-of-birth, year-of-birth, census-year and gender cells and regressions are weighted by the square root of cell size.

Figure B.3: —Effects of Coffee Price Shocks on Completed Schooling
(Robustness to using 1970 coffee cultivation)



Notes. This figure plots estimates of the effects of coffee price shocks at different ages of exposure on years of education. The regression includes the baseline controls. Coffee cultivation is measured using the 1970 coffee census and normalized by total municipality land. The dashed line represents the respective 95 percent confidence intervals of the baseline estimates, where robust standard errors are clustered at the municipality-level. I group ages to increase precision: 0-1, 2-5, 6-9, 10-13, 14-17, 18-21. Sample includes individuals who are 22-56 years old at census time. Microdata are collapsed into municipality-of-birth, year-of-birth, census-year and gender cells and regressions are weighted by the square root of cell size.

Figure B.4: —Effects of Coffee Price Shocks on Completed Schooling
(Robutnes to using coffee cultivation in levels rather than normalized)



Notes. This figure plots estimates of the effects of coffee price shocks at different ages of exposure on years of education. The regression includes the baseline controls. Coffee cultivation is measured in levels rather than normalized by total municipality land. The dashed line represents the respective 95 percent confidence intervals of the baseline estimates, where robust standard errors are clustered at the municipality-level. I group ages to increase precision: 0-1, 2-5, 6-9, 10-13, 14-17, 18-21. Sample includes individuals who are 22-56 years old at census time. Microdata are collapsed into municipality-of-birth, year-of-birth, census-year and gender cells and regressions are weighted by the square root of cell size.

C Selective Attrition

A concern with my results is mortality selection at older ages changing the composition of the sample. In practice, however, selective attrition is thought to play a limited role in explaining my results given that it most likely leads me to use a positive selected sample of high "quality" individuals, making it more difficult detect any change in education. Indeed, comparing changes in cohort size between the 1993 and 2005 censuses across low and high education groups, I observe that less educated individuals attrite at higher rates. More of a concern for my empirical approach is whether there is differential mortality selection across cohorts exposed to different coffee market conditions in childhood. If attrition rate is higher for more educated individuals who were exposed to coffee booms in childhood, then one could observe negative effects of coffee price shocks on long-run schooling even in the absence of a causal relationship. Alternatively, if less-educated cohorts exposed to coffee booms attrite at higher rates, then my differences-in-differences approach would lead to underestimates of the true effects of coffee price shocks on education.

To investigate this issue, I first evaluate whether attrition is systematically related to childhood coffee market conditions and then determine whether there are meaningful differences in this relationship across more- and less-educated individuals. I estimate the following specification:

$$D_{jgt} = \beta \left(\mathbb{P}_t \times \mathbb{I}_j\right) + \kappa \, \mathbf{T}_{jt} + \lambda_j + \gamma_g + \mu_t + \xi_{jgt} \tag{C.5}$$

where D represents the relative change in size of cohort t in municipality j and gender g between the 2005 and 1993 censuses – i.e., $D = (cohort_{1993} - cohort_{2005})/cohort_{1993}$. All of the other variables and coefficients are the same as in equation (1). The key parameter of interest is β. If coffee booms lead to increased mortality, then one would expect to see negative and significant estimates for β. I estimate this specification for the full sample and separately for cohorts above and below the median years of education.

These results are presented in Appendix Table C.1. Column (1) shows that increases in the international price of coffee price lead to increases in the rate of "deceased" cohorts. This effect is entirely driven by less educated cohorts. Indeed, the estimated effect for more-educated cohorts is statistically insignificant and only 8 percent the magnitude of the corresponding effect for less-educated cohorts (columns 2-3). These results suggest that mortality selection is likely to introduce a downward bias in my estimates of the effect coffee price shocks on educational attainment.

To gauge the extent to which selective mortality may be important in practice, I re-estimate the relationship between coffee price shocks and educational attainment for young individuals. If mortality selection introduces a significant bias in my estimates, then the results from this subsample should differ significantly relative to the baseline, since mortality selection should be less important for younger individuals. I find that the magnitude and standard errors are remarkably similar to the baseline. Indeed, I find a coefficient of -0.040 in a sample that excludes individuals who are over 40 years old, an estimate that is virtually identical to the baseline (see Appendix Table C.2). I conclude that mortality selection induced by coffee price shocks is not large enough to affect significantly my analysis.

Table C.1: Coffee Price Shocks and Changes in Cohort Size

	Dependent variable: relative change in cohort size between 1993 and 2005		
	Full sample	Low education individuals	High education individuals
	(1)	(2)	(3)
log school-age coffee price × coffee cultivation intensity	0.0243 [0.0069]***	0.0398 [0.0091]***	0.0056 [0.0089]
Observations	24107	23921	23448
R^2	0.4864	0.4929	0.1156

Notes. Relative change in size of cohort t is measured as $D = (cohort_{1993,t} - cohort_{2005,t})/cohort_{1993,t}$. School-age coffee price of the cohort born in year t is the average real-world coffee price observed between years $t+5$ and $t+16$. Coffee cultivation intensity is measured as the total hectares of land used for cultivating coffee in each municipality in 1932 scaled by the total land area. Sample restricted to 1993 and 2005 census data on individuals born between 1949 and 1983, who are 22-56 years old at the time they are observed in the census. The data are collapsed into municipality-of-birth, year-of-birth, census-year, and sex cells, and the observations are weighted by the square root of the 1993-cell sizes. All regressions include controls for municipality-of-birth, year-of-birth,and gender fixed effects as well as municipality-specific linear time trends. Low and high education individuals refer to those below and above the median years of education in 1993 and 2005, respectively. Robust standard errors (in brackets) are clustered at the municipality level.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table C.2: Coffee Price Shocks and Completed Schooling
(Robustness to Dropping Older Individuals)

	Dependent variable: years of education attained		
	Baseline	22-50 yrs. old sample	22-40 yrs. old sample
	(1)	(2)	(3)
log school-age coffee price ×	-0.04	-0.039	-0.04
coffee cultivation intensity	[0.0093]***	[0.0092]***	[0.0096]***
Observations	64234	57927	43196
R^2	0.7319	0.7501	0.7894

Notes. Dependent variable is total years of education attained. School-age coffee price of the cohort born in year t is the average real-world coffee price observed between years $t + 5$ and $t + 16$. Coffee cultivation intensity is measured as the total hectares of land used for cultivating coffee in each municipality in 1932 scaled by the total land area. Sample restricted to 1973, 1993 and 2005 Census data on individuals born between 1949 and 2005, who are 22-56 years old at the time they are observed in the census. The data are collapsed into municipality-of-birth, year-of-birth, sex and census-year cells, and the observations are weighted by the square root of the cell sizes. All regressions include controls for municipality-of-birth, year-of-birth, census-year and gender fixed effects. Robust standard errors (in brackets) are clustered at the municipality level.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

D Alternative Clustering

In the baseline model, standard errors are clustered at the municipality level to account for serial correlation in error terms at the municipality level. A possible concern is that the intensity of coffee cultivation may be spatially correlated and ignoring it might lead to misleading inference. As a simple check, Table D.1, column (2) reports standard errors clustered by department, which account for correlation across municipalities within the same department. I find that clustering at the department level reduces slightly the standard error and thus leads to more precise results. A possible problem with these standard errors is that they do not account for correlation between municipalities across different departments. As an alternative approach, column (3) computes two-way clustered standard errors by municipality and year of birth to account flexibly for both serial and spatial correlation. This increases the standard error relative to the baseline, but the estimated relationship remains highly significant at the conventional levels of significance.

As a final check, column (4) reports standard errors that account for arbitrary spatial and serial correlation using the method proposed by Conley (1999). This approach computes a covariance-variance matrix that is spatially weighted, with weights starting at 1 and declining to 0 until a given cutoff. I find that Conley standard error is significantly lower than the baseline. Overall, the conclusions are largely unchanged across different assumptions about the covariance-variance matrix, suggesting that spatial correlation is not a potentially important issue in my setting.

Table D.1: Coffee Price Shocks and Completed Schooling
(Alternative Assumptions about Standard Errors)

	Dependent variable: years of education attained			
		Alternative clustering		
	Baseline	SE clustered by department	Two-way clustering	Conley SE
	(1)	(2)	(3)	(4)
log school-age coffee price × coffee cultivation intensity	-0.04 [0.0093]***	-0.04 [0.0073]*** (0.000)	-0.04 [0.0103]***	-0.04 [0.0048]***
Observations	64234	64234	64234	64234
R^2	0.7319	0.7319	0.7319	0.7319

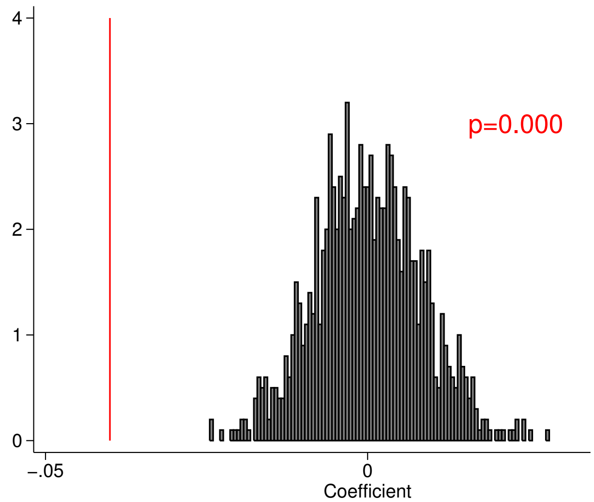
Notes. Column (1) replicates the baseline results (Table 2, column (3)). Column 2 clusters standard errors by department of birth. Column 3 uses a toway clustering method developed by Cameron,Gelbach, and Miller (2011) and clusters standard errors along the municipality and year of birth. Column 4 reports results using Conley (1999) standard errors to account for spatial correlation with cutoff of 250 kilometers. Two-tailed p-values based on the wild cluster bootstrap-T method in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

E Permutation Tests

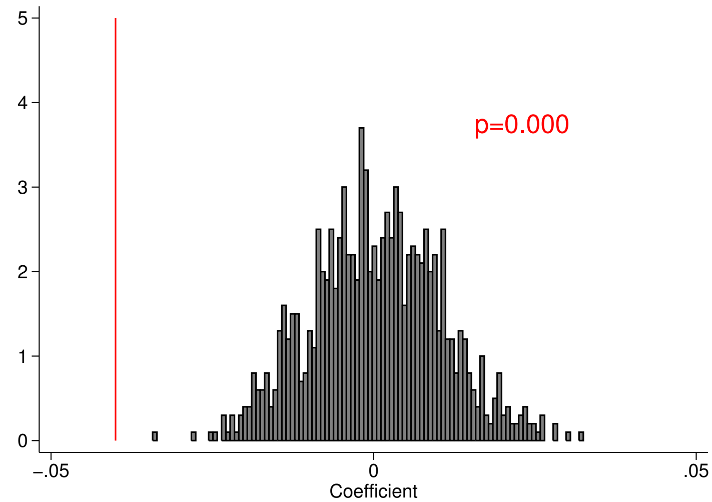
To further evaluate the robustness of the results, I conduct a non-parametric permutation test that is similar in spirit to Chetty et al. (2009) and Dell and Querubin (2018). To do so, I sample the set of possible coffee prices and coffee cultivation intensity, re-assigning randomly chosen values to municipalities and birth cohorts. I then regress years of schooling on the re-assigned values using the baseline controls, and repeat this procedure 1000 times. The share of the 1000 absolute placebo coefficients that are larger than the absolute actual coefficient can be taken to be the p-value for the hypothesis that $\beta = 0$. If coffee price shocks had a significant effect on completed human capital, one would expect the actual coefficient to be in the lower tail of the placebo coefficient distribution. A major strength of this approach is that it does not make parametric assumptions about the error structure, minimizing any concern about serial and spatial correlation.

Appendix Figure E.1 plots of the placebo coefficients, with the vertical line denoting the actual coefficient reported in Table 2. Panel (a) shows the results from an exercise in which only coffee prices are randomly re-assigned, while that panel (b) only re-assigns coffee cultivation intensities. Panel (c) randomly re-assigned both coffee prices and the intensity of coffee cultivation. In all cases, the actual coefficient falls far in the tail of the placebo coefficient distribution, with p-values extremely similar to those obtained using conventional inference. This suggests that the estimated effects of coffee prices on educational attainment are very unlikely to have arisen by chance.

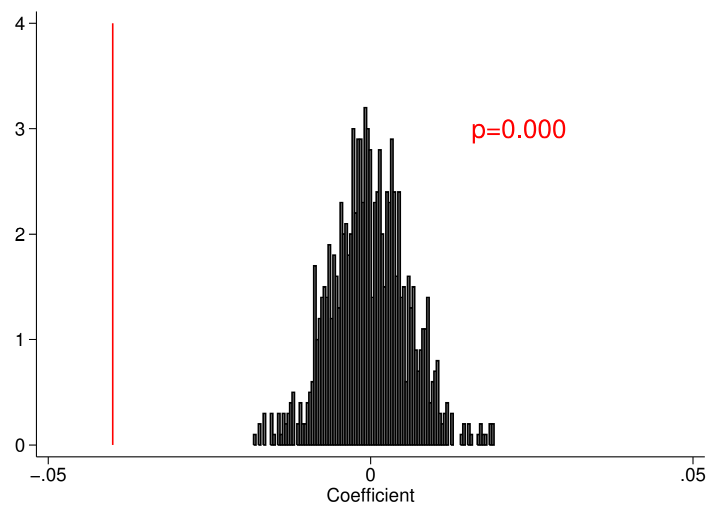
Figure E.1: —Randomization Inference: Completed Schooling



(a) Random assignment of coffee prices



(b) Random assignment of coffee cultivation



(c) Random assignment of coffee cultivation and prices

Notes. Figures plot the distribution of reduced form placebo coefficients on the interaction of coffee cultivation and log school-age coffee prices. The red line plots the actual coefficient.

F Additional Tables

Table F.1: Coffee Price Shocks and Completed Schooling
(Alternative Treatment Definitions)

	Dependent variable: years of education attained						
Treatment intensity definitions:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log school-age coffee price × **1**(coffee cultivation>50th pct.)	-0.2424 [0.0699]***						
1(coffee price>50th pct.) × coffee cultivation intensity		-0.0144 [0.0035]***					
1(coffee price>50th pct.) × **1**(coffee cultivation>50th pct.)			-0.0815 [0.0242]***				
1(coffee price>50th pct.) × **1**(coffee cultivation>75th pct.)				-0.1325 [0.0252]***			
1(coffee price>50th pct.) × **1**(coffee cultivation>90th pct.)					-0.1942 [0.0331]***		
1(coffee price>75th pct.) × **1**(coffee cultivation>50th pct.)						-0.0719 [0.0264]***	
1(coffee price>90th pct.) × **1**(coffee cultivation>50th pct.)							-0.0675 [0.0293]**
Observations	64234	64234	64234	64234	64234	64234	64234
R^2	0.7318	0.7319	0.7318	0.7319	0.7319	0.7318	0.7317

Notes. Each row corresponds to a different treatment definition based on school-age coffee price and coffee cultivation intensity. All the specifications include the full set of baseline controls. Robust standard errors (in brackets) are clustered at the municipality level.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table F.2: Coffee prices and Enrollment Rates
(Role of Teacher Supply Responses)

	Dependent variable:		
	Enrollment rates		Teacher rates
	(1)	(2)	(3)
log coffee price ×	-0.0909	-0.0868	-0.0182
coffee cultivation intensity	[0.0320]***	[0.0192]***	[0.0696]
teacher rates		0.2369	
		[0.0240]***	
Observations	486	484	484
R^2	0.8508	0.9105	0.9125

Notes. Enrollment rates represent children is the number of children in primary-schools per 5-11 children. Teacher rates refer to the number primary-school teacher per 100 children aged 5-11. These variables are at the department/year level. All regressions control for department and year fixed effects as well as department-specific linear time trends. The observations are weighted by the square root of the number of 5-11 children. Robust standard errors (in brackets) are clustered at the department level.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table F.3: Coffee prices and School Attendance
(Role of Household Work Decisions)

	Dependent variable: School attendance		
	(1)	(2)	(3)
log coffee price ×	-0.0112	-0.0109	-0.0115
coffee cultivation intensity	[0.0027]***	[0.0028]***	[0.0039]***
	(0.000)	(0.000)	(0.025)
Adult employment rate		-0.1114	-0.1132
		[0.0667]	[0.0700]
		(0.1161)	(0.1311)
log coffee price ×			0.0006
coffee cultivation intensity ×			[0.0021]
Adult employment rate			(0.7718)
Observations	2203	2202	2202
R^2	0.9396	0.9398	0.9398

Notes. Sample limited to 1973, 1985 and 1993 census data on children in rural areas aggregated at the the department/census-year/cohort/gender level, and the observations are weighted by the square root of the cell sizes. Adult employment rate refers to the fraction family members over 17 years old who are employed at census time. All regressions include controls for department-specific time trends, and department, gender, census-year and cohort-census fixed effects. Robust standard errors (in brackets) are clustered at the department level. Two-tailed p-values based on the wild cluster bootstrap-T method in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table F.4: Coffee Price Shocks and Completed Schooling
(Role of Local Violence)

	Dependent variable: years of education attained					
	(1)	(2)	(3)	(4)	(5)	(6)
log school-age coffee price × coffe cultivation intensity	-0.04	-0.0519	-0.0485	-0.0488	-0.0492	-0.0471
	[0.0093]***	[0.0098]***	[0.0090]***	[0.0087]***	[0.0095]***	[0.0112]***
log school-age coffee price × coffee cultivation intensity × attacks		0.0036				0.0068
		[0.0127]				[0.0233]
log school-age coffee price × coffee cultivation intensity × clashes			-0.0045			-0.009
			[0.0106]			[0.0181]
log school-age coffee price × coffee cultivation intensity × massacres				-0.0618		-0.0749
				[0.0852]		[0.1029]
log school-age coffee price × coffee cultivation intensity × political kidnappings					-0.0282	-0.0261
					[0.0882]	[0.0893]
Observations	64234	60336	60336	60336	60336	60336
R^2	0.7319	0.656	0.656	0.656	0.656	0.6567

Notes. Column (1) replicates the baseline results (Table 2, column (3)). Columns (2)-(6) include additional interactions. Attacks refers to the number of violent event carried out by either guerrillas or Unilateral violent event carried out by either guerrillas or paramilitaries, in which there is no direct armed combat between two groups. Clashes refers to the number of direct encounter between two or more groups that results in armed combat. Massacres refers to the number of intentional killing of four or more civilians in a single event by either guerrillas or paramilitaries. Political kidnappings refer to the number of kidnapping of government officials, political candidates running for office, and other community leaders by guerrillas or paramilitaries. These conflict intensity variables are averaged over the 1984-2004 period. Besides the baseline controls, columns (2)-(6) include year-of-birth fixed effects interacted with the conflict intensity variables. Robust standard errors (in brackets) are clustered at the municipality level.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table F.5: Municipality-level Variables

Variable	Description
La Violencia	Two variables measuring intensity of the Colombian civil war known the “La Violencia” in the 1950s (before and after 1955).
Manufacturing Employment	Total individuals employed in the manufacturing industry per resident in 1945.
Market Access	Six ordered categories measuring the ease of transport to major markets in 1960.
Economic Development	Six ordered categories measuring local economic development in 1960.
Leishmaniasis Disease	The fractions of territory within each municipio in which transmission of Leishmaniasis occurs.
Hookworm	The fractions of territory within each municipio in which transmission of Hookworm occurs.
Non-hookworm helminth	The fractions of territory within each municipio in which transmission of Non-hookworm helminth occurs.
Malaria ecology	Index of malaria ecology based on climatic factors.
Attacks	Unilateral violent event carried out by either guerrillas or paramilitaries, in which there is no direct armed combat between two groups. This variable is averaged ob the 1984-2004 period.
Clashes	Direct encounter between two or more groups that results in armed combat, averaged over the 1988–2004 period.
Massacres	Intentional killing of four or more civilians in a single event by either guerrillas or paramilitaries, averaged over the 1988–2004 period.
Political kidnappings	Kidnapping of government officials, political candidates running for office, and other community leaders by guerrillas or paramilitaries, averaged over the 1988–2004 period.

Notes. Data on *La Violencia*, manufacturing employment, market access, economic development, disease incidence, and malaria ecology are from Bleakley (2010), which originate from Oquist (1976), the Colombian Census of manufacturing, Banco de la Republica (1964), Gallup et al. (1999) and Poveda et al. (2000). Data on attacks, clashes, massacres and political kidnappings are from Dube and Vargas (2013), which originates from the Conflict Analysis Resource Center (CERAC) and Center for Study of Economic Development (CEDE).

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