Present Bias and Underinvestment in Education? Long-run Effects of Childhood Exposure to Booms in Colombia

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Resumo

Este artigo analisa os efeitos de longo prazo de choques de renda usando mudanças nos preços do café em Colômbia em uma estratégia de diferenças-em-diferenças. Os resultados indicam indivíduos que foram expostos a maiores retornos no mercado de trabalho do café na idade escolar completaram menores anos de escolaridade e baixa renda quando adultos. Isto sugere que deixar a escola durante "booms" temporários implica numa perda significativa de renda de longo prazo. Estes resultados são consistentes com a possibilidade de que estudantes ignoram ou pesadamente descontam as futuras consequências de deixar a escola quando encarado com ganhos imediatos de renda.

Abstract

This paper examines the long-run impacts of income shocks by exploiting variation in coffee cultivation patterns within Colombia and world coffee prices during cohorts' school-going years in a differences-in-differences framework. The results indicate that cohorts who faced higher returns to coffee-related work during school-going years completed fewer years of schooling and have lower income in adulthood. These findings suggest that leaving school during temporary booms results in a significant loss of long-term income. This is consistent with the possibility that students may ignore or heavily discount the future consequences of dropout decisions when faced with immediate income gains.

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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, but at the same time higher wages can significantly raise the opportunity cost of schooling and thus discourage educational investments. 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 (e.g., Black et al., 2005; Edmonds et al., 2010).[1] To date, however, there is little systematic evidence documenting the extent to which these shocks translate into long-run differences in total human capital, and much less evidence on how they affect subsequent labor market success in adulthood.

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 after these economic opportunities are gone.[2] This will alter the timing of schooling without any consequence on completed education. Furthermore, many youths may delay schooling and enter the workforce during booms to accumulate savings and finance subsequent education that otherwise would have 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 may lead to larger long-run impacts of income shocks.

This paper examines this question by exploring the long-term effects of plausibly exogenous income shocks in Colombia generated by changes in world coffee prices. Colombia is a major exporter of coffee, a relatively homogeneous good whose 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

[1]Other studies include Ferreira and Schady (2004), Kruger (2007), Rosenzweig and Evenson (1977), and Soares et al. (2012). A comprehensive review of this literature can be found in Ferreira and Schady (2009). 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.

[2]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.

coffee prices, including weather shocks that decimated Brazilian coffee crops, the collapse of the international coffee price agreement, and the unprecedented expansion in Vietnam's coffee industry. During this period, the real price of coffee fluctuated between 55 percent below and 130 percent above its historical average, and previous studies have documented that these shocks significantly affected local wages across coffee-growing areas (Miller and Urdinola, 2010; Dube and Vargas, 2013). Since all these events 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. Colombia is a geographically heterogeneous country, such that, some regions are well suited for coffee cultivation, while others are not. Individuals in areas where the intensity of coffee cultivation is high should be more affected by changes in coffee prices, but those in low-intensity or non-growing areas serve as a comparison group. In addition, because the coffee price shocks were large and of varying intensities, different birth cohorts witnessed different world coffee market conditions during their school-going years. The empirical strategy therefore compares cohorts based on the intensity of coffee cultivation in their place of birth, and world coffee prices during their schooling-going years. 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. I provide evidence supporting the plausibility of this assumption.

The estimates suggest that a rise in international coffee prices reduces educational attainment disproportionally in municipalities cultivating more coffee. The estimates imply that the increase in childhood coffee prices from individuals born in 1954 (preboom cohort) to those born in 1970 (boom cohort) led to a decline in completed schooling of 0.045 years. This estimate is very precise and of comparable magnitude to that of well-documented interventions targeting education in developing countries. For example, the magnitude of this effect is quite similar to those produced by the Colombian PACES program, which randomly assigned private school vouchers (Angrist et al., 2002).

After establishing that coffee price shocks have a robust effect on completed education and the likely mechanism behind these patterns, I then examine potential changes in labor market earning profiles in adulthood. 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 0.45-percent reduction in earnings. Combined with the schooling results, this finding suggests that a one year decrease in schooling as result of a coffee boom would reduce adult earnings by about 10 percent.

This paper is connected to a broad literature on the determinants of human capital formation. This literature has focused on the effects of changes 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). This study is more directly related to previous studies linking economics shocks and child labor in developing countries (Edmonds and Pavcnik, 2005, 2006; Kruger, 2007; Edmonds et al., 2010; Soares et al., 2012). The findings of this paper add to this literature by documenting the extent to which economic shocks that increase child labor have persistent impacts.

2 Data

2.1 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 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, the 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 in size. Since coffee cultivation intensity is measured before the major coffee price shocks, endogenous production responses to variation in coffee prices is not a concern. Still, this measure is likely to accurately capture 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 largely determine the suitability and thus distribution of coffee cultivation (de Graaff, 1986).

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 coffee cultivation is 4.56 hectares per each 100 hectares of total municipality area (relative to a mean of 2.8). 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.

2.2 Census 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).[3] Another available census is that of 1985. I do not use this census in my long-run analysis because it does 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 frequency weights 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.[4]

To estimate the extent to which individuals were exposed to coffee market conditions 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.[5] 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.[6] Moreover, the vast majority

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

[4]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 includes cohorts born between 1949 and 1983, or individuals aged 22-56.

[5]In Colombia, children must start school the year they turn 6. Since I use age at census time to infer individuals' year of birth, I am not able to identify the exact year they turned 6. Consequently, I assume that the school-age period begins when an individual is $t + 5$ years old, where t = census - age at census. The results are essentially the same if I use instead $t + 6$.

[6]The census enumerators asked respondents whether "the actual municipality of residence is the same as the one where they were born." This information allows to directly identify "movers." The 1964 and 1985 censuses did not ask the birth municipality for those individuals whose current municipality of residence is different from their municipality of birth. In this case, it is even possible to identify "movers", but not their place of birth.

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.[7] 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.[8] 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 childhood 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.[9] Childhood exposure is calculated as the interaction between the average world coffee prices observed during cohorts' school-going years (ages 5-16) and the time-invariant measure of coffee cultivation intensity in the municipality of birth. As shown above, coffee cultivation patterns are highly persistent over time, so the pre-determined measure of coffee cultivation intensity is likely to be a good approximation of "real" baseline market size and accurately capture differences in childhood exposure across areas.

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 the effects of coffee price shocks on adult income. Since the Colombian census does not collect any information about income, I follow Bleakley (2010) and assign income scores based on the average earnings of individuals in the same industry, class of worker and gender cell, drawn from other Latin American censuses with available information on income.[10] The resulting indicator represents (log) average earnings

[7]These migration patterns are not exclusive to Colombia. These patterns are similar to that observed in other Latin American countries such as Bolivia, Brazil, Chile, and Mexico (see, for example, case-count view of the " place of residence 5 years ago" variable for these countries at https://international.ipums.org/international-action/variables/MIGRATE5#description_section, last accessed on February 18, 2019). Other studies have also used the place of birth to identify childhood exposure effects in other settings, including Duflo (2001) for Indonesia, and Bleakley (2010) for the United States, Mexico, and Brazil.

[8]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.

[9]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 aggregate the coffee census data into this broader definition of municipality.

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

across industry/class-of-worker/gender cells (after removing census-country specific effects), or simply log earnings.

The baseline sample consists of approximately 2.7 million observations.[11] Since the key identifying variation relies on changes across birth cohorts and municipalities, I aggregate the data into cell means by birth cohort, municipality of birth, census-year, and gender to ease the computational burden.[12] 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. The results are identical if the regressions are estimated using individual-level data.

2.3 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.[13] It reports the total number of students enrolled in public and private schools at the department level.[14] 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. Because these records also contain information on the number of teachers, 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 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.

[11] The expanded sample with census weights consist of about 33 million observations. These weights are employed in all analyses. The results remain virtually the same if the census weights are not used.

[12] In generating these aggregate data, I first expand the sample using the frequency weights given by the census IPUMS.

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

[14] There are 33 departments in Colombia. However, in the education statistics, some departments are combined with neighboring departments, leading to a total of 22 geographical units or simply departments.

3 Empirical Strategy

Short-run Effects. Previous literature has so far focused on the short-run effects of local income shocks on human capital formation. Following this literature, an analogous short-run specification of the effects of coffee price shocks would be as follows:

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

where Y is either the proportion of children who are currently attending school or working in area j, cohort aged a and gender g at the time of the year t census. The key independent variable is given by the interaction between (log) real world coffee prices, $\mathbb{P}_t$, and the (time-invariant) measure of coffee cultivation intensity, $\mathbb{I}_j$. This interaction term measures the prevailing coffee market conditions at the census-year t. The specification includes controls for area fixed effects (λ_j), and cohort-census fixed effects (μ_{at}), which capture any time-invariant differences across areas and common changes over time. The area-specific time trends, $\mathbf{T}_{jt}$, account for possible long-run dynamics in socioeconomic and other characteristics across areas.

This model is basically a differences-in-differences (DID) setup that uses two continuous measures of "treatment" intensities, thereby exploiting exploit greater variation in the data than the standard two-group/two-period DID. In Section 4.1, I present results from estimating this model. In doing so, I use data from the 1973, 1985 and 1993 census data.[15] Unlike the long-run analysis described below, I consider the department rather than municipality as unit of analysis for two reasons. First, information on the municipality of birth was not collected in 1985. Second, matching individuals with coffee cultivation data 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.

Long-run Effects. The model above, however, does not allow to infer the extent to which local income shocks induced by changes in coffee prices have long-run effects on human capital. To examine this question, I adopt an intention-to-treat (ITT) design that compares long-run outcomes of cohorts with varying coffee cultivation intensities in their municipality of birth, and different world coffee prices during their school-going years. In particular, I employ the following specification:

$$Y_{jgct} = \tilde{\alpha} + \tilde{\beta} \, \left(\bar{\mathbb{P}}_t \times \mathbb{I}_j\right) + \tilde{\kappa} \, \mathbf{T}_{jt} + \tilde{\lambda}_j + \tilde{\gamma}_g + \tilde{\mu}_{ct} + \tilde{\xi}_{jgct} \tag{2}$$

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

where Y is average years of completed schooling or log earnings for individuals in municipality j, born in year t, gender g, and observed in census-year c. Now, the key independent variable is given by the interaction of childhood coffee prices, $\bar{\mathbb{P}}_t$, and the (time-invariant) measure of coffee cultivation intensity in the municipality of birth, $\mathbb{I}_j$.[16] 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 ($\tilde{\lambda}_j$), birth cohort $\times$ census-year fixed effects ($\tilde{\mu}_{ct}$), and municipality-specific time trends ($\mathbf{T}_{jt}$). The results from estimating equation (2) are presented in Section 4.2.

This specification is an extended version of the model (1) that allows to analyze long-run effects. The key parameter of interest is $\tilde{\beta}$, which summarizes the magnitude of the long-term impacts of coffee price shocks. 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 originated outside of Colombia, and the timing of such shocks was plausibly unanticipated. Moreover, since the potential coffee market of an individual is given by her or his municipality of birth, it is not endogenous to future erratic changes in coffee price shocks.

Since treatment intensity varies across areas and cohorts, this fuzzy differences-in-differences estimates a weighted average of Wald-DIDs (De Chaisemartin and D'HaultfŒuille,

[16] As mentioned above, the intensity of coffee cultivation is measured as the total of land used for cultivating coffee (scaled by municipality area) in 1932. An alternative approach would be to use coffee cultivation intensity in 1970 as the key treatment variable and generate an instrumental variable using data on coffee cultivation in 1932. The key independent variable would be constructed using a more recent measure of coffee cultivation intensity, and the instrumentation strategy would eliminate any possible bias induced by endogenous production responses to past coffee prices. The results are quantitively and qualitatively similar when following this approach. Hence, one can interpret equation (2) as a reduced-form expression, which is my focus for simplicity.

2017).[17] In addition to the common trend assumption discussed above, identification in my setting also requires the absence of heterogenous effects over time. Results in Section 4.2 suggest that this seems to be the case (i.e., there is no evidence that the effects of coffee price shocks are heterogeneous across birth cohorts).[18]

After showing the baseline results, I also present results from a more flexible specification that allows to examine how the long-rung effects of coffee price shocks vary with children's exposure age:

$$Y_{jgct} = \tilde{\alpha} + \sum \tilde{\beta}^a \left(\bar{\mathbb{P}}_t^a \times \mathbb{I}_j\right) + \tilde{\kappa}\, \mathbf{T}_{jt} + \tilde{\lambda}_j + \tilde{\gamma}_g + \tilde{\mu}_{ct} + \tilde{\xi}_{jgct} \tag{3}$$

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 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 outcomes driven by other factors.

Throughout the analysis, I use standard errors that are clustered at the municipality level (or department level when using equation (1)) to account for potential serial correlation. The preferred specification includes a robust set of fixed effects and municipality-specific linear time trends, but results are almost unaffected if a number of additional controls are included.

4 Results

4.1 Short-Run Effects on Schooling and Child Labor

Before showing the long-run estimates, the focus of this paper, I examine the effect of coffee price shocks on contemporaneous school attendance and child labor as in previous studies. Table 1 shows the results from estimating model (1). 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 (33 departments), I also calculate two-tailed p-values using the wild cluster bootstrap-T method (Cameron et al., 2008). Column (1) documents that

[17] I also consider binary definitions of the treatment by classifying areas into high and low cultivation intensities groups as well as periods into low and high coffee price categories. The conclusions are the same under these alternative definitions.

[18] I also estimate the time-corrected Wald ratio (Wald-TC) estimator proposed by De Chaisemartin and D'HaultfŒuille (2017), which does not rely on any assumption on treatment effects. I find point estimates that are extremely similar to the baseline, although the Wald-TC results are somewhat imprecise.

increases in real coffee prices are associated with reduced school attendance. The estimated coefficient is precisely estimated and thus highly significant at the conventional levels of 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 ($-0.011 \times 0.82 \times 2.6 = -0.023$).

In columns (2)-(3), I estimate the model separately for children aged 5-11 and 12-16. The estimates suggest that both young and old children are negatively affected by increases in the real price of coffee, with magnitudes and significance that are extremely similar. One potential reason for this is that, as discussed above, the most labor-intensive activity in coffee farming is relatively simple and can be performed both for young and old children. This reasoning is consistent with previous reports documenting similar employment rates in the coffee sector among children aged 5-11 and 12-16 (Bernal and Cárdenas, 2006). Hence, changes in coffee prices can plausibly affect the opportunity cost of schooling both for young and old children.

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 (1) that uses a department-level panel of school enrollment rates. Consistent 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 employment 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.

4.2 Long-Run Effects on Completed Schooling

4.2.A Main Findings

I now turn to the main question of whether income shocks induced by changes in coffee prices have long-term effects on completed human capital. I begin by examining graphically the relationship between these variables. In doing so, I estimate a semi-parametric and simpler version of equation (2) where the intensity of coffee cultivation is interacted with birth cohort dummies, adjusting for birth cohort $\times$ census-year fixed effects and municipality of birth fixed effects. The coefficients on these interactions compare the trends in schooling over time in municipalities with different coffee cultivation intensities. Figure 1 plots the coefficients and respective 95 percent confidence intervals. There are no differential trends in schooling among cohorts who were born between 1949 and 1955 across municipalities cultivating coffee more and less intensively. 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 intensities. 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 born between 1978 and 1983, who faced lower coffee prices in childhood compared to the boom cohorts. Overall, these patterns in schooling mirror the trends in childhood coffee prices.

Table 2 reports formal estimates of the effect of coffee price shocks on educational attainment based on equation (2). 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 during school-going years lead to fewer years of completed schooling in areas with greater intensity of coffee cultivation. Columns (2)-(4) add other controls sequentially to this specification. The addition of municipality-specific linear time trends in column (2) has small effects on the estimated coefficient, which is now -0.040 (standard error =0.009). Point estimate is similarly little affected 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) drops 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).

To explore how the effects of coffee price shocks vary with children's exposure age, Figure 2 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 insignificant.[19] 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 during school-going years lead to reduced educational attainment. This suggest that income shocks induced by changes in the real price of coffee have persistent effects on completed human capital. To interpret the results, 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 relatively low coffee prices 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 ($0.5 \times -0.04 \times 4.5 = -0.09$).

[19] The fetal origins literature suggests that income shocks during the prenatal period should have long-run repercussions on schooling (see Almond and Currie (2011) for a review of this literature). 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.

4.2.B Interpretation of Magnitude

To gauge the magnitude of the results, I perform a simple exercise that measures what would have been the level of completed schooling of individuals born in 1970 (boom cohort) if they had been exposed to the same childhood coffee prices as those born in 1954 (pre-boom cohort) —a difference of about 50 percent in childhood coffee prices. I can use the baseline result reported in column (3) of Table 2 to compute the counterfactual level of schooling of the boom cohort for each municipality. This is equal to the observed level of schooling of the boom cohort minus the estimated parameter of $\tilde{\beta}$ multiplied by the intensity of coffee cultivation and the change in (log) childhood coffee prices: $years\ of\ education_{jgc,1970}$ $-\hat{\tilde{\beta}} \times \mathbb{I}_j \times 0.5$. These counterfactuals are then averaged across all municipalities to obtain an overall counterfactual measure of completed human capital. The calculations suggest that, on average, the 1970 cohort would have completed an additional 0.035 years of schooling without the shock.

This estimate represents an ITT effect because exposure to coffee market conditions is based on an individual's place of birth (rather than the childhood municipality of residence). Using information on geographical mobility rates, I can calculate an approximate estimate of the treatment-on-the-treated (TOT) effect by dividing the ITT impact by the fraction of school-age children residing in their place of birth at census time.[20] As discussed in Section 2.2, about 75 percent of individuals were residing in their place of birth when they were of school-going age. This suggests a TOT effect of 0.045 years of schooling $(0.045 = 0.035/0.75)$.

To place the magnitude of this effect in perspective, I can compare it with well-documented interventions targeting education in developing countries. Perhaps, the Sekolah Dasar INPRES program in Indonesia and the Colombian PACES program are two of the best-known and well-documented examples of such interventions. 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 (Duflo, 2001). In the PACES program, which is particularly relevant to my setting given its focus on Colombia, students were randomized to receive private school vouchers that reduced school fees by about 80 percent (Angrist et al., 2002). The INPRES program raised educational attainment by 0.12 years in high intensity program regions, while children treated in the PACES program completed an additional 0.1

[20]This is a 'back-of-the-envelope" calculation of the TOT effect. Ideally, if one had information on an individual's place of residence in childhood, one could instead estimate the TOT effect using a 2SLS regression. Under this approach, the first stage would be the specification (2), but using childhood coffee market conditions (based on an individual's childhood municipality of residence) as dependent variable. The coefficient on this first stage would be approximately equal to the fraction of school-age children residing in their place of birth. To extent to which information on geographical mobility rates is accurate, the 'back-of-the-envelope" estimate would tend to be similar to that produced by this 2SLS approach.

years of schooling. Scaling these effects by a 50 percent variation in treatment intensity, the INPRES program had an impact of 0.10 years on educational attainment, and the PACES program had an effect of 0.06 years of education.[21] Therefore, the magnitude of my results is quite similar to that of the PACES program, and approximately one half the effect of the INPRES program.

4.3 Long-Run Effects on Earnings

4.3.A Main Findings

After establishing that coffee price shocks have a robust effect on completed education and the likely mechanism behind these patterns, I then examine potential changes in earnings in adulthood. 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 (2), but use the industry-based earnings score as dependent variable. The results are shown in Table 3. Column (1) shows the results from a specification that controls for a basic set of fixed effects (i.e., municipality, birth year, gender and census-year fixed effects). I find an estimate of $\tilde{\beta}$ of -0.0023 (standard error =0.001), which is statistically significant at the 5 percent level. This suggests that higher coffee prices in childhood are associate with lower adult earnings. Columns (2)-(3) document that the coefficient is slightly larger in magnitude and relatively more precise when municipality-specific time trends and census $\times$ birth cohort fixed effects are included. The coefficient of interest is now -0.0037 (standard error=0.0013) and statistically significant at less than the 1 percent level.

In column (4), I evaluate the robustness of the results to considering an alternative source of data to assign income scores. The baseline earnings score is constructed using data from other Latin American census with available information on income. One might be concerned that these earnings scores do not accurately reflect relative incomes across industry, class of worker and gender cells in Colombia. As a robustness check, I generate an alternative income score using data from the 1973 census, the only Colombian census with information on income. This alternative measure is highly correlated with the baseline one, with a

[21] Duflo (2001) estimates the baseline effects of the INPRES program by comparing high and low intensity regions (see the coefficient reported in column (1) of Table 4). The difference in treatment intensities between low and high intensity regions is about 60 percent, so I can normalize the baseline estimate by multiplying the baseline coefficient by 0.5/0.6. Given that the PACES program reduced on average school fees by 80 percent, I normalize the treatment effect by multiplying the baseline estimate of 0.1 by 0.5/0.80.

correlation coefficient of 0.93. Given this high correlation, the results are unsurprisingly similar in magnitude when the alternative income score is used as an outcome (column 4).

In columns (5)-(6), I explore the gender specificity of the results. As shown above, the schooling results are larger in magnitude for males than for females. Hence, if the changes observed in earnings are driven primarily by changes in completed schooling, then one should observe a similar gender heterogeneity in the effect of coffee price shocks on earnings. To examine this question, I estimate the effects of coffee price shocks on income separately for men and women. Consistent with the schooling results, I find that, although both male and female earnings significantly decline with higher childhood coffee prices, male earnings decline more than female with coefficients of -0.004 and -0.0027, respectively.

In sum, the results of this section suggest that cohorts who faced sharp rises in the return to coffee-related work during school-going years have lower earnings in adulthood. 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. Performing the same counterfactual calculation as that in Section 4.2.B, I find that the boom cohort born in 1970 would have had, on average, 0.34 percent higher earnings if they had been exposed to the same coffee prices as the pre-boom cohort born in 1954. Using information on geographical mobility rates in childhood, the corresponding TOT effect is approximately 0.45 percent.

4.3.B Implied Returns to Schooling

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 baselines estimates of $\tilde{\beta}$ are -0.04 and -0.0037 for schooling and log earnings, respectively. Together, these estimates imply that the marginal effect of an extra year of schooling on income is about 10 percent (-0.0037/-0.04). 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 between coffee cultivation intensity and childhood coffee prices 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).

5 Conclusion

This paper has provided new evidence on the long-term impacts of local income shocks during school-going years. In doing so, this study exploits variation in local economic conditions in Colombia generated by dramatic fluctuations in the international price of coffee. The results indicate that cohorts who faced sharp rises in the return to coffee-related work during school-going years completed fewer years of schooling and have lower income in adulthood. Collectively, these findings suggest that educational decisions made early in life based on local labor market conditions can have persistent impacts.

The negative effects both on long-run schooling and income are difficult to reconcile with a human capital model where education is viewed as a financial investment (Eckstein and Wolpin, 1999). Rather, these findings are consistent with the possibility that children and adolescents ignore or heavily discount the future. This behavior may be driven by credit-constrained youths rationally trading off between immediate income gains and future returns to extra schooling, with no overall welfare consequences. Alternatively, it may be that individuals drop out of school when faced with immediate income gains without realizing that in a few years their salaries will be lower than if they had stayed at school. This interpretation is made somewhat more plausible by the evidence in neurology that executive brain functions responsible for abstract reasoning, self-control and patience skills are underdeveloped among children and adolescents (Fuster, 2002; Giedd et al., 2010; Romine and Reynolds, 2005; Teffer and Semendeferi, 2012). As a consequence of poor abstract reasoning, children and adolescents may be more likely to overemphasize immediate rewards and engage in risk-taking behaviors that may lead to suboptimal outcomes (Lavecchia et al., 2016).

The present-biased behavior may be exacerbated if students focus too much on negative identities, in the sense that they may make educational investments based not only on their own benefits but also on immediate social gratification from their peer group (Haun et al., 2013; Lavecchia et al., 2016). If dropping out of school during booms is consistent with the behavior of some peer groups, then other students may want to do the same to conform. As a result, rises in the opportunity cost of schooling may massively increase dropout rates.

Similarly, interrupting or delaying school may create an inertia in subsequent individuals' decisions if children and adolescents rely too much on routine and automatic thinking (Lavecchia et al., 2016). In particular, employment during temporary booms may lead individuals to stick to a routine of "not attending school" even after these economic opportunities are gone. And this routine may be difficult to change. This may help explain why temporary economic shocks have persistent impacts.

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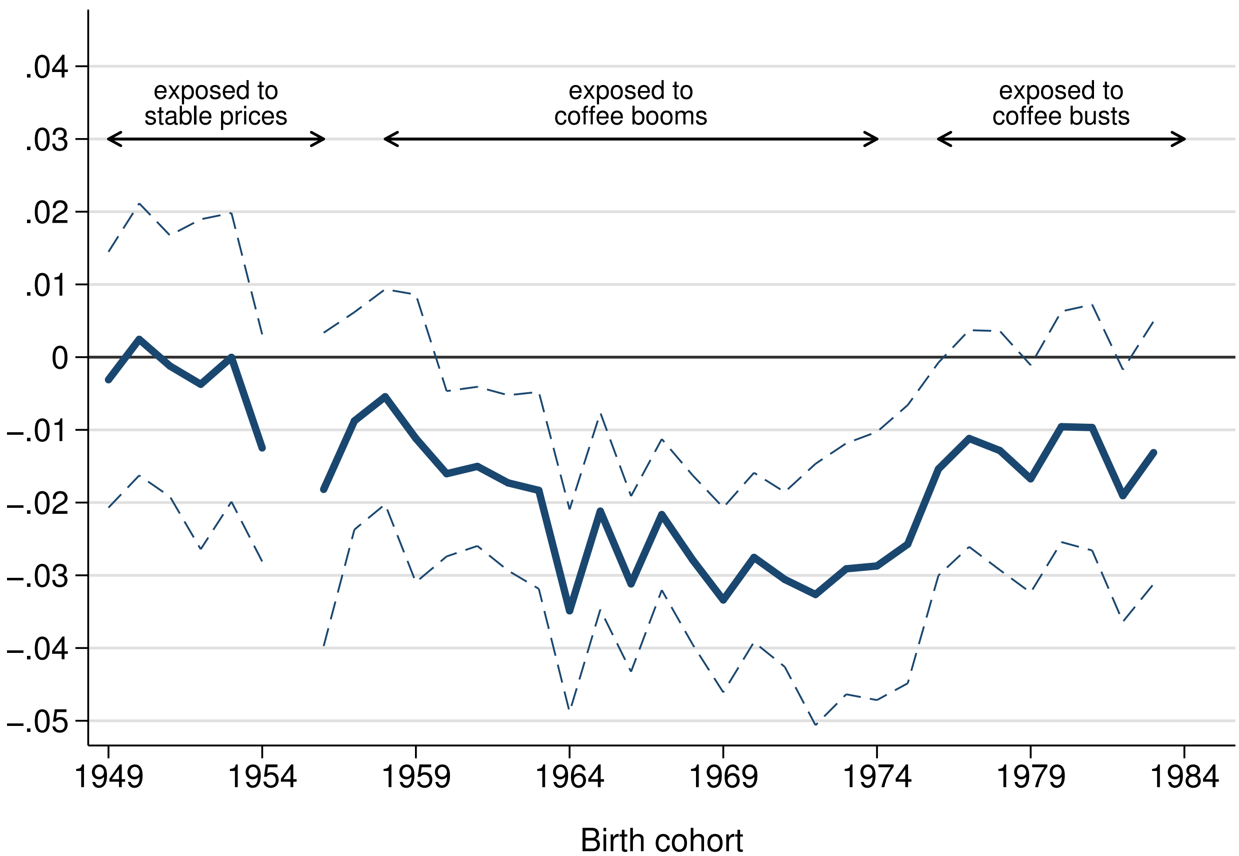
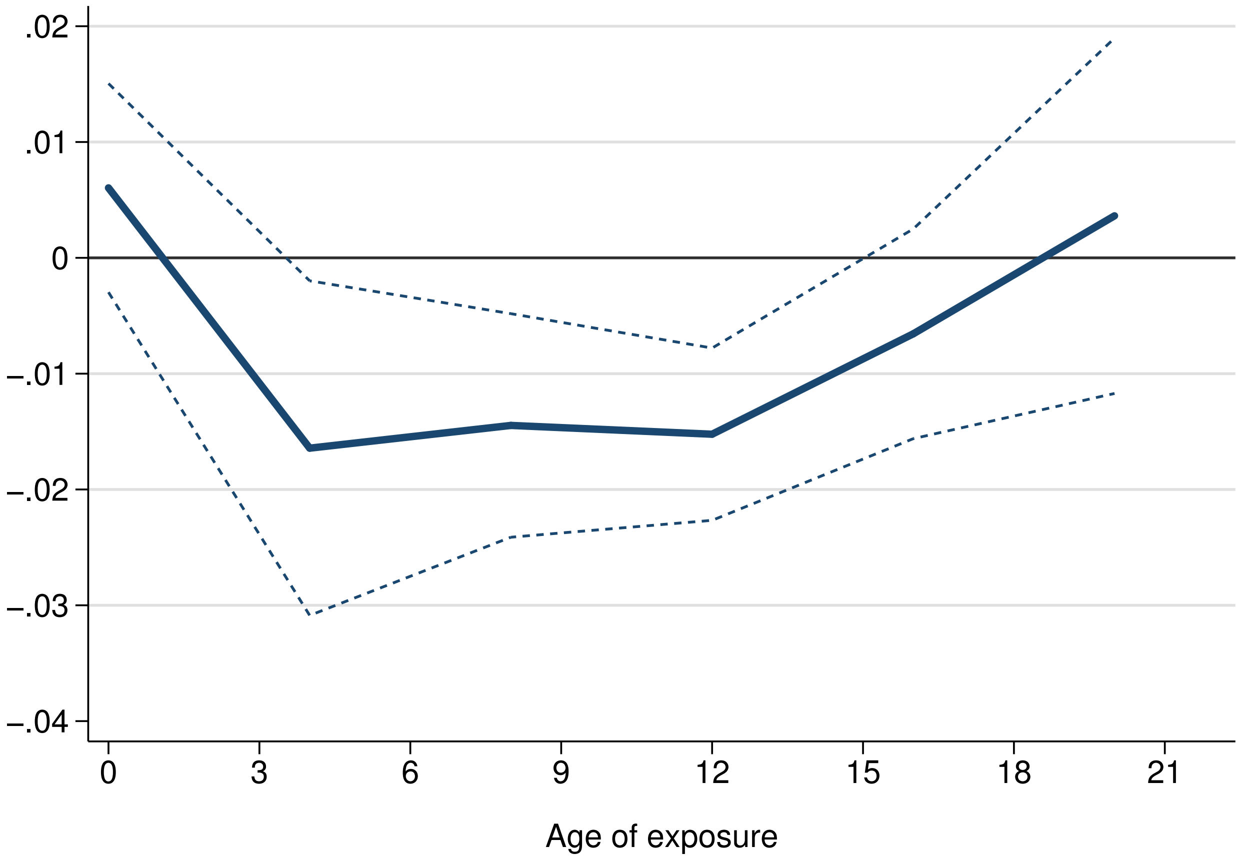
Figure 1: —Cohort Schooling and Coffee Cultivation



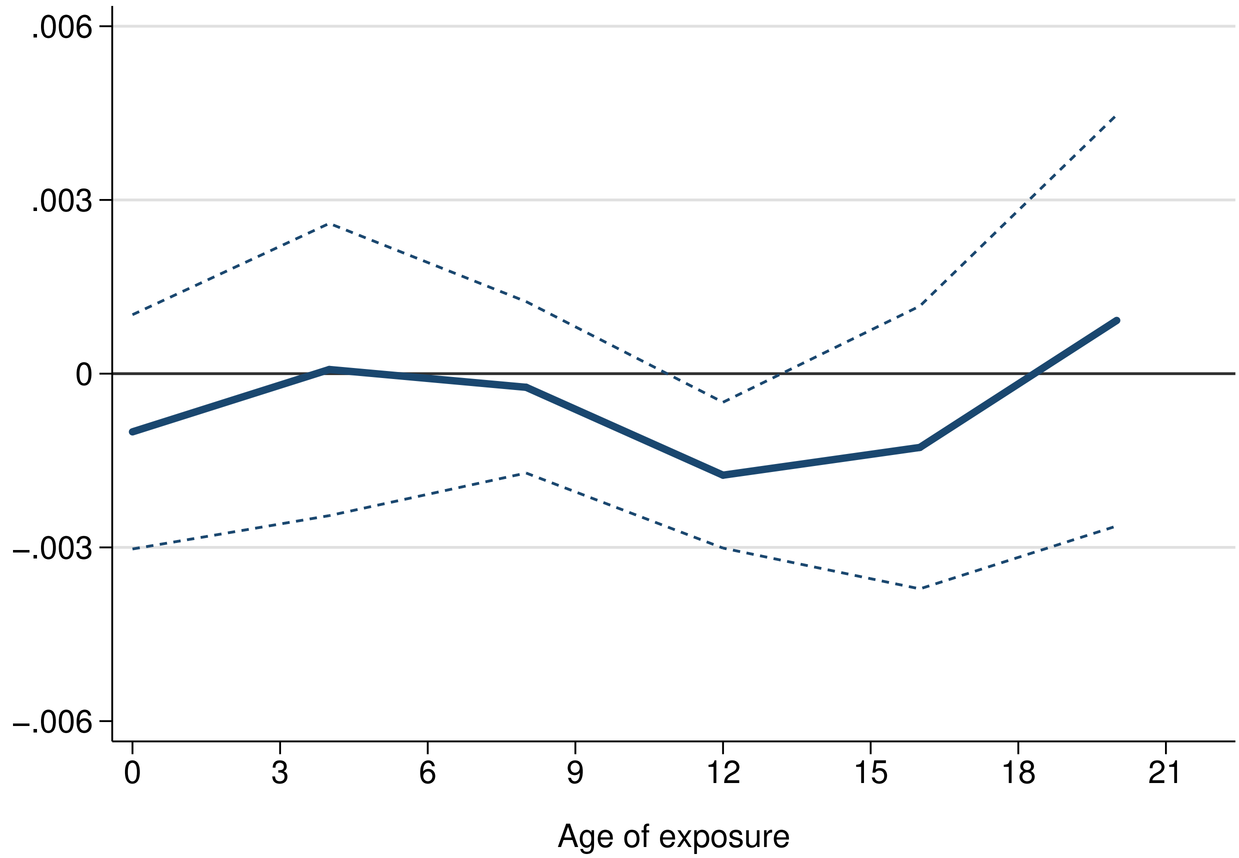
Notes. This figure presents estimates of $\tilde{\beta}^t$ from $S_{jtc} = \tilde{\alpha} + \sum_{t=1949}^{1983} \tilde{\beta}^t \left(\mathbf{1}(t = \tau) \times \mathbb{I}_j\right) + \tilde{\lambda}_j + \tilde{\mu}_{ct} + \tilde{\xi}_{jtc}$. The omitted group is the 1955 birth cohort. Dependent variable is average years of schooling for cohort t born in municipality j observed in census year c. Coffee cultivation intensity is given by $\mathbb{I}_j$, which is measured as total coffee cultivation in 1932 (in hectares) per 100 hectares of total municipality area. The specification includes municipality fixed effects, λ_j, and cohort $\times$ census-year fixed effects, μ_{ct}. Sample includes individuals born between 1949 and 1983 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. There are 531 municipalities. The total number of observations is 64234. Robust standard errors are clustered at the municipality level. Dashed lines plots 95 percent confidence intervals for estimates of β^t.

Figure 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. It shows estimates of $\tilde{\beta}^a$ from model (3). 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. Coffee cultivation intensity is measured as total coffee cultivation in 1932 (in hectares) per 100 hectares of total municipality area. 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 born between 1949 and 1983 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. There are 531 municipalities. The total number of observations is 64234.

Figure 3: —Effects of Coffee Price Shocks on (log) Industrial Income Score



Notes. This figure plots estimates of the effects of coffee price shocks at different ages of exposure on (log) industrial earnings score. It shows estimates of $\tilde{\beta}^a$ from model (3). 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. Coffee cultivation intensity is measured as total coffee cultivation in 1932 (in hectares) per 100 hectares of total municipality area. 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 born between 1949 and 1983 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. There are 531 municipalities. The total number of observations is 61472.

Table 1: 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 ×	-0.0112	-0.0112	-0.011	-0.0909	0.0033
coffee cultivation intensity	[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. Coffee cultivation intensity is measured as total coffee cultivation in 1932 (in hectares) per 100 hectares of total land area. 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. These 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. There are 33 departments in columns (1)-(3) and (5), and 22 departments in column (4). The number of departments differ because some departments are grouped with neighboring departments in the formal education statistics, the source for school enrollment data.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

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 × coffee cultivation intensity	-0.047 [0.0132]***	-0.0404 [0.0094]***	-0.04 [0.0093]***	-0.0382 [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 × coffee cultivation intensity	-0.0424 [0.0126]***	-0.034 [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 total coffee cultivation in 1932 (in hectares) per 100 hectares of total municipality 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. There are 531 municipalities. 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 Adult Earnings

	Dependent variable: (log) industrial income score			
	No controls	Add municipality × linear trends	Add census × cohort fixed effects	Alternative income score
	(1)	(2)	(3)	(4)
log school-age coffee price × coffee cultivation intensity	-0.0023 [0.0010]**	-0.0037 [0.0013]***	-0.0037 [0.0013]***	-0.0037 [0.0016]**
Observations	61472	61472	61472	61471
R^2	0.3956	0.4083	0.4177	0.3422
	Male subsample	Female subsample		
	(5)	(6)		
log school-age coffee price × coffee cultivation intensity	-0.004 [0.0015]***	-0.0027 [0.0015]*		
Observations	31624	29848		
R^2	0.5836	0.2345		

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 total coffee cultivation in 1932 (in hectares) per 100 hectares of total municipality 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. Column (4) repeats the baseline specification, but uses an alternative income score based on the 1973 Colombian census. Columns (5)-(6) estimate the baseline specification separately for males and females. Robust standard errors (in brackets) are clustered at the municipality level. There are 531 municipalities.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.