Household Income and Investments in Child Health and Education in Ivory Coast

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Abstract

This article asks how household income affects household investments in child schooling and health in developing countries. In particular, we look at three outcomes: school attendance, child anthropometric status and child health condition. Using Ivorian data from the 1985-1988 *Côte d'Ivoire Living Standards Survey* and the 1993 and 1998 *Enquêtes Prioritaires*, we are able to develop an instrumentation strategy that identifies the causal effect of income. Ultimately, we find that household income has a strong impact on the three variables we examine.

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1 Introduction

In most developing countries, despite the profusion of arguments in favor of large investments in human capital, performances with regard to child schooling and health are quite bad. None or late primary schooling enrolment, low school attendance, dropouts, undernourishment, disease-prone biogeographical and social environments, poor quality of state-owned medical infrastructure, expensiveness of private care, etc. are the problems that usually and broadly characterize their education and health systems. Indeed, in a rather precarious environment, for households as well as public administration, child schooling and health may temporarily or permanently not appear as a key family or social choice. As it occasions direct expenditures (tuition fees, supplies, medicines, transport costs to the nearest doctor, etc.) and indirect costs (implicit loss of child labor wages or child labor household production, etc.), household income theoretically proves to be decisive, regardless of household intrinsic preferences towards human capital.

The Ivorian context of varying cocoa producer prices in the 1980s and the 1990s constitutes a natural experiment which is helpful in scrutinizing this issue empirically. Particularly, in consequence of the boom in international cocoa prices in the 1970s, Ivory Coast has considerably developed its cocoa industry, and has become (from 1977) the world leading country as regards cocoa beans exports. In the period 1985-1994, cocoa beans exports amounted to 29.2% of Ivorian total exports, which is momentous as total exports could stand for between 20 and 40% of total GDP over the period. As such, all the Ivorian economy was dependent on cocoa international prices. As those latter were plummeting over the 1980s, the Ivorian government, which was more or less in charge of setting the national producer price, did dramatically lower it in 1990, before raising it in 1994 and 1997. Thus, income can be instrumented with belonging to a cocoa-producing household in a given year, and its causal effect can be identified.

Using data from the 1985-1988 Côte d'Ivoire Living Standards Survey and the 1993 and 1998 Enquêtes Prioritaires, we are therefore in a position to assess the impact of income on three specific outcomes: school attendance, child anthropometric status and child health condition. Amongst other things, we show that a 10% rise in household income leads to : (a) a 0.023-0.030 increase in the likelihood to attend school for 5-17 year-old children, (b) a 0.06 diminution in the likelihood that a 3-5 year-old child has been malnourished since her birth, and (c) a 0.018-0.02 reduction in the probability that a 0-18 year-old child is sick. Specifically, OLS estimated effects of income on education and health outcomes are always downward-biased. Eventually, we believe that our paper is one of the first to provide unbiased estimates of the effect of household income on child-related health outcomes in developing countries.

2 Schooling and Health in Ivory Coast: Facts

Ivory Coast, like its adjacent Western African countries, is a demographically young country to the extent that the share of children aged 0 to 14 is high in the total population: 46.1% (UN 2007). Then, the children who are schooled may theoretically achieve the following curriculum: from 5 to 11, they complete primary schooling, from 12 to 15, they complete secondary schooling, from 16 to 18, they complete high school, and from 19 to more, they can complete University. Actually, children enter rather lately the first grade of primary schooling. In our specific sample, the average entry age into primary schooling is 7.12 (and not 5 as in theory)[1]. Then, as shown in table 1, less than half of children attend primary schooling, and even less achieve the full cycle. Lastly, those who do attend school may also work, the adjustment variable being leisure time[2]. From an

[1]Girls do not tend to enter sooner than boys (7.08 vs. 7.15 for the latter), contrary to what Bommier and Lambert (2001) find on Tanzanian data.

[2]In our 1988 sample of cocoa and non-cocoa agricultural producers, amongst the children aged 12 to 17, 25.42% only attend school, 7.03% both attend school and works, 51.07% only work, while the remaining 16.48% do not do anything.

anthropometric and mortality viewpoint, Ivory Coast performs well in comparison with other West African countries. Nevertheless, not all the children receive the adequate treatment when affected by a specific illness. Then, health-related private expenditures broadly exceed government expenditures (80% vs. 20%). But the production of health has essentially remained a state activity[3].

Lastly, in our sample, the education and health variables of interest appear strongly correlated with household wealth and household chef education, as reported in table 2. A remarkable fact is the positive correlation between household income and illness. We attribute this to the fact that the rich tend to overreact to plausible symptoms and overreport that their children are sick, while the poor tend to underreact and underreport.

3 Previous Empirical Evidence

If we ultimately posit that investing in education and health would be a rational social choice, the question becomes how to foster it in developing countries, specially in a context of household poverty and public administration meagre resources. Several articles have particularly studied the role of parental income, thus privileging a demand-side approach.

Gradually, it has become apparent that supply factors: the quantity and quality of schools, the abilities and motivation of teachers, etc. were insufficient to augment the level of education. Education demand factors had to be explored as well: household-specific shadow price of schooling (opportunity cost, etc.), parental income, parental education, etc. Yet, the identification of causal

[3]This paradox can be easily explained. In the 1980s, structural deficits led several African countries to raise resources. As they decided to implement fees for the users of the health care system (Cissé, Luchini and Moatti 2002), a part of former government expenditures was transferred to individuals. In addition, the supply of private care has noticeably grown in urban centers for wealthier individuals. As the latter spend much more on health than others, the share of private expenditures has mechanically augmented. Public care is indeed an inferior good, especially in Ivory Coast where public care is deemed as inefficient.

effects of demand-side factors on schooling decisions is subject to many biases that must be accounted for (Behrman and Knowles 1987, Behrman and Knowles 1997, Blau 1999). Subsequently, most studies have elaborated innovative identification strategies to disentangle the interrelated effects of parental income and parental education. A first solution is to attempt to control for heterogeneity by comparing diverse individuals within a same household or family (Behrman and Wolfe 1987). Another solution is to explore the set of familial variables to find out an instrumental variable of parental education and/or parental income (Cogneau and Maurin 2001, Dumas and Lambert 2005). A third solution is to establish how aggregate or idiosyncratic income shocks may act as an instrument for income and permit to gauge the causal effect of income (Rucci 2004, Loken 2007). In the end, those studies tend to show that OLS underestimate the causal effect of parental income on education, one exception being Dumas and Lambert 2005.

In relation to the previous framework, many studies have chosen to concentrate on the reduced-form model: they examine how negative or positive changes in income or schooling shadow price affect child schooling and even child labor (Jacoby and Skoufias 1997, Funkhouser 1999, Thomas and al. 2003, Duryea and al. 2003, Edmonds 2006, Hupkau 2007). These studies mainly coincide with the literature on liquidity constraints and imperfect credit and insurance markets. Then, other articles have only addressed the effects of income shocks on child labor (Dehejia and Ghatti 2002, Guarcello and al. 2002 and Beegle and al. 2003). In general, negative shocks decrease schooling and increase child labor, while positive shocks act conversely. Third, some studies link past income variability and present schooling choices: households facing inherently more income volatility become more risk-adverse and consequently more reluctant to invest in their child education as its return may be quite uncertain (Fitzsimons 2003, Kazianga 2005). Lastly, others studies evaluate the impact of cash and non-cash conditional transfers programs: households receive money or goods as long as their children attend school (Ravaillon and Wodon 2000, Parker and Skoufias 2001, Schultz 2004, De

Janvry and al. 2004).

While in the literature on education demand most studies have too much focused on demand-side variables instead of supply-side ones, one can say that the literature on health demand has too much focused on supply-side factors. Then, most studies rely on data from developed countries, since health data are rarely extensive in developing countries surveys. And amongst the studies that indeed involve developing countries, most deal with global health demand, without accounting for the specificities induced by the child-related health demand. After, amongst the studies that research the role of household characteristics on health demand, an even lower number of studies adopt identification strategies that correctly answer for household income endogeneity.

First, many articles opt for examining health status and anthropometric outcomes. The rationale is that health status and anthropometric outcomes tend to represent global purposed and non-purposed past investments in health and nutrition. For instance, being sick may be influenced by access to water, while the likelihood of being small or slender is affected by past calorie intakes. Studies on developed countries validate the persistence of a health gradient (Case, Lubotsky and Paxson 2001, Case 2001, Lindhal 2002, Frijters and al. 2003). Then, existing studies on developing countries display the impact of income on food intakes (Behrman and Deolalikar 1987, Subramanian and Deaton 1996) and nutritional status (Case 2001, Duflo 2003). Second, several papers illustrate how income determines the decision of consulting or not and the choice of a care supplier (private care vs. public care, modern care vs. traditional care). Since we only observe outcomes for those who are sick, estimations should be done conditionally on being ill. Examples of such strategy are Dor, Gertler and van der Gaag 1987, Akin, Guilkey and Denton 1995, Hallman 1999, Jensen 2000, Havemann and van der Berg 2002, and Lindelow 2002. Third, few studies relate household characteristics to the amount of health expenditures (Mocan, Tekin and Zax 2000)[4].

[4]Doing so is problematical as a high amount of health expenditures is difficultly interpretable.

4 The Theoretical Obstacles to the Identification of the Causal Effect of Parental Income

Clearly, the identification of the causal effect of income on schooling and health decisions is subject to many biases: a simultaneity bias, an omission bias, a measurement error bias and an over-controlling bias. The two latter produce the same effects for education and health: indeed, classical measurement errors and over-controlling (the fact to econometrically insert variables that are strongly collinear with income) both provoke a downward bias. We now detail the simultaneity and omission biases for education and for health. We then list all biases in table 3.

4.1 Education

(i) The simultaneity bias: child schooling and household income are jointly determined through the joint determination of child schooling and child labor. In other words, the more a child works, the lower its schooling attendance but the higher its total household income (*downward bias*).

(ii) The omission bias: the blindly estimated impact of household income on schooling may result from the correlation between household income and an omitted variable that is itself correlated with child schooling. (a) High income may derive from familial abilities that are transmitted to children and which will encourage parents to school them (*upward bias*). (b) High income may be the product of parental education, which makes parents more well-informed and expect sufficiently elevated education returns to incite them to school their children (*upward bias*). At the same time, the fact that parents are educated may set an example for children, who will make more effort at school, which will push parents to keep schooling them (*upward bias*). Parental education may also stand for inherent familial (dynastic) preferences towards education, and thus be correlated with the education of children (*upward bias*). (c) Household income may be a proxy for owned physi-

It could mean that households are willing to invest in health, but it could also represent the global inefficiency of the local health system.

cal assets (land, house, etc.) that may serve as collateral and allow the household to borrow money and cover costly schooling costs, even if the household current income is not adequate to cover these (*upward bias*). (d) Household income may be associated with more informal assets such as social capital (access to professional networks, etc.), which will raise the child expected return to education and thus increase schooling (*upward bias*). (e) High income may also induce that the household possesses farms and /or businesses, which provide present and future opportunities for children. It may augment the return to education and subsequently favor schooling (*upward bias*). But it could also amplify the opportunity cost of schooling and make useless future education as anything useful (for managing a farm or a business) will be directly taught by parents to their children, eventually lowering schooling (*downward bias*). (f) High income may signify that the family is located in districts that benefit from improved physical and social infrastructure, especially better schools. Schooling is then encouraged as income is correlated with a lower shadow price of education (*upward bias*). (g) Education could be a superior good, implying that education expenditures increase as income grows since growing income positively alters current household preferences towards education (*upward bias*). Those preferences must be distinguished from dynastic preferences since they only concern parents and are not inherited from grand-parents nor transmitted to children. (h) Rich people could be less risk-adverse and less discouraged by the variance of the expected returns to schooling (*upward bias*). (i) Rich people could be more able to cope with future stochastic events. Even if their children fail one day, it will not be as detrimental as it would be for poorer children, who will have no choice but to leave school (*upward bias*).

4.1 Health

For the sake of concreteness, we consider the sole relationship between health investments and household income.

(i) The simultaneity bias: (a) ill children may occur health expenditures while being unable to work, thus diminishing the household income (*downward bias*), and (b) sick children prevent parents from earning labor income as they need to be nursed. Health expenditures derive from poor child health, which is associated with reduced parental income (*downward bias*).

(ii) The omission bias: (a) Parents are wealthy because they are healthy thanks to genetically inherited beneficial health endowments that are transmitted to children, which will necessitate less health investments. Thus, parental income is fallaciously correlated with more health and less health expenditures (*downward bias*). (b) Parents and children are both healthy as a result of familial (dynastic) preferences towards health. As parental health make them more productive, child health is positively associated with income and health expenditures are negatively associated with income (*downward bias*). (c) Parents are healthy and subsequently wealthy due to intrinsic familial abilities to transform health expenditures in an increased stock of human capital. As a result, their children are also healthier, and income is spuriously associated with less health expenditures (*downward bias*). (d) Parental wealth is associated with parental education, and parental education may induce parents to better understand the need to invest in child health (*upward bias*). At the same time, their education may make them more efficient in transforming health expenditures in an increased stock of human capital for their children: they are more well-informed about the spectrum of diseases and the way of healing them (*downward bias*). (e) Household income is a proxy for owned physical assets (land, house, etc.) that may serve as collateral and thus permit the household to borrow money and cover costly health costs, even if the household current income is not suitable to cover these (*upward bias*). (f) Household income may be associated with entrepreneurial assets (business, farm, social capital, etc.), which will lift up the child expected return to health and thus increase health expenditures (*upward bias*). (g) High income may signify that the family is located in districts that benefit from improved physical and social infrastructure, especially better health care supply. Health expenditures are then encouraged as income is correlated with a lower shadow price of health. (*upward bias*). But we could also imagine that better health infrastructure are located close to poor people if the state considers that government health expenditures should favor poor districts instead of rich districts (*downward bias*). In addition, poor people could suffer from a disease-prone environment as poor districts are often more characterized by overcrowding, antiquated sewage systems, close garbage dumps, contaminated tap water, etc. (*downward bias*). (h) Health could be a superior good, implying that health expenditures increase more than proportionally as income grows since growing income positively alters preferences towards health (*upward bias*). Especially, richer people may opt for private and thus more expensive care. (i) Rich people could be less risk-adverse and less discouraged by the variance of the expected returns to health (*upward bias*). At the same time, the fact that poor people are highly risk-averse could make them overreact and spend too much on health if one of their members shows some symptoms (*downward bias*). But we could also imagine that wealthier parents are the most risk-adverse regarding health, and overreact by overreport illness and over-consult (*upward bias*). (j) Children of poorer households may be more undernourished, thus making them more vulnerable and entailing higher health expenditures (*downward bias*). (k) Poorer households may be accommodated in unsanitary living conditions (no WC, promiscuity, etc.), which will weaken the health of children and occur more health investments (*downward bias*). (l) Higher income may be correlated with child labor, which affects child health and incurs health expenditures (*upward bias*). (m) Poor parents will be more likely to work for the extralegal sector, which does not provide health insurance. They will have to pay for the care of their offspring, unlike wealthy parents working for the legal sector (*downward bias*).

5 The Changes in Cocoa Prices and the Design of our Identification Strategy

From independence till 1978, Côte d'Ivoire has experienced dramatic growth thanks to the development of cocoa and coffee exports in a context of rising primary commodity prices. However, as regards cocoa, the Ivorian State was the main beneficiary of this surge in international prices. First, the annual producer price was administered by the state-owned national office of commercialization (the "Caistab"), which usually fixed it much below the international price[5]. Over the period 1974-1980, the producer price only represented 44.7% of the export price (Cogneau 2002). Second, cocoa exports were extensively taxed by the State itself. From 1979, the decline in international cocoa prices and the subsequent mounting deficits of the Caistab as well as macroeconomic problems resulting from structural debts designated the end of the "Ivorian Miracle". Despite a small break in 1985-1986, the Ivorian economy kept declining, and in 1990, the State diminished the cocoa producer price for the first time in 25 years. In 1994, the devaluation of the CFA franc and a new rise in cocoa prices permitted Côte d'Ivoire to recover growth. This evolution is more or less depicted in figure 1.

Then, it is logical that income changes due to varying cocoa prices were different for cocoa producers (who were directly affected) and other farmers (who were indirectly affected). As a result, cocoa households may have differentially less invested in their offspring human capital from 1988 to 1993 (in comparison with non-cocoa households), and differentially more invested from 1993 to 1998. This is a priori confirmed in our figures from 2 to 5, which exhibit the average evolution of several variables for cocoa and non-cocoa households: per capita consumption, school attendance, child health status (sick or not in the last month) and child growth failure (corresponding to a Height-for-Age Z-score inferior to -2). Comparing figures 2 and 3, one can notice that the dynamic gap between cocoa and non-cocoa households is larger for school attendance than for household income.

[5] One of the goals of such institutions is to allow cocoa producers to smooth their revenues by fixing a producer price that would prevail even if the international prices had to fall. Thus, the mission of such offices is to save money when international prices are high and dissave money when these same prices are low. Unfortunately, the State used the original monetary reserves to fund large-scale unproductive projects as well as massive recruitment of civil servants.

Our intuition is that the difference is merely due to a dynamic gap in the supply of education faced by both types of households. Especially, cocoa households were located in 1993 in villages whose schools had less money and whose quality decreased. Thus, if we control for time-village effects, we hope that the dynamic gaps are the same for income and school attendance. We thus regress our school attendance and income measures on village-time fiexed effects, and we then plot the residuals for cocoa and non-cocoa households on wave dummies. Figures 6 and 7 corroborate our reasoning for the period 1988-1993. The period 1993-1998 is much more problematic, as we will explain later.

Eventually, here lies the core of our IV strategy. We can instrument household income with belonging to a cocoa-producing household in 1993 and 1998. That is why we call our econometric strategy difference-in-difference instrumentable variables (DiD-IV)[6]. More concretely, we define a treatment group and a control group, the treatment group being more susceptible to be affected by changing cocoa prices. Our treatment group is made up of all the cocoa producers, that is to say all the households that produce more than 0 kilos of cocoa beans ($COCOA = 1$), and our control group is made up of all the other farmers: subsistence farmers and agricultural producers specialized in another export crop such as coffee or cotton ($COCOA = 0$). In order to be a valid instrument, belonging to the treatment group a given year must influence income, and must be uncorrelated with the error term in the main equation: once we control for a set of observable variables, belonging to the treatment group in a given year should not affect education this same year through another channel than income. It gives for child i in household h in village v at time t, with S being the outcome, Y income, $\mathbb{1}(COCOA = 1)$ a dummy valuing 1 if the household produces cocoa and 0 if not, X a set of child and household variables, Z a set of exogenous instruments, V village-time fixed effects and u a disturbance term:

$$S_{ihvt} = a + X_{hvt}\beta + \alpha Y_{hvt} + \gamma \mathbb{1}(COCOA = 1) + V_{vt} + u_{ihvt}$$

[6] Other examples of such a strategy are Duflo 2000, Chen 2004, Hupkau 2007 and Losen 2007.

$$Y_{ihvt} = a' + X_{hvt}\beta' + \gamma' \mathbb{1}(COCOA = 1) + Z\pi + V_{vt} + v_{ihvt}$$

$$with\ Z = [\mathbb{1}(COCOA = 1,\ t = 93), \mathbb{1}(COCOA = 1,\ t = 98)],$$

$$which\ we\ also\ call\ Z = [cocoa93, cocoa98]$$

Theoretically, DiD estimators are used to exploit "natural experiments" in order to assess the effects of a certain public policy. A treatment group (supposedly affected by the policy) and a control group (allegedly unaffected by the policy) are defined, and the DiD estimator permits to obtain an unbiased estimate of the policy effect. Indeed, with only one group and two periods (before and after the public policy), we cannot ascertain whether the estimated policy effect is contaminated by a temporal change. With two groups and assuming that both groups have known the same temporal change, we can clear the estimated effect from the temporal change effect and deduce the true policy effect. With only two periods and D a dummy being 1 if this is the treatment group, we get:

$$Policy\ Effect\ DD\ = [E(Y/X, D = 1)_{t+1} - E(Y/X, D = 1)_t]$$

$$-[E(Y/X, D = 0)_{t+1} - E(Y/X, D = 0)_t]$$

Surely, resorting to such strategy imposes a certain number of hypotheses that need to be scrutinized more carefully. First, as mentionned above, it is necessary that both groups would have known the same evolution of outcomes, absent the policy change. In the following section, we will show that our cocoa and non-cocoa households are enough similar to deduce that the latter hypothesis is satisfied. Second, one must ensure that our instruments are uncorrelated with the residual in the main equation. If we posit that the error term u can be decomposed into a household unobservable heterogeneity effect H and an individual disturbance v: $u_{ihvt} = H_{hvt} + v_{ihvt}$, we must satisfy:

$$E(Z, u_{ihvt}) = E(Z, H_{hvt} + v_{ihvt}) = 0$$

By definition, $E(Z, v_{ihvt}) = 0$. Thus, the main possible limit to our analysis could be that:

$$E(Z, H_{hvt}) \neq 0$$

Concretely, it means that there can be a bias between cocoa and non-cocoa producers regarding child outcomes as long as this bias remains constant over time (since we control for being a cocoa producer in the main equation). If this bias is different in 1993 or in 1998 relatively to 1988, then our DiD-IV strategy is questionable. For instance, because of lower cocoa prices in 1990-1993, we could suppose that cocoa-producing households in 1993 are more credit-constrained, anticipate a lower return-to-education for their children, have altered their preferences towards education or live in communities whose school have less money than before and whose quality has decreased. We actually believe our strategy to be valid, as we expect all those terms to be non-influenced by the crisis, at least in the short-run, which is our framework of analysis (specifically, as such assumption is less strong for 1988-1993 than for 1988-1998, we will actually restrain all of our regressions to our first two waves 1988 and 1993). First, credit is never allocated on potential profits generated by future cocoa production, hence the irrelevance of cocoa prices for access to credit. Second, cocoa and non-cocoa households do not educate their children so that they may later work on the farm. Education is more seen as a way to enter the wage sector. Therefore, we do not see why the return-to-education perceived by cocoa households would differ from the one perceived by non-cocoa households in 1993 or 1998 in comparison with 1988. Third, we think that preferences towards education or health are not adjusted in the short run, as it is the case in our analysis on our data 1988-1993. Fourth, we could indeed think that education and health supply was differentially altered in regions with a high proportion of cocoa producers and regions with a low proportion, implying a non-constant bias between cocoa and non-cocoa households. That is why we introduce village-time fixed effects. Doing so, we do not include village or time fixed effects individually, since they would be collinear with our latter fixed effects.

6 Data

Our principal sources of data are the Côte d'Ivoire Living Standards Survey (CILSS) 1985-1988, jointly developed by the World Bank and the Direction de la Statistique of Côte d'Ivoire, and the Enquête Prioritaire (EP) 1993 and 1998, jointly conducted by the World Bank and the Ivorian Institut National de la Statistique. As we are only interested in the comparison of households in 1988, 1993 (after the negative shock) and 1998 (after the positive shock), we stack all the household data for 1985-1988 and label them 1988. Then, in order to elaborate our difference-in-difference first stage strategy, we only keep the households that were farmers (cocoa and non-cocoa farmers). Lastly, as cocoa production is concentrated in the regions "Eastern Forest" and "Western Forest", we will also provide estimates for our models considering those sole regions.

Vis-à-vis our income variable, we opt for using annual household expenditures instead of more direct income measures (other examples are Glewwe and Jacoby 1995a, 1995b, Tansel 1997). Doing so, we are aware that we miss some risk-coping mechanisms that could operate in a context of income volatility. Indeed, if a cocoa producer is confronted with a sudden and exogenous diminution in cocoa producer prices, its agrarian income will fall as a result, but its total income will not fall thanks to ex-ante and ex-post economic strategies to offset this primary loss of income: increase in the number of household members working, increase in the number of hours worked by members, dissaving, sale of divisible assets, formal or informal borrowing, use of informal insurance networks, etc. Eventually, total expenditures correspond to income once these strategies have fully functioned. Consequently, we are able to control the household abilities to cope with risks (if it can borrow or not, etc.). Accordingly, we are estimating neither the impact of current income on education and health (since some risk-coping strategies may have operated), nor the impact of permanent income (since agents may still remain liquidity-constrained). Our measure of income is more permanent than current income but more current than permanent income, and we believe it to be the most determining income variable with regard to schooling and health decisions.

7 Supporting Evidence for the DiD-IV Strategy

In our case, the recourse to a DiD-IV strategy imposes that both groups are rather similar ex ante the shocks, so that it is likely that they would have known the same temporal change, absent the shocks. If there is a significant bias between both groups ex ante, we must show or infer that this bias is likely to remain constant across periods. Since we restrain our analysis to rural households (by comparing cocoa producers and non-cocoa farmers), we have already considerably reduced the latent heterogeneity that could contaminate our analysis. It could still remain some heterogeneity, which we investigate now.

7.1 Comparison of Main Observable Characteristics between Both Groups in 1988

Table 4 shows the mean characteristics for 5-17 year-old children in both treatment and control groups[7]. Mean-equality t-test p-values were also reproduced[8]. Except for wealth variables, differences are not significant, or they are significant but they represent little compared to the mean. Cocoa households are less Ivorian which confirms that cocoa production concern more international migrants from Burkina-Faso. Then, cocoa household chefs have often relatively more ever been to school but such education-related differences between cocoa and non-households disappear for the following grades. In addition, cocoa households are richer than non-cocoa households, which is reasonable since cocoa is an exportable crop culture. Lastly, as regards educational supply, cocoa households seem to be more

[7] We have also looked at such mean characteristics for other age categorizations since the categorization we use depends upon the outcome we test. In general, results are similar.

[8] The null hypothesis is that means are equal across both groups. A p-value inferior to 0.15 thus means that both groups are different at 15% for the concerned variable.

favored in general, except when they live very far from schools (surely, deep in the forest).

7.2 Testing the Parallel Trends Assumption

We also look at outcomes and per capita consumption on the period 1985-1988 so as to check whether trends were parallel before the first shock in 1990. Figures 8 and 9 show that although this hypothesis is not perfectly respected, it remains satisfying. For the sake of simplicity, we have only shown the figure with school attendance (others figures are available upon request).

7.3 Analysis of Education-Related Observables

We are interested in specific education-related observable characteristics in 1988. We try to verify that both groups do not have specific preferences towards education ex ante the shock (in addition, we have previously demonstrated that household chef education variables were rather close for cocoa and non-cocoa households). Table 5 shows that cocoa children are more schooled than non-cocoa children but this is probably attributable to higher incomes amongst cocoa producers. Then, non-cocoa children seem more active than cocoa children, which could denote asymmetric preferences towards child labor. Actually, once we regress "being active" on per capita consumption, dummies for age and village-time fixed effects, the coefficient of Cocoa is almost nil (-0.007) and not significant (p-value: 0.461). Surely, consumption is endogenous but we think that the results on the coefficient of Cocoa are not altered. Therefore, cocoa children are not more active than non-cocoa children. Lastly, we demonstrate that the proportion of children both not going to school and not working (our variable "abandon") does not differ much between both groups.

7.4 Analysis of Health-Related Observables

We are now interested in specific health-related observable characteristics in 1988. Table 6 illustrates that cocoa children may be less often sick than non-cocoa children. Moreover, when they are sick, cocoa households tend to more often consult someone (doctor, nurse, midwife) and buy medicine than non-cocoa households. Again, this is presumably attributable to relatively higher incomes for cocoa households. Then regarding the number of induced inactive days when sick, this difference disappears once we regress "the number of inactive days" on per capita consumption, dummies for age and village-time fixed effects (coefficient of COCOA being equal to 0.129 but with a p-value of 0.821). It is possible that cocoa children live farther in the forest, and must include a higher travel time to the nearest health center in their number of induced inactive days. In within regressions, such distinction vanishes. Then, figure 10 demonstrates that cocoa and non-cocoa children aged from 0 to 5 have the same distribution of their Height-for-Age Z-score, indicating no differences as regards nutrition preferences.

7.5 DiD-IV and Professional Migration

Actually, it is plausible that the bias between both treatment and control groups was not constant over time due to professional migration, the fact that some individuals switch from one culture to another as a result of price shocks (cocoa $\rightarrow$ non-cocoa between 1988 and 1993, and non-cocoa $\rightarrow$ cocoa between 1993 and 1998). In reality, such phenomenon is unlikely to appear since cocoa production imposes irreversible investments. A cocoa tree needs 3-5 years to produce cocoa beans, is mature after 7-10 years, and may live much more longer. Therefore, since cocoa prices were high before 1990, households that were producing cocoa before 1990 are likely to remain so in 1993 and even in 1998[9]. However, we must acknowledge that the professional migration may have happened between 1993 and 1998 due to exogenous phenomena such as urbanization and industrialization. Some cocoa and non-cocoa farmers may have joined the industrial or service sector, but we do not know if our treatment group was relatively more or less affected

[9]Even if the cocoa producer price has strongly decreased, cocoa producers have surely preferred to wait before being sure to leave the cocoa sector.

than our control group. Again, we reiterate our will to focus on our first two waves, 1988 and 1993. Table 7 illustrates the professional distribution of households in the global Ivorian sample as well as in our more restricted sample including only the Eastern and Western Forests. It is comforting to see that the share of cocoa producers has not changed between 1988 and 1993. Then, it seems that many coffee producers became non-cocoa and non-coffee farmers between 1988 and 1993. That is why we prefer to take as a control group all of them (our non-cocoa farmers), instead of the sole non-cocoa and non-coffee farmers. We thus avoid a plausible selection bias, induced by endogenous alterations of our control group.

Then, since we run within-village models, villages with no cocoa production (and full cocoa production) will be automatically dropped from our observations. Because of the shock (if we do not believe the "irreversibility" story), we could imagine that some villages that were producing cocoa in 1988 were not doing so anymore in 1993, thus entailing a change in our villages composition between 1988 and 1993. We would then estimate effects for different populations in 1988 and 1993. Figure 11 fortunately confirm our irreversibility story. The distribution of cocoa production amongst villages did not evolve across periods.

Nonetheless, we are aware that such stability could hide "exchange" of individuals between sectors. That is why we also look at how some education-related observables are altered between cocoa households and non-cocoa households in 1993 and 1998 in comparison with 1988. Table 8 exposes slight modifications of our variables, thus denoting a non-constant bias between cocoa and non-cocoa households. However, such bias is very small and we hope to account for by directly including these variables (especially, the household chef educational ones) in our IV regressions. In the end, we believe our instruments to be uncorrelated with the residual in our main equation.

7.6 DiD-IV and Geographical Migration

A second selection bias, when considering the sole model with the regions Eastern

Forest and Western Forest, could come from the fact that cocoa producers have more migrated than their control counterparts from regions Eastern and Western Forests to other regions as a result of the negative shock 1989-1993, and also more migrated from other regions to these regions as a result of the positive shock 1994-1998. If those migrants have specific unobservable characteristics that influence their child schooling, then it would make our instrument endogenous to the extent that the cocoa population is more or less different from the non-cocoa population in 1993 in comparison with other years: 1988 and 1998. Table 9 a priori substantiates the idea that regions Eastern and Western Forests ("Forest") do not represent significantly distinct shares of the Ivorian population across years. We are thus confident that geographical migration was not problematical.

8 Econometric Results

The econometric models that we run are derived from the DiD-IV within (including village-time fixed effects) model that we have written previously:

$$S_{ihvt} = a + X_{hvt}\beta + \alpha Y_{hvt} + \gamma \mathbb{1}(COCOA = 1) + V_{vt} + u_{ihvt}$$

$$Y_{ihvt} = a' + X_{hvt}\beta' + \gamma' \mathbb{1}(COCOA = 1) + Z\pi + V_{vt} + v_{ihvt}$$

Specifically, we study several outcomes, and for each one, we test six specifications that we make all robust to heteroscedasticity: OLS (column 1), OLS + regional dummies (column 2), OLS within (column 3), DiD-IV (column 4), DiD-IV within without additional controls except age, age squared and sex[10](column 5),

[10]We thought it important to report results without additional controls. If our instrument is uncorrelated with the residual in the main equation, then we may even use a model without any controlling variable. As we hypothesize that there may remain some heterogeneity deriving from a slight correlation between the instrument and the residual in the main equation, we add classical controlling variables and verify whether punctual estimates are altered or not. Ideally, we would like them to be non-modified or very slightly changed.

DiD-IV within with additional controls (column 6)[11]. The baseline specification includes age, age squared, time dummies and belonging to the treatment group ($COCOA = 1$)[12]. DiD-IV are performed using the Generalized Moments Method (GMM)[13]. We also resort to the Limited-Information Maximum Likelihood (LIML) method but results are quite similar to GMM results. Thus, we have decided to not report them. When our outcome is a binary variable, we only reproduce the results for the linear probability model, since IV-Probit or IV-Logit results are also quite similar. Then, when it was pore or less interesting to do so, we have tested the same models on the restricted spatial sample including the Eastern and Western Forests.

8.5 School Attendance

We now study school attendance, using two age categorizations: 5-17 year-old (theoretical primary plus secondary schooling) and 5-11 year-old (theoretical primary schooling). For each, we have reported the results obtained on the restricted spatial sample of Eastern and Western Forests. Lastly, since results for the global sample 1988-1993-1998 were much less satisfying (high standard errors, weak instruments), we have only reported results for the restricted sample 1988-1993. Table 10 presents the specific coefficient for log per capita consumption (our income measure), its associated p-value, the R^2 _ whether simple, adjusted (OLS within) or centered (DiD-IV) _, and the Kleibergen-Paap Wald rk F statistic which must be compared with the F statistic Stock-Yogo critical value to test whether instruments are "weak" (Stock and Yogo 2005, Baum, Schaffer and Stillman 2007b).

[11]IV estimators were calculated using Stata modules Ivreg2 (Baum, Schaffer, and Stillman 2007a) and Xtivreg2 (Schaffer 2007)

[12]The list of additional controls we use is reported under each table.

[13]Given S an outcome, X a set of observable variables, β the coefficients vector of the regression of S on X, U the vector of residuals and Z the vector of instruments (included and excluded), then the GMM calculates the coefficients vector B so that all moment conditions are satisfied: $g(B_{GMM}) = \sum_n Z_i(Y_i - X_i B_{GMM})/N = Z'U/N = 0$. Theoretically, IV-GMM generates more efficient estimates than 2SLS.

More specifically, we need the calculated F statistic (called "IV F-stat" in our tables) to be superior to the critical value (called "Threshold" in our tables) so that we may reject the null hypothesis of weak instruments. In our IV models, we use the most conservative critical value given by the Stock-Yogo weak-instrument test (10% maximal IV size). Except for three models in column 4, all our F statistics are sufficiently high to undoubtedly reject the null hypothesis of weak instruments.

First, comparison of columns 1-2 vs. 3, and 4 vs. 5-6, confirm our intuition that not taking into consideration educational supply generates an upward bias (the rich are located in better-endowed or more emulating areas). Then, comparison of columns 1-2-3 vs. 4-5-6 demonstrate that OLS underestimate the causal effect of income on education (although the extent of underestimation is not so large). Lastly, comparison of columns 5 and 6 confirm that our results are not due to a statistical artefact that would come the erroneous inclusion of controlling variables that should be included. Ultimately, columns 5 and 6 display what we think to be the true estimates of such effect. They suggest that a 10% rise in household income entails a 0.023-0.033 increase in the likelihood to be schooled, contrary to a 0.011 increase in the case of OLS[14]. It is indeed comforting to see that running our regression on the global sample ("All") or the more restricted spatial sample ("Forest") does not alter much our results. Then, the household income effect does not seem to differ for 5 to 11 year-old children (theoretical primary schooling) and 12 to 17 year-old children (theoretical secondary schooling). The younger the child, the lower the opportunity cost of education (since younger children are less productive on the labor market), and thus the lower the effect of income. At the same time, families may be willing to privilege younger children by sending them to school; household income will then prove to be determining and the effect of income will be higher. One can imagine that both effects com-

[14]If S is a linear dependant variable, Y, an explanatory variable expressed in log, and b the estimated coefficient of Y, then a 10% increase in Y causes a 0.1 × b rise in Y. Indeed, it can be shown that b $\times\frac{\Delta X}{X} \approx \Delta Y$.

pensate each other, thus resulting in more or less equal effects for younger and older children. Lastly, we have also tested that our results were robust to the age categorizations retained. E.g., if we consider 6 to 12 year-old children instead of 5 to 11 year-old children, are our new results sufficiently close to our former results? We have tested many combinations (lowest age; highest age) and our results are indeed robust to the age categorization we use.

8.5 Anthropometric Status

For each age category in months, for 0-5 year-old children in 1988 and 1993 (data were only available for those years), we construct height-for-age and weight-for-height Z-scores[15]. Those represent standard deviations from the median of the NCHS reference population, a group of well-nourished American children (Duflo 2003)[16]. For instance, a height-for-age Z-score more than 2 standard deviations below the reference median mean that the child is malnourished (Jensen 2000)[17]. Height-for-age captures long-run nutritional and health status, while weight-for-height is more a measure of the current status. Indeed, as shown by Ashworth (1969), Martorell and Habicht (1986) and recalled by Duflo (2003), weight-for-height reflects short-term undernourishment and illness episodes, and quickly resumes with food and health investments. To the contrary, height-for-age is a stock that mirrors its genetic endowments, its physical and socio-cultural environment and, above all, the past investments in child nutrition and health (Martorell and

[15] Such estimators are classical in the literature on anthropometry. They are easily calculable thanks to a Stata macro provided by the WHO Multicentre Growth Reference Study Group (2006). Details of such calculations are available on the Internet WHO website: http://www.who.int/childgrowth/software/en/

[16] This normalization does not alter our analysis, and allow domestic as well as international comparisons.

[17] Malnutrition, which is equivalent to undernourishment, consists of protein-energy malnutrition (PEM) and micronutrient malnutrition (MM). Whereas PEM is characterized by a lack of energy and proteins in the body, MM denotes the absence of vital nutrients such as vitamins (vitamin A, vitamin C) and trace elements (iron, iodine, etc.).

Habicht 1986, Duflo 2003). Especially, it has been demonstrated that such factors were extraordinarily decisive before the age of 5 (Barker 1990, Scrimshaw 1997). Thus, episodes of undernourishment and illnesses before the age of 5 could have everlasting consequences for children affected by such phenomena. For instance, early iodine deficiency is a cause of mental retardation, while recurrent hunger may definitively weaken the immune system and provoke the so-called deficiency diseases

We can now examine how anthropometric outcomes for children aged 0 to 5 depend on household income. First, for each Z-score from -6 to 6, I construct a difference-in-difference density that indicates the reduced-form effect of the instrument (belonging to a cocoa-producing household in 1993) on Z-scores. Clearly, for a Z-score equal to x and $f(x)_g$ various density functions (with g = {cocoa in 1993, cocoa in 1988, non-cocoa in 1993, non-cocoa in 1988}), we have[18]:

$$DiD - density(x) = [f(x)_{cocoa,93} - f(x)_{cocoa,88}] - [f(x)_{non-cocoa,93} - f(x)_{non-cocoa,88}]$$

This is not a pure DiD strategy as none controlling variable is included. But it may be informative about the impact of changing cocoa prices. Figure 12 indicates that results are as expected. Children of cocoa producers have seen their height-for-age Z-score deteriorate between 1988 and 1993 relatively to children of non-cocoa farmers. Indeed, the DiD-density is negative for positive Z-scores and positive for negative Z-scores. Concerning weight-for-height (results not reported here), such a detrimental effect is much less visible, confirming that the latter indicator is more related to very present factors.

We now turn to the same econometric models as for school attendance. We test two outcomes: the Height-for-Age Z-score and the likelihood that this Z-score be inferior or equal to -2 (meaning that the child has experienced growth failure).

[18]This is a generalization of what Jensen 2000 does in his article on agricultural volatility and investments in child human capital. Indeed, he only performs difference kernel distributions ($\text{group}_{A,93}$ - $\text{group}_{A,88}$), while I perform difference-in-difference kernel distributions ($\text{group}_{A,93}$ - $\text{group}_{A,88}$) - ($\text{group}_{B,93}$ - $\text{group}_{B,88}$).

Table 11 presents coefficients, p-values, R^2 and weak-instrument F statistics[19]. Since instruments were too weak for all regressions on the global spatial sample, we only report the results for the more restricted spatial sample. Coefficients are less precisely estimated than for school attendance, but this is surely due to the much lower number of observations. Indeed, with few observations, instrumentation and numerous village-time fixed effects (195), standard errors are likely to be high. That is why we interpret our coefficients as if they were significant, even if they do not automatically appear as such.

Clearly, blind OLS without controlling for village-time effects show no effect of income on nutritional status variables (whether we directly use the Z-score as an outcome, or the likelihood that the latter is inferior to -2). Then, including village-time fixed effects reveals a slight positive effect of income on the latter status, indicating a logical upward bias (richer children live in healthier biogeographic and social environments, thus making them healthier). However, once we instrument income, the effect appears very strong. Considering again village-time fixed effects, the effect is less strong, confirming the need to control for the environment of children. The model is more precisely estimated and instruments are stronger when we consider 3-5 year-old children instead of 0-5 year-old children. That is why we focus our interpretation on the former age category. It may be so because that the inequality in nutritional status due to inequality in permanent income is more likely to appear with time (when the child is old enough). In particular, F statistics in columns 5 and 6 (our within DiD-IV estimators) for 3-5 year-old children make us completely reject the weak-instrument hypothesis. Again, the inclusion of controls in column 6 does not alter much the results. Specifically, a 10% rise in household income induces a 0.17-0.18 rise in the Height-for-Age Z-score of 3-5 year-old children[20] and a 0.06-0.061 decrease in the likelihood to have

[19]Since nutritional status is strongly determined by the socio-ethnical group to which one belongs (through genetics and alimentary habits), we have included dummies for various ethnical groups.

[20]In other words, if income doubles (+ 100%), the Height-for-Age Z-score will decrease by 1.7-

experienced a growth failure.

8.5 Child Health Status

We now investigate the causal effect of household income on the child likelihood to have been sick in the last month. Since instruments were weaker for regressions on the global temporal sample 1988-1993-1998, we only reproduce results for the sample 1988-1993. Table 12 presents coefficients, p-values, R^2 and weak-instrument F statistics. Results are reported for the global and the restricted spatial samples. Blind OLS estimates suggest that household income has a positive impact on child health condition, a surprising feature that we have already mentionned above (see table 2). Then, including village-time fixed effects, these positive effects are almost not diminished. Once we instrument household income, effects become significantly negative. Again, the inclusion of additional controls in column 6 is almost inconsequential. Our best estimates are provided in column 5 and 6 and correspond to within DiD-IV. Especially, it should be noted that the null hypothesis of weak instruments is always rejected. Then, results for the global and restricted spatial samples are quite similar. In particular, a 10% rise in household income leads to a 0.018-0.019 point decrease in the likelihood to be sick, confirming a preliminary upward bias due to overreporting by the richer households.

9 Interpreting the Results and Public Policy Implications

Whichever outcome we retain, it is obvious that household income impacts investment decisions in child education and health. Besides, it should be noticed that OLS underestimate the causal effects of household income, but not as much

1.8 points. If a child has a Z-score around -2 (which is the upper limit for having experienced a growth failure), a doubling of household income may almost make her become "normal" (Z-score around 0).

as what we would expect given previous estimates in the literature. However, we are really confident in our estimates. First, we have ex ante provided significant evidence of the validity of our DiD-Iv strategy. Second, by combining IV with village-time fixed effects, we absorb dynamic supply effects, which few studies have done before. Third, weak-instrument tests entirely validate our analysis.

A very interesting feature of the education economics literature is that studies on developed countries tend to find that OLS upward bias the causal effect of income, while studies on developing countries tend to find the contrary (OLS conduce to a downward bias). This fact is unfortunately never explained in the literature. Yet, there are obvious reasons why such discrepancy exists (see table 3). First, the simultaneity bias is much more pregnant in developing countries since many children do work. Second, richer parents tend to not school their children since they prefer to learn them directly what they perceive as mandatory for being able to run the household farm and/or business. It should not be omitted that our estimated effects are conditional on our population of study, a sample of rural households in a Sub-Saharan country. Besides, richer parents could have a low opinion of community schools, and may not want their offspring to be in contact with poorer children. Then, we are certainly not contrasting comparable objects of study, but, interestingly, IV estimates based on education and health outcomes appear quite similar.

Furthermore, our analysis raises the issue of parental altruism. Theoretically, parents may invest in children for egoistic and altruistic reasons. In models that posit parental egoism, parents must arbitrate between more consumption today and more consumption tomorrow. Indeed, present child labor permits parents to increase their current consumption of goods. At the same time, more educated children will get higher earnings later, which they may share with retired parents: parents then invest in children, thus anticipating the potential income that their offspring may get and the share of it they will be able to claim (Nugent 1985, Cox 1987, Lillard and Willis 1997). In models that hypothesize parental altruism (Becker and Tomes 1986, Basu and Van 1998, Baland and Robinson 2000), parents intrinsically prefer that their children get educated. In other words, child education directly enters the parental utility function. Actually, investments in child education motivated by income pooling could be labeled as unintentional altruism[21]. Then, altruism would be broadly defined as an action of investing more in child education than in other goods (especially, unproductive consumption such as tobacco and alcohol) when income increases (Bhalotra 2004). We do not have tested a specific model with education or health expenditures, but our estimates rather suggest that parents indeed invest in their child human capital when they are monetarily allowed to do so.

Lastly, in terms of public policy, it shows that supply-side policies may prove to be limited to expand education and health, particularly amongst poor districts. Indeed, once we control for village characteristics and once we correct for income endogeneity, household income remains a determining factor. However, designing and implementing demand-side programs to raise education and health investments is neither a cheap nor an easy task. Surely, more research is needed, in particular in applied health microeconometrics.

10 Conclusion

In this paper, we develop an instrumentation strategy to assess the causal effect of household income on child education and health. Our estimates show that household income has a more or less strong effect on the three outcomes we test. In particular, in our specific sample of rural households in Ivory Coast, we show that a 10% increase in household income results in a: (a) a 0.023-0.030 point in the likelihood to attend school for 5-17 year-old children, (b) a 0.06 points in the likelihood that a 3-5 year-old child is malnourished, and (c) a 0.018-0.02 point reduction

[21] Similarly, we could label intentional altruism as unintentional egoism. Parents send their children to school because they want to augment their own utility by raising the intertemporal utility level of their offspring.

in the probability that a 0-18 year-old child is sick. First, OLS downward biases the effect of household income and this is explainable. Second, supply-side public policies are insufficient to raise school and health investments in developing countries. Third, our analysis illustrates the impact of income volatility on child school and health, in a global context of credit and insurance markets incompleteness. This is especially true in African countries whose economies remain little diversified and are then vulnerable to changing international prices for their exports. In Ivory Coast, a considerable part of the population still works for the cocoa sector, and directly undergo the fluctuations of international cocoa prices. By the past, the state-owned stabilization fund, the Caistab, proved to be a cash cow for the state and corrupted officials, and did not really served its original mission. It was thus dismantled in 1998. Obviously, new insurance schemes should be invented to protect cocoa producing households from unexpected shifts in cocoa prices. Then, parents would be more able to continuously invest in their offspring.

Table 1: Investments in Education and Health for Five West African Countries (UN 2007, WHO 2007).

	Burkina-Faso	Ivory Coast	Ghana	Guinea	Mali
Net primary education enrolment ratio, 1990 (%)	26.2	45.6	52.4	25.5	20.4
Completion rate of primary schooling, 1991 (%)	21.3	43.4	62.8	16.8	10.8
Completion rate of primary schooling, for girls only, 1991 (%)	16.1	32.2	54.9	9.1	8.5
Percentage of pupils starting grade 1 and reaching grade 5, 1991	69.7	72.5	80.5	58.6	69.7
% of children under 5 who are stunted for age	43.1 (2003)	31.5 (1999)	35.6 (2003)	39.3 (2005)	42.7 (2001)
% of children under 5 who are underweight for age	35.2 (2003)	18.2 (1999)	18.8 (2003)	22.5 (2005)	30.1 (2001)
% of newborns with low birth weight, 2002	19	17	11	12	23
Under-5 mortality rate (per 1000 live births), 1990	210	157	122	240	250
Childre under 5 years of age with diarrhoea who received oral rehydratation therapy (%)	62.8 (2004)	66.1 (2000)	63.3 (2004)	56.7 (2005)	65.7 (2002)
Childre under 5 years of age with acute respiratory infection and fever taken to facility (%)	32.6 (2004)	34.9 (2000)	44 (2004)	34.5 (2005)	42.8 (2002)
General govt. expenditure on health: % of total exp. on health, 2000	43.5	20	37.4	14	32.9
Private expenditure on health: % of total exp. on health, 2000	56.5	80	62.6	86	67.1
General govt. expenditure on health: % of general govt. total exp., 2000	9	5.2	9.4	4	9.5
Physicians (density per 1000 population), 2004	0.05	0.12	0.15	0.11	0.08

Table 2: Schooling and Health Outcomes following Household Chef Wealth and Education (%).

Wealth Quintile	1	2	3	4	5
Never Schooled, 12-17 yo.	56.33	47.56	45.51	37.46	28.17
Malnourished, 0-5 yo.	45.74	45.21	39.84	33.9	24.09
Sick, 0-18 yo.	10.64	12.47	12.98	14.17	15.99
Consulted if Sick, 0-18 yo.	25.05	29.73	32.19	37.71	48.59
HH Chef Education	None	Full Prim.	$\geq$ 1 Year of Sec.	BEPC	$\geq$ BAC
Never Schooled, 12-17 yo.	50.1	22.26	16.22	18.58	8.95

Table 3: The Various Theoretical Biases.

EDUCATION	Upward	Downward	**HEALTH**	Upward	Downward
Simultaneity:		Y	**Simultaneity** :		Y
Omission:			**Omission** :		
Familial school-related abilities	Y		Genetically inherited health endowments		Y
Familial educational experience	Y		Dynastic preferences		Y
Familial educational preferences			Parental health-related abilities		Y
Assets and collateral	Y		Parental education	Y	Y
Social assets and education return	Y		Collateral assets	Y	
Entrepreneurial assets and education	Y	Y	Entrepreneurial assets	Y	
Endogenous locational choices	Y		Health infrastructure	Y	Y
Education is a superior good	Y		Physical and social environment		Y
Less risk aversion	Y		Health is a superior good	Y	
Ex post insurance	Y		Risk aversion	Y	Y
Over-controlling:		Y	Undernourishment		Y
			Insalubrious accommodation		Y
			Child labor	Y	
			Extralegal sector		Y
			Over-controlling:		Y
Measurement error:		Y	**Measurement error**:		Y

Table 4: Mean Characteristics for Cocoa and Non-Cocoa Children Aged from 5 to 17, in 1988.

	Non-Cocoa	Cocoa	T-test
Age	10.22	10.3	0.35
Sex	0.53	0.55	0.15
HH Chef is born out of Ivory Coast	0.07	0.13	0
HH Chef has migrated in the last 3 years.	0.14	0.14	0.38
HH Chef has migrated last year	0.08	0.08	0.79
Age of HH chef	50.43	51.23	0.01
HH Chef is a woman	0.081	0.022	0
Share of women in the HH	0.51	0.49	0
Extended family / HH Size	3.07	3.08	0.95
HH Chef is literary	0.16	0.22	0
HH Chef has never been to school	0.81	0.72	0
HH Chef has achieved primary schooling	0.12	0.15	0
HH Chef has achieved at least one year of secondary schooling	0.06	0.05	0.05
Chef has obtained the Bepc	0.04	0.02	0
HH Chef has obtained the Bac or plus	0.04	0.02	0
Size of the HH	10.18	11.37	0
HH owns its accomodation	0.88	0.93	0
Log Total HH Consumption (annual)	13.73	14.02	0
Log Per Capita HH Consumption (annual)	11.53	11.74	0
Av. distance to school if $\leq$ 100 Kms	2.72	4.61	0
Av. distance to school if $\leq$ 50 Kms	2.03	2.01	0.862
Av. distance to school if $\leq$ 20 Kms	1.48	0.92	0
Av. distance to school if $\leq$ 5 Kms	0.77	0.42	0

Table 5: Mean Education-Related Characteristics for Cocoa and Non-Cocoa Children, in 1988.

	Non-Cocoa	Cocoa	T-Test
Schooled, 5-17 yo.	0.28	0.41	0
Schooled, 5-11 yo.	0.28	0.43	0
Schooled, 12-17 yo.	0.28	0.38	0
Active, 7-17 yo.	0.47	0.31	0
Active, 7-11 yo.	0.33	0.14	0
Active, 12-17 yo.	0.63	0.5	0
Abandon, 7-17 yo.	0.25	0.28	0.02
Abandon, 7-11 yo.	0.34	0.36	0.26
Abandon, 12-17 yo.	0.15	0.19	0.01

Table 6: Mean Health-Related Characteristics for Cocoa and Non-Cocoa Children 0-18 yo., in 1988.

	Non-Cocoa	Cocoa	T-test
Sick, 0-18 yo.	0.2	0.16	0
Consult if Sick, 0-18 yo.	0.31	0.46	0
Medicine was Bought if Sick	0.47	0.56	0
Number of Inactive Days due to Illness	4.76	6.1	0

Table 7: Professional Distribution for Households with Children Aged from 5 to 17 (%).

Global	Cocoa	Coffee	Other Farmers	Workers	Total
88	28.13	11.1	25.24	35.53	100
93	28.65	5.18	29.62	36.56	100
98	22.73	5.17	31.1	41	100
Forests	Cocoa	Coffee	Other Farmers	Workers	Total
88	63.09	16.69	14.52	5.7	100
93	63.09	7.82	24.32	4.78	100
98	55.45	8	26.05	10.49	100

Table 8: Relative Changes in Observables for Cocoa and Non-Cocoa Households, 1988-1993-1998.

All	Cocoa	p	Cocoa93	p	Cocoa98	p
Chef is literary	-0.017	(0.277)	-0.028	(0.162)	-0.015	(0.575)
Chef has never been to school	0.017	(0.293)	0.031	(0.147)	0.018	(0.511)
Chef has achieved prim. school.	-0.014	(0.322)	-0.043	(0.023)	-0.013	(0.585)
Chef has obtained the Bepc	-0.047	(0)	0.026	(0.013)	0.046	(0.002)
Chef has achieved at least one year of secondary schooling	-0.029	(0.002)	0.022	(0.081)	-0.017	(0.375)
Chef has obtained the Bac or plus	-0.041	(0)	0.015	(0.14)	0.051	(0)
Chef has migrated in the last 3 years	-0.026	(0.079)	0.009	(0.573)	-0.042	(0.027)
Chef has migrated last year	0.006	(0.606)	-0.012	(0.300)	-0.051	(0.001)
Chef is born out of Ivory Coast	0.001	(0.905)	-0.034	(0.022)	-0.035	(0.023)
HH owns its house	0.104	(0)	0.079	(0)	0.023	(0.323)
HH owns betail	0.051	(0.004)	0.056	(0.011)	0.018	(0.477)
Forest	Cocoa	p	Cocoa93	p	Cocoa98	p
Chef is literary	-0.009	(0.632)	-0.044	(0.070)	-0.031	(0.338)
Chef has never been to school	0.007	(0.718)	0.044	(0.082)	0.031	(0.345)
Chef has achieved prim. school.	-0.016	(0.320)	-0.047	(0.034)	-0.011	(0.696)
Chef has obtained the Bepc	-0.053	(0)	0.029	(0.023)	0.01	(0.515)
Chef has achieved at least one year of secondary schooling	-0.014	(0.213)	-0.007	(0.622)	-0.078	(0.001)
Chef has obtained the Bac or plus	-0.051	(0)	0.019	(0.128)	0.026	(0.044)
Chef has migrated in the last 3 years	-0.015	(0.37)	-0.024	(0.184)	-0.058	(0.009)
Chef has migrated last year	0.019	(0.138)	-0.025	(0.054)	-0.063	(0)
Chef is born out of Ivory Coast	-0.011	(0.300)	0.009	(0.617)	-0.006	(0.718)
HH owns its house	0.081	(0)	0.095	(0)	0.091	(0)
HH owns betail	0.031	(0.126)	0.067	(0.012)	0.043	(0.168)

Regressions: OLS, within (including time-village fixed effects), robust to heteroscedasticity. Obs. All: 24246. Obs. Forest: 11751.

Table 9: Geographical Distribution for Households with Children Aged from 5 to 17 (%).

	Non-Forest	Forest	Total
88	45.08	54.92	100
93	44.23	55.77	100
98	46.62	53.38	100

Table 10: Results for School Attendance, 1988-1993.

5 - 17 yo, *All*	(1)	(2)	(3)	(4)	(5)	(6)
coeff.	0.117	0.108	0.082	0.75	0.291	0.228
p-value	(0)	(0)	(0)	(0)	0.07	(0.101)
R^2 main eq.	0.167	0.170	0.319	0.053	0.074	0.114
IV F-stat				27.35	25.89	33.32
(Threshold)				(16.38)	(16.38)	(16.38)
5 - 11 yo, *All*	(1)	(2)	(3)	(4)	(5)	(6)
coeff.	0.114	0.107	0.082	0.95	0.304	0.236
p-value	(0)	(0)	(0)	(0)	(0.122)	(0.163)
R^2 main eq.	0.201	0.203	0.347	0.09	0.105	0.151
IV F-stat				14.50	17.3	22.48
(Threshold)				(16.38)	(16.38)	(16.38)
5 - 17 yo, *Forest*	(1)	(2)	(3)	(4)	(5)	(6)
coeff.	0.114	0.113	0.096	0.711	0.299	0.241
p-value	(0)	(0)	(0)	(0.033)	(0.023)	(0.052)
R^2 main eq.	0.154	0.154	0.316	0.122	0.091	0.128
IV F-stat				8.43	37.42	39.53
(Threshold)				(16.38)	(16.38)	(16.38)
5 - 11 yo, *Forest*	(1)	(2)	(3)	(4)	(5)	(6)
coeff.	0.111	0.111	0.089	0.674	0.331	0.266
p-value	(0)	(0)	(0)	(0.141)	(0.032)	(0.069)
R^2 main eq.	0.2	0.2	0.356	0	0.114	0.163
IV F-stat				4.20	27.94	29.30
(Threshold)				(16.38)	(16.38)	(16.38)

Columns: (1) OLS, (2) OLS + regional dummies, (3) within OLS (including village-time fixed effects), (4) IV-GMM, (5) within IV-GMM (including village-time fixed effects) without additional controls except age, age squared and sex, (6) within IV-GMM with additional controls: dummies equal 1 if the household chef is a woman, literate, has achieved primary schooling, has spent at least one year in secondary schooling, has obtained the BEPC (at the end of secondary schooling), has obtained the BAC or more (at the end or after high school), dummies equal 1 if the household owns livestock, a business, has a household member being a civil servant, the ratio of extended family over household size and the share of women in the household. Obs. 5-17, All: 18992. Obs. 5-11, All: 11782. Obs. 5-17, Forest: 9111. Obs. 5-17, Forest: 5802.

Table 11: Results for Height-for-Age Z-score (haz) and the Likelihood to Have Experienced Growth Failure (p(haz ≤ -2)), 1988-1993.

***haz*, 0-5 yo**	(1)	(2)	(3)	(4)	(5)	(6)
coeff.	0.02	0.021	0.104	4.097	1.261	1.01
p-value	(0.762)	(0.747)	(0.162)	(0.071)	(0.237)	(0.239)
R^2 main eq.	0.095	0.101	0.159	.	.	.
IV F-stat				4.93	11.63	17.27
(Threshold)				(16.38)	(16.38)	(16.38)
p(haz≤-2), 0-5 yo	(1)	(2)	(3)	(4)	(5)	(6)
coeff.	-0.029	-0.029	-0.051	-0.787	-0.283	-0.272
p-value	(0.106)	(0.102)	(0.012)	(0.104)	(0.307)	(0.240)
R^2 main eq.	0.094	0.0987	0.1345	.	.	.
IV F-stat				4.93	11.63	17.27
(Threshold)				(16.38)	(16.38)	(16.38)
***haz*, 3-5 yo**	(1)	(2)	(3)	(4)	(5)	(6)
coeff.	0.154	0.156	0.138	5.187	1.8	1.71
p-value	(0.086)	(0.081)	(0.160)	(0.172)	(0.034)	(0.028)
R^2 main eq.	0.121	0.124	0.168	.	.	.
IV F-stat				2.12	16.373	18.12
(Threshold)				(16.38)	(16.38)	(16.38)
p(haz≤-2), 3-5 yo	(1)	(2)	(3)	(4)	(5)	(6)
coeff.	-0.035	-0.035	-0.028	-1.38	-0.596	-0.611
p-value	(0.159)	(0.153)	(0.316)	(0.182)	(0.018)	(0.012)
R^2 main eq.	0.11	0.111	0.134	.	.	.
IV F-stat				2.12	16.37	18.12
(Threshold)				(16.38)	(16.38)	(16.38)

Columns: (1) OLS, (2) OLS + regional dummies, (3) within OLS (including village-time fixed effects), (4) IV-GMM (2SLS for models with parental data), (5) within IV-GMM (including village-time fixed effects) without additional controls except age, age squared and sex (6) within IV-GMM with additional controls: dummies equal 1 if the household chef is literate, has achieved primary schooling, has spent at least one year in secondary schooling, has obtained the BEPC (at the end of secondary schooling), has obtained the BAC or more (at the end or after high school), dummies equal 1 if the household owns livestock, a business, has a household member being a civil servant, or belongs to a certain ethnical group (Akan, Krou, Mande, Voltaïque, Other, Foreigner), and the share of women in the household. Obs. 0-5, Forest: 2824. Obs. 3-5, Forest: 1434.

Table 12: Results for Health Status (Sick = 1/0), 1988-1993.

All	(1)	(2)	(3)	(4)	(5)	(6)
coeff.	0.019	0.016	0.011	-0.376	-0.195	-0.185
p-value	(0)	(0)	(0.018)	(0.003)	(0.195)	(0.174)
R^2 main eq.	0.041	0.049	0.100	0.162	.	.
IV F-stat				35.72	25.89	31.92
(Threshold)				(16.38)	(16.38)	(16.38)
Forest	(1)	(2)	(3)	(4)	(5)	(6)
coeff.	0.02	0.014	0.011	0.232	-0.183	-0.183
p-value	(0.001)	(0.017)	(0.074)	(0.300)	(0.119)	(0.097)
R^2 main eq.	0.046	0.057	0.110	0.056	.	.
IV F-stat				9.75	45.15	51.01
(Threshold)				(16.38)	(16.38)	(16.38)

Columns: (1) OLS, (2) OLS + regional dummies, (3) within OLS (including village-time fixed effects), (4) IV-GMM, (5) within IV-GMM (including village-time fixed effects) without additional controls except age, adge squared and sex, (6) within IV-GMM with additional controls: dummies equal 1 if the household chef is literate, has achieved primary schooling, has spent at least one year in secondary schooling, has obtained the BEPC (at the end of secondary schooling), has obtained the BAC or more (at the end or after high school), dummies equal 1 if the household owns livestock, a business, has a household member being a civil servant, and the share of women in the household. Obs. All: 28002. Obs. Forest: 13681.

Figure 1: National Producer Prices and GDP, PPP and Constant Prices, base = 1998 (Data: Berthélemy and Bourguignon 1996, World Bank 2001, IMF 2007).

Figure 2: Average Per Capita Consumption (Annual) for Cocoa and Non-Cocoa Households.

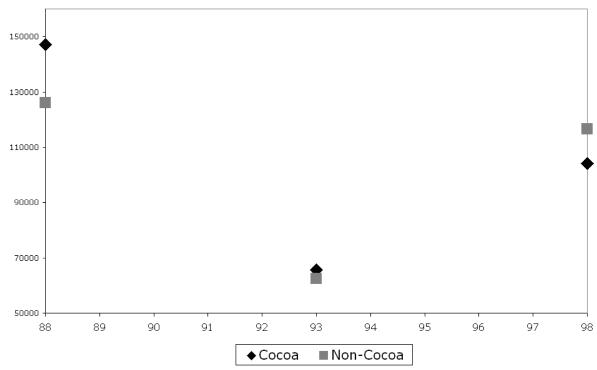


Figure 3: Average School Attendance for Children 5-17 yo. for Cocoa and Non-Cocoa Households.

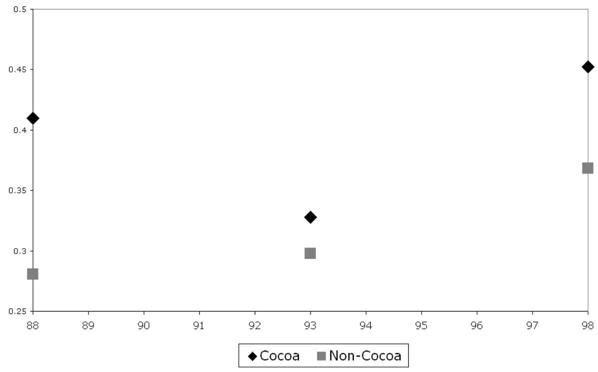


Figure 4: Average Health Status (Sick = 1/0) for Children 0-17 yo. for Cocoa and Non-Cocoa Households.

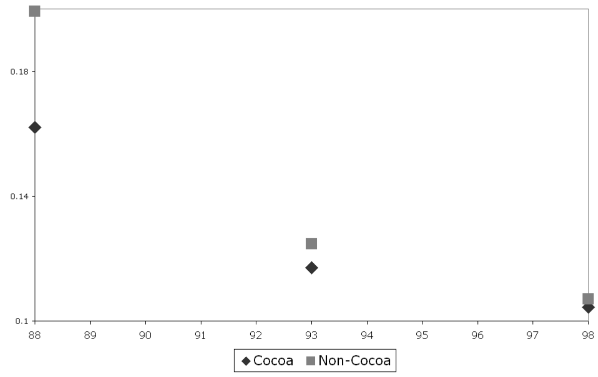


Figure 5: Average Proportion of Children 0-5 yo. with a Height-for-Age <= -2 for Cocoa and Non-Cocoa Households.

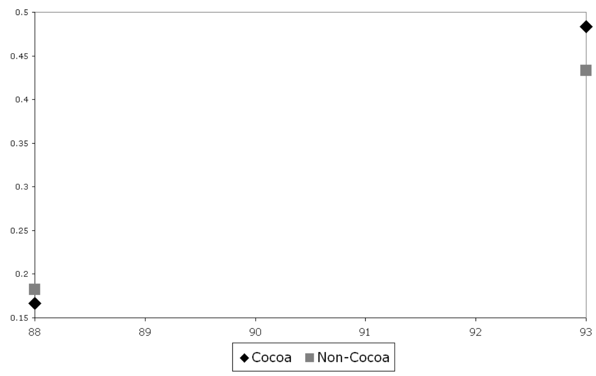


Figure 6: Evolution of the Individual School Attendance Residual, 5-17 Year-Old Children, 1988-1993-1998.

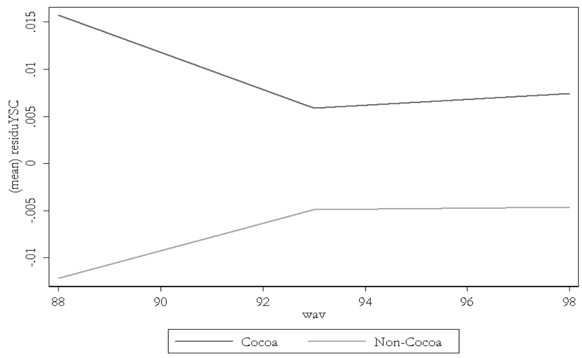


Figure 7: Evolution of the Individual Log(Per Capita Consumption) Residual, 1988-1993-1998.

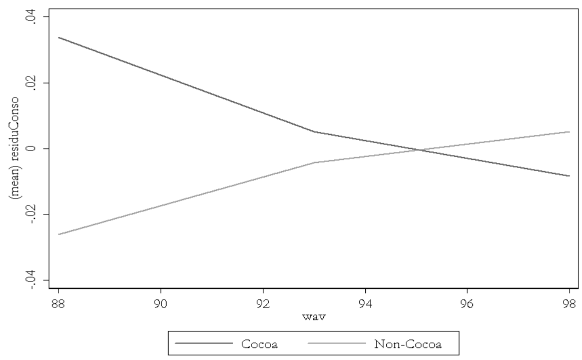


Figure 8: Evolution of Average Log Per Capita Consumption (Annual) for Cocoa and Non-Cocoa Households, 1985-1988-1993-1998.

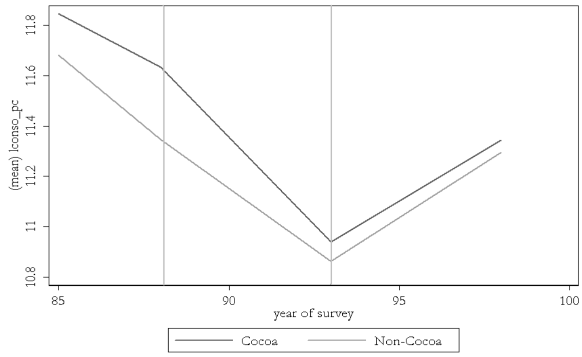


Figure 9: Evolution of Average School Attendance for Children 5-17 yo. for Cocoa and Non-Cocoa Households, 1985-1988-1993-1998.

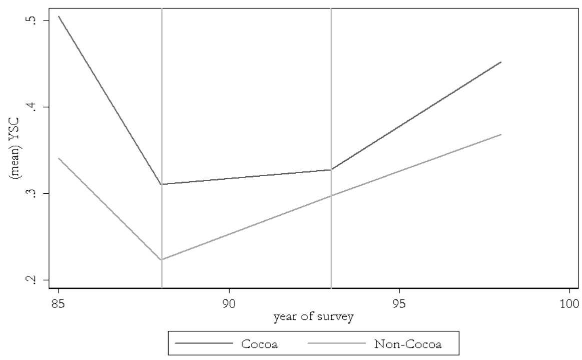


Figure 10: Kernel Distribution of Height-For-Age Z-Score for Children 0-5 yo. in Cocoa and Non-Cocoa Households, 1988.

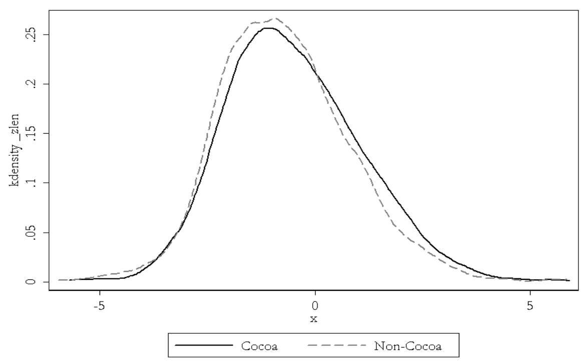


Figure 11: Kernel Distribution of Cocoa Households across Villages.

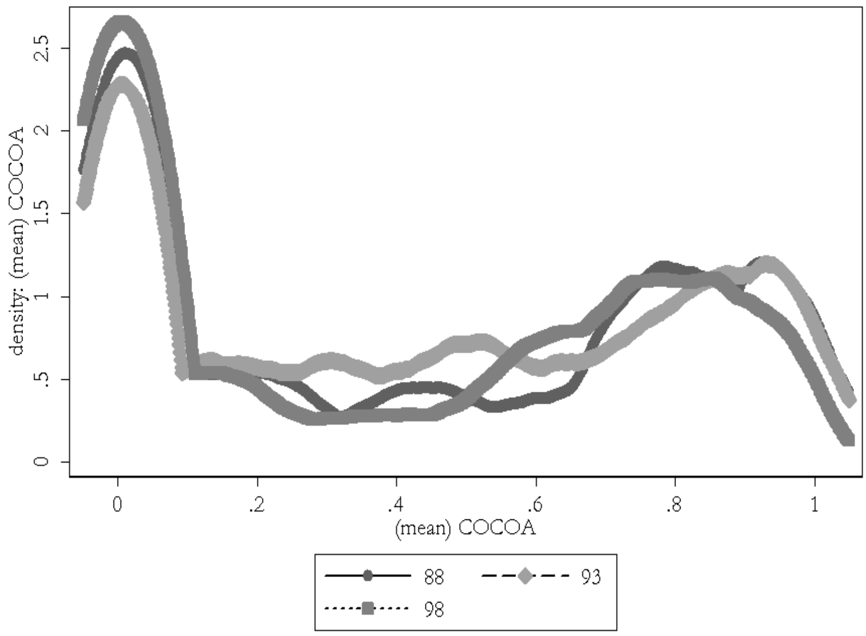
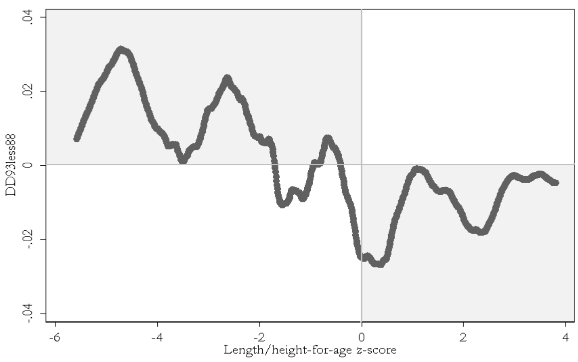


Figure 12: Height-For-Age Z-Score, DiD Kernel Densities (Cocoa - Non-Cocoa, 93 vs. 88).



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