Family Income and Child Outcomes: The Impact of Cocoa Price Shocks in Cote d'Ivoire

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Abstract

We study the drastic cut of the administered cocoa producer price in 1990 Cote d'Ivoire and investigate the extent to which cocoa producers' children suffered from this severe income shock in terms of school enrollment, increased labor, height stature and sickness. Comparing pre-crisis (1985-88) data and post-crisis (1993) data, we propose a difference-in-difference within-village strategy in order to identify the causal effect of family income on children outcomes. We find a strong impact of family income variation for at least two out of the four variables we examine.

JEL classification codes: I21, I12, J13, J22, 012, O15, Q12

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1 Introduction

In many low-income countries and in particular in Africa, performances with regard to child education and health are still very much disappointing (see Appendix 1). While the disease-prone environment and the low availability and quality of infrastructures bear a large responsibility in this situation, on the demand side low parental resources also constitute a direct limiting factor. A large body of econometric works has already addressed the issue of estimating the impact of parental income on child outcomes in developing countries. This literature has long recognized that the statistical correlations between these two latter variables are merely an indirect and potentially biased reflection of the causal impact of income (Behrman and Knowles, 1999). One reason is the contamination of income indicators by relatively large measurement errors or idiosyncratic transient components. Another reason is the possible endogeneity of parental income: Some unobservable preferences and resources may simultaneously determine parental income, child work, child schooling, and child care.

Randomized experiments are a first answer to this endogeneity problem. The evaluations of the famous Mexican conditional cash transfer program Progresa have revealed a strong and causal sensitivity of school enrollment to the transfers delivered to families that send their children to school (e.g., Schultz, 1994; De Janvry, et al. 2004). However, the impacts of unconditional income variations and of negative income shocks, the impacts on other outcomes than schooling such as health, and the influence of the socioeconomic context (e.g. Africa vis-a-vis Latin America) are still not well known. In the absence of randomized experiments, a bunch of recent works exploits the income variability generated by macroeconomic crises (Thomas and al. 2004), commodity price changes (Edmonds and Pavcnik 2005; Kruger, 2007), shocks on production (Jensen, 2000; Beegle, Dehejia and R. Gatti, 2006) or targeted policy reforms (like that of the South-African pension-system: Duflo 2000 and 2003; Case, 2001; Edmonds 2006) in a variety of contexts.

Most of these works suggest that income has direct and large effects on child outcomes, and are suggestive of the strong liquidity constraints that weight on poor households (with the exception of Kruger, op.cit., in the case of child labor, and Dumas and Lambert, 2005, in the case enrollment).

Our work pertains to this family. We study the drastic cut of the administered cocoa producer price in 1990 Cote d'Ivoire and look at the extent to which cocoa producers' children suffered from this severe income shock in terms of school enrollment, increased labor, height stature and sickness. Cote d'Ivoire is the world leading exporter of cocoa beans. In the period 1985-1994, cocoa beans exports amounted to more than one third of Ivorian total exports; as such, the Cote d'Ivoire economy was and still is highly dependent on cocoa international prices. As those latter were plummeting over the 1980s, the parastatal marketing board finally decided to halve the producer price in 1990, from 400 to 200 CFA francs per kilogram. We exploit two datasets from nationally representative large sample household surveys that were implemented before and after the cocoa crisis, in 1985-88 and 1992-93 respectively.

We implement two kinds of identification strategies of the impact of income shocks. Our preferred strategy is a double difference, whereby we compare the evolution of outcomes of children living in cocoa producing households with that of children living in other agricultural households. We even compare children living in the same villages, in order to absorb the potential variation in supply-side factors. Of course, given the weight of cocoa in Cote d'Ivoire, the comparison group (non-cocoa farmers' children) is also affected by the cocoa crisis, so that we do not measure the overall impact of the cocoa crisis but only use it in order to identify the causal impact of a negative private income shock. A second identification strategy exploits the weight of cocoa production in the district of birth of the children, in keeping with previous works that also rely on regional variation (Jensen, 2000; Kruger, 2007), although they do not correct for potentially endogenous migration; this second strategy offers results that are broadly consistent with the first.

We not only study the reduced-form impact of the cocoa income shock, but also take advantage of good data on consumption, and present instrumented estimates of the household income-elasticity of children outcomes. We find a strong reaction of school enrollment to the income shock, especially for children between 5 and 11 years old, and indifferently for boys and girls. For instance, at 7 years of age, a 10 percent decrease in income induces a 3 percentage points fall of the probability of school enrollment. The estimated income elasticity of child labor is negative but less significant. We also find a large income effect on the height stature of children between 3 and 5 years old: a 10 percent variation in income here leads to an average 0.4 to 0.9 cm change or to a 2.5 to 7.5 change in the probability of being stunted. The remainder of this paper is organized as follows. Section 2 proposes a very simple theoretical model of school enrollment that illustrates the main endogeneity bias that may affect the econometric estimation of the causal impact of household income on children outcomes. Section 3 presents the data and the construction of the main variables. Section 4 describes the socioeconomic context of the natural experiment and some suggestive descriptive statistics about the long-term consequence of the cocoa shock. Section 5 presents our two double-difference identification strategies. Section 6 discusses the the assumption that underlie the validity of our identification strategies and provide supportive evidence in their favor. Section 7 presents the results. Section 8 summarizes and concludes.

2 Theoretical Framework and Identification Issues

We here write the simplest microeconomic model of school enrollment decision, in order to raise the main identification questions that we have to solve out, like in Cogneau and Maurin (2001). A child care model (including nutrition and medical expenditures) could be devised the same way. Let us consider families (indexed by i) which have to decide whether they send their children to school ($S_i = 1$) or not ($S_i = 0$), depending on their ability to pay the costs of schooling (γ_i) and on the impact of the schooling decision on their utility. Parents determine the allocation of their permanent income (Y_i) between consumption (C_i) and schooling in order to maximize a utility function $U(C_i, S_i)$. The maximization is performed subject to a budget constraint $C_i + \gamma_i S_i = Y_i$. Assuming that U is concave and additively separable ($U(C_i, S_i) = C_i^\alpha + \beta_i S_i$) and that γ_i remains small with respect to Y_i, it is not difficult to check that:

$$S_i = 1 \Longleftrightarrow U\left(Y_i - \gamma_i, 1\right) > U\left(Y_i, 0\right) \Longleftrightarrow \ln Y_i > \frac{1}{1-\alpha} \ln\left(\frac{\alpha\gamma_i}{\beta_i}\right) \qquad (1)$$

Parents send their children to school if and only if their income is sufficiently high for the impact of schooling cost on family consumption to be small enough. One straightforward extension of this school enrollment model is to assume that the net cost γ_i/β_i depends on the characteristics of the child and that the parental decision is taken in two steps: in a first step, they evaluate the optimal timing of their children's schooling (i.e., the timing that minimizes γ_i/β_i) and, in a second step, they chose to send or not their children to school depending on whether condition (1) holds true or not. In particular, we will consider that the optimal timing is not necessarily the same for cocoa producers compared to other farmers. It should however be acknowledged that such a model is more adapted to explaining delayed entry, i.e. the probability of not being schooled on time (at 5 years old, at the first compulsory primary level called CP1) or at any age conditional on a given timing. It is less suited to explaining school dropouts, as the model should then be dynamic and include past school experience into the net cost of school enrollment. However, the data will not allow us to distinguish late entries and early drop-outs, unlike Bommier and Lambert (2002), as the age of entry into school and the school curriculum of children are not available. We will essentially analyze the probability of attending school in a given year and relate it to the household current income, but will consider the heterogeneity of the income treatment with respect to the age of children, as well as to his/her gender, relation to the household head and birth order. To specify our empirical models, we will assume that the net cost γ_i/β_i of schooling can be expressed as a linear function of (a) the children's exogenous characteristics X_i such as the child's gender and her/his actual age (b) head's education and other household characteristics H_i, and (c) location characteristics V_i.

$$S_i = I(aX_i + b_1Y_i + b_2H_i + b_3V_i + \varepsilon_i > 0) \tag{2}$$

where $I(x > 0)$ is a dummy that takes the value 1 whenever x is positive. From an econometric point of view, the main problem for estimating (2) is the potential endogeneity of income, parental education and some other household characteristics (Behrman and Knowles 1987; Behrman and Knowles 1999; Blau 1999). In this paper, we are only interested in the estimation of the causal effect of the former. The reasons for such endogeneity of income are the classical simultaneity, omission and measurement errors biases: some of these bias may either lead to the underestimation (*downward bias*) or the overestimation (*upward bias*) of the income effect. An example of simultaneity bias is the fact that 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 enrollment but the higher the total household income (*downward bias*). A then example of omission bias is the fact that income is correlated with parental abilities and parental preferences towards education, which could either positively influence child schooling (*upward bias*) or else negatively (*downward bias*) if skilled parents put their children to work early in order to transmit their *savoir-faire*. Another example is the fact that richer parents may locate in villages with a better school (V_i), thus implying a relatively lower net cost γ_i/β_i (*upward bias*). Lastly, our measure of income could be subject to measurement errors (*downward bias*). Therefore, the identification of our model requires the construction of instrumental variables which are correlated with household income but uncorrelated with unobserved family-specific factors and measurement errors. We shall consider all other characteristics as exogenous controls, or more precisely we shall argue that our instrumental variable strategy for estimating the causal impact of income is valid once we control for them (even if we do not estimate their true causal impact).

3 Data

Our main sources of data are the four Cote d'Ivoire Living Standards Surveys (CILSS) from 1985 to 1988, and the Enquête Prioritaire (EP) 1993, conducted by the Institut National de la Statistique of Cote d'Ivoire with the support of the World Bank. As we are only interested in the comparison of children between the pre-crisis and the post-crisis period, we stack all the household data for 1985-1988 and label them 1988; some panelized household being present twice in the 1985-1986 and 1986-1988 datasets, we only keep the most recent date of observation for them.

Regarding children outcomes, the surveys ask the same questions about school enrollment and child work during the previous year. Our definition of child work includes domestic work. As already noted, the surveys unfortunately do not provide details on the children school curriculum nor on age of entry into school.[1] With respect to health outcomes, the questions about sickness episodes during the preceding month are the same, and height and weight are measured for every child between 0 and 5 years. We can then construct height-for-age Z-scores following the procedure recommended by the World Health Organization.[2]

In each of the two datasets, we are able to define in an homogeneous way

[1]Moreover, the question "Have you ever been at school?" that is asked in 1993 is formulated in a much wider manner in 1988 as "Have you ever followed any kind of training?" and thus includes apprenticeship and koranic schools. Likewise, level attained can not be used as for informal curricula formal equivalent levels have been coded. The questions on literacy are not comparable either as the ability of reading and writing is asked without any precision in 1993, whereas it is characterized as the capacity to read a *newspaper* and write a *letter* in 1988.

[2]See 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/

the group of cocoa producing households, whether they are landowners who grow their own cocao trees or sharecroppers. As the district (*"département"*) of birth is available in each survey, we are also able to know whether a child was born in a cocoa-producing district or not.

Our preferred income variable is consumption per capita at 1988 prices; consumption is much better measured than income in that kind of surveys (see e.g. Deaton, 1997). Our consumption concept includes consumption of own food production, and all cash expenditures including an imputed housing rent, but excluding very infrequent durable goods acquisition and health expenditures. We are thus aware that we miss some risk-coping mechanisms. Indeed, if a cocoa producer is confronted with a sudden and exogenous cut of the cocoa administered price, its cocoa income will fall as a result, but its total income will not thanks to ex-post strategies that mitigate this ex-ante loss: increase in household labor supply, dissaving and sale of assets, borrowing, etc. Income available for consumption more or less corresponds to the ex-post income obtained once these coping strategies have been tried. If for instance cocoa producers are more (resp. less) able to cope with ex-ante income shocks in order to preserve the investments they make in their children, our identification strategy will underestimate (resp. overestimate) the true impact of ex-ante income. As household size is larger in cocoa households, they could be a little more able to increase labor supply. We also use the cash expenditures variable excluding consumption of own food production, knowing that the cocoa income loss directly affects cash income. The results obtained with this latter are largely consistent with those obtained with total consumption.

4 The Cocoa Shock

So as to solve income endogeneity, we use the natural experiment provided by the exogenous changes in cocoa producer prices in Cote d'Ivoire over the period 1985-1993. From independence till the mid-1970's, Cote d'Ivoire has experienced dramatic growth thanks to the development of cocoa exports in a context of rising primary commodity prices. Migration from Northern regions and neighboring countries (Burkina-Faso and Mali) was encouraged in order to provide the necessary workforce to this expanding sector. The expansion of production also relied on the extensive exploitation of new forest areas in the South-Western part of the country. The producer price was administered by the state-owned marketing board (the "Caisstab"), which fixed it much below the international price: Over the period 1974-1980, the producer price only represented 45% of the export price (Cogneau et Mesplé-Somps, 2002). The benefits of the Caisstab constituted extra-budgetary resources which were extensively used to finance the fiscal deficit, aside to the taxes also levied on cocoa exports. This allowed the Ivorian government to pay high wages to its highly skilled civil servants and to fund a wide expansion of the education sector. Starting from a very low colonial level, Cote d'Ivoire managed to catch up with the neighboring Ghana where the British colonial ruler had much more developed education. From 1979, the decline in international cocoa prices and the subsequent increasing deficits of the Caisstab designated the end of the "Ivorian Miracle". Many public investments that had been financed through international debt revealed at the same time not very efficient. Cote d'Ivoire progressively entered in a period of financial crisis and ajustement that would last almost twenty years (Berthélemy and Bourguignon, 1996; Cogneau and Mesplé-Somps, 2003). After a short-lived rebound in 1985-1986, and despite a governmental attempt to influence them by rationing cocoa exports in 1987, international cocoa prices kept falling. In June 1989, the cocoa producer price was abruptly cut for the first time in 25 years, first from 400 to 250 CFA francs per kilogram; then in 1990, it was purely halved to 200 CFA francs. In 1994, a new rebound of international prices, combined with the CFA franc devaluation, authorized to increase at new the producer price. In 1998, the Caisstab was dismantled and the cocoa trade liberalized. But this is another story (Grimm, 2004).

Between 1985 and 1993, we expect cocoa-producers' income to have fallen much more than the rest of the population. In order to examine the income consequences of the shock, we define a treatment group, the sample of cocoa-producing households (defined as producing at least 1 kilo of cocoa beans), and a comparison group, the sample of non-cocoa agricultural households (defined as being households whose head is a farmer but do not produce cocoa at all). Figure 1 confirms that cocoa households has fallen more than their non-cocoa agricultural counterparts, by 56% against 47%, although each category has been very much affected by the cocoa-induced macroeconomic crisis.

We now look at whether cocoa households have comparatively less invested in their offspring's health or education.

In the case of education, let us first point out that assessing the long-term consequences of the cocoa shock turns out to be difficult. Indeed, the data does not allow tracing back the type of household (cocoa vs. non-cocoa) where an adult individual actually lived at school age years. This precludes comparing definitive educational attainments (literacy, completed primary level) between treated adults and non-treated adults. Conversely, if we restrict to children less than 15 years, for whom mobility outside of household of birth is still limited, the number of cohorts who do not suffer from rightward truncation is very much limited. We are therefore bound to focus on current school enrollment and child work for ages 5-15. In the case of health however, average differences in height-for-age deficits mirror the past investments in child care from the parents and the quantity and quality of nutrition received (Martorell and Habicht, 1986). At ages 3-5, a height-for-age Z-score inferior to -2 means that a child has experienced a severe growth failure, and this kind of accident is widely considered as a health handicap in adult age.

Table 1 shows that in 1988, 5-15 y.o. children living in cocoa producing households were more often attending school and 7-15 y.o. children were much less often at work than their non-cocoa counterparts. 3-5 y.o. "cocoa children" were also taller, while 0-17 y.o. were also less often declared sick. When looked at within each village, Table 2 reveals that three of these differences were still significant, and only a bit lessened, the exception being child work. The right columns of Table

2 reveal that in 1993 the situation of cocoa children had significantly deteriorated in two cases: school enrollment and height-for-age. These within-village change of fortune for cocoa children is even confirmed in Table 3 where a full set of gender and age dummies, all interacted with the cocoa status, are controlled for.

Those two figures about schooling and stature deprivation suggest that the cocoa price shock might have had very serious consequences on the capabilities of the children living in cocoa-producing districts or cocoa-producing households. Although we can not definitively prove it, it is fairly possible that some minimal education was not received and could not be recovered; it is the same for the small stature inherited from stunting in that it reflects an irreversibly diminished health capital. The cohorts who were the most at risk in terms of primary education or nutrition and health care in the beginning 1990s could be doomed to carry all along their life the handicaps brought about by this unfavorable period. It remains to be checked whether the shock on parental income channel can credibly explain these long-term consequences.

5 Identification Strategies

The previous section has already circumscribed the core of our IV strategy. The price shock has had a relatively more negative impact on the income of cocoa households (relatively to our sample of non-cocoa agricultural households), who would have in turn relatively less invested in the education and health of their children. Therefore, we propose to instrument household income with belonging to a cocoa-producing household in 1993, rather than to another farming household, i.e. implement a difference-in-difference instrumental variable strategy (DiD-IV) on the sub-sample of farmers' households. We estimate the following econometric model, for child i in household h in village v at time t:

$$S_{ihvt} = a + X_{hvt}b + \theta Y_{hvt} + \delta COCOA + V_{vt} + u_{ihvt} \qquad (3)$$

$$Y_{ihvt} = a' + X_{hvt}b' + \delta' COCOA + \pi COCOA93 + V'_{vt} + v'_{ihvt} \quad (4)$$

where S is the outcome, X a set of child and household exogenous variables, Y stands for household income, COCOA is a dummy variable taking the value 1 if the household produces cocoa, V is a vector of village-time fixed effects and u is a residual. COCOA93 indicates if the household produces cocoa in 1993, and is the instrumental variable. As such, it must be reasonably correlated with income in 1993, and uncorrelated with the residual in the main equation: once we control for a set of observable variables, belonging to the treatment group in 1993 (COCOA93) should not affect our outcome (S) this same year through another channel than income (Y). Whether non-cocoa farmers are indirectly affected by the cocoa crisis or simultaneously affected by a specific price or income shock is irrelevant, provided that the evolution of the difference in outcomes between cocoa and non-cocoa children is only affected by the evolution of their relative income over the period 1988-1993. This of course requires that the outcomes of the two groups had *parallel within-village trends* before the cocoa price shock.

We also implement a second instrumental variable strategy for comparative purposes. Instead of instrumenting by "belonging to a cocoa household vs. belonging to another farming household in 1993" (the strategy which we label IV1), we instrument by "being born in a department specialized in cocoa production vs. being born elsewhere in 1993" (IV2). We replace COCOA(93) by the dummy variable COCOADEP(93) corresponding to this latter definition, and we use the full sample of households instead of a sub-sample restricted to farmers. Then, since we can no longer consider village-time fixed effects, as they would absorb too much of the instrument variation, we just include a time dummy:

$$S_{ihvt} = \alpha + X_{hvt}\beta + \theta' Y_{hvt} + \gamma COCOADEP + \lambda t + u_{ihvt} \quad (5)$$

$$Y_{ihvt} = \alpha' + X_{hvt}\beta' + \gamma' COCOADEP + \pi' COCOADEP93 + \lambda t + v_{ihvt} \quad (6)$$

When referred to the previous literature, this strategy echoes the exploitation of local aggregate shocks instead of individual shocks (Jensen, 2000; Kruger, 2007).

We measure cocoa specialization by the number of tons of cocoa produced per squared kilometer. As figure 3 reveals, the cocoa-producing districts are all located in the Southern part of the country (brown regions). Given the political economy of Cote d'Ivoire, we run the risk of confounding the effect of the cocoa shock and of the differential evolutions of Southern and Northern regions over the period. We thus divide the coco-producing districts in two classes with the same number of districts (light brown vs brown in figure 2), and also exploit the variability in production within the Southern districts. We also check that regressions run on a sample restricted to cocoa-producing districts give similar results although the obtained coefficients are little bit higher and a little less precisely estimated. Finally, it should of course be stressed that district of birth can influence private household income but also a whole bunch of contextual factors: educational and sanitary infrastructures, aggregate income and demand for products and for labor, etc. It may also reflect social interactions effects whereby neighbors in the same district imitate each other in terms of schooling, child work or child care behaviors. First, we do not have enough data to compute triple-differences in order to check whether cocoa and non-cocoa districts already displayed specific trends before the shock, like for instance non-cocoa districts catching up with their cocoa counterparts in the 1980s. Second, even if trends are parallel, the cocoa shock could have affected the contextual factors aside to household income. This is why we think that this IV2 strategy should usually produce an overestimation of the household income effect, by attributing too much of the variation in outcome to this latter variable. And this is why we give our preference to the within-village IV1 strategy. Table 3 confirms that the magnitude of within-village double-differences is indeed lower than pooled estimates, except in the case of stunting.

6 Supporting Evidence for the Double-Difference Strategy

We examine here whether other factors than income can plausibly have influenced the evolution across time of the difference between children living in cocoa-producing households and their non-cocoa counterparts.

6.1 Occupational Mobility and Migration

Some households may have switched from cocoa to non-cocoa farming / non-farming as a result of the price shock. In fact, such a move is unlikely in the short-term 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 longer. Since cocoa prices were high before 1990, households who were producing cocoa before 1990 are likely to remain so in 1993. Anecdotal evidence from the field says that many cocoa producers were waiting for a price upturn. Furthermore, the shares of cocoa and non-cocoa households in the total population have remained stable between 1988 and 1993: respectively 28.1 and 28.6 for cocoa households, 36.3 and 34.8 for non-cocoa agricultural households. We also calculated the share of cocoa households in each village and checked the density distributions of this share did not change between the two years (Figure 3). We are nevertheless aware that such stability could hide some compositional change of sectors. Table 2 indeed reveals slight differential evolutions in observable variables, the most significant being the household head ageing and the increased ownership of livestock in cocoa-producing households. Cocoa household heads also seem a little less educated than their counterparts in 1993, but as it has already been noted this education variable is not fully comparable between the two years. When a non-constant difference is observed, even at 10% confidence, we chose to additionally control for this variable in our IV regressions (see columns (5) or (6) in Table 5).

6.2 Selection and Fostering [To Be Completed]

Then, it could be that cocoa households have fostered more children in 1993 compared to 1988, relatively to non-cocoa households. Our identification strategies would then be contaminated by the endogeneity of household composition. Let us point out that our IV2 strategy should not be subject to this type of bias, as it includes all children living in Cote d'Ivoire in a given age range, and corrects for endogenous migration by using district of birth as the instrumental variable. In the case of IV1 however, this possibility of selectivity bias linked to household composition and fostering is indeed supported by the fact that the relative size of cocoa households in 1993 has slightly diminished compared to 1988 relatively to non-cocoa households (Table 2). 78% of the 0-17 year-old are the children of the household head. Then, within, the remaining 22%, 28% are the stepchildren, (step-)sister/brother or niece/nephew of the household chef, 70% are other members of the family (grandchildren, cousins, etc.), while only 2% do not come from the extended family. The probability that a child is not the direct offspring of the household head is stable over time, as the regression of the binary variable "non-direct child" on age and gender dummies, COCOA, COCOA93 plus village-time fixed effects gives a non-significant effect of COCOA93 (0.008, with a p-value equal to 0.642). In the end, we believe that household composition is not very much influenced by our natural experiment. In the econometric estimations, we will include the whole sample of children but will also look at the heterogeneity of the household income with respect to the relation to the head.

7 Results

We estimate the household income effect under six model specifications, although we do not consider that all of them provide a valid identification of the causal impact of family income. All the specification includes a full set of age and gender dummies interacted with a dummy indicating whether the household produces cocoa, as well as a 1993 time dummy or village-time dummies when appropriate. The six specifications are the following: OLS (column 1), OLS-within village (column 2), IV1-pooled (column 3), IV1-within-village without additional controls (column 4), IV1-within-village with additional controls (column 5) and IV2 with additional controls (column 6).[3] We report the coefficient for the logarithm of per capita consumption (PCC), our income measure; the list of additional controls is given below each table (estimated coefficients for such variables are not reported since they are not of primary interest, but they are available upon request). Table 5 provides a detailed example of the IV1-within-village estimation and of the IV2 estimation in the case of school enrollment, with the first stage in the bottom panel and the second stage in the top panel. DiD-IV are performed using the Generalized Moments Method (GMM).[4] Double Least-Squares (2SLS) were also tried and gave similar results, even in terms of efficiency. For each IV regression, we report the Kleibergen-Paap Wald rk F statistic which must be compared with the F statistic Hausman, Stock and Yogo critical values to test for the weakness of instruments (Hausman, Stock, and Yogo, 2005). Actually, our F statistic almost always passes the 10% maximal IV size threshold. Lastly, we only reports results for the linear probability model, since IV-Probit or IV-Logit results are also quite similar (but much time-consuming to perform with village-time fixed effects).

7.1 School Enrollment and Child Labor

We now describe the results of our estimations of the income effect for school enrollment and child labor. Regarding the former, we consider the sample of 5-15 year-old children, 5 being the theoretical age of entry in CP1 the first class of the 6 years of primary school cycle, and 15 being the theoretical age of termination of 3e the last class of the 4 years of secondary school cycle. As for child work, we focus on 7-15 year-old children, as employment data only starts at age 7. First, taking into account the sole five first columns, comparison of columns (1) vs. (2), and (3)

[3] IV estimators were calculated using Stata modules Ivreg2 and Xtivreg2 (Schaffer 2007)

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

vs. (4)-(5), confirms 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 (2) vs. (4)-(5) suggests that OLS strongly underestimate the causal effect of income on enrollment, the IV point estimate being threefold the OLS coefficient. Then, comparison of columns 4 and 5 confirms that our IV1-within-village results are robust to the inclusion of control variables whose difference in means between the treatment group and the comparison group varies across time. Lastly, column (6) confirms that IV2 also may lead to overestimated income effects. In particular, if we refer to the results from column (5), a 10% increase in income leads to a 2.9 percentage points increase in school enrollment, and a 2.4 decrease in child labor. But this former elasticity is hardly significant (at 10% confidence only), while the latter is not. A within-village standard deviation of income is around 0.44, so these coefficients should be multiplied by 4 to correspond to a one standard deviation change in income: 13 percentage points in the instance of school enrollment. It should however be pointed out that such standard deviation of income is probably overestimated due to measurement errors in noisy data. However, when income is interacted with the age of the child, the elasticity of school enrollment gains significance. When age goes from 5 to 15, the elasticity ranges between 0.36 to 0.21 and is significant at 5% percent confidence between age 5 and age 11. Other forms of heterogeneity of treatment were tried, with respect to the gender of the child, to his/her relation to the head and to his birth order, but without success. Whatever the form of the heterogeneity that is considered, child labor does not seem to be sensitive to family income. This result echoes with the relatively disappointing performance of conditional cash transfer programs in terms of child labor.

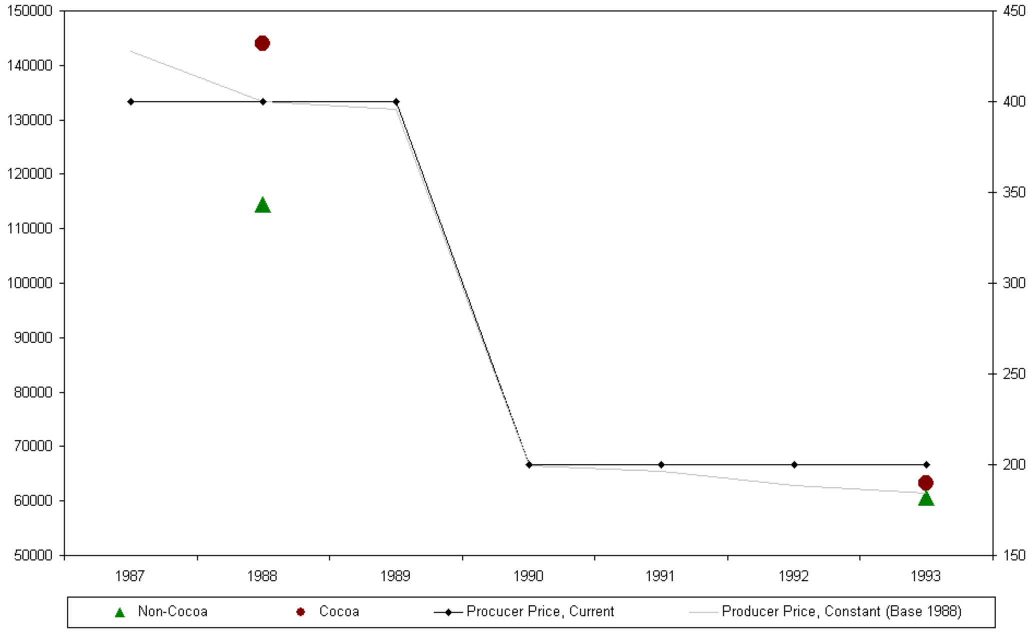
7.2 Height Stature, Stunting and Sickness

We turn to our results for height, stunting and declared sickness (having been sick or not in the previous month). Regarding the anthropometric variables, we focus on the sample of 3 to 5 year-old children, because the results we obtain when also considering 0-2 year-old children are much less efficient; we control for age in months dummies and gender dummies. We also tried analyzing height-for-age Z-scores, that gave very similar results as height stature regressions. Regarding declared sickness, we use the sample of 0-17 year-old children. Again, comparison of columns (1) vs. (2), and (3) vs. (4)-(5), confirms our intuition that not taking into consideration village fixed effects generates an upward bias. Comparison of columns (2) vs. (4)-(5) also reveals that OLS very strongly underestimate the causal effect of income on height stature or stunting in comparison of IV estimates, actually by a factor of fifteen. We found no indication of weak instruments bias that could underlie this result. In the case of declared sickness, while the OLS estimates are flawed by a positive correlation between income level and sickness self-assessment (a hypochondriac bias from the rich or some preference attrition bias from the poor), IV estimates interestingly establish a more plausible negative impact of income variation on sickness. While rightly signed, our preferred IV1-within-village estimates are however never statistically significant. Coming back to the height stature and stunting outcomes, column (6) surprisingly shows that IV2 leads to lower point estimates of the income elasticity. In the end, whether one refers to the results from column (5) or from column (6), a 10% increase in income would lead a height increase of 0.4 to 0.9 centimeter at 3-5 years of age, and a decrease in the likelihood of stunting of 2.5 to 7.5 percentage points (remember fourfold these values correspond to a one standard deviation change in income, although this estimate is certainly overestimated). The non-significance of estimates for the youngest children (0-2 years old) could suggest that breastfeeding may smooth the variation of income, and that *in utero* growth is not very much affected by the economic situation of mothers. In contrast, 3-5 years old children were born just around the cocoa shock and had to endure low and bad quality nutrition during 2 or 3 years.

8 Conclusion

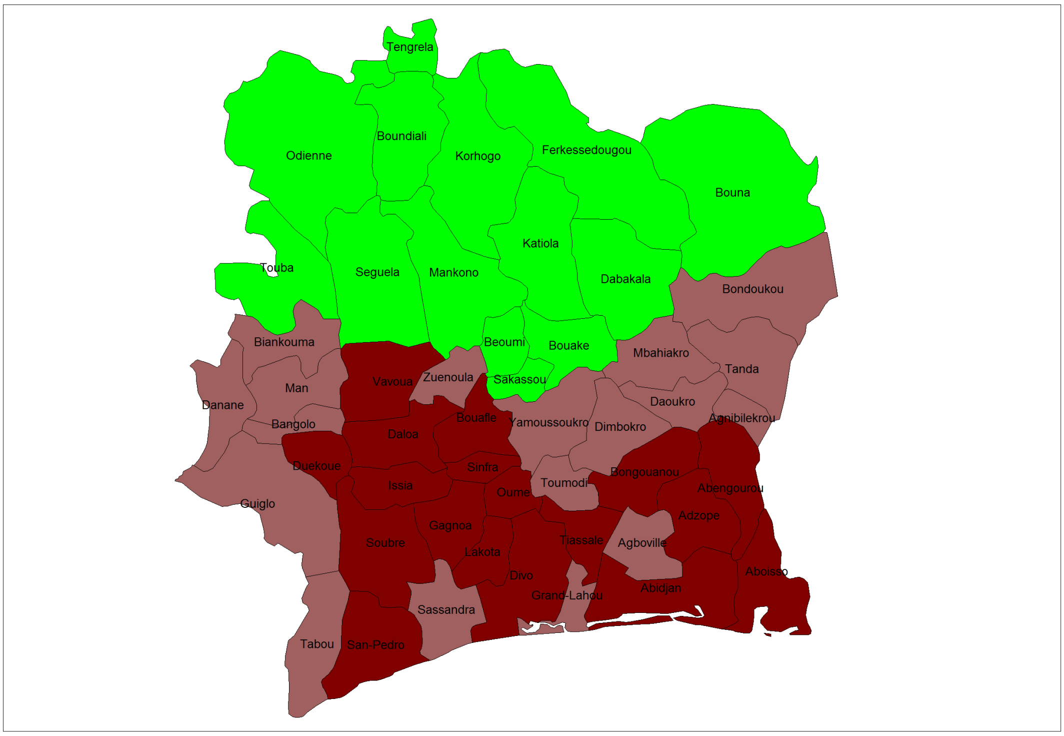
We study the drastic cut of the administered cocoa producer price in 1990 Cote d'Ivoire and look at the extent to which cocoa producers' children suffered from this severe income shock in terms of school enrollment, increased labor, height stature and sickness. Comparing pre-crisis (1985-88) data and post-crisis (1993) data, we propose a difference-in-difference within-village strategy in order to identify the causal effect of family income on children outcomes, whereby we compare the evolution of outcomes of children living in cocoa producing households with that of children living in other agricultural households of the same village. A second identification strategy exploits the weight of cocoa production in the district of birth of the children. With both strategies, we find a strong and significant impact of family income variation for at least two out of the four variables we examine. In particular, we find a strong reaction of school enrollment to the income shock, especially for children between 5 and 11 years old, and indifferently for boys and girls. For instance, at 7 years of age, a 10 percent decrease in income induces a 3 percentage points fall of the probability of school enrollment. The estimated income elasticity of child labor is negative but less significant. We also find a large income effect on the height stature of children between 3 and 5 years old: a 10 percent variation in income here leads to an average 0.4 to 0.9 cm change or to a 2.5 to 7.5 change in the probability of being stunted. Last, our difference-in-difference strategies allow correcting for the large bias underlying the positive correlation between declared sickness and income: our instrumented estimates turn out to have a negative sign, although they are not always significant. In comparison with the previous literature, we believe that our analysis offers several advantages. First, we exploit a negative income shock , for which no randomized experimental data will ever exist. Second, we not only examine child schooling and child labor, but also child care and child health, which are under-represented issues in the literature, especially in African countries. Third, using good microeconomic data on income, we are able to derive direct estimates of the causal effect of family income on children education and health. Fourth, we show that instrumenting with aggregate shocks may underestimate or overestimate the individual income effect, since contextual effects are not accounted for, hence our preference for the within-village strategy. Fifth, we indeed confirm that naive OLS estimation tends to underestimate the effect of household income. African economies remain little diversified and vulnerable to changing international prices for their exports. In Cote d'Ivoire, a considerable part of the population still works in the agricultural sector, and directly undergoes the fluctuations of international prices. By the past, the national marketing board and price stabilization fund for cocoa and coffee, the Caisstab, did not really served its original mission; it was dismantled in 1998. Nevertheless, new insurance schemes and safety nets could be invented to protect households and children from unexpected shocks on income. If one believes in the income elasticities presented in this study, the transposition of conditional transfer programs already implemented in Latin America could deserve some attention, and could constitute a very defendable use of foreign aid money.

Figure 1: National Cocoa Producer Prices and Average Per Capita Consumption for Cocoa and Non-Cocoa Households (1988 and 1993).



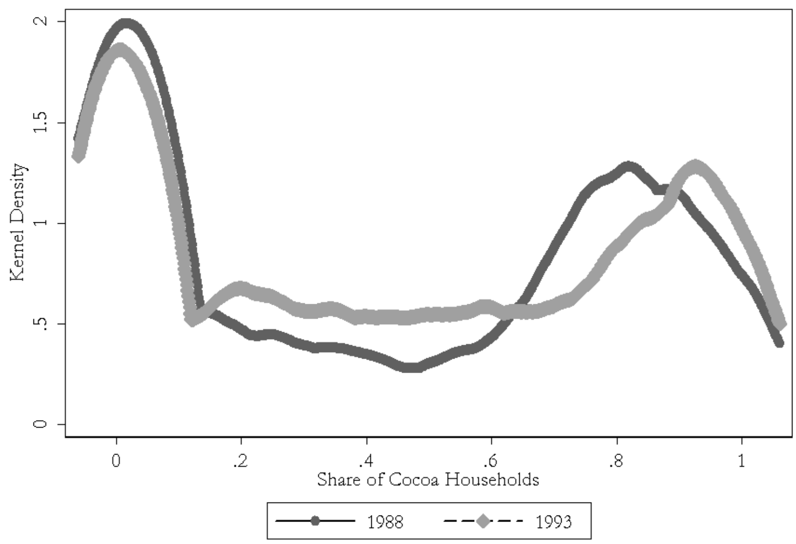
Sources: Berthélemy and Bourguignon 1996, World Bank 2001, IMF 2007. Authors' calculations. Population covered: 5 to 15 years old children.

Figure 2: Average Cocoa Production by Department in 1987-1988-1989.



Reading: Green = no cocoa production, Light Brown = low density of cocoa production, Brown = high density of cocoa production. Production expressed in thousands of tonnes of cocoa beans per squared kilometer. Sources: CSSPPA (1990), DCGTx (1995). Authors' calculations.

Figure 3: Kernel Density of the Share of Cocoa Households Within Villages.



Population covered: 5 to 15 years old children.

Table 1: Mean Characteristics for Cocoa and Non-Cocoa 5-15 yo Children and Mean Outcomes for Specific Age Categorizations, in 1988.

	Non-Cocoa	Cocoa	T-test
Age	9.55	9.58	0.550
Male	0.531	0.542	0.159
Was born out of Ivory Coast	0.012	0.009	0.090*
Age of HH head	49.36	51.23	0.000**
HH head is a woman	0.097	0.024	0.000**
Share of women in the HH	0.508	0.497	0.000**
HH head is literate	0.218	0.225	0.282
HH head has ever been to school	0.225	0.248	0.000**
HH head has at least achieved prim. school	0.182	0.161	0.000**
HH head was born out of Ivory Coast	0.147	0.182	0.000**
HH head has migrated in the last 3 years.	0.051	0.022	0.000**
HH head has migrated last year	0.014	0.005	0.000**
Size of the HH	9.64	11.56	0.000**
HH owns livestock	0.497	0.508	0.187
Child (5-15 yo) attends a school	0.325	0.393	0.000**
Child (7-15 yo) works	0.417	0.253	0.000**
Child (3-5 yo) stunted	0.188	0.108	0.000**
Child (0-15 yo) sick in the last month	0.197	0.163	0.000**

T-test p-values are reported in column 3. Here, the null hypothesis is that the means for both groups are different. ** significant at 5%, * significant at 10%.

Table 2: Relative 1988-1993 Change in Observables for 5-15 yo Children and Mean Outcomes for Specific Age Categorizations, in Cocoa and Non-Cocoa Households.

	Cocoa	s.e.	Cocoa93	s.e.
Age	0.15	0.133	0.001	0.168
Male	0.011	0.021	-0.017	0.027
Was born out of Ivory Coast	-0.017**	0.006	0.011	0.008
Age of HH head	2.496**	0.504	3.448**	0.624
HH head is a woman	-0.108	0.012	-0.021	0.015
Share of women in the HH	-0.007	0.007	0.002	0.009
HH head is literate	-0.02	0.017	-0.035	0.022
HH head has ever been to school	-0.019	0.018	-0.039*	0.022
HH head has at least achieved prim. school	-0.016	0.015	-0.048*	0.02
HH head was born out of Ivory Coast	0.005	0.01	-0.038*	0.016
HH head has migrated in the last 3 years.	-0.026**	0.011	0.006	0.013
HH head has migrated last year	0.003	0.007	-0.009	0.007
Size of the HH	2.306**	0.205	-0.431*	0.236
HH owns livestock	0.05**	0.019	0.059**	0.023
Child (5-15 yo) attends a school	0.067**	0.019	-0.044*	0.024
Child (7-15 yo) works	-0.027	0.02	0.034	0.026
Child (3-5 yo) stunted	-0.131**	0.043	0.181**	0.057
Child (0-15 yo) sick in the last month	-0.038**	0.013	0.019	0.016

Regressions: OLS-within-villages (including time-village fixed effects), robust to heteroscedasticity. Obs. 5-15: 17098, 7-15: 13501, 3-5: 2932, 0-15: 25257. ** significant at 5%, * significant at 10 %.

Table 3: Results for the Reduced-Form Model, School Enrollment (School 5-15), Child Labor (Work 7-15), Stunting (Stunted 3-5) and Health Status (Sick 0-15).

School		Work		Stunted		Sick	
Farmers sample: cocoa producers vs. non-cocoa producers, in 1993 vs. 1988 (IV1)							
Pooled	Within-village	Pooled	Within-village	Pooled	Within-village	Pooled	Within-village
-0.103** (0.016)	-0.041* (0.022)	0.152** (0.016)	0.036 (0.023)	0.127** (0.034)	0.173** (0.058)	0.026** (0.010)	0.018 (0.015)
Full sample: born in cocoa district (low or high) vs. non-cocoa, in 1993 vs. 1988 (IV2)							
Pooled		Pooled		Pooled		Pooled	
Low cocoa density	High cocoa density	Low cocoa density	High cocoa density	Low cocoa density	High cocoa density	Low cocoa density	High cocoa density
-0.119** (0.017)	-0.07** (0.015)	0.074** (0.019)	0.101** (0.016)	0.105** (0.012)	0.065** (0.010)	0.086** (0.045)	0.139** (0.043)

Standard errors in parentheses. Regressions: OLS, pooled or within, robust to heteroscedasticity, including dummies for age, gender and cocoa specialization and their multiple interactions. Obs. IV1: 5-15: 17098, 7-15: 13501, 3-5: 2932, 0-15: 25257. Obs. IV2: 5-15: 28984, 7-15: 23136, 3-5: 3001, 0-15: 42527. ** significant at 5%, * significant at 10 %.

Table 4: IV1 and IV2 Second-Stage and First-Stage Results for School Enrollment, 5-15 yo Children.

School 5-15	(4)		(6)	
Second-stage	Coeff.	s.e.	Coeff.	s.e.
pcc	0.286*	0.163	0.511**	0.094
Cocoa	-0.038	0.024		
Cocoasup1			0.022	0.029
Cocoasup2			-0.101**	0.033
Was not born in Ivory Coast	-0.107**	0.032	-0.162**	0.035
Age of HH head	0.004	0.002	0.01**	0.002
Age^2 of HH head	-0.00*	0.00	-0.000**	0.000
HH head is a woman	0.03	0.016	-0.007	0.016
HH head has ever been to school	0.027	0.019	0.05**	0.021
HH head has at least achieved prim. school	0.059**	0.023	-0.021	0.037
HH head was born out of Ivory Coast	-0.147**	0.019	-0.105**	0.009
HH owns livestock	-0.005*	0.015	0.013	0.017
First-stage	Coeff.	s.e.	Coeff.	s.e.
Cocoa_{93}	-0.138**	0.027		
Cocoa	0.133**	0.042		
Cocoasup1_{93}			-0.194**	0.024
Cocoasup2_{93}			-0.073**	0.021
Cocoasup1			0.078	0.055
Cocoasup2			0.26**	0.049
Was not born in Ivory Coast	-0.031	0.04	0.279**	0.033
Age of HH head	-0.007**	0.002	-0.005**	0.002
Age^2 of HH head	0.00**	0.00	0.000*	0.000
HH head is a woman	-0.043**	0.017	0.117**	0.015
HH head has ever been to school	0.066**	0.018	0.154**	0.018
HH head has at least achieved prim. school	0.087**	0.02	0.364**	0.019
HH head was born out of Ivory Coast	-0.089**	0.016	-0.017	0.011
HH owns livestock	0.076**	0.009	-0.168**	0.009
IV F-stat	26.19		36.19	
Hansen J-stat			0.292	
Chi-sq(1) p-value			0.589	

Columns: (4) IV1-GMM-within without additional controls except dummies for age, gender and cocoa specialization and their multiple interactions, (6) IV2-GMM with the additional controls. Controls: age and age squared of the household head, dummies equal 1 if the child was not born in Ivory Coast, if the household head was not born in Ivory Coast, is a woman, has ever been to school, has achieved at least primary schooling and if the household owns livestock. Obs. column (4) : 17098. Obs. column (6): 5-15: 28984. ** significant at 5%, * significant at 10 %.

Table 5: Results for School Enrollment (School 5-15), Child Labor (Work 7-15), Height (Height 3-5), Stunting (Stunted 3-5) and Health Status (Sick 0-15).

School 5-15	(1)	(2)	(3)	(4)	(5)	(6)
pcc	0.148**	0.09**	0.757**	0.31*	0.286*	0.511**
s.e.	0.006	0.007	0.142	0.172	0.163	0.094
IV F-stat			48.89	23.59	26.19	36.19
Work 7-15	(1)	(2)	(3)	(4)	(5)	(6)
pcc	-0.11**	-0.037**	-1.04**	-0.268	-0.241	-0.263**
s.e.	0.007	0.008	0.179	0.177	0.172	0.093
IV F-stat			45.17	18.81	20.28	27.80
Height 3-5	(1)	(2)	(3)	(4)	(5)	(6)
pcc	0.743**	0.596**	11.606**	8.536**	9.06**	4.579**
s.e.	0.24	0.288	3.71	4.195	4.401	1.732
IV F-stat			17.9	13.08	12.63	20.48
Stunted 3-5	(1)	(2)	(3)	(4)	(5)	(6)
pcc	-0.051**	-0.045**	-0.661**	-0.737**	-0.746**	-0.267**
s.e.	0.017	0.021	0.225	0.31	0.319	0.12
IV F-stat			17.9	13.08	12.63	20.48
Sick 0-15	(1)	(2)	(3)	(4)	(5)	(6)
pcc	0.023**	0.015**	-0.199**	-0.158	-0.168	-0.572**
s.e.	0.004	0.005	0.08	0.138	0.136	0.09
IV F-stat			70.94	30.06	31.72	46.75

Interactions	(2)		(4)		(5)	
School 5-15	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
pcc	0.107**	0.016	0.46**	0.188	0.437**	0.179
pcc‖pcc×age	-0.002	0.002	-0.015**	0.004	-0.015**	0.004
Work 7-15	Coeff.	s.e.	Coeff.	s.e.	Coeff.	s.e.
pcc	0.048**	0.022	-0.316*	0.191	-0.275	0.178
pcc‖pcc×age	-0.008**	0.002	0.004	0.006	0.004	0.005

Columns: (1) OLS, (2) OLS-within, (3) IV1-GMM, (4) IV1-GMM-within; (1) to (4) models include dummies for age, gender and cocoa specialization and their multiple interactions (in the case of height stature and stunted, only dummies for cocoa specialization, and the interactions between age in months and gender); (5) IV1-GMM-within with additional controls (list provided below) and (6) IV2-GMM with additional controls. Additional controls: age and age squared of the household head, dummies equal 1 if the child was not born in Ivory Coast, if the household head was not born in Ivory Coast, is a woman, has ever been to school, has achieved at least primary schooling and if the household owns livestock. Obs. columns (1) to (5) : 5-15: 17098, 7-15: 13501, 3-5: 2932, 0-15: 25257. Obs. column (6): 5-15: 28984, 7-15: 23136, 3-5: 3001, 0-15: 42527. ** significant at 5%, * significant at 10 %.

APPENDIX 1: Schooling and Health in Cote d'Ivoire: Facts

Cote d'Ivoire, like its neighboring 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). The Ivorian educational system proposes the following curriculum: from 5 to 11, "*école primaire*", from 12 to 15, "*collège*", from 16 to 18, "*lycée*" (high school), and from 19, "*université*". Actually, children enter rather late into the first grade of primary schooling ("*Cours Préparatoire 1ère année*", CP1). In our specific sample, the average entry age into primary schooling is 7.12 (and not 5 as in theory). Girls do not seem to enter sooner than boys (7.08 vs 7.15 for the latter). Then, less than half of children attend primary schooling, and even less achieve the full cycle. Lastly, those who attend school may also work, the adjustment variable being leisure. In our 1988 sample of agricultural producers, amongst the children aged 12 to 17, 25.42% only attend school, 7.03% both attend school and works, and 51.07% only work. Lastly, as for nutritional and mortality indicators, Cote d'Ivoire performs well in comparison with other West African countries, even if this country is the Western African country where the AIDS epidemics is the most widespread.

Table 6: Investments in Education and Health for Five West African Countries

	Burkina-Faso	Cote d'Ivoire	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, 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	43.1 (2003)	31.5 (1999)	35.6 (2003)	39.3 (2005)	42.7 (2001)
% of children under 5 who are underweight	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
Children 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)
Children 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)

Sources: UN, 2007 and WHO, 2007

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