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Food price inflation and schooling

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Abstract

In the middle of the nineties the rural population in Burkina Faso was seriously hit by rising food prices. Whereas cotton farmers were able to cope with this shock given the simultaneous boom in the cotton sector, food crop farmers had to withdraw children from school and to let them work more intensively. Using the exogenous character of the income variation as an instrument allows to disentangle the pure effect of parental income from effects related to parental education, family background and other unobservables. A set of simple policy simulations illustrates the potential of unconditional cash transfers to raise schooling levels and to protect investment in children's education against transitory income shocks. Although the involved effects are not negligible and much higher as simulations based on the pure OLS effect would suggest, they also show that making transfers conditional on attendance might largely increase the efficiency of such transfers.

***JEL* Classification:** I21, O12, Q12.

Key words: Child Labor, Education, Income Elasticity of Education, Agricultural Shocks, Cotton Production, Burkina Faso.

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1 Introduction

The United Nations' World Food Programme warns of the catastrophic effects of rising food prices on hunger and poverty.[1] In 1997/98 Burkina Faso was hit by a severe drought, even by Burkinabè standards. Following that drought prices of the three main food crops, sorghum, millet and maize increased by more than 40 percent. These food crops account in normal times for about 30 percent of total expenditures (including imputed expenditures for own production) of rural households in the poorest quintile of the expenditure distribution. This surge in food prices signified a dramatic increase in the poverty line in nominal terms. In consequence, rural poverty increased between 1994 and 1998 by 5.3 percentage points (Grimm and Günther, 2007a). In the same time cotton exports increased tremendously driven by the devaluation of the Franc CFA in 1994, a favorable development of the world market price for cotton and a significant expansion of land used for cotton production. This boom mitigated the effects of the drought for cotton farmers and prevented expenditures to decline as much as those of food crop farmers. Although Burkina Faso knew another drought in 2000/01, though less severe, real expenditures of food crop and cotton farmers were higher in 2003 than in 1998.

Many empirical studies have shown that if households operating in an environment of incomplete financial markets as it is the case in most parts of Sub-Saharan Africa are affected by such shocks, they rely on strategies such as the depletion of assets, increased labor supply and the withdraw of children from school to meet their basic needs (see e.g., Jacoby and Skoufias, 1997). I analyze whether in Burkina Faso delayed enrolment and school dropouts were also a strategy of food crop farmers to compensate the adverse effects of the drought and whether, in contrast, children of cotton farmers were sheltered, at least to some extent, against that shock, given the simultaneous cotton boom.

The fact that food crop and cotton farmers where hit so differently by exogenous shocks during that period provides a natural experiment which may also allow to quantify accurately the income elasticity of children's school enrolment. Estimates of that elasticity based on a simple regression of schooling on income are usually biased due to simultaneity, unobserved heterogeneity and possibly measurement error. Using the income shocks as instruments can help to remove these biases.

Un unbiased estimate of the income elasticity of schooling can help to design safety nets which allow parents to keep their children at school in times of economic hardship. More generally, such an estimate is of course also needed if policy makers see conditional or unconditional cash transfer programs, of the type implemented in many parts of Latin America and Asia, as one option to increase education levels in Sub-Saharan Africa. There is still a lively debate about the role of parental income for children's schooling. So far there is only little evidence for Sub-Saharan Africa, although this region has the lowest education levels in the world. Low education perpetuates social mobility and inequality and has many other adverse effects which have not to be repeated

[1] See http://www.wfp.org/.

here.

Enormous financial resources will be allocated by governments and international donors to investments in children's education in Africa and hence information of the type mentioned above is important to think how to spend these resources in the most efficient way. Institutional constraints make it still difficult to implement (conditional) cash transfer programs (Schubert and Slater, 2006), but the need for such interventions is in principle high, given the usually extreme vulnerability of poor rural households.

This paper will show that children of food crop farmers were seriously affected by the drought. Food crop farmers knew on average a decline by more than 30 percent of their income which led to a drop of more than 10 percent in enrollment rates. This corresponds to about 100,000 children which have not been enrolled or have been withdrawn from school. The income effect is high and in particular much higher than a simple OLS regression would suggest. Thus the World Food Programme is right in warning of the detrimental effects of rising food prices.

The reminder of the paper is organized as follows. Section 2 presents a short review of the relevant literature. Section 3 discusses the economic context in Burkina Faso and provides some information about the education system. Section 4 analyzes the effect of the drought and the cotton boom on children's schooling in cotton and food crop farming households. Section 5 estimates using an original IV approach the income elasticity of enrollment. Section 6 presents based on the estimated income effects, some simple policy simulations of fictive cash transfer programs designed to mitigate the effects of shocks on children's schooling. The paper ends in Section 7 with some concluding remarks.

2 A review of the literature

Various papers have analyzed the impact of transitory and unanticipated income shocks on indicators related to children's schooling in poor rural settings. The theoretical background of these papers is that under liquidity constraints, caused by the lack of insurance and limited possibilities to smooth consumption through credit and savings, the standard human capital investment model of child labor and schooling decisions introduced by Schultz (1960) and formalized by Ben-Porath (1967) does not apply. As pointed out by Baland and Robinson (2000), if parents face such constraints then in a case of a negative shock they have to trade off the future benefits of educating their children against their current consumption needs. Therefore children are not enrolled or drop out of school in order to contribute to household income and to help to maintain current consumption, even if the return to child labor is below the return of education.

Jacoby (1994) was one of the first who empirically showed using data for Peru that income shocks can have a notable impact on school attendance in poor settings. He emphasized that this effect stemmed mainly from those households which were credit constrained, as measured by the predicted probability of having positive savings or outstanding nonbusiness loans. In another paper, Jacoby and Skoufias (1997) focused in particular on the impact of seasonal fluctuations in the income of agrarian households distinguishing aggregate from idiosyncratic and anticipated from unanticipated income shocks. Using panel data from India, the authors found that small farm households were inadequately insured ex ante, and, hence, unanticipated income shocks significantly affected children's school attendance. They also found that households, again in particular smaller farm househoolds, faced serious credit market constraints, sometimes combined with limited storage opportunities, which again had adverse impacts on children's school attendance.

Kazianga (2005) used similar panel data for six villages in rural Burkina Faso, the country on which this paper also focuses on. He also pointed to the potential benefit of informal insurance mechanisms. He showed that for households without any access to insurance the frequency of income shocks, as measured by the predicted income variance, reduced educational investments in boys and, in particular, in girls.

Beegle, Dehejia and Gatti (2006) used panel data for Tanzania to examine the extent to which transitory income shocks led to increases in child labor and whether household's asset holdings mitigated the effects of these shocks. For this purpose they regressed child labor hours on an interaction term of asset ownership and shocks controlling for household fixed effects and a number of time varying household characteristics. They find that crop shocks led to a significant increase in the level of child labor, but that households with assets were able to offset at least a large part of that shock. Richer households tended to use their assets rather as collateral against credit, whereas poorer households tended to use them rather as a buffer. However, school enrollment decreased less than expected because many children were able to combine school and work.

Other papers focused on child labor and children's school enrollment in the context of economic crisis. Thomas, Beegle, Frankenberg *et al.* (2004), for instance, analyzed the effects of the financial crisis in Indonesia on children's school enrollment, by relating income to school enrollment and education spending. They found a substantial effect on schooling in particular in poorer households. However, poorer households had a tendency to protect education investments of older children at the expense of younger ones.

All these studies show convincingly that household income matters for children's schooling whenever households have only limited insurance and limited possibilities to smooth their consumption through credit and savings. However, these studies do not provide an accurate estimate of the income effect, which could be used to design safety nets, such as conditional or unconditional cash transfer programs, which would help households to overcome those shocks without withdrawing their children from school. To get an idea how much income has to be transferred, one needs to know the income elasticity of enrollment, once the effects of reverse causality and omitted variables, in particular parents' unobserved preferences and abilities, have been controlled for. An issue which will be addressed in this paper.

For industrialized countries the causal impact of parental income on children's educational attainment was intensively studied using either panel data (see e.g., Blau, 1999) or instrumental variables (see e.g., Shea, 2000; Maurin, 2002; Chevalier, Harmon, O'Sullivan and Walker, 2005), the latter more and more often based on natural experiments (see e.g., Black, Devereux and Salvanes, 2005; Løken, 2007). The different identifying assumptions made in these studies can of course be subject to debate and it is difficult to draw a sharp conclusion. It is also plausible that the effect differs by country and time even within the group of industrialized countries. However, it seems the income effects in richer countries are relatively weak and that the effect of omitted parental abilities dominates, i.e. the OLS estimate of the income effect is rather upward than downward biased.

For developing countries this literature is relatively limited. Behrman and Wolfe (1987) used siblings data to analyze the respective roles of unobserved family backgrounds, parental education and income in Nicaragua. They showed that the impact of parental education is strongly reduced once family fixed effects are introduced. They did not find an effect of various measures of parental income on children's schooling. However, the use of family fixed effects in such a context is often criticized for inappropriately assuming that unobserved abilities are constant across siblings (see e.g., Ermisch and Francesconi, 2001).

Cogneau and Maurin (2003) is one of the rare studies which investigated the issue for a Sub-Saharan African country, Madagascar. They instrumented parental income by the difference in parental and grand-parental education. Using past educational achievements ensures that the instrument is uncorrelated with the error term and transitory income. By taking the first difference, they eliminate the family fixed effect. In contrast to the results of Behrman and Wolfe (1987), their instrumented income effect is three to four times larger than the non-instrumented effect. Parental education becomes almost insignificant, suggesting that parental education is rather a proxy for permanent income.[2]

Other studies relied on natural experiments, as this study will do. Rucci (2004) looked at changes in enrollment rates during the Argentinean crisis and instruments household income by the lagged Brazilian Real-US Dollar exchange rate. She also found the IV estimate, depending on age and gender of the child, to be two to seven times larger than the OLS estimate. Cogneau and Jedwab (2007) took cocoa price shocks in Côte d'Ivoire as an instrument for income and explored the difference in investments in children's education and health in families of cocoa and food crop farmers. Regarding the effects on education, they find an income elasticity of primary school enrollment which is three to four times higher than an elasticity estimated by OLS. For instance, for the age group five to eleven years old, they found that an increase in income by ten percent increased enrollment by almost three percent.[3]

For Latin America and to a minor extent also for South-East Asia there are of course also all those studies which examine the impact of conditional cash

[2] Behrman and Knowles (1999) survey a large number of other studies. However, most of them do not address the limitations regarding the use of current annual income or expenditures.

[3] Jensen (2000) and Kruger (2007) also rely on natural experiments to investigate the impact of income on education, however, they used reduced form estimators and thus did not provide an estimate of the income elasticity of enrollment.

transfer programs, where the condition is on school enrollment (see e.g., Schultz 2004). While these studies provide many interesting insights they do not allow to conclude on the impact of unconditional income transfers[4] and they also do most likely not allow to conclude on such income effects in the Sub-Saharan African context. Although enrollment rates in that region, in particular in rural areas, are by far the lowest in the world.

This paper contributes in several respects to the literature discussed above. First, it shows, as have some other papers before, that unanticipated transitory shocks on household income have immediate effects on children's school enrollment suggesting that other risk-coping instruments are insufficient. Second, it provides a relatively accurate estimate of the causal impact of income on children's enrollment in a very poor rural Sub-Saharan African setting. Third, based on the estimated income effects, the paper also provides some simple policy simulations to illustrate how cash transfers could mitigate the adverse effects of such income shocks.

3 Background

3.1 Agricultural production and prices

Burkina Faso is one of the poorest countries in the world. In 2005, GDP per capita in was estimated at only PPP US$ 1,213 and according to the Human Development Index, the country was ranked 176th out of 177 countries (UNDP, 2007). It is a landlocked country in the middle of West-Africa with a population of roughly 13,4 million. It has a very low human capital base and only very few natural resources. The country depends highly on cotton exports, which account for almost 60 percent of total export earnings, as well as on international aid. More than 80 percent of the Burkinabè population lives in rural areas working predominantly in the agricultural sector, which, again, suffers from very limited rainfall and recurrent severe droughts.

Figure 1 shows that as a result of the severe drought in 1997/98 total production of the three main food crops decreased by almost 20 percent. Although the production of maize increased during that period, given its relative low weight in food consumption, maize production could not compensate for the decline in millet and sorghum production. In the same time cotton production increased by more than 70 percent.

[insert Figure 1]

Figure 2 shows that the prices of cereals rose tremendously between 1994 and 1998. This rise was caused first of all by the production shortage following the drought. But even before prices tended to rise due to a general lack of productivity increases in cereal production, accompanied by high population growth and intra-annual price fluctuations. Demand from neighboring countries also put pressure on cereal prices in Burkina Faso during that time.

[4] Bourguignon, Ferreira and Leite (2003) for instance show that in the case of *Bolsa Escola* the unconditional effect is much lower than the conditional effect.

[insert Figure 2]

Following the devaluation of the Franc CFA in 1994 and the favorable development of the world market price of cotton, the Burkinabè cotton marketing board '*Société Burkinabè des fibres textitles*' (SOFITEX), which was in place at that time,[5] increased in several steps the producer price. Although the costs of inputs increased as well, given that most of them have to be imported, but the rise in prices still provided enough incentives to expand cotton production, mainly by the expansion of land allocated to cotton production. Returns per hectare increased by only 20 percent between 1994 and 1998 (Grimm and Günther, 2007b).

After 1998 cereal prices fell back to their normal level, before rising again due to a second drought in 2000/01 though less severe. The immediate consequences of that second drought are difficult to assess, since household survey data only exists for 1994, 1998 and 2003. As the Figures 1 and 2 show, in 2002 the cereal market was already back to normality.

Obviously such price hikes in food staples can always have two types of consequences. Households who are net producers of these cereals will benefit, i.e. the income effect will more than outweigh the price effect. Households who are net consumers will, in turn, suffer real income losses. Household survey data for 1998 shows, that in rural areas 94 percent of all households produced cereals, but only 15 percent sold any on the market. In contrast, the share of purchased cereals in total cereal consumption was on average 49 percent (Grimm and Günther, 2007b). Thus, in rural Burkina Faso most of the households should be losers of such price increases. It also suggests that households often have some other non-agricultural activities to generate the cash income necessary to purchase food.[6].

It is important to emphasize that the cereal prices shown in Figure 2 are consumer and not producer prices. The latter are often much lower given the negotiation power of traders and the information asymmetries prevailing between traders and farmers. Farmers do often also not have appropriate storage possibilities and thus are forced to sell their cereals directly after the harvest and, hence, prices tend to fall. Traders in turn, are able to store cereals, to speculate in the market and to drive the price up by allocating their supply over the whole year.

To conclude, given the price and production movements of cereals and cotton, the hypothesis is that cotton farmers were much less affected by the drought than farmers who cultivate only food crops. Given the lack of formal insurance and credit and, as shown by Kazianga and Udry (2006), the only limited evidence of risk sharing and consumption smoothing in Burkina Faso (see also Reardon, Matlon and Delgado (1996)), food crop farmers may have withdrawn their children from school or not enrolled in a first place even if they expected that this shock is only transitory. As discussed in Section 2, there is similar

[5] At that time SOFITEX was the only importer of agricultural inputs such as fertilizer and pesticides and the only buyer of cotton. For a detailed description of the sector, see e.g. Kaminski (2007).

[6] See Reardon, Matlon and Delgado (1988) for similar evidence on Burkina Faso

evidence for a number of other countries. If cotton farmers expected the shock on cereal prices to be transitory and the shock on cotton prices to be permanent, which is however not very likely given the dependance of the producer price on the world market price, they may even have increased the schooling of their children. Given the fact that cotton farmers knew a different income shock during the period under consideration, it is possible under some further assumptions, which will be discussed below, to make an inference on the causal relationship between income and children's schooling.

3.2 Schooling system

In Burkina Faso basic education includes pre-school classes for a maximum duration of three years, normally children from three to six years can attend. Primary school starts officially at the age of seven and lasts in total six years. Upon successful completion, children receive the *Certificat d'Etudes Primaires* (CEP) which qualifies also for the entry in secondary school.

Secondary school comprises two types of curricula: the general curriculum and a technical curriculum. Lower secondary education lasts four years and ends with the *Brevet d'Etudes du Premier Cycle* (BEPC) in case of the general curriculum and with the *Certificat d'Aptitude Professionnelle* (CAP) in case of the technical curriculum. General higher secondary education lasts three years. Technical higher secondary education can be three years (long) or two years (short). The respective diploma are *Baccalauréat* (BAC), *Brevet de Technicien* (BT) and *Brevet d'Etudes Professionnelles* (BEP). The BAC allows to enter tertiary education.

In principle school is compulsory for the age group six to sixteen. But the law explicitly states that this is conditional on the availability of schools, teaching material and teachers (see e.g., Pilon, 2002). De facto, many children never go to school or if they do, only a few years, in particular in rural areas. In addition school entry is often delayed, repetition rates are high and there is still an important gender gap in rural areas, although it is decreasing.

The schooling system comprises public and private schools. Private schools charge fees. Public schools were always free of charge and parents only had to buy pens, books and a school uniform. Until 2007 it was also custom that parents paid each year 1,000 CFA F (about 10 PPP US$) into the parents association. However, this was abolished in 2007. Now, public and private schools are receiving text books by the government.

4 Data

I use three nation-wide representative household survey data sets, so-called *Enquête Prioritaires* (EP), undertaken in 1994 (EP I), 1998 (EP II) and 2003 (EP III) covering around 8,500 households in each year. These surveys were conducted by the *Institut National de la Statistique et de la Démographie* (INSD) with technical and financial support of the World Bank. These surveys contain relatively detailed information on household's socio-demographic characteristics, education, employment, agricultural and non-agricultural activities as well as consumption, income and some assets. A detailed description of these data sets can be found in Grimm and Günther (2007a).

Information on school enrollment is provided for all children older than five years. For all individuals who ever attained school, the surveys asked for the highest education level achieved. For children older than 9 years, the surveys also inform whether a child worked, e.g. on the household's farm or non-farm business or outside the household.

Given the usual low quality of income data in poor rural settings, I use household expenditure per capita as an indicator of households' living standards. The expenditure aggregate includes self-produced consumption and imputed rents. Expenditures were deflated over time and space using temporal and regional price deflators. Given the above mentioned tremendous changes in relative prices in the second half of the nineties, e.g. the substantial rise in cereal prices, and the significant differences of consumption habits across the income distribution I use decile-specific price indices to deflate expenditures over time. Using simply the general consumer price index would over-estimate the living standard of the poor. This is shown in detail in Grimm and Günther (2007a).

All those farmers who produced at least one kg of cotton in the survey year are coded as cotton farmers. All other farmers are coded as food crop farmers. However, I will test the sensitivity of that definition to alternative assumptions. It should be noted that cotton farmers are, as food crop farmers, usually small family farms with in most cases not more than a view hectares of land.

In what follows I restrict my sample to food-crop and cotton farmers and exclude pure livestock farmers and all other socio-economic groups. I only consider rural areas and limit the sample to households in the south and south-west of the country, excluding the relatively dry tropical savanna in the north. Applying those criteria reduces the sample to in total 6,610 households for all three years together. The area is indicated on the map in Figure 3. Although one can find cotton cultivation almost everywhere in the country, in this area more than 80 percent of the total cotton exports are produced. The oldest cotton provinces are Houet, Kénédougou and Mouhon.

[insert Figure 3]

5 Empirical Evidence

5.1 Income

Table 1 shows the development over time of real household expenditures per capita. The effect of the drought on the purchasing power of farm households is clearly visible. On average expenditures decreased by almost 25 percent in real terms. Afterwards households recovered and attained in 2003 a living standard which was slightly above the one in 1994.

[insert Table 1]

Comparing cotton farmers with food crop farmers, one can state that expenditures of the latter were depressed much more. Whereas expenditures of cotton farmers decreased between 1994 and 1998 by only 16 percent, expenditures of food crop farmers decreased by almost 30 percent. In 2003 cotton households had expenditures 14.5 percent above those in 1994, food crop household just met again the level of 1994.

The shares of the two groups of farmers are presented in brackets of Table 1. As one should expect, households have responded to the opportunities provided by the cotton sector. The share of cotton households, i.e. households which produced at least 1kg of cotton, increased by 13.4 percentage points between 1994 and 1998. Given that households who joined the group of cotton farmers probably allocated less land to cotton production than those households who had cultivated cotton already for a few years, one can assume that the average decline of expenditures observed among cotton farmers between 1994 and 1998 would have been smaller without these 'new' cotton farmers. In a next step, I will analyze whether the income shock between 1994 and 1998 affected children's school enrollment, and if so, whether this effect was different for children of food crop farmers and cotton farmers.

5.2 School enrollment

Column (1) in Table 2 shows the temporal pattern of enrollment rates for children six to thirteen years old of food crop and cotton farmers. This pattern is obtained by regressing enrollment status on cotton and year dummies and cotton and year interaction terms. The regression also controls for age (coefficients not presented in table), relationship to the household head i.e. child of the head or not (coefficients not presented), gender and the interaction of gender, cotton and year effects. Note that on average 22 percent of the boys and 15 percent of the girls within that age group are enrolled.

[insert Table 2]

The results suggest that children of cotton households have on average the same probability to be enrolled than children of food crop farmers. The cotton dummy is not significantly different from zero. In 1998 all children were significantly less likely to be enrolled than in 1994 and 2003. In that year the enrollment probability was on overage 8.7 percent lower than in the two other years. However, for children of cotton farmers this effect was on average much lower. The corresponding coefficient of the cotton-year interaction indicates that these children had in 1998 a probability to be enrolled which was higher by 6.4 percent. This interaction effect is insignificant in 2003.[7]

The other control variables indicate as one can expect in the given context, that boys have in general a higher probability to be enrolled in school. But it is interesting to see that girls were in 1998 less affected by delayed entry and school drop outs than boys. This could be explained by higher opportunity costs of schooling for boys. Below I will analyze this issue in more detail. However, it

[7] The results are qualitatively the same, if a probit model is estimated.

should already be noted that the interaction term of ‘being a boy’ and ‘being a child of a cotton household’ is insignificant. That suggests that boys in cotton households are on average not more likely to be enrolled or to be at work than boys in food crop households.

5.3 Education expenditures

Columns (2) and (3) in Table 2 show the differential development for schooling expenditures per household member and per child enrolled. The result is consistent with the impact observed for school enrollment: A general decline in expenditures in 1998 (although not significant in column (3), but a positive and highly significant impact of the cotton-year interaction term in 1998. Thus in 1998 cotton households reduced significantly less schooling expenditures than food crop farmers. The linear year effect in combination with the interaction effect even suggest that there was no reduction at all for cotton farmers. The cotton dummy is insignificant, i.e. in normal times, there is no difference in schooling expenditures between food crop and cotton farmers.

5.4 Child work

Table (3) shows the results of a multinomial logit regression. The dependent variable is a categorial variable taking the value ‘one’ if the child only attends school, ‘two’ if the child attends school and works, ‘three’ if the child only works and ‘four’ if the child neither attends school nor works. Information on activities, other than schooling is available for children 10 years and older. Hence, I include the age group ten to thirteen in the regression.

[insert Table 3]

Child work is relatively widespread in this age group. In the study area between 60 and 70 percent of all children do some work without attending school and another five to ten percent combine school and work. Children who work, help in most cases (more than 90 percent) on the family farm. Work outside the household is rather an exception. The surveys do not contain any information on working hours. Children were only asked what has been the principal activity, but it was not distinguished between those working and attending school whether one or the other activity was the main activity. I use the same control variables than in Table 2.

The multivariate analysis shows that children in cotton households were working slightly more often than attending school or being inactive than children in food crop households. Moreover, in 1998 all children were more likely to work relative to be at school. They were also less likely to combine work and school or to be inactive, implying that children either worked or attended school in that year. At the sample mean, the probability of working relative to attending school is in 1998 almost 19 percent higher than in 1994 and 2003.

The cotton-time interaction shows that in 1998 children of cotton farmers were much more likely than children of food crop farmers to attend school rather than to work. This effect is highly significant. In that year, children of cotton farmers were also more likely than children of food crop farmers to combine school and work than attending only school. However, this effect is only weakly significant. In 2003 these differential effects between children of cotton and food crop farmers disappeared again.

Regarding the gender differences, boys had a slightly higher probability than girls to combine school and work than to attend school only. Boys were less likely than girls to be inactive. There were no differences between boys and girls specific to the year 1998. However, in 2003 it seems that boys were less likely than girls to work relative to attend to school.

5.5 First conclusions

All results above suggest that food crop farmers were significantly hit by the drought in 1997/98 and that they responded to the associated loss in purchasing power by reducing children's school enrollment and letting them work more often. This suggests, as some other studies have found before, that most rural households in Burkina Faso are unable to smooth consumption over time through credit and/or savings (Reardon, Matlon and Delgado, 1996; Kazianga and Udry, 2006). Moreover, the results imply that risk sharing, which would in this case be possible with cotton farmers, does not happen or, again, happens only to a very limited extent. However, it is possible that food crop farmers benefitted to some extent from the income development of cotton farmers through higher demand for labor or goods produced by food crop farmers and that without these effects the impact of the drought would have been even worse.

5.6 Income elasticity of school enrollment

The simultaneous shocks induced by the drought and the cotton boom constitute a natural experiment which caused an exogenous variation in income over time and household groups. This variation may help to identify the causal impact of income on children's schooling. As discussed in Section 2, there is still an intense debate about the respective roles of income, parent's education, and parents preferences for children's schooling. So far there is almost no evidence for Sub-Saharan Africa, although this region has the lowest education levels in the world.

I only use the period 1994 to 1998 for identification as this is the period in which both shocks occurred. I use expenditure as a proxy for income, thus ignoring the role of savings. This implies that, if consumption smoothing or insurance (or both) takes place, for which again there is only weak evidence in Burkina Faso, the relationship which is analyzed is rather between permanent or average income and schooling than between current income and schooling.

A standard OLS model of the income effect can be written as follows:

$$S_{ijt} = \alpha + \beta Cotton_{ijt} + \gamma Year_{ijt} + \delta \ln Inc_{ijt} + \sum_{k=7}^{13} \zeta_k Age_{ijtk} + X'_{ijt}\eta + \theta_j + \varepsilon_{ijt}. \tag{1}$$

Enrollment, S_{ijt} is a binary variable taking the value one, if the child i, living in province j is enrolled in school in year t. $Cotton_{ijt}$ takes the value one if the child lives in a cotton household. $Year_{ijt}$ takes the value one if the child is observed in 1998. The variable $\ln Inc_{ijt}$ stands for the logarithm of household expenditures. I do not express expenditures in per capita terms, because I assume that this could lead to identification problems in an enrollment equation, given that fertility and educational investments might be jointly determined and have the same unobservable determinants. However, if household composition responds to income shocks, income may have a omitted variable bias (Akresh, 2005). Whether this is an issue will be examined below. The coefficient δ measures the income elasticity of school enrollment.

Age_{ijtk} are age-specific dummies for each age group between seven and thirteen years with the age of six being the reference group. X'_{ijt} is a vector of other household and individual control variables, including parental education, livestock and non-agricultural business ownership, wealth, access to credit, position of the child in the household, and variables reflecting the composition of the household. θ_j are province fixed effects to account for differences in education supply and other province-specific effects which otherwise might be picked up by the remaining included variables if those are correlated over time.[8] Given that the unobservables of children living in the same household are likely to be correlated, I use robust standard errors for inference.

An OLS estimate of the income effect above is obviously subject to number of biases. In principle, the most important ones are the simultaneity bias and the omitted variables bias. The simultaneity bias arises if enrollment is a substitute for child work and thus has a negative impact on household income. Simultaneity would bias δ downward. Given the extent of child work in Burkina Faso, it is likely that this bias arises. However, each of the activities, school and work on the family farm need not necessarily the whole day, both can be combined as seen above, reducing probably the downward bias.

Omitted variable bias can stem from a number of reasons and can introduce a down or an upward bias. For instance, unobserved parental abilities may have a positive impact on income and make it more likely that parents send their children to school. This would upward bias the income effect. Household income could also be correlated with better opportunities for children to get a job which requires a certain level of education. This would increase the expected returns to education and thus again bias the income effect upward. The omission of parental education, household assets and location-specific characteristics could also lead to a biased estimate of δ, but those factors can, at least to some extent, be controlled.[9]

[8]One might prefer to include cluster-specific instead of province-specific fixed effects. However, those units are not constant over time and, hence, I would have to mix the time with the fixed effects. In addition, that would entail problems for the identification strategy explained below, given that many of these cluster would not have enough farmers from either group.

[9]A detailed discussion why marginal private benefits of schooling are likely to be associated with household income in the context of a low income country and why a simple OLS estimate of the income elasticity of schooling might be biased is provided by Behrman and Knowles, 1999).

A further downward bias of the income effect may result from measurement error in the income variable. Although for most industrialized countries that bias should be negligible, in the case of a poor agrarian country that bias can be very important and may even dominate the two other biases. Household surveys of the type undertaken in Burkina Faso ask households for pre-specified recall periods how much they spent on a specific good or group of goods. The recall period is for food usually a week, for clothes, transport etc. a months and for durables, education etc. a year. Obviously the potential error in these types of questions is huge (see e.g., Deaton, 1997; and Deaton and Grosh, 2000).

It is not straightforward to get a rough estimate to what extent measurement error could bias the income effect. Validation surveys are frequently conducted in industrialized countries (see e.g., Bound, Brown and Mathiowetz, 2001), but do not exist for the Sub-Saharan African context. One possibility would be to rely on simulated errors and to make a sensitivity analysis under various assumptions about the variance of the error term and its correlation with income. This is however left for future work.

In order to obtain an unbiased estimate of the income effect on school enrollment, I make use, as mentioned above, of the natural experiment provided by the simultaneous occurrence of the drought and the cotton boom. As an instrument for income, I use 'being a cotton farmer in 1998' conditional on 'being a cotton farmer' and 'being observed in 1998'. Using this instrument, Equation (1) reads:

$$S_{ijt} = \alpha + \beta Cotton_{ijt} + \gamma Year_{ijt} + \delta \ln \hat{I}nc_{ijt} + \sum_{k=7}^{13} \zeta_k Age_{ijtk} + X'_{ijt}\eta + \theta_j + \varepsilon_{ijt}, \quad (2)$$

where the first stage equation is given by

$$\ln Inc_{ijt} = \vartheta + \iota Cotton_{ijt} + \kappa Year_{ijt} + \lambda (Year_{ijt} \times Cotton_{ijt}) + \sum_{k=7}^{13} \mu_k Age_{ijtk} + X'_{ijt}\nu + \xi_j + \omega_{ijt}.$$

This instrumentation yields in fact a 'difference-in-difference' estimator, since it compares within provinces the difference between 1994 and 1998 in the difference in school enrollment between children of food crop farmers and cotton farmers. This kind of instrumentation is also used by Cogneau and Jedwab (2007) to identify the income elasticity of schooling using cocoa price shocks in Côte d'Ivoire.[10] The principle of this approach is of course also very similar to the technique employed by Duflo (2001).

Obviously a number of assumptions have to be verified such that this instrumentation is valid and yields an unbiased income effect. First, the instrument

[10] Rucci (2004) used both in her study, a simple difference estimator and a difference-in-difference estimator. To implement the latter she assumes that children of well educated parents have been differently affected by the crisis than children of less educated parents. She argues that this difference arises, because educated parents might be less credit constrained. However, if this was the case, the measured effect would not be anymore a pure income effect, but an effect due to incomplete markets.

needs to be relevant. That this is the case, was shown in Table 1. Whereas food crop farmers and cotton farmers had a similar living standard in 1994, cotton farmers were significantly richer than food crop farmers in 1998. Second, being a cotton farmer in that particular year 1998 has to be uncorrelated with the error term in the main equation of (2), i.e. the instrument should not have any direct impact on school enrollment other than its impact through income, once the linear effect of time and 'being a cotton farmer' is controlled for. Table 2 shows that the cotton dummy is not significant in a simple enrollment equation suggesting that the instrument is indeed exogenous. In fact, even if the cotton dummy was significant, the instrumentation would be valid, as long as the level effect between food crop and cotton farmers is constant across time.

The instrumentation could still be invalid, if food crop farmers and cotton farmers differed with respect to unobservables which are correlated with the instrument. For example if cotton farmers would have assets which food crop farmers do not have and if these assets allowed cotton farmers to cope more easily with shocks. Table 4 shows the education related observables in our sample. If for those variables we state no significant differences between the children of food crop farmers and cotton farmers, we may hope that there are also no substantial differences in the unobservables, but of course we cannot be sure that this is true.

[insert Table 4]

Table 4 shows that for most variables there are indeed no significant differences. The difference in the mean age between 1994, 1998 and 2003 corresponds to the difference of the time in which these surveys have been carried out. The differences in the relationship of the children to the household head and household size between food crop and cotton farmers and the change of these differences over time, suggest that some children from food crop farmers were fostered by cotton farmers after the drought. Akresh (2005) has provided evidence that, for instance, households in the Bazega province—which is outside the area which is covered by this paper—rely on child fostering to mitigate shocks.[11] There is no significant age difference for household heads between both groups, but household heads in cotton households are more often a male. Household heads have slightly more education in cotton households, but this is mainly due to the difference in household head's gender. Cotton households are a bit wealthier, own more often livestock and run more often also a non farm business. However, they are more often credit constrained than food crop farmers, although being credit constraint is only rudimentarily measured. A household is considered as being credit constrained if it requested an agricultural credit, but did not get one.[12] All these differences in observables will be controlled.

[11] In another paper Akresh (2004) shows that the foster children are equally likely as their host siblings to be enrolled and they are slightly more likely to be enrolled than their biological siblings, but both the foster children and their biological siblings experience increased enrollment after the fostering exchange.

[12] In 2005, in Burkina Faso only 22.5 percent of the households benefitted from credits from micro-lending institutions (African Development Fund, 2006). Cotton farmers have in

Finally the instrumentation would also be invalid, if those households who became 'new' cotton farmers between 1994 and 1998 did switch to that activity for the purpose of ensuring that their children can go or stay in school. Professional mobility, like geographical mobility, could in addition invalidate the instrumentation strategy, if both processes led to differences in the unobservable determinants of enrollment within each group between both years. In other words the control and treatment groups would not exactly be the same over time. Again, it is difficult to rule out completely this potential bias, but I will provide below a number of robustness checks which all indicate that there is no substantial bias in that direction. For instance, I will introduce an interaction term for having been a cotton farmer in 1998, but not in 1997. I will also run the regression by excluding completely from the estimation those who joined after 1997. I will also change the definition of cotton farmers according to the amount of cotton produced or according to the share of cotton income in total agricultural income.

Table 5 shows the estimation results. Given the differences in enrollment patterns for boys and girls, I run the estimations separately for boys and girls. For boys the OLS estimate in column (1) suggests that a 10 percent increase in household income leads to an increase in the probability of being enrolled of 0.64 percent, controlling for age, household composition characteristics, parental education, livestock ownership, non-farm business ownership and province fixed effects. For girls (column (7)) this elasticity is 0.49 percent. Both coefficients are highly significant. The cotton dummy is insignificant, supporting the identification strategy for the income effect below. The 1998 year dummy indicates that in 1998 enrollment rates were on average lower by 7 percentage points for boys and 4.5 percentage points for girls. Parental education has (or more precisely education of the household head and his/her spouse) a significant positive impact on enrollment rates, in particular for girls. Ownership of a private non-farm business has also a positive impact, in particular for boys. This could suggest two things. First, a wealth effect or, second, a returns to education effect. If education is particularly valuable for running a non-farm business, parents owning such a business may invest more in the education of their children. Livestock ownership has no significant impact.

[insert Table 5]

If income is instrumented (columns (2) and (8)) the income effect rises substantially. For boys the income effect increases to 0.268, suggesting that an increase of income by 10 percent increases the probability of enrollment, on average, by 2.7 percent, that is roughly four times the effect suggested by the OLS regression. The F-statistic in the corresponding first-stage regression is far above the critical value of ten, indicating again that the used instrument is relevant. For girls the instrumented income effect is even higher than for boys, and again, the F-statistic indicates that the instrument is relevant. The cotton dummy is still insignificant. The effects of parental education decrease

addition access to credit to finance their inputs such as seeds and fertilizer, but these credits cannot be used to cover other expenditures.

significantly, for both boys and girls, showing that a simple OLS estimation understates the income effect and overstates the parental education effect. Business ownership is not anymore significant. The comparison of the OLS income effect with the instrumented income effect implies that the simultaneity bias and the measurement error bias dominate. Of course, it would be interesting to disentangle both, but again this will be left for future work. But, it is likely that in particular the measurement error bias leads to such a low OLS estimate.

To investigate in more detail whether wealth ownership or access to credit introduces a bias into the instrumented income effect, I add as regressors in columns (3), (4), (9) and (10) a wealth index and the above mentioned measure of credit restrictions. Introducing these controls can avoid a bias which can stem from the fact that cotton households have on average higher wealth holdings than food crop households. Introducing these controls lowers the OLS income effect, but does not alter the instrumented income effect, again supporting the identification strategy. Finally, I add the square root of household size as a control variable (columns (5), (6), (7) and (8)) accounting for the fact that households may respond to income shocks by adjusting household structure. Indeed the income effect goes further up, showing that income and household size are positively correlated. Cotton households might have received foster children of food crop households after the drought leading to a lower growth in per capita terms than in absolute terms. This can also explain the lower F-statistic, though still above ten for boys and girls.

5.7 Robustness checks

As discussed above, activity changes, and in particular farmers who started to grow cotton following the price incentives may bias the estimates of the income effect. To test the robustness of the estimates in Table 5, I re-estimate the model under various alternative assumptions. Table 6 shows the results.

First, I introduce in Equation (2) an interaction term between 'being a cotton farmer in 1998' and 'having not been a cotton farmer the season before'. For boys, the income effects for both estimations, OLS and IV, remain more or less unchanged. The F-statistic of the first-stage regression of the IV estimation goes even further up, showing that once controlled for the 'newcomer status', 'being a cotton farmer in 1998' is even more correlated with income. For girls, the interaction term is significant. The F-statistic goes down and the instrumented income effect becomes insignificant, although the size of the income effect is not much altered. In the next row, I exclude all households who joined the cotton sector after 1997 from the estimation. The F-statistics for boys and girls increase substantially. The instrumented income effect for boys is close to the estimate in Table 5 and for girls the effect converges to that for boys.

[insert Table 6]

Another way of dealing with the problem of households which switched to cotton production is to exclude those households which produced only relatively small quantities of cotton. The assumption is that new cotton farmers allocate on average less land to cotton than well-experienced cotton farmers.[13] Of course, the risk is to exclude also systematically cotton farmers who have not much land and are thus relatively poor. I use two alternative cut-off points. The first eliminates all farmers who produced less than 50kg of cotton, which removes 10 percent of the cotton farmers from the sample. The second cut-off point eliminates all farmers who produced less than 250kg of cotton, which removes 25 percent of the cotton farmers from the sample.[14] With the first cut-off point the results for boys lead to a lower IV estimate and a similar First-stage F-statistic. For girls the instrument becomes weak and the income effect turns out be insignificant. Using the second cut-off point, the instrumentation looses also its power for boys and the instrumented income effect goes substantially up. However, the second cut-off point is really far beyond the upper bound and removes obviously not only many 'newcomers', but many poor cotton farmers in general.

Finally, I exclude those households from the cotton sample which draw only a relatively small share of their total agricultural income from cotton. Again I use two cut-off points, the first at 10 percent (removing 9 percent of all cotton households from the sample) and the second at 50 percent (removing 27 percent). In all cases, for boys and girls, the F-statistics go up, that is what one expects. Of course the higher the share of income a farmer draws from cotton, the more his income was (positively) affected in 1998. The IV estimates of the income effect go down for boys and girls, and more so, when the higher cut-off point is used. With the 50 percent cut-off point the income elasticity of enrollment is 0.219 for boys and 0.276 for girls.

These robustness checks suggest that activity changes did not drive the results and did not lead to a substantial bias. Thus, income matters. The true average income effect seems to be between 0.22 and 0.26, that is between three and four times the OLS effect. However, the OLS estimate might be strongly downward biased, not only because of a 'simultaneity bias' but also because of 'measurement error' bias. Again, exploring the pure role of measurement error is left for future work.

6 Policy simulations

In what follows, I use the estimated income effect on children's schooling to explore the potential effects of unconditional transfer programs on school enrollment rates of children six to thirteen years old. At this stage I rely on a pure accounting micro-simulation method. I neglect any further behavioral reactions by households for instance regarding labor supply, crop choice or household composition. I also discard any general equilibrium effects, which might result from such transfer programs and I ignore how such programs could be financed. However, the simulation relies on an estimated income effect which

[13] Note that the size of the plots allocated to cotton and cultivated land size in general is not available in the surveys.

[14] Note that quantities can only be computed approximately since respondents had the possibility to provide the quantities in terms of the number of baskets, sacks etc. These information where then converted in number of kg.

should be closer to the true causal effect than income effects based on simple cross-section correlations between income and enrollment. This quantitative difference between both effects is illustrated in Figure 4.

[insert Figure 4]

Kakwani, Soares and Son (2006), for instance, perform similar simulations for a large set of African countries (including Burkina Faso). They rely on the non-instrumented income effect, which is similar to the one estimated in this paper when simply OLS is used. Thus, their simulations should systematically underestimate the effects of unconditional cash transfers on enrollment rates.

I focus on the predicted average effect and assume that residuals are normally distributed. In each simulation, I compute for each individual the score of being enrolled, $\tilde{S_{ijt}}$:

$$\tilde{S}_{ijt} = \hat{\delta}\tilde{Inc}_{ijt} + \phi\bar{\Omega}_{ijt}, \tag{3}$$

where $\hat{\delta}$ stands for the estimated income effect in Equation (2), $\tilde{Inc}_{ijt}$ stands for the household income after cash transfers have been made and ϕ stands for the effect of all other variables in Equation (2), $\hat{\Omega}_{ijt}$. The latter are held constant in the simulations. Thus, the principle of the simulations is to compute what happens to the average enrollment rate $\bar{S} = \tilde{S}_{ijt}/n$ if income for each individual i is shifted from Inc_{ijt} to $\tilde{Inc}_{ijt}$.

I simulate three types of transfer schemes. First, one where all households with children in the relevant age group receive the same amount of cash, expressed as a share of the yearly per capita poverty line of 53219 CFA Francs (Grimm and Günther, 2007a). Second, one where specific households are targeted dependent on their income or location they live in. Third, one where cash transfers are higher for girls than for boys. Table 7 shows the results. The population on which the simulations are done, is that of 1998.

[insert Table 7]

First of all one can see that the prediction for the baseline case accurately represents the observed average enrollment rate. The average enrollment rate is very low, but it should be noted that I look only at rural households. Cash transfers of 25 percent of the poverty line tend to increase the average school enrollment rate in the study region by 4 percent (simulation 1). A transfer of 200 percent of the poverty line is necessary to lift that average by 30 percent (simulation 4).

If the transfers are targeted to the children of the poorest 25 percent of all households and the same total amount of money is distributed than in the case of a 100 percent-of-the-poverty-line-transfer-to-all-children (simulation 3), the average enrollment rate goes up by 23.7 percent (simulation 5). That is much more than in the case where the same amount of cash is universally distributed to all children. The effect is even higher if not the poorest 25 percent but the poorest 50 percent are targeted holding constant the total amount of transfers (simulation 6).

Obviously, the involved administrative costs in such a targeting conditional on income might be high. An alternative might be to target entire provinces or villages conditional on their average enrollment rates. If the 11 provinces with the lowest enrollment rates are targeted covering roughly 50 percent of all children, the enrollment rate goes up by 17.2 percent (simulation 7) and if instead of provinces, the most needy villages are targeted it goes up by 16.1 percent (simulation 8). Thus, it is better (and probably cheaper) to target entire provinces than entire villages (ignoring all other aspects, which might justify an intervention at a specific location). However, both 'policies' are worse than the transfers targeted conditional on household income.

The last two simulations favor girls relative to boys. The first simulation examines the effects of providing transfers to all children but to provide twice as much to girls than to boys. The average effect is not much different than under simulation 3, however, girls' enrollment should of course be higher. The last simulation targets girls in the most needy provinces, thus it combines simulation 7 and 9. Again the effect is similar to the balanced transfers to provinces (simulation 7), but again girls' enrollment should be higher.

In summary, although these results should be seen in relation to each other and in terms of their direction and should certainly not be interpreted as an accurate specification of the potential effects if such policies were really implemented, they have some interesting implications. First, the results suggest that unconditional transfers have a higher potential effect than often believed. Kakwani *et al.* (2006) found only very modest effects of income transfers. This study shows that they probably underestimated the income effect and hence underestimated also the potential impact of cash transfer programs. However, even if the involved effects are higher in this paper, it seems likely that the efficiency of such transfers can be increased if they can be made conditional on enrollment, given that this would reinforce the income effect through a reduction of the shadow price of schooling.[15] This of course requires that institutions are built which can implement such schemes. The simulations in this study also show that transfers targeted to the poor population in general might be more efficient than universal transfers or location-specific targeting. However, in practices one has to account for the costs of such a targeting. An issue not addressed in this paper.

7 Concluding remarks

The distributional effects of rising food prices between 1994 and 1998 in Burkina Faso, driven by among other things the drought of 1997/98, have been overseen by several previous studies. This paper shows that there was not only a substantial impact on household income but also on children's schooling. The exact effects are identified using a natural experiment, provided by the fact that in the same time the drought occurred the cotton sector knew an notable boom caused by the CFA Franc devaluation in 1994 and a favorable development of

[15] Bourguignon Fereirra and Leite (2003) find a similar result for Brazil. See also De Janvry, Finan, Sadoulet and Vakis (2006).

the world market price for cotton. Thus, cotton farmers had an exogenous variation in income which was different from the variation faced by food crop farmers. Incomes of the latter declined almost twice as much as those of the former.

This study provides evidence that in order to cope with the shocks food crop farmers significantly decreased enrollment of their children (either by withdrawing enrolled children or by not enrolling children at the first place) and let them work in higher numbers, mostly on their family farm. This differential effect is also reflected in schooling expenditures.

This study also shows that the income elasticity of enrollment is much higher than a simple OLS regression would suggest. Thus income matters. The substantial downward bias of the OLS estimate is probably caused by both, a simultaneity bias and a non-negligible measurement error bias. So far, the causal impact of income on schooling has only rarely be shown for the case of Sub-Saharan Africa. It suggests that cash transfer programs, could in principle also be successful in this region. This is illustrated using simple policy simulations under various targeting rules. The main obstacle for their implementation is in most countries of that region obviously the lack of institutional and financial capacity to run such a program.

Besides the need for interventions to raise enrollment levels in general, which again could in principle be achieved through (conditional) cash transfer programs, there is of course a need for safety nets to ensure that children have not to be withdrawn from school in periods of economic hardship. Even delayed enrollment and enrollment interruptions can have adverse effects on educational achievements. Safety nets could be provided by income assistance during periods of crisis or by improved access to credit and savings to allow households to smooth more easily consumption.

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Tables and Figures

Table 1
Yearly household expenditure per capita
(in 1000 CFA Francs, population weighted)

	1994		1998		2003	
All Farm Househ.	61.7		47.0		64.6	
	(100.0)		(76.2)		(104.8)	
Cotton Househ.	62.0	[0.182]	52.0	[0.316]	71.0	[0.370]
	(100.0)		(83.8)		(114.5)	
Food Crop Househ.	61.5	[0.674]	43.4	[0.599]	59.4	[0.517]
	(100.0)		(70.5)		(96.6)	

Notes: In parentheses changes over time (1994=100). In brackets, share of cotton and food crop households respectively.

Source: EP1, EP2, EP3; estimations by the author.

Table 2
Temporal pattern of differential enrollment rates and school expenditures
Cotton vs. Food Crop Households
Regression effects

	(1) OLS School enrollment 6-13 years old			(2) Tobit Schooling expend. per househ. member			(3) Tobit Schooling expend. per child enrolled		
	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.
Cotton Househ.	-0.001		0.029	0.795		0.511	0.865		0.594
Year 1998	-0.081	***	0.022	-1.398	***	0.327	-0.232		0.385
Year 2003	-0.005		0.024	-0.305		0.330	0.616		0.383
Cotton × Year 1998	0.064	**	0.032	1.462	**	0.620	1.388	*	0.730
Cotton × Year 2003	-0.019		0.033	-0.372		0.617	-0.502		0.722
Boy	0.101	***	0.023						
Boy × Cotton	0.011		0.017						
Boy × Year 1998	-0.044	*	0.025						
Boy × Year 2003	-0.026		0.026						
Household Head Male				0.513		0.423	0.873	*	0.496
Observations	12273			6645			3897		

Notes: Standard errors are robust to arbitrary forms of heteroskedasticity and permit within-family correlations among unobservables. $*$ significant at the 10% level, $**$ significant at the 5% level, $***$ significant at the 1% level. Stars refer to standard errors of regression coefficients, not to those of marginal effects. Regression (1) also controls for age and relationship of child to household head. Intercept included but not reported here. Baseline year is 1994.

Source: EP1, EP2, EP3; estimations by the author.

Table 3
Temporal pattern of school enrollment, child work and inactivity
Cotton vs. Food Crop Households, Children 10 to 13 years old
Marginal effects from a multinomial logit, enrollment is baseline outcome

	School and work			Work			Inactivity		
	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.
Cotton Househ.	0.012	*	0.012	0.093	*	0.045	-0.028		0.017
Year 1998	-0.030	*	0.011	0.143	*	0.039	-0.056	**	0.013
Year 2003	-0.053	***	0.011	0.038		0.040	-0.018	*	0.010
Cotton × Year 1998	0.027		0.021	-0.185	***	0.058	0.038		0.033
Cotton × Year 2003	-0.019		0.009	-0.055		0.057	0.006		0.023
Boy	0.014	*	0.008	0.013		0.038	-0.075	***	0.017
Boy × Cotton	-0.013	*	0.008	-0.012		0.030	0.016		0.017
Boy × Year 1998	0.004		0.010	-0.061		0.045	0.036		0.025
Boy × Year 2003	0.059		0.048	-0.122		0.054	0.056	*	0.029
Observations					5319				

Notes: Standard errors are robust to arbitrary forms of heteroskedasticity and permit within-family correlations among unobservables. $*$ significant at the 10% level, $**$ significant at the 5% level, $***$ significant at the 1% level. Regression also controls for age and relationship of child to household head. Intercept included but not reported here. Base year for year effects is 1994.

Source: EP1, EP2, EP3; estimations by the author.

Table 4
Characteristics of children (6 to 13 years old) and the households they live in

	1994		1998		2003	
	Food crop	Cotton	Food crop	Cotton	Food crop	Cotton
Age	9.054	9.055	9.202	9.184	9.205	9.113
Boy	0.540	0.552	0.533	0.545	0.514	0.516
Child of househ. head	0.783	0.821	0.872	0.773	0.880	0.899
Household Size	9.101	10.959	8.795	11.213	8.051	9.067
Share women in househ.	0.496	0.477	0.509	0.492	0.522	0.505
Share children 6-13 in househ.	0.294	0.277	0.323	0.289	0.314	0.298
Household head male	0.914	0.967	0.919	0.974	0.896	0.978
Household head age	46.358	45.229	47.711	47.318	47.830	46.260
Househ. head migrated last 5 years	0.062	0.025	0.066	0.055	n.a.	n.a.
Househ. head some primary	0.061	0.089	0.060	0.090	0.066	0.097
Househ. head primary completed	0.028	0.056	0.029	0.055	0.030	0.057
Spouse of head some primary	0.045	0.054	0.047	0.051	0.047	0.059
Household owns livestock	0.669	0.776	0.798	0.920	0.796	0.933
Household owns non-farm business	0.516	0.596	0.266	0.276	0.391	0.358
Value of asset index	-0.757	-0.514	-0.836	-0.400	-0.785	-0.544
Household credit constrained	0.035	0.046	0.036	0.072	0.269	0.384

Notes: The means of 'Age', 'Boy' and 'Child of household head' are computed over all children. The remaining variables are means over all households to which the children belong. The questionnaire on 'Non farm business' varies slightly from year to year. The asset index includes ownership of the following assets: radio, TV, bike, motorbike, fridge, connection to electricity, connection to taped water, modern toilet, good floor and wall materials. The index is computed on the national level using principal component analysis. The national average is normalized to zero in each year. A household is defined as credit constrained, if it asked for agricultural credit, but did not get it.

Source: EP1, EP2, EP3; computations by the author.

Table 5
The income elasticity of school enrollment, 1994 – 1998
Children 6 to 13 years old

		(1)			(2)			(3)			(4)			(5)			(6)	
		OLS			2SLS			OLS			2SLS			OLS			2SLS	
	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.
Boys																		
Main equation																		
Cotton Househ.	-0.012		0.022	-0.040		0.028	-0.010		0.021	-0.040		0.027	-0.011		0.022	-0.019		0.021
Year 1998	-0.071	***	0.019	-0.092	***	0.021	-0.075	***	0.018	-0.094	***	0.020	-0.072	***	0.019	-0.114	***	0.031
Ln Expenditure (IV)	0.064	***	0.012	0.268	**	0.120	0.029	**	0.013	0.265	**	0.127	0.067	***	0.015	0.373	**	0.187
HH head some primary	0.101	***	0.034	0.088	**	0.035	0.079	**	0.034	0.081	**	0.034	0.100	***	0.034	0.065		0.041
Spouse some primary	0.190	***	0.053	0.151	***	0.051	0.154	***	0.059	0.140	***	0.047	0.189	***	0.053	0.137	***	0.053
HH owns livestock	-0.016		0.022	-0.099	*	0.053	-0.011		0.021	-0.100	*	0.052	-0.015		0.022	-0.074	*	0.042
HH owns non-agri. bus.	0.072	***	0.019	0.041		0.025	0.053	***	0.018	0.035	*	0.019	0.073	***	0.019	0.050	**	0.022
Wealth index							0.075	***	0.010	0.020		0.031						
Credit constrained							0.036		0.054	-0.014		0.057						
HH size (Square root)													-0.006		0.012	-0.136	*	0.081
Provincial fixed effects		yes			yes			yes			yes			yes			yes	
First stage																		
IV: Being a cotton																		
farmer in 1998				0.291	***	0.050				0.274	***	0.048				0.196	***	0.044
F-Stat					33.5						32.9						19.7	
Observations		4359			4359			4359			4359			4359			4359	

Notes: See next page.

Table 5 (... *continued.*)

	(7)	OLS		(8)	2SLS		(9)	OLS		(10)	2SLS		(11)	OLS		(12)	2SLS	
	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.
GIRLS																		
Main equation																		
Cotton Househ.	0.009		0.022	-0.042		0.043	0.007		0.022	-0.038		0.034	0.015		0.022	-0.005		0.026
Year 1998	-0.045	**	0.018	-0.077	***	0.027	-0.047	***	0.018	-0.074	***	0.022	-0.049	***	0.018	-0.094	***	0.036
Ln Expenditure (IV)	0.049	***	0.012	0.308	*	0.183	0.019		0.012	0.297	*	0.160	0.066	***	0.015	0.363	*	0.217
HH head some primary	0.161	***	0.040	0.140	***	0.038	0.147	***	0.039	0.147	***	0.035	0.155	***	0.039	0.108	**	0.048
Spouse some primary	0.158	***	0.048	0.123	**	0.054	0.128	***	0.047	0.134	***	0.048	0.161	***	0.047	0.142	***	0.047
HH owns livestock	0.037		0.023	-0.067		0.076	0.038	*	0.023	-0.059		0.059	0.040	*	0.023	-0.034		0.057
HH owns non-agri. bus.	0.030	*	0.017	-0.016		0.036	0.017		0.017	-0.009		0.021	0.033	*	0.017	0.009		0.023
Wealth index							0.058	***	0.012	-0.018		0.044						
Credit constrained							0.048		0.040	0.003		0.047						
HH size (Square root)													-0.028	**	0.012	-0.152	*	0.092
Provincial fixed effects		yes			yes			yes			yes			yes			yes	
First stage																		
IV: Being a cotton farmer in 1998				0.189	***	0.057				0.213	***	0.053				0.160	***	0.049
F-Stat					11.1						16.3						10.6	
Observations		3708			3708			3708			3708			3708			3708	

Notes: Standard errors are robust to arbitrary forms of heteroskedasticity and permit within-family correlations among unobservables. $*$ significant at the 10% level, $**$ significant at the 5% level, $***$ significant at the 1% level. Regressions control also for age, being the eldest child in the household, being a child of the household head, the share of female household members, the share of children 6-13 years old in the household and whether the household head is a male. Intercept included but not reported here. The first-stage regression includes also all other variables from the main equation as instruments. Base year for year effects is 1994.

Source: EP1, EP2, EP3; estimations by the author.

Table 6
The income elasticity of school enrollment, 1994 – 1998 — Robustness checks
Children 6 to 13 years old

	Boys						Girls					
	(1) OLS			(2) 2SLS			(3) OLS			(4) 2SLS		
	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.
Interaction term: Household was not a cotton household in previous year												
Cotton Househ.	-0.015		0.023	-0.047		0.030	-0.010		0.024	-0.054		0.044
Cotton × No Cott. in $t-1$	0.013		0.033	0.041		0.033	0.081	**	0.037	0.097	***	0.034
Ln Expenditure (IV)	0.064	***	0.012	0.249	**	0.111	0.050	***	0.012	0.253		0.171
First stage F-Stat					37.9						11.8	
Without those households having not been a cotton household in previous year												
Ln Expenditure (IV)	0.060	***	0.013	0.269	***	0.103	0.050	***	0.012	0.239	*	0.144
First stage F-Stat					44.6						16.3	
Without those households having produced less than 50kg cotton (removes 10% from sample)												
Ln Expenditure (IV)	0.066	***	0.013	0.226	*	0.119	0.053	***	0.012	0.271		0.231
First stage F-Stat					30.4						5.9	
Without those households having produced less than 250kg cotton (removes 25% from sample)												
Ln Expenditure (IV)	0.062	***	0.013	0.410	*	0.224	0.051	***	0.012	0.600		0.394
First stage F-Stat					10.3						3.1	
Without those households where cotton income share less than 10% (removes 9% from sample)												
Ln Expenditure (IV)	0.061	***	0.013	0.229	**	0.109	0.048	***	0.012	0.347	**	0.149
First stage F-Stat					39.3						17.9	
Without those households where cotton income share less than 50% (removes 27.7% from sample)												
Ln Expenditure (IV)	0.065	***	0.012	0.219	**	0.126	0.052	***	0.012	0.276	*	0.147
First stage F-Stat					27.6						17.0	

Table 6 (... *continued.*)

Notes: Standard errors are robust to arbitrary forms of heteroskedasticity and permit within-family correlations among unobservables. $*$ significant at the 10% level, $**$ significant at the 5% level, $***$ significant at the 1% level. Regressions correspond to those presented in Table 5.

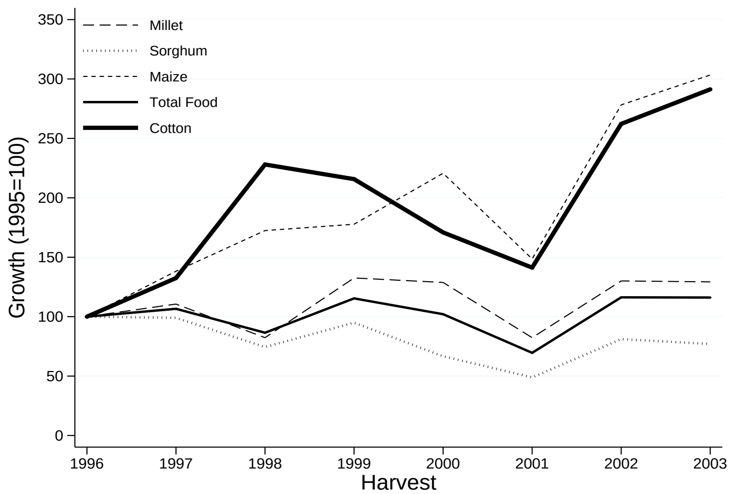
Source: EP1, EP2, EP3; estimations by the author.

Table 7
Simulation of various unconditional transfer programs
Children 6 to 13 years old, population 1998
(Simulations use parameters of Table 5, cols. (4) and (10))

				Enrollment rate
	Baseline 1998 observed			0.189
	Baseline 1998 predicted			0.187
		Percentage of children benefitting	Transfer per child as percentage of poverty line	Percentage deviation from baseline
(1)	Universal transfer of 25 percent of poverty line	100	25.0	4.4
(2)	Universal transfer of 50 percent of poverty line	100	50.0	8.6
(3)	Universal transfer of 100 percent of poverty line	100	100.0	16.5
(4)	Universal transfer of 200 percent of poverty line	100	200.0	30.7
(5)	Transfer sum of (3) distributed to households of poorest quartile	13	750.2	23.7
(6)	Transfer sum of (3) distributed to households of poorest two quartiles	34	293.2	24.4
(7)	Transfer sum of (3) distributed to 11 provinces with lowest enrollment rates	50	213.1	17.2
(8)	Transfer sum of (3) distributed to 65 villages (EU) with lowest enrollment rates	50	201.8	16.1
(9)	Transfer sum of (3) universally distributed, but double transfer to girls	100	68.6 (Boys) 137.2 (Girls)	16.7
(10)	Transfer sum of (3) distributed to 11 provinces with lowest enrollment rates, but double transfer to girls	50	144.7 (Boys) 289.4 (Girls)	17.2

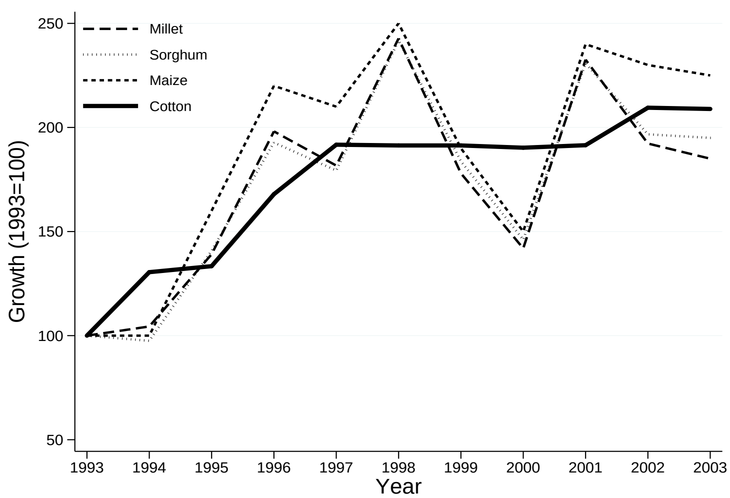
Source: EP1, EP2, EP3; estimations and simulations by the author.

Figure 1
Production of main cereals and cotton (in tons)



Notes: Total food means tons of millet, sorghum and maize.
Source: Economic Accounts for the Agricultural Sector, based on Enquête Agricole (data not available for harvests before 1996).

Figure 2
Consumer prices of main cereals and cotton producer price

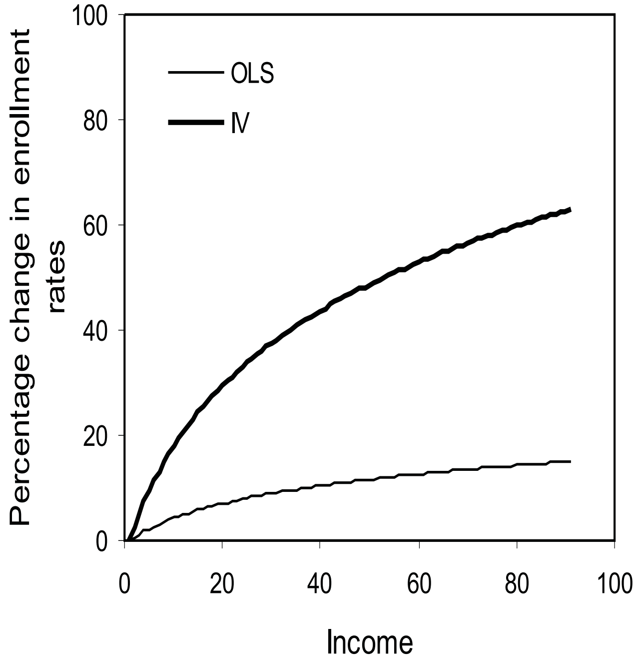


Notes: Annual average prices (collected on 37 different regional markets).
Source: Cereal prices: Grain Market Price Surveillance System, Ministry of Trade. Cotton price: Ouedraogo, Sanou and Sissao (2003).

Figure 3
Provinces included in empirical analysis

Source: United Nations.

Figure 4
Relative change in enrollment rates by income



Source: Simulations by the author.