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Who Gets to Stay in School? Long-run Impact of Income Shocks on Schooling in Rural Tanzania

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Abstract

This paper shows that income shocks to rural households have permanent effects on the educational attainment of 7-15 year old children within the household. Using a 13 year panel survey of households in rural Tanzania, I find that idiosyncratic crop shocks such as pests, theft and fire cause changes in the distribution of schooling among children within the household that persist 10-13 years after the shock. They affect older (12-15) girls and younger (7-11) boys most adversely. The effects are remarkably persistent in households affected by shocks of varying magnitudes. An investigation of plausible channels for these effects suggests that an increase in the chore burden of older girls within the household in response to a crop shock is likely to be part of the explanation for the adverse effect of shocks on this cohort.

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1.1. Introduction

There is a growing body of evidence showing that income shocks can have adverse effects on child welfare in households that are unable to smooth consumption (e.g. Jensen, 2000; Alderman et al, 2006; Dercon & Hoddinott, 2004), and that these effects differ by individual characteristics of the child. The evidence on the permanence of these effects and the channels for them is much more limited. This paper focuses on whether adverse effects of idiosyncratic income shocks on education persist into adulthood and whether these effects differ by gender and age at the time of the shock. I utilise a 13 year individual level panel data-set from rural Tanzania to examine the impact of crop shocks on the within childhood household distribution of final schooling attainment among those who were between the ages of 7 and 15 at the time of the shock.

Much of the existing literature on the effects of income volatility emphasises household level responses. Seminal works by, among others, Townsend (1994), Kochar (1999), and Udry (1990), focus on the ex-ante and ex-post coping strategies adopted by households in dealing with uninsured risk in the context of volatile incomes and imperfect or absent credit and insurance markets. Existing work on individual level impacts of income shocks is less well-established, although some persistent trends are emerging.

Broadly, the results indicate that at the individual level income volatility can have substantial adverse effects on health and educational achievement, especially among children (Jensen, 2000; Hoddinott & Kinsey, 2001; Beegle et al, 2006; Bengtsson, 2010; Guarcello et al, 2010). In addition, there are strong indications that these effects are highly heterogeneous. This is entirely consistent with more general evidence that individual characteristics such as gender, age and birth-order are important determinants of investment into children and the role of children within the household (Edmonds, 2006; Ejrnaes & Portner, 2004; Kaestner, 1997). The existing evidence on heterogeneity of shock effects on children singles out younger children and girls as particularly vulnerable groups (Rose, 1999; Hoddinott & Kinsey, 2001; Dercon & Hoddinott, 2004; Duryea et al, 2007; Bengtsson, 2010).

There are two important gaps in this literature. The first relates to the permanence of the short-term effects. Existing findings do suggest that some of the short-term effects are likely to have consequences that persist into the medium-term. These include loss of height, delayed enrolment, reduction in grade completion and increased probability of dropping out of school (Jocoby & Skoufias, 1997; Alderman et al, 2006; Dureya et al, 2007). The persistence of these effects into adulthood and their permanence, remain strikingly underexplored.

The second gap is methodological; the majority of existing estimates of individual and household level shock effects do not address the endogeneity of shocks. While the shocks considered, whether covariate or idiosyncratic, tend to be unpredictable to the household, household vulnerability and response to them cannot be considered random. This source of endogeneity is explored more widely in literature on health shocks which emphasises the power of certain characteristics, such as wealth and education, in predicting future health shocks (Mohanan, 2008; Adams et al, 2003; Smith, 2003). The same concerns apply in considering income shocks; the estimated relationships between shocks and child outcomes may reflect spurious correlations, if the characteristics that make some households more likely to be affected by income shocks also affect the outcomes of interest[1]. Without explicitly addressing this potential source of bias in the empirical strategy, causal interpretation of the findings is difficult.

This paper aims to address both of these gaps by examining whether idiosyncratic household income shocks have permanent effects on children's schooling in rural Tanzania at a within childhood household level. It builds on earlier findings by Beegle, Dehejia and Gatti (2006), henceforth referred to as BDG, that income shocks in rural Tanzania have short-term adverse effects on child time-allocation; they find that these shocks increase the amount of time spent on chores and reduce the probability of school attendance. I utilise a follow-up round of data on the same children collected 10-13 years after the shock to examine whether the effects of these

[1] For instance, a well-established view in the education literature is that in rural settings returns to education are realised through enabling households to make decisions that improve their capacity for dealing with disequilibrium (Schultz, 1975). Since education is a key determinant of many other behaviours and outcomes, omission of a complete set of controls for education in investigating the effect of shocks may lead to wrongly attributing the impact of education to shocks.

shocks persist into adulthood. All of the individuals in the sample completed their schooling by this time, so that the measure of schooling reflects final educational attainment with little likelihood of further catch-up. Further, the shock effects are identified from within household variation in outcomes only, which greatly reduces the risk of the results being confounded by unobservable household characteristics.

In line with the literature, I find strong evidence of gender and age-cohort heterogeneity in the within household impact of income shocks on two measures of final educational attainment – primary school completion and total years of schooling. A household income shock results in a permanent decline in the probability of primary school completion of older girls (age 12-15) relative to most of the other children in the household, eliminating the advantage they have relative to younger girls in non-shock households and creating a 10 percentage point gap relative to their male peers. The exception to this trend are younger boys (age 7-11), who themselves suffer some adverse shock affects. For them, however, the effect comes through most strongly with respect to completed years of schooling, expanding the gap with older boys by roughly one year and eliminating the advantage they have relative to younger girls in non-shock households.

These effects constitute new evidence that the inability of households to insure themselves against income fluctuations can have persistent and even permanent effects. How precisely these set in, however, remains a question. The last part of the paper is dedicated to a careful consideration of this issue. This part of the paper is strictly exploratory and does not aim to robustly identify the mechanism(s) for the estimated effects. Instead, it utilises insights from the existing literature on how decisions regarding child schooling are made within the household (Orazem & King, 2008; Edmond, 2006) and what effects income shocks can have in this context (Jacoby & Skoufias, 1997) to suggest a plausible narrative for some of the findings. In particular, I look at whether in the short-term shocks affect allocation of child time between school and work and examine whether these short-term fluctuations have permanent effects on schooling[2]. I also consider the possibility that shocks have adverse effects on health in the short-run which translate into long-term loss of schooling; the intuition for this is founded on the persistent findings in the literature that shocks have adverse effects on child health and that these effects differ by age and gender of the child (e.g. Rose, 1999; Bengtsson, 2010; Hoddinott & Kinsey, 2001).

The most suggestive finding that emerges is the positive relationship between shocks and time spent doing chores by older girls. Although older girls in non-shock households already spend up to 5.5 hours a week more time on chores than the other children in the household, these gaps increase significantly following a shock. I use a 2SLS model to explore whether this shock-induced variation in child work has any power in explaining the longer term impact of shocks on schooling. I utilise the within household gender and age-cohort specific variation in shock effect on time spent on chores to instrument for time spent on chores in childhood and estimate its long-run impact on final schooling. The results do indeed suggest that time spent on chores in childhood is negatively associated with the probability of primary school completion.

I do not draw definitive conclusions from these results because, although they are consistent with other findings in the literature on the relationship between child labour and schooling (Patrinos & Psacharopoulos, 1995, 1997; Ray & Lancaster, 2005; Bezerra 2007; Beegle et al 2009), and the instrument set is relevant, there are concerns about excludability of the instruments. Firstly, there is evidence to suggest that shocks also have adverse effects on the health of older girls in the short-term. Secondly, the pattern of within household impact of shocks on chores does not entirely match that of the long-term effect of shocks on education. I, therefore, interpret these findings as suggestive evidence that the long-term impact of shocks on schooling may, at least in part, be driven by the short-term effect they have on how much children work. This mechanism is likely to be most relevant for the older girls within the household, since the power of the instrument is driven primarily by this cohort.

[2] Since BDG find for this sample that shocks reduce the probability of school attendance and increase the amount of time spent on chores, a-priori there are good grounds to hypothesise that the long term effects that I find reflect persistence of these short-term responses.

This paper is motivated by the importance of understanding the full-scale of the effects that the inability of household to protect themselves from negative income shocks can have on children, and who is most vulnerable. Further, I offer some analysis of the plausible channels for these effects, since effective alleviation of the adverse impacts of shocks requires an understanding of the ways in which they set in.

The next section presents a brief overview of the related literature. It is followed by Section 1.3 which outlines the setting for the study, and offers a detailed description of the data-set used. Section 1.4 highlights some descriptive statistics for the sample of interest, and discusses the main variables of interest. The empirical strategy is discussed in Section 1.5, followed by Section 1.6 in which the main results are examined. The following section (1.7) extends the analysis further, exploring the mechanisms for the main results. Section 1.8 concludes.

1.2. Review of Related Literature

Traditionally the effects of income shocks have been examined with a focus on the household. The starting point for this analysis is the overwhelming evidence that households in rural communities in poor countries are unable to achieve Pareto-efficient risk pooling. Townsend (1994), and Ravallion & Chaudhuri (1994), for instance, use ICRISAT Indian data to show that there is imperfect covariance in consumption across households within the villages as evidence of imperfect risk pooling. Similarly Deaton rejects the hypothesis that efficient risk pooling is achieved within villages or ethnic groups in Cote D'Ivoire (Deaton, 1992). In fact, according to a summary by Bardham & Udry (1999), all existing studies of risk-pooling in rural parts of poor countries reject the hypothesis of efficient risk-pooling.

Nevertheless, there is equally strong evidence that households manage to smooth consumption to a degree using numerous strategies. Among others these include accumulation of assets such as bullocks as buffer stocks (Rosenzweig & Wolpin, 1993), saving transitory income (Paxson, 1992), informal credit and insurance arrangements (Udry, 1990), crop portfolio diversification (Dercon, 1996; Larson & Plessmann, 2009), diversification of household members' time/employment portfolios (Kochar, 1999) and child labour (Beegle et al, 2006; Guarcello et al, 2010). The adverse effects of income volatility, which are the focus of this study, may, therefore, stem not only from the inability of households to perfectly smooth consumption, but also from the impact that some of the coping strategies households adopt in order to attain partial consumption smoothing, such as, for instance, asset sales or child labour.

The studies examining these adverse effects at the individual level tend to focus on the impact of shocks on health and education. Since these are very closely linked, I discuss findings relating to both. In one of the most recent contributions, Bengtsson analyses the short-run relationship between transitory income shocks and child body weight, using the same data-set as this study. He finds high income elasticities of weight among some children in the household, reporting that a 10 percent decrease in income results in up to a 0.4 kg reduction in weight (Bengtsson, 2010). These results are entirely consistent with an older selection of studies on the short-term effects of shocks. Jensen's study of Cote D'Ivoire, for instance, shows that income shocks, proxied using rainfall variation, result in a doubling of the rate of malnutrition among children (Jensen, 2000). Hoddinott and Kinsey show that the 1994/95 drought in Zimbabwe had a particularly adverse effect on children in the 12-24 month age group, lowering their growth by between 1.5 and 2 cm per year (Hoddinott & Kinsey, 2001)

A selection of studies also finds strong negative effects on education. Beegle et al (2006) and Guarcello et al (2010) use data from rural Tanzania and Guatemala, respectively, and find that negative idiosyncratic income shocks push children into work and reduce the probability of school attendance. Using a different approach, Edmonds finds that relaxation of liquidity constraint in South Africa in the form of eligibility for "fully anticipatable" pension income results in reduced child labour hours and increased school attendance in poor black families, indicating that these are being utilised as coping mechanisms (Edmonds, 2006b).

In addition to consistently finding negative shocks effects, the existing evidence also strongly suggests that there is substantial impact heterogeneity along various child characteristics such as age-cohort and gender. One recurring finding is that girls are often more vulnerable. Rose (1999) finds that the positive association between rainfall and higher mortality in rural India only holds among girls. Bengsston's results suggest that it is girls whose weight has the highest income elasticity (Bengsston, 2010). Porter finds that the famine in Ethiopia in 1984 had the most adverse long-term health effects on children who were in utero or under 36 months at the time of the famine (Porter, 2009). Finally, Duryea's work on urban Brazil suggests that 16 year-old girls are particularly vulnerable to household employment shocks which significantly increase the chances that they drop out/fail to advance at school (Duryea et al, 2007).

A problem with the majority of these studies is that they do not address the endogeneity of household vulnerability to income shocks. While the shock itself may be unpredictable to the household, the extent to which a household is affected by it depends on characteristics that also may determine key outcomes. This is explicitly acknowledged in the literature on health shocks. In fact there is a whole body of work here examining persistent strong associations between various indicators of socio-economic status and health. Smith (2003) for instance uses the Health and Retirement Survey for the U.S. to show that education predicts future onset of disease. Although establishing causal links between health and socio-economic indicators is notoriously difficult (Adams et al, 2003), increasingly analysis of health shock effects treats the shock as endogenous, finding sources of exogenous variation in who is affected for identification of causal impacts (e.g. Mohanan (2008)). for instance proposes

This issue is not entirely ignored in the literature on income shocks. For instance, Duryea et al (2007) identify the impact of household unemployment shocks on child schooling and work by comparing households where the shock occurred during the school year to those where it occurring after the school year. To the extent that the timing of the shock is exogenous, their strategy eliminates concerns about unobservable characteristics of households that experience unemployment shocks. Beegle et al (2006) partially control for unobservable characteristics of households that experience crop shocks by conducting the analysis at a within household level, but pooling four rounds of data. This addresses concerns about unobservables which do not vary in that period. While there are, therefore, examples of less and more direct acknowledgements of the endogeneity of income shocks in the existing literature, there is still an overall lack of emphasis on this potential source of bias.

Moving away from methodological limitation, the scope of the analysis in the existing studies does not often extend beyond short-term effects. Not enough is known, therefore, about the permanence of the adverse effects of shocks and the gender and age-cohort gaps that they create. There are a few notable exceptions. Alderman et al (2006), for instance, offer more medium-term results. They show that loss of stature at pre-school age in response to exposure to drought and civil war in Zimbabwe leads to reduced height in adolescents. They also find that these children suffer from delayed school enrolment and reduction in grade completion. Although beginning to examine the longer term, these findings still relate to a time-horizon in which catch-up is possible.

The lack of literature on the longer-term also means that little is known about the channels through which permanent shock effects set in; this study considers the relevance of child labour, schooling attendance and health. Within a household lifetime utility maximising framework a negative shock can have an impact on schooling through affecting the marginal productivity of child labour and/or reducing current and anticipated future wealth and, therefore, overall consumption (Jacoby & Skoufias, 1997). Further, there is a large body of literature suggesting that working more in childhood is associated with worse school outcomes in the short and medium-term with respect to a variety of measures of school attainment, including school attendance, chance of grade repetition, and reading and mathematics tests (Patrinos & Psacharopoulos, 1995, 1997; Akabayashi & Psacharopoulos, 1999; Heady, 2003). A number of attempts have also been made to identify the impact of child labour on education more causally. The most common way of doing this has been using various instrumenting strategies, including household measures of income, assets, access to infrastructure, and community level price, education and literacy indicators (Ray & Lancaster, 2005; Bezerra, 2007; Beegle et al, 2009). Overall, the evidence overwhelmingly points to an inverse relationship between child labour and schooling.

The motivation for also considering health as a channel is that while, as discussed above, it is often found to be adversely affected by shocks, there is a large and established literature in developmental psychology, and, increasingly, economics linking early child health, especially nutrition, and mental development including development of skills necessary to perform well at school (e.g. Engle et al, 1979; Dercon & Sanchez, 2010)[3].

This paper contributes to the current understanding of the impact of income shocks in rural communities in poor countries in a number of ways. Most importantly I utilise an unusually long individual panel survey to explore the permanence of shock effects by looking at adult outcomes of those affected by shocks in childhood. Secondly, the low levels of attrition in the data allow estimation at a within household level, which addresses concerns about endogeneity of income shocks more comprehensively. Finally, I start investigating links between the existing theoretical and empirical literature on short-term shock effects and that on the determinants of schooling in search of likely channels for the shock effects that I find.

1.3. The Setting and Data

The data utilised in this study comes from the longitudinal Kagera Health and Development Survey (KHDS) collected between 1991 and 2004. The Kagera region is located in North-western Tanzania, on the western shore of Lake Victoria, bordering Uganda to the north and Rwanda and Burundi to the west.

Tanzania is one of the more politically and economically stable countries among its neighbours, which include Kenya and Uganda to the north, Rwanda, Burundi and the Democratic Republic of Congo to the west, and Zambia, Malawi and Mozambique to the south. While during the survey period real GDP growth was indicative of rapid macroeconomic growth at around 4.2 percent per year, poverty reduction in rural areas over the 10 year period between 1991 and 2001 was a

[3] An important caveat is that this literature emphasises the importance of health for a younger cohort of children than is considered in this study.

negligible 2 percentage points (NBS, 2001; URT, 2004). Most of the macroeconomic growth experienced during this period benefitted those in more urban areas, with poverty falling by as much as 10 percentage points in Dar es Salaam. This is also reflected in the high returns to migration from less to more connected areas, estimated by Beegle et al (2010) to result in growth that is 66 percentage points higher than among those who do not move.

Kagera region is among the more remote parts of Tanzania, with 1500km between its capital and Dar es Salaam. The region is overwhelmingly rural with a population of 2 million in 2004 living on 30,000 square kilometres of land, up from 1.3 million in 1988 with a constant age composition of just under half of the population aged 0-14 years and around 5 percent over 65 (URT, 1998, 2006). The primary activity in the region is agriculture; 75 percent of the households in rural Kagera classifying self-employment in agriculture as the main activity. Those in agriculture are primarily engaged in producing bananas and coffee in the north and rain-fed annual crops (maize, sorghum, cotton) in the south (Ellis, 1998, 2000). In 2004 about 40 percent of the rural population of Kagera were found to be living in poor households i.e. with consumption below the poverty line, and 43 percent of children under five were found to be chronically malnourished (stunted) (EDI, 2004).

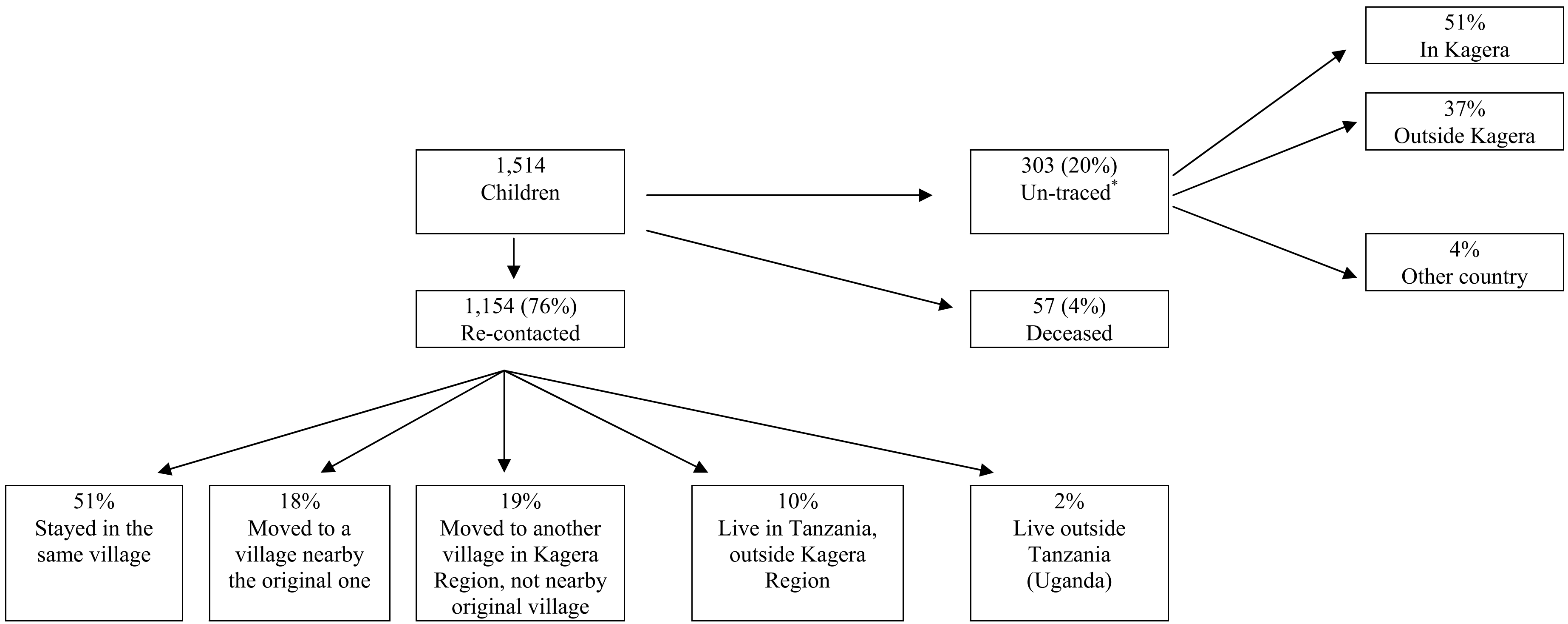
The Kagera Health and Development Survey (KHDS), was originally conducted by the World Bank and Muhimbili University College of Health Sciences (MUCHS) in four waves between 1991 and 1994. It is a socioeconomic survey of 912 household designed at the outset to examine the impact of HIV/AIDS. A re-survey, conducted in 2004, turned the KHDS into an individual longitudinal survey by administering a full household questionnaire to all households inhabited by any of the household members from the households in the baseline surveys (World Bank, 2004).

There are two key features in particular that distinguish KHDS from other panel surveys. Firstly, re-interviewing in 2004 was done at the individual rather than the household level; this resulted in an increase in the sample from 912 households to 2,719. Secondly, a large effort was made to find *all* panel individuals irrespective of where they resided in 2004, rather than just those who remained in or close to the original villages. This feature ensured significantly lower attrition rates than is characteristic of other longitudinal studies. As summarized in Alderman et al (2001), many of the longitudinal surveys conducted during the period spanned by KHDS did not attempt to re-interview households that moved and had household level attritions rates of over 30 percent over much lower time-spans of 2-3 years (e.g. Bolivian Pre-School Evaluation Household Survet (PIDI) and Kenyan Diffusion and Ideational Change Project (KDICP). Even comparing like with like, the KwaZalu-Natal Income Dynamics Study (KIDS) conducted starting from 1993, which like KHDS had a tracking feature, re-surveyed 84 percent of the households after 5 years. In KHDS, after 10-13 years (depending on which part of the baseline survey the individual appears in) 93 percent of the original households were tracked. Though tracking was done at the individual level and a household is not a well-defined unit of measurement over the long-term, the household level attrition rate comparisons are useful to highlight the effectiveness of the tracking feature of the KHDS survey relative to other longitudinal surveys, even ones spanning less than half the time spanned by KHDS.

This study does not utilise the whole KHDS sample so the attrition that is most important to consider is that in the sub-sample analysed here. I examine the effects that shocks have in the long-run on educational attainment. I am particularly interested in exploring the persistence of the effects into adulthood. I, therefore, define the study-sample to include those who were children in the baseline (under the age of 16), but were old enough by the follow up round of the survey to have completed their schooling or at least have little room for catch-up (no younger than 17). The analysis sample thus consists of all individuals from the first wave of the survey who were between the ages of 7 and 15 at the time of the baseline survey and who were re-contacted in 2004; the majority (94 percent) are at least twenty by 2004 (if they were interviewed in 1991).

Figure 0.1 shows the re-contact rates and location of all the individuals in this sub-sample. Overall, 76 percent of the children aged 7 to 15 in the first wave of the baseline survey were re-interviewed in 2004, 4 percent had passed away, and 20 percent were not found. The distribution of those who were tracked by the location in which they were found shows that had the extensive tracking exercise not been undertaken and only the individuals who had stayed in or near the original village were found, the attrition rate would have been twice as high.

Figure 0.1: Tracking of analysis sample



*21 (8%) = Not Known

1.4. Descriptive Statistics

Table 0.1 presents summary statistics for the key analysis variables including baseline individual characteristics, such as age, sex, health, parental schooling and age-rank within the household, as well as baseline household level characteristics such as assets and wealth. The analysis sample consists of 1,150 “panel” individuals who were tracked in 2004 out of 1,514 individuals, age 7-15, who were interviewed in the first wave of the baseline survey between 1991 and 1994[4]. For completeness, Table 0.1 shows summary statistics for both the panel and the complete baseline samples.

The panel sample is evenly split by gender with a mean age of about 11 years, ranging from 7 to 15. There are slight differences in the numbers of children in each age-group; 9 year-olds constitute the smallest group at 8.8 percent of the sample, while 12 year olds are the largest group at 14.7 percent. By 2004 the majority of these individuals are in their twenties, with a mean age of 23.7, ranging between 17 and 30. Over a tenth (14 percent) of the individuals were severely stunted[5] during the baseline. Further, a quarter was living in a different household from their parents[6]. Finally, the majority of individuals in the sample have parents with none or incomplete primary schooling; mothers are much more likely to have no formal schooling than fathers at 33 and 14 percent respectively.

[4] Although 1,154 children were re-contacted in 2004 (Figure 0.1), complete information is only available for 1,150 of them.
[5] Had a height for age z-score below -3 standard deviations, which indicates severe chronic malnutrition.
[6] 45 percent of these children were maternal orphans, 50 percent paternal, and 34 percent were double orphans.

Table 0.1: Descriptive statistics

	Panel Sample		Whole Sample	
	mean	sd	mean	sd
Female	0.49	0.50	0.49	0.50
Baseline (1991)*				
Age	11.09	2.54	11.05	2.55
Age: 7-8	0.22	0.42	0.22	0.42
Age: 9-11	0.30	0.46	0.30	0.46
Age: 12-13	0.26	0.44	0.25	0.44
Age: 14-15	0.22	0.42	0.22	0.41
Severely Stunted	0.14	0.35	0.14	0.35
Age rank within the household	3.65	2.09	3.57	2.11
Neither parent present in household	0.25	0.43	0.27	0.44
Father's schooling: incomplete primary	0.44	0.50	0.42	0.49
Father's schooling: complete primary	0.30	0.46	0.31	0.46
Father's schooling: secondary plus	0.12	0.33	0.12	0.33
Mother's schooling: incomplete primary	0.37	0.48	0.35	0.48
Mother's schooling: complete primary	0.28	0.45	0.29	0.46
Mother's schooling: secondary plus	0.02	0.14	0.02	0.14
Log per capita physical stock excluding land, durables, & cash	10.01	1.58	9.98	1.60
Log per capita expenditure	10.92	0.61	10.96	0.63
Log per capita value of land owned	10.03	2.09	9.80	2.53
Follow-up (2004)				
Age in 2004	23.66	2.86	NA	NA
Number of Observations	1,154		1,514	

*Note: Baseline data is from the first wave of the survey, which, for most households, was in 1991; for a small minority, however, it was later, between 1992 and 1994.

Table 0.2 shows the variables of particular interest for this study. I estimate the effect of idiosyncratic income shocks on final educational attainment. Since agriculture is the primary occupation in the great majority of household in the sample (81 percent), self-reported crop shocks are used as indicators of idiosyncratic income shocks. Crop shocks are defined as loss of crop due to insects, rodents, fire or theft as reported by the household. In the first wave of the baseline, just over three-quarters of the households reported having experienced a crop shock in the year preceding the survey. Further, these shocks were of a non-negligible magnitude, destroying, on average, over a quarter of the total harvest.

In addition to the problem of endogeneity of household vulnerability to shocks, self-reported shock data may also be contaminated by reporting bias. As discussed in more detail below, these are less of a concern in this study since the analysis is at a within household level.

Final educational attainment is measured by total years of schooling, as well as primary school completion. On average, by 2004, three out of four individuals in the sample had completed primary school (i.e. had at least 7 years of schooling), 95 percent were no longer attending school, about a tenth had never gone to school and the average duration of schooling was just over 6 years.

In trying to explain the long-term effects of income shocks on education, I explore a selection of short-term effects that shocks had been found to have in the broader literature as well as in relation to this particular sample. These include nutrition, disruption to schooling as well as a change in the child work burden. Nutrition is measured as standardized deviation of the Body Mass Index from the reference population mean[7]. The negative mean BMI z-score indicates that on average the sample was under-nourished in childhood, with an average BMI z-score that was nearly a standard deviation below the reference group mean.

I use two indicators of disruption to schooling: whether the child went to school during the year for which shock data is collected (i.e. the year preceding the survey), and the number of hours that the child spent at school in the week preceding the survey. The average amount of time spent at school among the 7-15 year olds in the baseline was roughly 15 hours per week. However, more than a third of the 7-15 year-olds in the sample had not attended school in the year preceding the survey.

[7] The US version of the CDC Growth Charts is used as the reference. BMI below -2 standard deviations is indicative of acute malnutrition. (Kuczmarski R. J., 2002).

Finally, child labour data collected in the baseline of KHDS includes hours spent on economic activities and chores in the week preceding the survey. Economic activities for children consist predominately of farming and looking after livestock, which typically includes tasks such as tending crops and herding livestock, as well as processing crop and livestock products. Chores include activities such as fetching water and firewood, cleaning, looking after the house and preparing meals. Children in the baseline sample tend to spend slightly more time working than at school. On average they work 18 hours per week, 11 of which are taken up by doing chores.

Table 0.2: Key Variables for the Analysis

	Panel Sample		Whole Sample	
	mean	sd	mean	sd
Baseline (1991)*				
Household experienced a crop shock	0.77	0.42	0.75	0.43
Share of crop lost because of crop shock	0.28	0.28	0.28	0.29
Body Mass Index z-score	-0.84	1.04	-0.84	1.03
Attended school in the past year	0.64	0.48	0.63	0.48
Hours spent at school in the last week	14.86	16.70	14.73	16.68
Hours spent working (chores and economic activities) in the last week	18.15	15.81	17.87	16.14
Hours spent on chores in the last week	11.06	10.03	10.87	10.08
Follow-up (2004)				
Completed primary school	0.75	0.44		
Total years of schooling	6.09	3.06		
Number of Observations	1,154		1,514	

*Note: Baseline data is from the first wave of the survey, which, for most households, was in 1991; for a small minority, however, it was later, between 1992 and 1994.

1.5. Empirical Strategy

This paper examines whether there is any evidence of permanent age-cohort and gender specific effects of experiencing crop shocks in childhood on schooling.

I start with the simple model in Equation (1), estimating the direct impact of shocks in childhood on educational attainment 10-13 years later, controlling for observable individual, household, and community characteristics.

$$S_{ij,t} = \alpha + \beta Shock_{j,t-l} + \gamma X_{ij,t-l} + \theta_{ij} + \epsilon_{ij,t} \qquad (1)$$

Where $S_{ij,t}$ is the schooling attainment of individual i, from household j, at the time of the follow-up survey t in 2004, $Shock_{j,t-l}$ is an indicator of whether household j had experienced an income shock in the baseline l years ago, where l=13 in most cases, and $X_{ij,t-l}$ is a vector of observable individual, household and community characteristics at the time of the shock. I then allow for age and gender specific effects by interacting these with the shock indicator.

The immediate concern with the robustness of the estimated shock effect in the above specifications is that while shocks such as pests and fire are exogenous in the sense that they are outside the control of the household and cannot be anticipated by the household, some households are nevertheless more vulnerable to the shocks both ex-ante and ex-post. Ex-ante vulnerability to shocks may depend, for instance, on the crop portfolio and employment diversification strategies adopted by the household (Schultz, 1975). Ex-post vulnerability is likely to depend on the household's coping capability, which is a function of for instance the extensiveness of the household's network (Goldstein et al, 2004). It is not unlikely that some of these factors are also relevant determinants of final schooling - the outcome of interest. While it is possible to include a variety of household level controls for characteristics relevant to its vulnerability, the presence of unobservables (θ_{ij}) is nevertheless highly likely.

In order to reduce the likelihood of such omitted variable bias, I examine the *within* household effect of shocks by gender and age-cohort of the children. Comparison of individuals who lived in the same household in childhood, allows to hold the unobservable characteristics that determine household vulnerability to shocks constant in the analysis, eliminating these as a potential source of bias. Further, individual level heterogeneity is controlled for by a range of individual characteristics including age, sex, health, age-rank within the household, parental education[8], and whether the child grew up with his/her parents.

[8] Note that not all children within a household have the same parent.

The last part of the paper examines the plausibility of various channels in explaining the main results including health, school attendance, and child labour. I use 2SLS estimation strategy to explore whether the shock induced short-term variation in these explains the long-run effects that shocks have on educational attainment:

$$L_{ij,t-l} = \alpha + \beta Shock_{ij,t-l} + \gamma X_{ij,t-l} + \theta_{ij} + \varepsilon_{ij,t-l} \qquad (2)$$

$$S_{ij,t} = \alpha + \beta \hat{L}_{ij,t-l} + \gamma X_{ij,t-l} + \theta_{ij} + \epsilon_{ij,t} \qquad (3)$$

Where $L_{ij,t-l}$ is the short-term childhood schooling, child labour and health outcomes of individual i from household j, following a shock (i.e. during the baseline), while the rest of the variables are defined as in Equation (1) so that $Shock_{ij,t-l}$ includes interactions of shock with gender and age-cohort of the individual and $X_{ij,t-l}$ is a vector of controls for individual characteristics at the time of the shock.

In the second stage, I examine whether shock induced variation in $L_{ij,t-l}$ has an impact on educational attainment that is comparable with the direct effect of the shock estimated in Equation (1). The outcome in the second stage, $S_{ij,t}$ is, therefore, the schooling attained by individual *i* from baseline household j in 2004 and $\hat{L}_{ij,t-l}$ is the instrumented channel for this effect using within household gender and age variation in the shock effect as instruments. As previously, the comparison in both stages is between individuals who were living in the same household at the time of the baseline.

Of course there are many robustness concerns with this strategy, discussed in Section 1.7. An important overall caveat is that in contrast to a related earlier paper co-authored with BDG (Beegle et al, 2008), the two-stage analysis of potential channels is offered only as an exploratory extension to the main findings of the estimated effects in Equation (1).

1.6. Results

The OLS estimates of Equation (1) show the overall, as well as age and gender specific associations between experiencing crop shocks in childhood and final schooling in adulthood (Table 0.3 and Table 0.4). After a brief discussion of these, I focus on investigating these relationships in a more causal framework, estimating a within childhood household fixed effects model, which controls for many sources of endogeneity that may bias the OLS estimates.

The mean, gender and age-specific associations between shocks in childhood and educational attainment 10-13 years later, in adulthood, appear to be insignificant at first glance, relative to both the probability of primary school completion and total years of schooling (Table 0.3).

Some marginally stronger correlations emerge once the functional form restriction on the age effect is relaxed. The specification in Table 0.4 allows for different levels of schooling by age-cohort and gender. Here instead of a linear age control, there is a dummy, taking the value of one if the child is in the older age-cohort (age 12 to 15) and zero if the child is in the younger cohort (age 7-11). Interactions of these with gender offer separate estimates of the levels of shock effects for younger and older girls as well as younger and older boys. Although the mean effects for each group remain insignificant, there is some evidence that shocks are correlated with a significant change in the existing schooling differences between groups. In particular, the results suggest that incidence of shocks is correlated with a reduction in the probability of primary school completion for older children relative to younger girls. Similar associations are observed with school years, although these are not statistically significant (Table 0.5).

Table 0.3: Impact of shocks in childhood on schooling attainment in adulthood (OLS)

	Completed Primary	Completed Primary	School years	School years
Crop shock (any crop lost)	-0.033	-0.002	-0.061	-0.543
	(0.036)	(0.192)	(0.243)	(1.300)
Crop shock * age		-0.006		0.018
		(0.015)		(0.107)
Crop shock * female		0.176		0.590
		(0.298)		(1.907)
Crop shock * female * age		-0.010		-0.000
		(0.025)		(0.164)
Age	0.020***	0.034**	0.096**	0.140
	(0.006)	(0.014)	(0.043)	(0.097)
Female	-0.015	0.055	-0.259	0.521
	(0.025)	(0.270)	(0.173)	(1.697)
Female * age		-0.011		-0.112
		(0.022)		(0.144)
Age rank	-0.018*	-0.018*	-0.142*	-0.151*
	(0.010)	(0.010)	(0.080)	(0.081)
Severely stunted	-0.090**	-0.099**	-0.614**	-0.666**
	(0.040)	(0.041)	(0.264)	(0.270)
Dad:1-6 yrs of education	0.007	0.007	0.007	-0.009
	(0.049)	(0.049)	(0.329)	(0.328)
Dad:7 yrs of education	0.126**	0.126**	1.017***	1.015***
	(0.049)	(0.049)	(0.339)	(0.338)
Dad:8+ yrs of education	0.154***	0.153***	1.502***	1.495***
	(0.056)	(0.056)	(0.396)	(0.396)
Mom:1-6 yrs of education	0.096***	0.097***	0.534**	0.529**
	(0.037)	(0.037)	(0.263)	(0.263)
Mom:7 yrs of education	0.139***	0.142***	1.000***	1.018***
	(0.040)	(0.039)	(0.269)	(0.268)
Mom:8+ yrs of education	0.195***	0.201***	1.940***	1.961***
	(0.069)	(0.068)	(0.752)	(0.751)
No parent	-0.097***	-0.096***	-0.523**	-0.509**
	(0.035)	(0.035)	(0.240)	(0.239)
Household Size	0.015***	0.016***	0.124***	0.131***
	(0.006)	(0.006)	(0.046)	(0.046)
Log per capita physical stock excluding land+durables+cash	0.029***	0.028**	0.249***	0.238***
	(0.011)	(0.011)	(0.077)	(0.077)
Log per capita expenditure	-0.017	-0.017	0.184	0.193
	(0.029)	(0.029)	(0.195)	(0.195)
Log per capita value of land owned	0.011	0.011	0.072	0.072
	(0.008)	(0.008)	(0.059)	(0.059)
Number of observations	1,150	1,150	1,150	1,150

Note: *** p<0.01, ** p<0.05, * p<0.1. Specification also includes controls for baseline district fixed effects.

Table 0.4: Impact of shocks in childhood on schooling attainment in adulthood (non-linear age, OLS)

	Completed Primary	School years
Crop Shock (any crop lost)	-0.064	-0.486
	(0.071)	(0.453)
Crop shock * age 12-15	-0.012	0.205
	(0.086)	(0.565)
Crop shock * female	0.123	0.840
	(0.097)	(0.598)
Crop shock * female * age 12-15	-0.126	-0.506
	(0.125)	(0.839)
Age:12-15	0.116	0.366
	(0.078)	(0.508)
Female	-0.087	-0.767
	(0.087)	(0.526)
Female * age 12-15	0.050	0.101
	(0.110)	(0.739)
Age rank	-0.012	-0.118
	(0.010)	(0.076)
Severely stunted	-0.088**	-0.607**
	(0.040)	(0.269)
Dad:1-6 yrs of education	0.008	0.008
	(0.049)	(0.329)
Dad:7 yrs of education	0.120**	0.994***
	(0.049)	(0.337)
Dad:8+ yrs of education	0.152***	1.492***
	(0.055)	(0.395)
Mom:1-6 yrs of education	0.098***	0.533**
	(0.037)	(0.263)
Mom:7 yrs of education	0.134***	0.981***
	(0.039)	(0.267)
Mom:8+ yrs of education	0.207***	1.983***
	(0.067)	(0.755)
No parent	-0.094***	-0.498**
	(0.034)	(0.239)
Household Size	0.013**	0.116***
	(0.006)	(0.045)
Log per capita physical stock excluding land+durables+cash	0.029***	0.244***
	(0.011)	(0.078)
Log per capita expenditure	-0.016	0.194
	(0.029)	(0.196)
Log per capita value of land owned	0.012	0.077
	(0.008)	(0.059)
Number of observations	1,150	1,150

Note: *** p<0.01, ** p<0.05, * p<0.1. Specification also includes controls for baseline district fixed effects.

Table 0.5: Interaction effects for Table 0.4

	Completed Primary		School Years	
	Shock household	Non-shock household	Shock household	Non-shock household
Female Young	0.059	-0.087	0.354	-0.767
Female Old	-0.079	0.079	0.052	-0.300
Male Young	-0.064	-	-0.486	-
Male Old	-0.076	0.116	-0.281	0.366
Female Old - Female Young	-0.139*	0.166***	-0.302	0.467
Female Old - Male Young	-0.015	0.079	0.538	-0.300
Female Old - Male Old	-0.003	-0.037	0.334	-0.666
Female Young - Male Young	0.123	-0.087	0.840	-0.767
Female Young - Male Old	0.136*	-0.203***	0.635	-1.133***
Male Young - Male Old	0.012	-0.116	-0.205	-0.366

note: *** p<0.01, ** p<0.05, * p<0.1, ^ p<0.13; derivations of the interaction effects are presented in Appendix B. "Young" =age 7-11, "Old"=age 12-15. Male Young in non-shock households is the omitted category.

Table 0.6 presents results of the age-cohort and gender specific shock effects at a within household level. The comparison here, therefore, is between individuals who grew up in the same household. While, at the time when the schooling outcome was measured they no longer necessarily live together, they were living in the same household at the time of the shock, facing the same observable and unobservable environmental characteristics, especially those which may have affected both the schooling decisions made in the household as well as the vulnerability of the household to income shocks. An example of such a characteristic is the education of the main decision maker in the household. There is evidence to suggest that in some settings, decision makers with less education may value schooling less and make sub-optimal agricultural decisions regarding, for instance, use of fertilizer and crop portfolio (Abdulai & Huffman, 2005; Neri et al, 2006). Insufficient controls for the characteristics of the decision maker, may, therefore, result in a spurious positive correlation between incidence of crop shocks and low schooling outcomes. Despite a large selection of controls for observable characteristics, one cannot hope to capture the complete relevant set, in part because not all of them are observable to the researcher. A within household comparison, however, ensures that the shock effect is identified only through variation in outcomes among those sharing the same within household conditions, facing the same constraints, risks and opportunity sets.

Table 0.7 presents calculations and significance tests of the relative within household shock effects between the four groups of children: younger and older boys and younger and older girls. The derivation of these effects is presented in Appendix B.

The results in Table 0.7 Column 1 firstly confirm the OLS results that shocks eliminate the gap in probability of primary school completion that otherwise exists between the younger girls and older children. In addition, they suggest that the disadvantage that younger girls have relative to younger boys with respect to chances of primary school completion is also redressed by incidence of crop shocks. In households that are not affected by shocks younger girls are 14 and 16 percentage points less likely to complete primary school than the older girls and younger boys respectively. A crop shock equalises these opportunities between the three groups through reducing the chances of primary school completion of older girls and younger boys relative to younger girls by 21 and 18 percentage points respectively. It is not possible to determine from the results what combination of changes in probability of primary school completion results in the elimination of these gaps. Intuitively, however, a decrease in the chances of primary school completion among the older girls and younger boys seems a more likely change in the context of income shocks, than a substantial increase among the older boys and younger girls.

Crop shocks do not only have equalising effects; the results suggest that they also create education gaps within the household. In particular, there is a significant negative impact of shocks on probability of primary school completion among older girls relative to older boys. While in non-shock households there is no difference between these two groups, shocks reduce the chances of primary school completion among older girls relative to older boys by 15 percentage points, creating a significant 10 percentage point gap. Similarly there is a widening of the disadvantage of younger boys relative to older boys, though even in households experiencing shocks, it is not quite statistically significant.

Overall, the initial set of results points particularly to adverse effects of shocks on the relative probability of primary school completion among the older girls in the household and somewhat weaker negative effects on younger boys. The older girls and younger boys also experience a decline in years of schooling attained in adulthood relative to the other two groups of children in the household. However, there is a reversal in the severity of the effects with most pronounced impact on younger boys, followed by much weaker evidence of an effect on the older girls.

Table 0.6 Column 2 shows that the largest and most significant effect of crop shock on school years is reflected in the closing of the gap between younger girls and younger boys. While in non-shock households younger boys on average get 1.3 years more schooling than younger girls, shocks eliminate this difference. Further, while the shock-induced change in relative chances of primary school completion between younger and older boys is not quite significant, it is highly significant with respect to school years. Younger boys suffer an average loss of nearly a year of schooling relative to older boys as the result of shocks which creates a significant gap between these two groups that does not exist in non-shock households. Finally, in contrast to the primary school results, while shocks have negative effects on the school years attained by older girls relative to the younger girls and older boys, these effects are not statistically significant[9].

The pattern emerging from results in Table 0.6, suggests that older girls and younger boys experience the highest permanent relative disadvantage in schooling as the result of shocks. However, while for the older girls this disadvantage comes through mainly in the impact of shocks on probability of primary school completion, the younger boy effect is strongest with respect to number of completed school years[10].

[9] It should be noted, though, that the decline of roughly three-fourths of a year in schooling of older females relative to younger females is marginally significant at 12 percent.

[10] Table 0.16 and Table 0.17 in Appendix C show a more detailed break-down of the age-cohort effects. These estimates are not included in the main results as at a within household level identification of effects with such a large number of groups is likely to be at best imprecise and perhaps even misleading. However, as an interesting detail, it appears that the younger boy effects are driven by 7-8 year old boys, while the older girl affects by the 12-13 year old girls. This is consistent with a hypothesis that the girl effect works through preventing girls who are almost finished with primary school from completing, although the total school years effect is insignificant, as 75 percent of children in the sample who attended school do not get

Table 0.6 Impact of shock in childhood on schooling attainment in adulthood (age-cohort, Childhood Household Fixed Effects)

	Completed Primary	School Years
Crop shock * female	0.182*	1.302**
	(0.099)	(0.536)
Crop shock * age 12-15	0.124	1.073*
	(0.093)	(0.551)
Crop shock * female * age 12-15	-0.332***	-1.852**
	(0.124)	(0.746)
Age:12-15	-0.069	-0.610
	(0.087)	(0.503)
Female * age 12-15	0.213**	1.099*
	(0.105)	(0.610)
Female	-0.160*	-1.289***
	(0.087)	(0.440)
Age rank	0.032**	0.080
	(0.015)	(0.115)
Severely stunted	-0.069	-0.293
	(0.045)	(0.269)
Dad:1-6 yrs of education	0.074	0.680
	(0.096)	(0.599)
Dad:7 yrs of education	0.139	1.658**
	(0.094)	(0.673)
Dad:8+ yrs of education	-0.066	0.289
	(0.156)	(0.777)
Mom:1-6 yrs of education	0.098	0.149
	(0.087)	(0.664)
Mom:7 yrs of education	0.198**	0.642
	(0.077)	(0.572)
Mom:8+ yrs of education	0.129	-0.925
	(0.097)	(1.023)
No parent	-0.233***	-1.759***
	(0.068)	(0.538)
Number of observations	1,150	1,150

note: *** p<0.01, ** p<0.05, * p<0.1

more than primary school education anyway. In contrast the boy effect works through delaying enrolment which is reflected in the school years completed as the result of imperfect catch-up.

Table 0.7 Interaction Effects for Table 0.6

	Completed Primary		School Years	
	Shock household	Non-shock household	Shock household	Non-shock household
Female Old - Female Young	-0.208***	0.144**	-0.779^	0.489
Female Old - Male Young	-0.027	-0.017	0.522	-0.80^
Female Old - Male Old	-0.15**	0.053	-0.55	-0.190
Female Young - Male Young	0.182*	-0.16*	1.302**	-1.289***
Female Young - Male Old	0.058	-0.091	0.229	-0.680^
Male Young - Male Old	-0.124	-0.069	-1.073**	-0.610

note: *** p<0.01, ** p<0.05, * p<0.1, ^ p<0.13; derivations of the interaction effects are presented in Appendix B

So far crop shocks have been treated in the same way irrespective of their magnitude. However, the severity of crop shocks varies substantially in the data, ranging from a loss of just over a tenth of the crop at the 25th percentile to nearly three fifths at the 75th percentile. I utilise this variation to examine the sensitivity of the results to magnitude of shocks.

Results in Table 0.8 and Table 0.10 show that the significant point estimates of the gender and age-cohort specific shocks effects on schooling are, in the main, remarkably stable even within subsamples. There is particularly little variation in the magnitude of the point estimates of the shocks, gender and age-cohort interaction effects on primary school completion (Table 0.8). Such stability suggests that the results are not entirely driven by a particular sub-group in the sample[11].

Some heterogeneity emerges in group specific effects, with the older girl effect coming through somewhat more strongly in households experiencing less severe shocks - those that are in the bottom two quartiles in terms of shock magnitude (Table 0.9). In contrast, the younger boy effect becomes significant in households experiencing more severe shocks - in the top two quartiles by shock magnitude (Table 0.11). However, these results are presented here mainly for

[11] While I do not directly test differences in point estimates by shock magnitude to avoid convoluting the estimation with more interaction terms, the evidence in Table 0.8 and Table 0.10 is strongly indicative of stability of estimates across the sample.

completeness, as effects identified in such small groups should be treated with caution and not over-interpreted.

Table 0.8 Impact of crop shock in childhood on primary school completion by magnitude of shock

	Shock Quartile 1	Shock Quartile 2	Shock Quartile 3	Shock Quartile 4
Crop shock * female	0.171	0.160	0.193*	0.200
	(0.133)	(0.144)	(0.115)	(0.123)
Crop shock * age 12-15	0.116	0.124	0.105	0.144
	(0.133)	(0.123)	(0.103)	(0.108)
Crop shock * female * age 12-15	-0.364**	-0.416**	-0.294*	-0.273*
	(0.167)	(0.179)	(0.158)	(0.155)
Age:12-15	-0.073	-0.058	0.016	-0.038
	(0.103)	(0.097)	(0.097)	(0.098)
Female	-0.174**	-0.153*	-0.162*	-0.158*
	(0.088)	(0.089)	(0.085)	(0.086)
Female * age 12-15	0.226**	0.201*	0.244**	0.244**
	(0.107)	(0.107)	(0.102)	(0.106)
Number of observations	489	485	486	485

note: *** p<0.01, ** p<0.05, * p<0.1. Additional controls are the same as in Table 0.6.

Table 0.9 Interaction effects for Table 0.8

	Shock Quartile 1	Shock Quartile 2	Shock Quartile 3	Shock Quartile 4
Female Old - Female Young	-0.247**	-0.292***	-0.189*	-0.129
Female Old - Male Young	-0.077	-0.133	0.003	0.071
Female Old - Male Old	-0.193*	-0.256**	-0.102	-0.073
Female Young - Male Young	0.171	0.160	0.193*	0.200^
Female Young - Male Old	0.054	0.036	0.087	0.056
Male Young - Male Old	-0.116	-0.124	-0.105	-0.144

note: *** p<0.01, ** p<0.05, * p<0.1, ^ p<0.13; derivations of the interaction effects are presented in Appendix 1

Table 0.10 Impact of crop shock in childhood on total school years by magnitude of shock

	Shock Quartile 1 (Lowest)	Shock Quartile 2	Shock Quartile 3	Shock Quartile 4 (Highest)
Crop shock * female	1.274	1.556*	1.305*	1.325*
	(0.821)	(0.876)	(0.738)	(0.736)
Crop shock * age 12-15	1.180	1.331	1.119	1.088*
	(0.819)	(0.819)	(0.685)	(0.655)
Crop shock * female * age 12-15	-1.905*	-2.945**	-1.490	-1.544
	(0.991)	(1.156)	(1.083)	(0.985)
Age:12-15	-0.814	-0.707	-0.075	-0.444
	(0.556)	(0.558)	(0.548)	(0.555)
Female	-1.481***	-1.350***	-1.313***	-1.312***
	(0.467)	(0.479)	(0.445)	(0.459)
Female * age 12-15	1.341**	1.192*	1.180*	1.227*
	(0.640)	(0.640)	(0.633)	(0.643)
Number of observations	489	485	486	485

note: *** p<0.01, ** p<0.05, * p<0.1, ^ p<0.13; Additional controls are the same as in Table 0.6.

Table 0.11: Interaction effects for Table 0.10

	Shock Quartile 1	Shock Quartile 2	Shock Quartile 3	Shock Quartile 4
Female Old - Female Young	-0.725	-1.615**	-0.370	-0.456
Female Old - Male Young	0.550	-0.059	0.935	0.869
Female Old - Male Old	-0.631	-1.390*	-0.185	-0.219
Female Young - Male Young	1.274	1.556*	1.305*	1.325*
Female Young - Male Old	0.094	0.225	0.185	0.237
Male Young - Male Old	-1.180^	-1.331^	-1.119*	-1.088*

note: *** p<0.01, ** p<0.05, * p<0.1, ^ p<0.13; derivations of the interaction effects are presented in Appendix B.

Overall, therefore, the evidence suggests that idiosyncratic crop shocks have some permanent effects on the within household distribution of education among those who were children at the time of the shock. Even in the context of the large body of evidence that exists on adverse effects of idiosyncratic income shocks, the permanence of the effects presented here is striking. The most adversely affected groups within the household are older girls and younger boys; they experience a reduction in their final educational attainment relative to the other children who they live with. For the older girls this effect comes through mainly with respect to chances of primary school completion. For younger boys, on the other hand it is most noticeable in the total number of years of schooling that they attain in adulthood. The presence of such strong gender and age differences in the impact of shocks suggests that the risk management and coping strategies adopted by households have heterogeneous effects on children. I begin to address the household response below in a discussion of the potential channels for the observed effects.

1.7. Discussion

I utilise insights from the existing literature on how decisions about child schooling are made within the household (Orazem & King, 2008; Edmond, 2006), what effects income shocks can have in this context (Jacoby & Skoufias, 1997), as well as previous findings for this sample in BDG to suggest at least a partial plausible narrative for the main results. It is important to note that this part of the paper is strictly exploratory and does not aim to robustly identify a comprehensive set of mechanisms for the estimated effects.

A natural starting for considering the mechanisms through which shocks may affect schooling is what the determinants of the schooling decision within the household are and how they may be affected by an income shock. In a standard static model of household behaviour households equalise ratios of marginal costs and marginal utility of consumption to schooling. In the presence of child labour (which is very prevalent in rural Tanzania), the cost of schooling includes both school prices and the marginal product of child time in the household (Orazem & King, 2008). Households will, therefore, reduce the amount of schooling in times when child labour productivity is relatively high[12]. A negative income shock can, therefore, have an impact on schooling through its effect on the productivity of child labour within the household[13]. Further, this effect can differ across children within the household if, for instance, they are engaged in different activities and there is heterogeneity in the effect of the shock on the productivity of

[12] Assuming a concave utility function for schooling.

[13] Whether this effect is positive or negative will depend on the effect of the income shock on the productivity if child labour. For instance, if children are predominantly engaged in helping with agricultural activities a crop shock may reduce the marginal product of child labour since there is less to do. In contrast, if a crop shock forces adult household members to seek temporary employment outside the household, the marginal product of child labour may increase as they can perform the tasks that adults used to undertake within the household.

these. The aggregate effect of the shock on schooling is not, however, entirely determined by its impact on the marginal productivity of child labour; it also encompasses a decrease in current and anticipated future wealth which reduces consumption including schooling (Orazem & King, 2008; Jacoby & Skoufias, 1997).

Within this framework, the long-term gender and age-cohort specific effects of crop shocks on schooling that I find are consistent with adjustment in the amount of child schooling and/or work in response to a crop shock. Further, BDG's results[14] are suggestive of such adjustments; they find that in the short-term these crop shocks had negative effects on probability of school attendance and positive effects on time spent working, especially on chores. Motivated by this intuition, I explore whether the short-term effects of crop shocks on school attendance, time at school, and time spent working are plausible mechanisms for the persistent long-term effects of the shocks on schooling. In addition, I consider the possibility that the long-term school effect is, in fact, an indirect consequence of the adverse health effect of shocks. Influenced by persistent findings of negative short and medium-term health effects of income shocks on children, I investigate whether there is evidence to suggest that these are relevant in explaining the main findings in this paper.

Empirically, I am looking for what it is that shocks affect in the short-run that then results in persistent education effects; specifically I consider school attendance, time spent at school, child labour and health. As discussed in Section 1.5, I use a 2SLS model to establish firstly whether shocks affect these in the short-term (Equation (2)) and secondly whether this effect explains the long-run impact of shocks on schooling (Equations (3)), circumventing the problem of endogeneity that arises in examining the relationship between these short-term factors and long-term schooling. The validity of this approach relies on the exogeneity and relevance of crop shocks interacted with gender and age-cohort as instruments, as well as their excludability. As discussed in the main findings, exogeneity of crop shocks is not overly concerning in within

[14] As mentioned before, I use the same baseline data as them and analyse a sub-sample of the individuals in their sample (those who were there in the first wave of the baseline and who were re-interviewed in 2004).

household level analysis; relevance of the instruments is therefore easily tested. However, the validity of this strategy also depends on the satisfaction of the exclusion restriction which requires there to be only one individual level channel for the effect of shock on schooling. While the exclusion restriction is never testable, in this case a-priori it is particularly concerning since from the outset I consider more than one plausible channel. Even if only one of these is relevant, the results are still only valid under the strong assumption that all the plausible channels are accounted for here. Given these limitations, I, therefore, treat the analysis in this section as entirely exploratory rather than conclusive.

The results are presented in Table 0.12 and Table 0.14; Table 0.13 shows the relative age and gender specific effects in the first stage results. School attendance is measured by whether a child went to school the year of the shock and the number of hours a child was spending at school per week at the time of the baseline interview up to a year after the shock. Two measures of child labour are used: a combined measure of the time spent on chores and economic activities in the week preceding the baseline survey up to a year after the shock[15], and a measure of the time spent on chores only. The inclusion of the latter measure is motivated by the findings in BDG that the relationship between child labour and shocks is driven by the impact that shocks have on time spent on chores in particular. Finally, health is measured using the standardised Body Mass Index (BMI).

Table 0.12 shows that shocks have by far the strongest short-term impact on the amount of time children spend on chores. In particular, shocks raise the amount of time that older children work relative to the younger children in the household. This effect is especially large and significant for older girls, increasing the already significant gap between them and the younger children. Relative to younger girls this increase is from 3 hours per week to 8.5 hours per week, and relative to younger boys from 4 hours per week to 7.6 hours. In addition, there is a reversal in the

[15] Economic activities for children consist predominately of farming and looking after livestock, which typically includes tasks such as tending crops in the field and herding livestock as well as processing crop and livestock products. Chores include activities such as fetching water and firewood, cleaning, looking after the house and preparing meals.

gap between older boys and younger girls; while in non-shock households younger girls spend approximately 2.5 more hours per week on chores than older boys, following a shock the amount that older boys work relative to the younger girls increases by just over 4 hours per week. Finally, there is also some weak evidence of an increase in the amount of time that older boys work relative to younger boys, but this effect is not quite statistically significant.

The evidence of a link between shocks and schooling in the short-run is much weaker. There is some indication that shocks may actually increase the amount of time that older boys spend at school relative to the other children in the household, although this change is only statistically significant relative to younger boys. There is no indication of the significant effect of shocks on probability of attending school in the year of the shock that BDG find[16].

Finally, there is evidence of an adverse short-term impact of crop-shocks on the health of older girls. There is a reduction in their BMI relative to all the other children in the household, although it is only statistically significant relative to the younger girls. While in non-shock households there if no significant difference in the BMI of these two groups, shocks reduce the BMI of older girls relative to younger girls by nearly half of a standard deviation.

To sum up, the results suggest that within the first year, crop shocks have the strongest impact on the amount of time that children, especially older girls, spend on chores. There is some additional evidence of an adverse effect on the health of these girls, as well as a weak positive effect on the time that older boys spend at school relative to the other children in the household.

[16] There are two main reasons why this may be the case. Beegle et al conduct the analysis at a within household level but across the four waves. The analysis here is at a within household *and* wave level. The differences in the findings may, therefore, reflect that shocks have an identical effect on school attendance of all children in the household and therefore the relative probabilities within the household do not change. The alternative explanation is that there is a bias from time-variant household level unobservables in BDG's results.

Table 0.12: Channels of long-run shock impact on education - first stage

	BMI(z-score)	School in past year	School hours in past week	Work hours in past week	Chore hours in past week
Crop shock * female	0.084	-0.006	1.783	-0.299	-1.868
	(0.275)	(0.129)	(3.307)	(3.102)	(1.734)
Crop shock * age 12-15	-0.033	0.061	5.068	4.875	2.365
	(0.281)	(0.122)	(3.642)	(3.479)	(1.731)
Crop shock * female * age 12-15	-0.401	-0.054	-5.265	-0.960	3.152
	(0.372)	(0.178)	(5.155)	(4.520)	(2.387)
Age:12-15	-0.321	0.041	1.494	-2.204	-1.496
	(0.272)	(0.121)	(3.653)	(3.406)	(1.710)
Female	-0.083	-0.078	-2.188	-2.200	0.991
	(0.255)	(0.118)	(2.930)	(2.752)	(1.515)
Female * age 12-15	0.710**	0.051	3.600	10.413***	4.499**
	(0.341)	(0.163)	(4.600)	(4.015)	(2.034)
Number of observations	1,088	1,150	1,150	1,150	1,150

note: *** p<0.01, ** p<0.05, * p<0.1. Additional controls are the same as in Table 0.6.

Table 0.13: Interaction effects for Table 0.12

	BMI (z-score)	School in past year	School hours in past week	Work hours in past week	Chore hours in past week	
	Shock	Shock	Shock	Shock	Shock	Non-shock
Female Old - Female Young	-0.434**	0.007	-0.197	3.916	5.517***	3.003**
Female Old - Male Young	-0.350	0.001	1.587	3.617	3.649*	3.993**
Female Old - Male Old	-0.317	-0.060	-3.481	-1.258	1.285	5.490***
Female Young - Male Young	0.084	-0.006	1.783	-0.299	-1.868	0.991
Female Young - Male Old	0.117	-0.067	-3.285	-5.174**	-4.232***	2.487**
Male Young - Male Old	0.033	-0.061	-5.068*	-4.875	-2.365	-1.496

note: *** p<0.01, ** p<0.05, * p<0.1. Derivations of the interaction effects are presented in Appendix B

The weakness of the within household age-cohort and gender specific effect of crop shocks on child health, schooling attendance, and time spent at school is confirmed by the low power that this variation has in predicting the within household distribution of these outcomes; the Cragg-Donald F-statistic barely exceeds one for any of these (Table 0.14). Unsurprisingly, it is highest for time spent on chores, at 4.6, suggesting that of all the short-term effects considered, only the impact on chores may be sufficiently strong to have some power in explaining the long-term effect of shocks on schooling.

Consistently with this, using crop shock interacted with child age-cohort and gender to instrument for the time spent on chores in the baseline, I find a significant negative association between this form of child labour and the long-term probability of primary school completion (Table 0.14). Further, it is evident from the first stage that the power of the instruments is driven by the increase in the chore burden among older girls relative to the younger children in response to a crop shock; the negative chore effect in the second stage is, therefore, likely to be most relevant in explaining the impact of shocks on educational attainment of these older girls. Accordingly, instrumented time spent on chores is not significantly associated with years of schooling – an outcome that shocks affect mainly through boys.

Although the age and gender specific shock effects have the highest predictive power with respect to time spent on chores, it is still too low for these to plausibly be used as instruments. However, this predictive power increases substantially in a first stage specification which allows more age and gender-specific variation in the effect of shocks on chores[17]. Table 0.15 shows that an instrument set consisting of a parsimonious sub-set of more detailed age-cohort and gender shock interactions[18] yields a somewhat more convincing first stage, with a Cragg-Donald F-statistic or 10.1. This increase in the power of the first stage, results in a slight decrease in the magnitude of the second stage estimate of the association between predicted time spent on chores in childhood and probability of primary school completion, from 2.6 percentage points to 2.1; the significance of the estimate, on the other hand, increases with the power of the instruments.

[17] This more detailed breakdown is motivated by earlier findings, presented in Appendix C Table 0.16 and Table 0.17, that a more detailed break-down by age reveals more age-cohort and gender specific variation in the long-run within household effect of shocks on schooling.

[18] The parsimonious specification contains only the most significant interactions in the instrument set. These are the interactions controlling for the older girl effect which drives the variation in the impact of shocks on chores.

Table 0.14: : Channels of long-run shock impact on education - second stage

	Completed Primary	School Years	Completed Primary	School Years	Completed Primary	School Years	Completed Primary	School Years	Completed Primary	School Years
BMI z-score	0.469	1.404								
	(0.345)	(1.608)								
Attended school last year			0.383	3.406						
			(1.194)	(7.268)						
Total hours at school in the last week					0.019	0.156				
					(0.023)	(0.154)				
Work hours in the past week							-0.011	0.003		
							(0.014)	(0.081)		
Chores hours									-0.026*	-0.072
									(0.015)	(0.086)
Cragg-Donal F-statistic	1.19		0.18		0.66		1.80		4.60	
Number of observations	1,088	1,088	1,150	1,150	1,150	1,150	1,150	1,150	1,150	1,150

note: *** p<0.01, ** p<0.05, * p<0.1. Additional controls are the same as in Table 0.6.

Table 0.15: 2SLS Impact of hours spent on chores in childhood on primary school completion in adulthood

	First-stage regression: chore hours	Primary	First-stage regression: chore hours	Primary (parsimonious)	First-stage regression: chore hours	Primary (parsimonious)
Home chores		-0.017		-0.013^		-0.021**
		(0.012)		(0.008)		(0.010)
Crop shock * female	-6.192**		-6.922***		-3.547***	
	(2.513)		(1.910)		(1.344)	
Crop shock * age 9-11	-4.234		-4.508*			
	(2.818)		(2.371)			
Crop shock * age 12-13	0.125					
	(2.508)					
Crop shock * age 14-15	0.371					
	(2.332)					
Crop shock * female * age 9-11	8.697**		9.443***			
	(3.718)		(3.336)			
Crop shock * female * age 12-13	7.322*		6.722***		5.294***	
	(4.046)		(1.668)		(1.716)	
Crop shock * female * age 14-15	7.814**		9.862***		8.380***	
	(3.582)		(1.703)		(1.588)	
Cragg-Donald F-statistic		2.5		7.8		10.2
Number of observations	913	913	913	913	913	913

note: *** p<0.01, ** p<0.05, * p<0.1, ^ p<0.12. Additional controls are the same as in Table 0.6.

Although it is possible to show with some level of credibility that within household age-cohort and gender variation in chores has sufficient predictive power to constitute a relevant instrument set, this alone does not ensure reliability of the second stage results; the exclusion restriction must also be satisfied. While the exclusion restriction can never be tested directly, in this instance there are two factors that call it into question. Firstly, as shown in Table 0.12, there is a clear indication that shocks also affect the health of the older girls. Combined with the evidence in Table 0.6, where severe stunting is included as a control and where it has a nearly significant negative effect on the probability of primary school completion, this first stage relationship suggests that the health effects may also be a relevant part of the story. Secondly, there are some notable disparities in the long-run shock effects on schooling and first stage shock effects on time spent on chores. In particular, while there is no worsening in the chances of primary school completion for older girls relative to younger boys, there is a significant increase in the amount that the older girls work relative to these boys. This generalises to the broader lack of any impact of shocks on younger boys through chores, despite some evidence of a worsening in their educational outcomes relative to the younger girls in the household as the result of shocks. Further the relative increase in the chore burden extends beyond older girls to all older children; however, there is no evidence of a matching adverse shock effect on the chances of primary school completion for older boys.

Finally, even looking at the effects that "match" in the two stages, there is a discrepancy in the magnitudes. While shocks result in a 21 percentage point decrease in the chances of primary school completion of older girls relative to younger girls (Table 0.6), shock induced variation in amount of time spent on chores explains only just over half of this decrease.

Overall, this exploration yields no insight into the mechanisms for the effects that crop shocks have on boys' schooling. The findings are, however, consistent with an increase in the work-burden of older girls in response to the shock. Within the outlined household decision making framework this can happen if crop shocks increase the marginal productivity of the work that is undertaken specifically by the older girls within the household[19]. Since there is no evidence of a shock impact on time spent at school in the short-term, the long-term effect of the increase in time spent on chores on the girls' schooling is more likely to reflect reduced capacity to perform at school due to for instance fatigue or insufficient time for homework. This is, however, no more than a plausible narrative for the main findings. The limited validity of shocks as instruments rules out definitive conclusions. While shocks may be a good source of external variation, the exclusion restriction has to be considered very carefully as there is evidence to suggest that even at the individual level they can have a wide variety of effects.

[19] See Edmonds (2006) for a model of sibling differences in child labour.

1.8. Conclusions

This paper looks at whether experiencing idiosyncratic income shocks in childhood permanently affects schooling. While there is an extensive literature on the short and medium-term household and individual level effects of income shocks, evidence on the longer-term persistence of these is sparse. I utilise a 13 year individual panel data-set from rural Tanzania to look at the effect of crop shocks in childhood (age 7-15) on schooling attained by adulthood (age 17-28). I add to the existing literature in two ways. Firstly, the long time-span of the data allows to estimate effects that are likely to be permanent as the chance of catch-up in schooling is very low by the time that individuals reach adulthood. Secondly, I emphasise the effect of shocks on the gender and age-cohort distribution of schooling within the household where individuals grow up. This enables me to analyse cohort-specific heterogeneity of shock effects and the evolution of schooling differences between cohorts on which there is limited evidence, while addressing many concerns about endogeneity of income shocks.

The results suggest that shocks have permanent effects on gender and age-cohort specific distribution of education within the household. The most adversely affected groups within the household are older girls and younger boys; they experience a reduction in their final educational attainment relative to the other children who they live with. For the older girls this effect comes through mainly with respect to chances of primary school completion. For younger boys, on the other hand it is most noticeable in the total number of years of schooling that they attain in adulthood.

The paper further explores a number of channels for these effects. I utilise data on the short-run effects of shocks on child schooling, work and health to examine the extent to which these may explain the longer-term effects. I use a 2SLS strategy to investigate whether there is evidence to suggest that the short-run impact of the shock on these outcomes explains the longer term effects that I find shocks to have on schooling. While there is only weak evidence of associations between shocks health and schooling in the short-term, they appear to have quite substantive effects on the gender and age-cohort distribution of child labour, and more specifically chores within the household. Further these effects appear to be persistent and relevant in explaining the long term impact of shocks on schooling. Because the excludability of the gender and age specific shock effects as instruments is questionable, these results are only treated as indicative of the likelihood that child labour is one of the channels for the long-run impact of shocks on schooling.

Existing work shows that shocks have a variety of adverse short and medium-term effects on child welfare, in particular through health and schooling. This paper offers evidence that the education effects in particular can persist into adulthood when they are unlikely to be reversed. In addition, the results suggest that there is substantial heterogeneity in the vulnerability of children within the household to shocks, which may, in part, be due to differences in their within household roles and responsibilities. Such striking results make a weighty contribution to the already hefty portfolio of reasons for the importance of providing households and communities with better means of coping with adversity in risky environments with poorly functioning credit and insurance markets.

Appendix A

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Appendix B

Derivation of interaction effects

Main Model:

$$S_{ijt} = \alpha_0 + \alpha_1 Shock_{j,t-13} + \alpha_2 Female_{ij} + \alpha_3 Old_{ij,t-13} + \alpha_4 Shock_{j,t-13} * Female_{ij} + \alpha_5 Shock_{j,t-13} * Old_{ij,t-13} + \alpha_6 Female_{ij} * \alpha_3 Old_{ij,t-13} + \alpha_7 Shock_{j,t-13} * Female_{ij} * Old_{ij,t-13} + \alpha_8 X_{ijt-13} + \theta_{ij} + e_{ijt}$$

Where the variables are defined as in Equation (1) and *Old* indicates being in the older age-cohort in the sample (i.e. age 12-15) at the time of the baseline survey.

$$\frac{\partial S_{ijt}}{\partial Shock_{j,t-13}}(female = 0, old = 0) = \alpha_1$$

$$\frac{\partial S_{ijt}}{\partial Shock_{j,t-13}}(female = 1, old = 0) = \alpha_1 + \alpha_4$$

$$\frac{\partial S_{ijt}}{\partial Shock_{j,t-13}}(female = 0, old = 1) = \alpha_1 + \alpha_5$$

$$\frac{\partial S_{ijt}}{\partial Shock_{j,t-13}}(female = 1, old = 1) = \alpha_1 + \alpha_4 + \alpha_5 + \alpha_7$$

$$S_{ijt}|_{female=0,old=0} = \alpha_0 + \alpha_1 + \alpha_8 X_{ijt-13} + \theta_{ij} + e_{ijt}$$

$$S_{ijt}|_{female=1,old=0} = \alpha_0 + \alpha_2 + \alpha_1 + \alpha_4 + \alpha_8 X_{ijt-13} + \theta_{ij} + e_{ijt}$$

$$S_{ijt}|_{female=0,old=1} = \alpha_0 + \alpha_3 + \alpha_1 + \alpha_5 + \alpha_8 X_{ijt-13} + \theta_{ij} + e_{ijt}$$

$$S_{ijt}|_{female=1,old=1} = \alpha_0 + \alpha_2 + \alpha_3 + \alpha_6 + \alpha_1 + \alpha_4 + \alpha_5 + \alpha_7 + \alpha_8 X_{ijt-13} + \theta_{ij} + e_{ijt}$$

From the above - the relative shock effects are as follows:

	Shock Effect	Non-shock household
Female Old - Female Young	$\alpha5+\alpha7$	$\alpha3+\alpha6$
Female Old - Male Young	$\alpha4+\alpha5+\alpha7$	$\alpha2+\alpha3+\alpha6$
Female Old - Male Old	$\alpha4+\alpha7$	$\alpha2+\alpha6$
Female Young - Male Young	$\alpha4$	$\alpha2$
Female Young - Male Old	$\alpha4-\alpha5$	$\alpha2-\alpha3$
Male Young - Male Old	$-\alpha5$	$-\alpha3$

Appendix C

Table 0.16: Impact shock in childhood on schooling attainment in adulthood (detailed age-cohort break-down, CHFE)

	Primary	School Years
Crop shock * age 9-11	0.179	0.958
	(0.121)	(0.759)
Crop shock * age 12-13	0.261**	1.948***
	(0.131)	(0.724)
Crop shock * age 14-15	0.225**	1.561**
	(0.114)	(0.746)
Crop shock * female	0.226*	1.105
	(0.136)	(0.729)
Crop shock * female * age 9-11	-0.074	0.257
	(0.171)	(0.974)
Crop shock * female * age 12-13	-0.530***	-3.178***
	(0.202)	(1.175)
Crop shock * female * age 14-15	-0.281*	-0.678
	(0.160)	(1.027)
Age: 9-11	-0.057	-0.228
	(0.110)	(0.645)
Age: 12-13	-0.131	-1.135*
	(0.126)	(0.651)
Age: 14-15	0.064	0.253
	(0.113)	(0.696)
Age rank	-0.005	-0.093
	(0.019)	(0.138)
Female	-0.206*	-1.011*
	(0.124)	(0.598)
Female * age 9-11	0.085	-0.352
	(0.152)	(0.798)
Female * age 12-13	0.469***	2.793***
	(0.179)	(0.999)
Female * age 14-15	0.089	-0.730
	(0.134)	(0.772)
Number of observations	1,150	1,150

note: *** p<0.01, ** p<0.05, * p<0.1. Additional control are the same as in main specification (Table 0.6)

Table 0.17: : Interaction Effects for Table 0.16

	Completed Primary		School Years	
	Shock household	Non-shock household	Shock household	Non-shock household
Boy Age 7-8 - Boy Age 9-11	-0.179	0.057	-0.958	0.228
Boy Age 7-8 - Boy Age 12-13	-0.261**	0.131	-1.948***	1.135*
Boy Age 7-8 - Boy Age 14-15	-0.225**	-0.064	-1.561**	-0.253
Boy Age 9-11 - Boy Age 12-13	-0.083	0.073	-0.990	0.907^
Boy Age 9-11 - Boy Age 14-15	-0.047	-0.122	-0.602	-0.481
Boy Age 12-13 - Boy Age 14-15	0.036	-0.195**	0.387	-1.388***
Girl Age 7-8 - Boy Age 7-8	0.226*	-0.206*	1.105^	-1.011*
Girl Age 7-8 - Boy Age 9-11	0.047	-0.148	0.147	-0.783
Girl Age 7-8 - Boy Age 12-13	-0.036	-0.075	-0.843	0.124
Girl Age 7-8 - Boy Age 14-15	0.000	-0.270**	-0.456	-1.264*
Girl Age 9-11 - Boy Age 7-8	0.330***	-0.179	2.320***	-1.591***
Girl Age 9-11 - Boy Age 9-11	0.152	-0.121	1.362**	-1.363***
Girl Age 9-11 - Boy Age 12-13	0.069	-0.048	0.372	-0.456
Girl Age 9-11 - Boy Age 14-15	0.105	-0.243***	0.760	-1.844***
Girl Age 12-13 - Boy Age 7-8	-0.043	0.133	-0.125	0.647
Girl Age 12-13 - Boy Age 9-11	-0.221^	0.190	-1.084	0.875
Girl Age 12-13 - Boy Age 12-13	-0.304**	0.263**	-2.073***	1.782***
Girl Age 12-13 - Boy Age 14-15	-0.268**	0.068	-1.686**	0.394
Girl Age 14-15 - Boy Age 7-8	0.170	-0.052	1.988***	-1.488**
Girl Age 14-15 - Boy Age 9-11	-0.008	0.005	1.029	-1.260*
Girl Age 14-15 - Boy Age 12-13	-0.091	0.078	0.040	-0.353
Girl Age 14-15 - Boy Age 14-15	-0.055	-0.117^	0.427	-1.741***
Girl Age 7-8 - Girl Age 9-11	-0.105	0.027	-1.216**	-0.580
Girl Age 7-8 - Girl Age 12-13	0.268*	0.339***	1.230	1.658**
Girl Age 7-8 - Girl Age 14-15	0.055	0.154	-0.883	-0.477
Girl Age 9-11 - Girl Age 12-13	0.373***	-0.312***	2.446***	-2.238***
Girl Age 9-11 - Girl Age 14-15	0.160	-0.127	0.333	-0.103
Girl Age 12-13 - Girl Age 14-15	-0.213^	0.185^	-2.113**	2.135***

note: *** p<0.01, ** p<0.05, * p<0.1, ^ p<0.13; derivations of the interaction effects are available on request from the author.