Impact of Child Labour on Educational Attainment in Adulthood: Evidence from Rural Tanzania

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By

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Abstract

This study examines whether and to what extend child labour affects educational attainment in adulthood. While there is a substantial body of literature investigating child labour, long-run effects remain almost entirely unexplored due to lack of panel data spanning a sufficiently long period of time. This study uses a newly available 13 year individual level panel data set from rural Tanzania to show that child labour reduces educational attainment in adulthood. Individuals who had worked an average amount as children (18 hours a week) attain just over half the number of years of education in adulthood compared to those who had not worked. This loss of education translates into a 7 percentage point loss in long-run consumption growth. The impact of child labour is, however, found to be substantially smaller among those who were already attending school at the time of the baseline survey.

1 Introduction

This study examines the impact of child labour on educational attainment at adulthood. The data used is from a Tanzanian household panel survey that spans 13 years (1991-2004). The findings show that working as a child affects final educational attainment adversely. Further, they suggest that child labour acts as a significant deterrent to starting school; the adverse effects of a given number of child labour hours worked on children not at school during the baseline are significantly stronger than on those already at school.

The study builds on a paper by Beegle et al (2005a)[1], that investigated the relationship between household income shocks and child labour using data on the same households, as that used in this study. Beegle et al find that transitory income shocks lead to increased use of child labour. In addition, they find that access to credit mitigates this effect. These findings suggest that child labour is used as an ex-post coping mechanism in the presence of imperfect credit and insurance markets. Their study only used data on short-term impacts, by focusing on a panel data set from 1991 to 1994. This study aims to investigate the long-run impact of the use of this strategy on accumulation of human capital, by answering the broader question of whether and to what extent child labour

[1] I would like to thank Kathleen Beegle for providing me with the tools and support necessary to build on that paper.

affects final educational attainment. It uses data on the same households, but has access to a further round from 2004, which contains data on educational attainment at adulthood.

The question addressed in this paper is important for a number of reasons. According to the International Labour Organisation (ILO, 2002) there were 211 million economically active children (aged 5–14) in 2000. Despite the high prevalence of child labour, there is little robust empirical evidence to support the general assumption that it has serious adverse long-term effects.

The theoretical literature explaining the existence and inefficiency of child labour is noticeably more comprehensive than the empirical literature. Theoretical studies of child labour tend to analyse it within the broader framework of intra-household resource allocation, pioneered, among others, by Becker, Murphy and Bergstrom (Becker and Murphy, 1988; Bergstrom 1989). Also related are some of the broader poverty trap models which present inefficiencies of resource allocation in the presence of market failures as explanations for poverty persistence. A well known example is the Eswaran and Kotwal (1986) production poverty trap model. Combined, these strands of theoretical literature present children and their life-time labour as a resource that may be used inefficiently in the presence of market failures. The market failures tend to affect poorer households more forcing them to use too much child labour; this in turn has adverse effects on the child's long-term human capital accumulation and, therefore, future earnings.

While there are a number of convincing models of the inefficiency of child labour, that developed by Baland and Robinson (2002), for instance, there is little consistent empirical evidence of these inefficiencies. Put simply, there is little robust evidence of the adverse effects that child labour is widely assumed to have.

The focus of this study is specifically on the relationship between child labour and educational attainment. This link is explored in the literature to an extent, by, among others, Ravallion and Wodon (2000), Canagarajah and Coulombe (1997), and Beegle et al (2005b). However, to my knowledge, this is the first study to analyses the causal relationship between child labour and a measure of final human capital accumulation – educational attainment at adulthood.

Section 2 of this paper provides an overview of the relevant theoretical and empirical literature. Section 3 contains the description of the data, as well as some summary statistics, while the empirical strategy and specifications used are explained in Section 4. Section 5 presents the main results which are discussed in detail in Sections 6. Finally, Section 7 focuses on the robustness of the findings and Section 8 concludes.

2 Literature Review

This section starts with an overview of the relevant theoretical literature on poverty traps and modelling of the child labour decision. This is followed by a discussion of the existing range of empirical evidence of the relationship between child labour and educational attainment.

A poverty trap is defined as an "equilibrium outcome of poor living conditions from which the poor, using their own resources cannot escape" (Dercon, (2003), p.2). A model developed by Eswaran and Kotwal (1986) is a clear example of the concept. They show that due to credit market imperfections poorer households are forced to use inputs inefficiently in production. In their model access to credit depends on the amount of collateral (land) a household possesses. As the result, poorer households are credit constrained and in equilibrium are forced to use too much of the inputs they have in abundance, such as labour, and too few of the inputs that have to be purchased, such as fertilizer. This is an equilibrium outcome in which the poor are locked into inefficient production methods and, therefore, poverty. By the same logic, if poorer households are forced to use inefficiently high levels of child labour in equilibrium, rather than letting the children build up human capital, they may be condemned to poverty persistence through loss of future earnings of the children.

Baland and Robinson (2000) develop a model of child labour demonstrating this narrative using a model of within household resource allocation. The model builds on work of Becker and Murphy (1988) and Bergstrom (1989) who were the first to investigate the conditions for efficient intra-household resource allocation. Baland and Robinson present a two period model in which parents and children live for both periods. The premise of the model is that in order for efficient levels of child labour to be used, parents need to be compensated for the loss of income that results from children going to school instead of working. Efficient use of child labour is defined as a level at which marginal return to education in terms of income is equal to its opportunity cost – lower level child labour[2]. This definition is used to demonstrate the circumstances under which the level of child labour that maximizes family income is inefficiently high.

Their first result shows that assuming parents cannot leave negative bequests, children will be made to work too much when there is no bequest. In this situation parents cannot compensate themselves for loss of income in period 1 by reducing the bequest and children cannot offer credible contracts for compensating parents in the future. In the model bequests are more likely to be at a corner when parents have low human capital endowments themselves, which, in turn is more likely among poorer parents.

Their second result shows that even with positive bequests, a combination of no savings and capital market imperfection also results in excessive use of child labour. If because of market imperfections parents are unable to transfer income from period 2 to period 1 they will use inefficiently high levels of child labour to ensure the desired level of consumption in period 1. The more impatient parents are to consume, i.e. the higher their discount rates are the more the level of child labour used will exceed the efficient level, in the presence of no savings.

This is a model of use of inefficiently high levels of child labour in equilibrium in poorer households. The consequent reduction in future earnings of the children could be viewed as something of a human capital poverty trap.

An alternative narrative of inefficient use of child labour is one presented in Beegle et al (2005a). Using the same data as that used in this study, the authors show that child labour is used as a mechanism for coping with transitory shocks. According to the permanent income hypothesis, the impact of transitory shocks on consumption should be smoothed by borrowing. However, Beegle et al present robust evidence of a causal positive relationship between transitory shocks and intensity of child labour use, especially among

[2] Working as a child is assumed to contribute to human capital accumulation through skills and experience gained.

households with few collateralizable assets. This trend indicates that in the presence of capital market imperfections, households are forced to cope using inefficient mechanisms.

Both of the above narratives assume the inefficiency of child labour. Whether this assumption is justified has been the subject of a number of empirical studies. As this study is concerned the impact of child labour on educational attainment specifically, I focus on this strand of the literature only, although other effects have also been examined; O'Donnell et al (2004), for instance, examine the impact of child labour on health outcomes 5 years later.

One of the most comprehensive attempts to empirically investigate the association between child labour and educational attainment was made by Ravallion and Wodon (2000). They test the assumption that child labour displaces schooling by examining the extent to which an increase in schooling in response to a schooling subsidy is matched by a decrease in child labour. They chose the Food For Education programme (FFE) as the schooling subsidy, information on which is included in the Bangladesh Household Expenditure Survey. A number of serious estimation issues are dealt with, such as finding suitable instruments for the villages and households that participate in the FFE, as well as maximizing the use of the scarce child labour data contained in the survey. In the end, they do robustly find that while FFE has a significant negative impact on child labour and positive impact on probability of being at school, the displacement of child labour represents only about a quarter of the increase in the enrolment rate. They conclude that child labour does not necessarily come at the expense of schooling, although there is a likelihood of it affecting school indirectly through, for instance, reduced preparation time.

An alternative method of examining the association between child labour and schooling was applied by Canagarajah and Coulombe to data from the Ghana LSMS. They use a bi-variate probit model with one latent variable representing the decision to go to school and the other decision to work. This specification allows the schooling and working decisions to be treated as interdependent, without having to make assumptions about the sequence of the schooling and working decision making process. As in Ravallion and Wodon's study (2000), no attempt is made to establish a causal relationship, only a correlation. They find a negative and significant correlation between the error terms of the two latent variable equations, which indicates that there is a trade-off between schooling and child labour.

I am not aware of many empirical studies of the causal relationship between child labour and educational attainment. An attempt was made by Psacharopoulos (1997), who used data from Bolivia and Venezuela to estimate the impact of child labour on schooling. However, as he does not control for the effect of unobserved community, household and individual characteristics no robust causal interpretation can be made. At best, the OLS results indicate some negative association between the two variables.

A far more thorough attempt is a study conducted by Beegle et al (2005b) using panel data from two rounds of the Vietnam LSMS. They estimate the impact of working between the ages of 7 and 15 while attending school in the first round of the survey on educational attainment 5 years later. The problem of omitted relevant unobservable variables is controlled for by instrumenting for child labour, as well as using fixed effects estimation. They find that child labour has a significant negative impact on educational attainment 5 years later.

A number of caveats are in order, however. Firstly, these results apply only to children who were already attending school in the first round of the survey. Secondly, the measurement of educational attainment is not a final one; catch-up of some of the education lost is still possible during the subsequent school-going years. Lastly, the instruments used in the main specification are community level variables. This means that while community level selection bias is controlled for, between and within household selection bias is not.

Overall, progress in determining the impact of child labour on human capital accumulation appears to be hindered by lack of suitable data. While robust results have been attained in determining whether there is an association between the two, establishing a causal relationship requires substantially more comprehensive data. Specifically, individual level panel data is necessary spanning from when individuals worked as children until the time that they complete their education. Furthermore, the dataset should contain detailed employment data, as well as data on individual, household and community characteristics. The completion of the 5^{th} round of Kagera Health and Development Survey (KHDS) has made such a dataset available. This study aims to use the new opportunities offered by the KHDS data to contribute to the existing work on child labour by establishing whether and in what way child labour impacts final educational attainment.

3 Data Description and Summary Statistics

The data used for this study come from the Kagera Health and Development Survey (KHDS) conducted by the World Bank, Muhimbili University College of Health Sciences (MUCHS) and University of Dar es Salaam. This is a panel Living Standards Measurement Survey (LSMS) conducted in Kagera region in the North West of Tanzania between 1991 and 2004. In total 5 rounds of the survey were conducted between 1991 and 1994 and again in 2004. In the first 4 rounds of the survey (KHDS1[3]), about 915 households were interviewed for up to 4 times at 6-7 month intervals. The aim of the 2004 round (KHDS2[4]) was to track members of the KHDS1 households. The extensive tracking phase of the 2004 round ensured very low attrition rates; 93 percent of the baseline households and 82 percent of individuals[5] were re-contacted. In addition, the baseline 832 households that were re-contacted expanded into 2,700 households in the 10 year between the rounds (Beegle et al, 2006).

This paper uses data on panel households that had at least one member between the ages of 7 and 15 during KHDS1 to examine the impact of child labour on human capital accumulation. The variable used to measure this outcome is total years of schooling completed by the time of KHDS2. As the great majority of those in the group of interest had completed their education by KHDS2, this variable is treated as an adequate measure of educational outcome[6].

The main input variable is amount worked as a child. The definition of child labour is consistent with that used by Beegle et al "…total hours spent working in economic activities and chores in the previous week (including fetching water and firewood, preparing meals, and cleaning the house)." (Beegle et al, (2005a), p.6). Although, past

[3] Throughout the study, the first four rounds of the survey (1991-1994) will be referred to as KHDS1.
[4] Throughout the study, the last round of the survey (2004) will be referred to as KHDS2
[5] This is excluding those who had died.
[6] Table 1 shows that only 8 percent of the group of interest were still at school in 2004. See Section 7.2 for further discussion of this variable.

studies have excluded chores from the definition of child labour, Canagarajah and Coloumbe (1997) for instance, they are included here for a number of reasons. Firstly, as argued by Beegle et al (2005a), the ILO definition of child labour includes all work that stunts or limits child's development or puts the child at risk. This definition does not preclude chores from being considered. Secondly, the distinction between economic activities and chores in the context of rural agricultural households is not as evident as it would be in an urban context. It may, therefore, be impossible to isolate the effect of child labour that does not incorporate chores. Finally, according to previous findings, inclusion of chores reduces the impact of child labour on human capital indicators (Canagarajah and Coloumbe, 1997). Consequently, inclusion of chores is not likely to lead to an overestimation of the impact of child labour.

Summary statistics are presented in Table 1 and Table 2. The first column in Table 1 presents aggregate data for all those who were between the ages of 7 and 15 in at least one round of KHDS1 and had been re-interviewed. These individuals constitute 76 percent of all those who were in the age-group of interest in the baseline sample. The second and third columns disaggregate the data by whether individuals worked more or less than the average number of hours[7].

Overall, individuals in the sample had worked an average of 18 hours per week as children. Among those who had worked more than average this figure is 27 hours per week, compared to 9 hours per week among those who had worked less than average. Mean age of individuals at last KHDS1 interview is just under 12. Those who had worked more than average were also older during the baseline than those who had worked less than average. Three-quarters of the individuals in the whole sample had attended school at some point by the time of the last KHDS1 interview[8].

[7] The data cannot be disaggregated by those who had and had not been engaged in child labour, since only 3 percent of those who were re-interviewed, are in the latter category; doing at least some work is the norm in these settings.

[8] Throughout the study the proportion of children who had ever attended school by the time of the last KHDS1 interview will be used as the KHDS1 schooling indicator. Note that this group includes those who were attending school at the time of the last interview, as well as those who were not attending school at the time of the interview. The reasons for using this as the schooling indicator are discussed in Section 4.

The middle section of the table presents statistics on the variables used to control for some observable initial household characteristics and those used to instrument for child labour. The variables used for the former purpose include measures of per capita consumption expenditure, value of land and collateralizable assets[9]. Out of these variables possession of collateralizable assets appears to be most correlated with amount worked as a child; those who had worked more appear to have also lived in households with slightly fewer collateralizable assets than those who worked less. The variables used as instruments for child labour include an indicator of whether a household experienced a transitory shock[10] and the interaction of this indicator with log of value of per capita collateral owned by household. Table 1 shows some evidence of a correlation between incidence of shocks and child labour; a higher proportions of individuals who had worked more than average as children also lived in households affected by shocks. An in-depth discussion of how the instruments were selected is presented in Section 5.

The last section of the table summarises the outcome of interest – final educational attainment. By 2004, 15 percent of respondents had still never attended school. This proportion is slightly higher among those who worked an above average amount of time as children. Further, an average of 7 years of schooling was attained; the difference in this outcome between those who had worked more as children and those who had worked less is negligible.

[9] Collateralizable assets include cash, physical assets and durables (e.g. Radio, bicycle, livestock) and exclude land.
[10] The shock indicator is equal to 1 for households that experienced some crop loss due to a transitory shock, such as fire, pests and other calamities, in the year preceding each of the KHDS1 interviews.

Table 1: Summary Statistic

	Full sample		Children working below mean number of hours		Children working above mean number of hours	
Proportion re-interviewed in KHDS2	0.76		0.73		0.78	
KHDS1						
Average labour hours per week (chores + economic activities)†	18.00	(11.56)	8.81	(4.99)	26.66	(9.04)
Age†	11.64	(2.76)	10.41	(2.70)	12.80	(2.27)
Female†	0.49	(0.50)	0.46	(0.50)	0.52	(0.50)
Had ever attended school (by last KHDS1 interview) †	0.75	(0.43)	0.68	(0.47)	0.82	(0.39)
Father's schooling: 1-6 years†	0.43	(0.50)	0.42	(0.49)	0.44	(0.50)
Father's schooling: 7 years†	0.31	(0.46)	0.33	(0.47)	0.29	(0.45)
Father's schooling: 8+ years†	0.12	(0.32)	0.13	(0.34)	0.11	(0.31)
Mother's schooling: 1-6 years†	0.35	(0.48)	0.35	(0.48)	0.35	(0.48)
Mother's schooling: 7 years†	0.31	(0.46)	0.32	(0.47)	0.30	(0.46)
Mother's schooling: 8+ years†	0.02	(0.13)	0.03	(0.17)	0.01	(0.08)
Shock: proportion of households experiencing crop loss due to a transitory shock†	0.84	(0.36)	0.82	(0.38)	0.86	(0.34)
Mean share of the value of crop loss to total value (if experienced shock) †	0.13	(0.11)	0.13	(0.11)	0.13	(0.11)
Mean log expenditure per capita†	10.37	(0.63)	10.39	(0.65)	10.35	(0.61)
Mean log value of collateral per capita†	10.26	(1.50)	10.38	(1.52)	10.14	(1.48)
Mean log value of land per capita†	10.67	(1.23)	10.66	(1.26)	10.69	(1.21)
KHDS2						
Had ever attended school by KHDS2	0.85	(0.36)	0.87	(0.34)	0.83	(0.37)
Still at school (KHDS2)	0.08	(0.28)	0.13	(0.34)	0.04	(0.20)
Highest grade attained by KHDS2	6.95	(2.02)	6.99	(2.00)	6.91	(2.04)
Observations	1,490		724		766	

*Standard deviations are in parentheses
†KHDS1 data

This study focuses on the impact of child labour on final educational attainment. Table 2 shows more detailed statistics on interaction between schooling and work among children from different age-groups. All statistics are disaggregated by school attendance status (i.e. whether a child had ever attended school by last KHDS1 interview), as well as age-group, separating those between the ages of 7 and 11 from those between the ages of 12 and 15.

Overall, those who had attended school at some point by the time of the last KHDS1 interview appear to have also worked slightly more on average. However, as can be seen from the Table, these individuals are also on average 3 years older than those who had not.

To control for the effect of the age differences the statistics are further broken down by age-group. The trends among the 7 to 11 year olds are similar to the overall trends. This is not, however, the case for the older children. In this group 12 percent of children had never attended school by the time of the last KHDS1 interview. These individuals worked noticeably more than those who had attended school. While the average time spent on economic activities among the latter group was 9 hours per week, among those in the former group it was nearly twice as much, at 16 hours. Further, as expected, there is a substantial difference in the final educational attainment of those who had and had not ever attended school by the last KHDS1 interview in the older group, at, on average, 1.5 and 6.6 years respectively.

Overall, a negative association between work and school appears to become pronounced among teen-agers. Before then trends are similar irrespective of work status. As children get older, however, those who are not at school begin to work substantially more than those who are. Further, as children get older, if they do not go to school, an increasing proportion of their time is spent on economic (wage earning) activities. This may act as a further deterrent to starting school if households become reliant on the wages earned by the children. However, thorough regression analysis is necessary before any causality can be attached to the observed trends.

Table 2: A closer look at interaction between working and school attendance†

	Total		Never been to school by last KHDS1 interview		Had been to school at some point by last KHDS1 interview□	
Overall						
Age	11.64	(2.76)	9.52	(2.62)	12.35	(2.42)
Mean hours worked	18.00	(11.56)	15.18	(13.97)	18.92	(10.48)
Mean hours worked: (economic activities)	7.27	(7.33)	6.69	(8.75)	7.47	(6.78)
Mean hours worked: (chores)	10.72	(7.44)	8.50	(8.36)	11.46	(6.96)
Final educational attainment in years (KHDS2) Δ	5.99	(3.05)	4.00	(3.42)	6.64	(2.60)
Age 7-11						
Age	8.98	(1.42)	8.26	(1.30)	9.5	(1.26)
Mean hours worked	12.45	(8.92)	10.33	(9.66)	13.96	(8.03)
Mean hours worked (economic activities)	4.54	(5.42)	3.89	(6.25)	5.0	(4.69)
Mean hours worked (chores)	7.91	(6.34)	6.44	(6.44)	8.96	(6.06)
Final educational attainment (KHDS2) Δ	5.94	(3.03)	4.76	(3.22)	6.77	(2.59)
Age 12-15						
Age	13.90	(1.12)	13.66	(1.19)	13.93	(1.11)
Mean hours	22.70	(11.46)	31.12	(14.12)	21.68	(10.67)
Mean hours worked (economic activities)	9.6	(7.92)	15.86	(9.54)	8.84	(7.35)
Mean hours worked (chores)	13.10	(7.48)	15.25	(10.23)	12.84	(7.04)
Final Educational attainment (KHDS2) Δ	6.03	(3.06)	1.49	(2.82)	6.57	(2.61)
Observations	1,490		685		805	

*Standard deviations are in parentheses

†Note all statistics, with the exception of final educational attainment, are for KHDS1

□Includes all children who reported having attended school at some point by the last KHDS1 interview. Some of the children included were not at school at the time of the survey, but had previously attended school.

ΔIndicates the total years of schooling attained

4 Empirical Strategy and Specification

This section presents the specifications used to investigate the relationship between child labour and final educational attainment[11]. In all the specifications the outcome of interest is years of schooling attained. This is the final educational attainment for the great majority of individuals in the sample and is, therefore, considered a suitable outcome measure[12]. The treatment[13] of interest is an indicator of the intensity of labour undertaken as a child.

4.1 Basic OLS specification

The main specification for examining the effect of child labour on educational attainment in this study is:

$$Y_{ijr+s} = \beta_0 + \beta_1 X_{ijt} + \beta_2 \overline{H}_{ij} + \beta_3 S_{ijt} + \beta_4 W_{jt} + \varepsilon_{ijt+s} \tag{1}$$

The outcome Y_{ijr+s} is measured up to 13 years after the treatment and constitutes years of schooling attained by individual *i* in household *j* in period *t+s*, where *t* is time of last KHDS1 interview and *s* is time since last KHDS1 interview, so that *t+s* is at the time of KHDS2. X_{ijt} and W_{jt} are vectors of observable individual, household, and community characteristics for individual *i* from household *j* at time *t*, where *t* is time of last KHDS1 interview. The characteristics included in the X_{ijt} vector are age and sex of individual *i*, parental education, as well as district and seasonal dummies. Vector W_{jt} contains control variables for household wealth and consumption, which include the log of the value of

[11]None of the estimation techniques used in the study account for the censoring of the educational attainment variable at 0. The significance of this problem was tested by using a tobit to estimate the main specification. The results do not differ significantly from those found using OLS. This is not surprising as the dependent variable only takes a value of zero in 14 percent of observations in the sample.

[12] This assumption is discussed in more detail in Section 7.

[13]In line with practice related to impact evaluations, child labour is referred to as the 'treatment' to be investigated.

per capita consumption, land and other collateral. Inclusion of these vectors of variables in the specification allows to control for observable individual, household and community characteristics relevant to the schooling and working decisions. $\overline{H}_{ij}$ is hours worked per week as a child during KHDS1 by individual *i* in household *j*, averaged across the number of times the individual was interviewed. Finally, S_{ijt} is an indicator variable of whether individual *i* from household *j* had ever attended school by time *t* (time of the last KHDS1 interview)[14].

The coefficient of interest in the OLS specifications is β_2. The literature tentatively suggests that child labour has a negative impact on human capital accumulation (Beegle et al, 2005b). In the context of this study, such a conclusion would be consistent with negative impact of child labour on final educational attainment i.e. $\beta_2 < 0$.

4.2 Potential Problems

There are a number of potential problems associated with the above specification and using OLS to estimate it. These include attenuation bias, between and within household selection bias, and endogeneity of the schooling and child labour decisions. Each is addressed in turn below.

To begin with, the analysis includes all children; those who were and were not at school during KHDS1. The child's schooling status at that time must explain a significant proportion of variation in educational attainment at adulthood and must, therefore, be included in the specification. However, as schooling and working decisions are endogenous, an optimal control for schooling status would be an identified instrument, that affects final educational attainment only through the instrumented schooling variable. It is very difficult to find such a variable as most factors influencing the schooling decision will also influence the working decision.

[14] This is the same indicator of school attendance as the one used in the summary statistics tables (Table 1and Table 2)

Further, it is likely that the amount worked as a child is measured with error. The child labour variable measures time spent on a variety of activities over the week preceding the interview; it is highly unlikely that there was a written record of the time spent on the majority of these. In addition, the respondent was not necessarily the child himself but rather the head of household for instance; this further increases the approximate nature of the statistic. Such inaccuracy may result in inconsistent estimates if the error in the measurement is correlated with the reported values.

Lastly OLS does not allow to control for unobservable individual and household characteristics. Unobservable individual characteristics may make one child in the household more likely to work than another. For instance, Ayalew (2000) attempts to explicitly model the impact of children's unobservable characteristics in allocation of human capital inputs. Using data from Ethiopia he finds that while parents tend to compensate for lower health endowments of children with higher inputs, the reverse is true in case of education. He concludes that concerns of efficiency are prioritized in allocation of educational inputs; therefore, more able children receive more schooling. In the context of this study such selection criteria is not observable, but, if not controlled for, may inflate the apparent negative impact of child labour on educational attainment. More generally, not controlling for such biases exposes OLS results to a risk of within-household selection bias

Finally, unobservable household characteristics may also partly explain whether and for how long a child is sent to school. For instance, Beegle et al (2005a) show that child labour is used as a coping strategy in the presence of credit market imperfections. In the presence of such imperfections, households with more extensive risk-sharing networks may be able to reduce the impact of the shocks that may affect child education adversely. At the same time, however, a household with fewer risk-sharing networks may also be forced to use more child labour. If the risk-sharing capacity of the household is not included in the analysis, child labour may again appear to have inflated impact on educational attainment. In the current specification household wealth variables are included to control for some of these effects. Past studies have found that wealthy household are able to protect themselves better and may have better networks for mutual support (De Weerdt, 2004). However, a high probability of significant unobservable characteristics remains.

Overall, OLS results are likely to be biased and inconsistent due to omission of unobservable characteristics as well as measurement error. They, therefore, at best indicate whether there is an association between the outcome and the treatment, but allow little causal inference to be made

4.3 Potential Solutions

An attempt is made to resolve each of these problems. Firstly, various approaches have been adopted in dealing with the problems associated with inclusion of schooling status in the main specification. Beegle et al (2005b), for instance, exclude all children who were not at school in the baseline from the analysis. However, this introduces selection bias issues and reduces the generality of the results. In this study, a decision was made to include the whole sample in the analysis. Instead, the variable chosen to control for the schooling status attempts to minimize the extent of the simultaneity in the making of the school and working decisions. Rather than including a variable indicating whether a child was at school at the time of the survey, a more general one is used, indicating whether a child has even attended school, irrespective of his schooling status at the time of the survey. As discussed above instrumenting this decision would be the optimal, but almost implausible solution[15]. Within the scope of this study, the most that can be done is to reduce the potential negative impact of including the schooling variable in the main specification. In addition, the problem is less serious than may seem at first since the attendance variable refers to a period *t-10* compared to the left hand side variable.

[15] See Section 7 for further discussion of this issue.

The problem of omitted variables and measurement error is addressed through use of instrumental variable specification and fixed effects estimation. The details are discussed below.

4.4 Instrumental Variables

The instrumental variable specification is:

$$H_{ijt} = \phi_0 + \phi_1 shock_{jt} + \phi_2 collateral_{jt} + \phi_3 X_{ijt} + \phi_4 shock * collateral + \phi_7 S_{ijt} + v_{ijt} \qquad (2)$$

$$Y_{ijr+s} = \beta_0 + \beta_1 X_{ijt} + \beta_2 \hat{\bar{H}}_{ij} + \beta_3 S_{ijt} + \beta_4 W_{jt} + \varepsilon_{ijt+s} \qquad (3)$$

The first stage, or reduced form regression includes $shock_{jt}$ and interaction between shock and collateral as instruments for child labour (H_{ijt}). $shock_{jt}$ is an indicator of whether household *j* experienced a transitory shock[16] in time *t*. This is almost identical to the specification used by Beegle et al (2005a) to explore the relationship between child labour, transitory shocks and access to credit. The significance and robustness of the relationship found in that study suggests these to be potentially good instruments. A detailed discussion of the selected instruments is presented in Section 5.

The second stage, or structural regression specification is the same as the original OLS specification with the exception of the child labour variable. Instead of constituting the reported number of hours worked, this variable is now the mean of *predicted* hours worked by child *i* in household *j* per week in time *t*.

A number of interactions are added to the second stage regression, equation (3) to explore variation in the effect of child labour on educational attainment by school attendance,

[16] A household is considered to have experienced a transitory shock if it had positive loss of crop due to insects, rodents, fire and other calamities in the year preceding at least one of the rounds of the baseline survey (KHDS1).

gender and the combination of the two. The interactions were initially also included in the first stage regression of the IV specification. However, since they had no significant impact on the outcome, they have been omitted in order to maintain clarity of both specification and results.

4.5 Fixed Effects

Instrumenting controls for unobservable household characteristics that may be correlated with the child labour variable. Fixed effects estimation controls for between household selection bias in the first stage regression. However, between household selection bias may still be a problem in the second stage regression if one of the other independent variables is endogenous. This can be controlled for using fixed effects estimation in the second stage as well, which is possible even though the data used to estimate the second stage regression is from one time-period, since there are multiple observations per household in the majority of cases[17]. In addition, controlling for unobservable initial household fixed effects allows to identify the effect of child labour through within-household variation i.e. variation between siblings or other children residing within the household. The fixed effect specification of the second stage regression is:

$$Y_{ijr+s} = \delta_j + \beta_1 X_{ijt} + \beta_2 \hat{\bar{H}}_{ij} + \beta_3 S_{ijt} + \beta_4 W_{jt} + \varepsilon_{ijt+s} \qquad (4)$$

where δ_j is a household dummy that controls for the unobservable time invariant characteristics of the household the individual grew up in (i.e. KHDS1 household).

4.6 Robustness Checks

A number of robustness checks follow the main investigation. These include an examination of whether the effects are consistent across sub-groups in the sample, testing the assumption of a linear relationship between child labour and final educational

[17] The great majority of individuals in the sample lived in a household with at least one sibling who was also in the age-group of interest and was also interviewed during KHDS1.

attainment, including individual level variables to control for within-household selection bias and examining the validity of the selected outcome variable as a measure of educational attainment at adulthood. Discussion of the specific methods used is presented in Section 7.

5 Results

This section examines the effect of child labour on final educational attainment estimated using three methods. The first of these is OLS, followed by IV and Fixed Effects with instruments.

5.1 OLS

Table 3 presents the OLS estimates for the main specification[18]. Since endogenous variation is not controlled for causality cannot be assumed in inference. However, the results do indicate that a one hour increase in mean hours worked per week is associated with a 3 percent reduction in the final educational attainment. In other words, a one standard deviation increase in child labour is associated with a 35 percent fall in final educational attainment[19]. This association is highly statistically significant and suggests that a child working over roughly 30 hours a week would be unable to attend school at all. Among other significant variables are age, whether the individual had ever attended school by the time of the last KHDS1 interview, parental education and ownership of collateralizable assets.

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[18] $Y_{ijr+s} = \beta_0 + \beta_1 X_{ijt} + \beta_2 \overline{H}_{ij} + \beta_3 S_{ijt} + \beta_4 W_{jt} + \varepsilon_{ijt+s}$ as in equation (1), Section 4.

[19] Standard deviation informs on the spread of the observations of the variable around the mean. If the variable is normally distributed, 68 percent of all observations fall within one standard deviation of the mean. In this instance, mean amount worked is 18 hours and the standard deviation is 11.6 (Table 1) indicating that 68 percent of individuals worked between 6.4 and 29.6 hours per week during KHDS1.

Table 3: Effect of child labour on final educational attainment – OLS without IV, OLS with IV and Fixed Effects with IV

	OLS without IV (1)	First Stage with Household Fixed Effects (2)	Second Stage OLS with IV (3)	Second Stage Household Fixed Effects with IV (4)
Dependent Variable	Final educational attainment in KHDS2	Hours worked in 7 days preceding KHDS1 interview	Final educational attainment in KHDS2	Final educational attainment in KHDS2
Specification				
Mean hours worked per week as child$^{\Delta}$	-0.029***		-0.050***	-0.135
	(0.007)		(0.009)	(0.10)
Had attended school at some point by last KHDS1 interview	2.576***	-1.496**	2.540***	1.567***
	(0.211)	(0.634)	(0.209)	(0.35)
Log per capita collateral value (cash, physical assets, durables - excluding land)	0.223***	0.465*	0.211***	-0.03
	(0.062)	(0.269)	(0.062)	(0.33)
Log per capita expenditure/consumption value	0.445***		0.443***	0.085
	(0.139)		(0.137)	(0.16)
Log per capita land value	-0.036		-0.02	-0.106
	(0.037)		(0.038)	(0.15)
Shock: any crop loss from transitory shocks		5.927**		
		(2.865)		
Shock x Log per capita collateral value		-0.455*		
		(0.276)		
Female	-0.141	2.089***	-0.093	0.071
	(0.136)	(0.444)	(0.136)	(0.28)
Age in years	-1.133***	7.501***	-1.002***	0.042
	(0.281)	(0.799)	(0.283)	(0.65)
Age in years squared	0.048***	-0.237***	0.044***	0.009
	(0.012)	(0.036)	(0.012)	(0.02)
Father: 1-6 years of education	0.086	-0.719	0.049	0.149
	(0.227)	(1.132)	(0.227)	(0.52)
Father: 7 years of education	0.958***	0.584	0.948***	0.912*
	(0.238)	(1.217)	(0.237)	(0.54)
Father: 8 or more years of education	1.360***	0.23	1.322***	0.951
	(0.282)	(1.498)	(0.282)	(0.66)
Mother: 1-6 years of education	0.317*	0.186	0.291	0.113
	(0.182)	(1.086)	(0.181)	(0.47)
Mother: 7 years of education	0.626***	0.462	0.627***	-0.278
	(0.201)	(1.064)	(0.200)	(0.46)
Mother: 8 or more years of education	1.130**	0.208	1.045*	-0.786
	(0.561)	(2.662)	(0.556)	(0.87)
Observations	1470	5591	1470	1470
Joint F-test on instruments		4.28		
F-test for significance of unobservable household fixed effects				1.71 (Prob>F=0.000)
Number of KHDS1: Cluster*household		716		620
R-squared	0.29	(within R^2)0.15	0.3	(within R^2)0.09

Robust standard errors in parentheses; †All explanatory variables in specifications (1) – (4) are from KHDS1; $^{\Delta}$ Instrumented in all specifications, with the exception of the first one; * significant at 10%; ** significant at 5%; *** significant at 1%

5.2 *Instrumental Variables*

Column (2) and (3) in Table 3 present the results of Instrumental Variable estimation. Before examining these, however, it is necessary to discuss the rationale for the selected instruments used in the first stage regression[20].

5.2.1 Validity of Instruments

Appropriate instruments satisfy two main conditions – those of relevance and exogeneity. The first means that an instrument must explain a substantial proportion of variation in the endogenous variable. The second means that the instrument must only have an effect on the outcome of interest through the endogenous variable and cannot be correlated with the residual. In order to be a good instrument, therefore, incidence of transitory shocks should satisfy both of these conditions[21].

Beegle et al (2005a) use a specification very similar to the first stage one used here to show that transitory shocks lead to a significant increase in child labour. They find that children living in households that experience shocks work 30 percent (or 6.1 hours) more per week. This result suggests that incidence of shocks is a significant source of variation in child labour and is thus a relevant instrument.

The same study also shows that incidence of transitory shocks is an exogenous source of variation in child labour; none of the endogenous variables, such as household characteristics or lagged child labour, predict occurrence of shocks. Shock is, therefore,

[20] $H_{ijt} = \phi_0 + \phi_1 shock_{jt} + \phi_2 collateral_{jt} + \phi_3 X_{ijt} + \phi_4 shock * collateral + \phi_7 S_{ijt} + v_{ijt}$ as in equation (2) Section 4.

[21] The second instrument, the interaction between shock and collateral, is not included in this discussion for the following reason. Collateral is included in both the first and the second stage regressions. Consequently, the interaction can only be a good instrument if shock is. The focus of the discussion is, therefore, the validity of shock as an instrument.

likely to only affects the outcome (final educational attainment) through the endogenous variable (child labour) and not through other variables in the second stage regression, satisfying the exogeneity restriction.

Beegle et al also show that ownership of collateralizable assets mitigates the effect of transitory shocks on child labour. As this interaction explains some of the variation in child labour, it is used as an additional instrument. Although, collateral appears in the second stage regression, shock does not. The interaction is, therefore, an identified instrument.

Since there are up to four rounds of data available for each individual, it is possible to estimate the first stage regression using household level fixed effects. Fixed effects estimation allows to extract household fixed effects from the residual and explicitly include them in the specification as a variable. This estimation method allows more accurate prediction of the dependent variable; a previously unaccounted for source of variation, relevant unobserved household characteristics, is now incorporated in the prediction.

Note, however, that because the selected instruments are household level variables they only control for between household selection bias, not within-household. An attempt is made to find identified individual level instruments for child labour in Section 6.

5.2.2 IV Results

Column (2) in Table 3 shows the results of the first stage regression that is estimated using fixed effects. The within R^2 shows that 15 percent of the variation in the time-variant variables is explained by this model. Further, both of the instruments, shock and the interaction between shock and collateral, are statistically significant. As such they are relevant instruments. F-test for joint significance of the identifying instruments (shock and shock*collateral) is 4.45 which is equivalent to a p-value of Prob >F =0.0386[22].

The results of estimating the structural equation (Column (3)) show that even when instrumented for child labour continues to be negatively associated with final educational attainment. Since endogeneity of child labour is controlled for, causality can be attached to these results; child labour has a negative impact on final educational attainment. While remaining significant at 1 percent, the magnitude of this effect nearly doubles compared to the OLS estimate without IV, increasing from 2.9 to 5 percent. This is equivalent to a loss of over 4 years of education from mean level of 7 years (assuming a nine month academic year) from an increase in child labour by one standard deviation. The significance and magnitude of the impact of the other variables in the specification do not change significantly between OLS and IV.

There are two possible explanations for the apparent downward bias in the OLS estimate of the effect of child labour. Firstly, negative bias can occurs when a relevant variable is omitted. In this instance, as proposed before, child ability is likely to be one such omitted variable. The second plausible explanation is attenuation bias. As discussed in Section 4, amount of child labour used may have been measured with error. If this error is systematically correlated with the reported values of child labour, the OLS estimate will always be lower than one which controls for the measurement error problem (e.g. IV)[23].

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[22] Staiger and Stock (1997) propose that strong instruments should have a joint F statistic of around 10. By such standards, the instruments used here are not as powerful as would be optimal. An attempt is made in the next section to introduce an individual level instrument to increase the joint power of the instruments.

[23] If there is measurement error such that $Cov(X_i\, u_i) \neq 0$ where X_i is the reported amount of child labour and u_i is the measurements error , $\text{var}(X_i) = \sigma^2_{X*} + \sigma^2_u$ where the first term is the variance of the explanatory variable (child labour) without measurement error, while the second term is variance of the measurement error. Using this it can be shown that $p\lim \hat{\beta}^{OLS} = \beta(\frac{\sigma^2_{X*}}{\sigma^2_{X*} + \sigma^2_u})$ indicating that with measurement error $\hat{\beta}^{OLS}$ will be biased downwards and inconsistent.

5.2.3 Interactive Variables (IV)

Results presented in Table 3 inform on the overall effect of child labour on final educational attainment. It may be revealing to disaggregate this effect by some individual level characteristics. The selected characteristics include whether the individual had ever been to school by the time of the last KHDS1 interview, sex and both of these combined into a 3-way interaction[24].

The results in presented in Table 4 show that there is a significant difference in the effect of child labour between those who had and had not ever been to school. The effect of child labour among those who had not been to school is magnified from 5 to 11 percent (F = 67.32). In contrast, the significance of the impact of child labour on final educational attainment is much lower among those who had attended school at some point falling from 11 to 2 percent. The impact of child labour on the final educational attainment of this group is only significant at 11 percent level.

Further, the results in Column (2) show that there is no significant difference in the effect that child labour has on final educational attainment of girls compared to boys. The three-way interaction in Column (3), however, shows that this is only the case among children who had not yet started school. Among those who had, boys continue to suffer from some adverse effects (F= 2.76)[25], while the effect looses its significance among girls (F=0.84).

Overall, the results consistently suggest that child labour adversely affects accumulation of human capital. Upon closer examination it becomes evident that this effect works primarily through children who had not yet started school by the end of the baseline survey. It appears that child labour acts as a deterrent to schooling among these children. The negative impact of child labour is weak among those who are at school and is only statistically significant among boys.

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[24] See Appendix 1 for demonstration of how interpretation of results changes with inclusion of interactive variables.

[25] $\beta_1 + \beta_4$ =-.024

The noticeable difference in the impact of child labour between children working while at school[26] and those working before starting school is consistent with the trend suggested by the descriptive statistics (Table 2); as children get older those who are still not at school begin to work increasingly more than those who are, further decreasing their chances of catching-up on schooling. The lack of substantive differences in final educational attainment of working younger children who had and had not started school, observed in the descriptive statistics, further indicate that there may be some threshold cumulative effect before which child labour does not adversely affect final educational attainment.

[26] Or at least having attended school at some point

Table 4: Effect of child labour on final educational attainment by school attendance and gender (IV)

	(1)	(2)	(3)
Dependent Variable	Final Educational Attainment in KHDS2	Final Educational Attainment in KHDS2	Final Educational Attainment in KHDS2
Specification†			
Mean hours worked per week as child (instrumented)	-0.112***	-0.119***	-0.108***
	(0.014)	(0.017)	(0.022)
Had attended school at some point by last KHDS1 interview	0.875**	0.873**	0.815*
	(0.345)	(0.346)	(0.470)
Female	-0.094	-0.299	-0.434
	(0.134)	(0.295)	(0.495)
Mean hours worked per week as child x Had attended school at some point by last KHDS1 interview (interaction 1)	0.095***	0.095***	0.083***
	(0.016)	(0.016)	(0.025)
Mean hours worked per week as child x Female (interaction 2)		0.011	-0.003
		(0.015)	(0.025)
Had attended school at some point by last KHDS1 interview x Female (interaction 3)			0.225
			(0.613)
Mean hours worked per week as child x Had attended school at some point by last KHDS1 interview x Female (interaction 4)			0.016
			(0.031)
Observations	1470	1470	1470
F-test (p-values) for when dummy in interaction term = 1: Interaction 1	0.112	0.089	0.097
F-test (p-values) for when dummy in interaction term = 1: Interaction 2		0.000	0.000
F-test (p-values) for when dummy in interaction term = 1: Interaction 4			0.359
R-squared	0.32	0.32	0.32

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
† All right hand side variables in columns (1)-(3) are from KHDS1

5.3 *Fixed Effects with IV*

Column (4) of Table 3 presents the results of estimating the structural regression using initial household fixed effects method, as in equation (4)[27]. As discussed in Section 4, initial household fixed effects estimation controls for the between-household selection bias that may have a significant impact on final educational attainment.

The results show that the overall impact of child labour on educational attainment in adulthood looses its significance once household fixed effects are controlled for. It should be noted, however, that the coefficient maintains a negative sign, nearly triples in magnitude compared to the second stage IV estimate of the same specification (Table 3, Column (3)) and remains significant at 20 percent level (F =1.68).

These results are further disaggregated through inclusion of the same interaction terms as in Table 4. In consistency with the findings discussed above, Table 5 shows that there is a significant difference in the impact of child labour on those who had started school by the end of the baseline survey compared to those who had not (Column (1)). Further, only the latter effect is statistically significant (F=4.19); an additional hour of work per week reduced the final educational attainment of these children (both boys and girls) by an average of 21 percent. The magnitude of this effect is nearly twice as large as the second stage IV estimate (Table 4, Column (1)).

The three-way interaction in Column (3) shows that irrespective of gender, the adverse effects of child labour are outweighed by the benefits of going to school; the effect of child labour is only significant for boys and girls who had not started school. The magnitude and the statistical significance of this adverse effect are higher for girls than boys. However, this difference is only significant at 20 percent level (F=1.72).

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[27] $Y_{ijr+s} = \delta_j + \beta_1 X_{ijt} + \beta_2 \hat{\bar{H}}_{ij} + \beta_3 S_{ijt} + \beta_4 W_{jt} + \varepsilon_{ijt+s}$

Table 5: Effect of child labour on final educational attainment by school attendance and gender (Household Fixed Effects with IV)

	(1)	(2)	(3)
Dependent Variable	Final Educational Attainment in KHDS2	Final Educational Attainment in KHDS2	Final Educational Attainment in KHDS2
Specification†			
Mean hours worked per week as child (instrumented)	-0.214**	-0.221**	-0.178*
	(0.10)	(0.11)	(0.11)
Had attended school at some point by last KHDS1 interview	-0.261	-0.266	0.103
	(0.52)	(0.52)	(0.69)
Female	0.05	-0.122	0.281
	(0.27)	(0.45)	(0.71)
Mean hours worked per week as child x Had attended school at some point by last KHDS1 interview (interaction 1)	0.117***	0.117***	0.061
	(0.03)	(0.03)	(0.04)
Mean hours worked per week as child x Female (interaction 2)		0.01	-0.055
		(0.02)	(0.04)
Had attended school at some point by last KHDS1 interview x Female (interaction 3)			-0.51
			(0.83)
Mean hours worked per week as child x Had attended school at some point by last KHDS1 interview x Female (interaction 4)			0.079
			(0.05)
Observations	1470	1470	1470
Number of KHDS1: Cluster *household	620	620	620
F-test (p-values) for when dummy in interaction term = 1: Interaction 1	0.351	0.324	0.268
F-test (p-values) for when dummy in interaction term = 1: Interaction 2		0.044	0.027
F-test (p-values) for when dummy in interaction term = 1: Interaction 4			0.368
Within R-squared	0.32	0.32	0.32

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
† All right hand side variables in columns (1)-(3) are from KHDS1

6 Discussion

Overall, the results definitively show that child labour has an adverse effect on final educational attainment. The magnitude and significance of this effect are particularly high for children who had not yet started school by the end of the baseline survey. The effect on this sub-group of children remains robust even after the unobservable household fixed effects are controlled for in both the reduced form and structural regressions.

The effect of child labour is much less pronounced among children who had started school by the time of the baseline survey. Using OLS to estimate the structural regression shows that there is still a significant negative effect, but a weak one. In fact, when unobservable household fixed effects are controlled for this effect, while remaining negative, looses its weak statistical significance. However, as discussed in the next section, this could in part also be due to weakness of the instruments and insufficient variation in the data. Overall, it is possible to conclude, somewhat tentatively, that children who are at school experience some adverse effects of child labour, especially boys. Unarguably, though the magnitude of this effect is substantially lower than that among working children who had not yet started school.

The preceding analysis of the effect of child labour on education does not inform on the implications of this effect. So far, it has only been shown that working as a child reduces total amount of time spent in formal education. Whether this is an adverse effect depends on whether variation in total amount of time spent at school affect future welfare.

As part of their study of poverty and wealth dynamics in Tanzania, Beegle et al (2006) use KHDS data to show that education has a positive impact on consumption growth. They find this effect be non-linear; education begins to have a positive and significant impact on long-term consumption growth after the first three years. The estimated quantitative relationship between consumption growth and education is used in this study to calculate the cost of lower educational attainment caused by child labour in terms of future loss in consumption growth.

The results presented in Table 3, Column (3) show that an extra hour of child labour per week reduces final educational attainment by 5 percent. Children who work the mean amount of time (18 hours per week) therefore have 90 percent less education than those who do not work. Since, on average, 7 years of education are attained, those who do not work would attain over 13 years of education. Using the estimated relationship between education and consumption growth, it can be calculated that children who do not work will have 7 percentage points more long-term consumption growth, than those who work the mean amount[28].

Although there is a substantial debate around the validity of consumption as a measure of welfare, it remains one of the most widely used indicators. It is beyond the scope of this study to conduct a thorough evaluation of the welfare impact of loss of education incurred by child labour. The above calculations are intended only as a brief demonstration of the type and scale of adverse long-term costs that a decrease in human capital accumulation incurs.

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[28] Beegle et al estimate the following regression: $\Delta \log y = \beta_0 + \beta_1 school + \beta_2 school^2 + \beta_3 X + \varepsilon$ where y=per capita consumption and the dependent variable is change in per capita consumption between 1991 and 2004, school=years of schooling, and X includes all other explanatory variable.
$\hat{\beta}_1 = -0.039$ and $\hat{\beta}_2 = 0.006$.
Conditional effect of education on consumption growth for non-working children:
$\beta_1 + 2\beta_2 X$ = -0.039+2(0.006*13.3)=.1206=12%
Conditional effect of education on consumption growth for children working mean amount of time:
$\beta_1 + 2\beta_2 X$ = -0.039+2(0.006*7)=.045=5%

7 Robustness

This section discusses the potential weaknesses of the specification and estimation methods used in this study.

7.1 *Within Household Selection Bias*

The first issue concerns the use of only household level variables to instrument for child labour. This leaves within-household selection bias, such as that found by Ayalew (2000), uncontrolled for.

At the individual level a valid instrument for child labour is a variable that explains some of the variation in child labour, but only affects schooling through child labour. An attempt was made to use a variable resembling birth order as such an instrument. Although data on birth order is unavailable, it is possible to rank children between the age of 7 and 15 living in the household during KHDS1 according to age. Age rank and the interaction between age rank and the shock indicator were used as additional instruments, as in (5):

$$H_{ijt} = \psi_0 + \psi_1 shock_{jt} + \psi_2 collateral_{jt} + \psi_3 age\_rank + \psi_4 X_{ijt} + \psi_5 shock * collateral + \psi_6 shock * age\_rank + \psi_7 S_{ijt} + u_{ijt} \quad (5)$$

Although the impact of age rank itself on amount worked as a child is not statistically significant, that of its interaction with shock is. The coefficient $\psi_6 = 0.56$, indicating that incidence of shock increases the amount worked by older children. The significance of this variable suggests that age-rank is a relevant instrument. However, the joint significance of the four instruments is lower than previously, at F=3.51 compared to 4.45.

Further, while adding the instruments does not change the results of the structural regression when OLS is used, the fixed effects results lose all significance. The directions of the effects remain consistent with the ones found previously. However, the magnitude of the coefficient on the instrumented variable decreases and the statistical significance of the impact observed with the previous set of instruments disappears.

There are two main reasons why this may be the case. The first is to do with identification. Age rank may, in fact, be a variable that should be included in the second stage regression. In other words it may be a variable that affects schooling independently of child labour, as well as through child labour. In this case it is not a valid instrument as it is correlated with the error term of the structural regression violating the over-identifying restriction. It is possible to test the over-identifying restriction since there is more than one instrument. However, it is difficult to do so within the context of this study because of computational restrictions imposed by the structure of the datasets used for first and second stage estimation. Usually, statistical packages, such as STATA, have pre-programmed commands that construct the restricted and unrestricted values for the maximum likelihood function of the model and test the validity of the over-identifying restriction using the Likelihood Ratio test. However, in this study the first stage regression was estimated using the data from the four rounds of KHDS1. The second stage regression was estimated using a dataset containing data averaged across the four KHDS1 rounds and some KHDS2 data. This arrangement precludes the use of the necessary command ("ivreg2" in STATA) so in order to conduct the test, the test statistic would have to be constructed manually. Due to the time constraint, constructing such a test statistic is beyond the scope of this study and will have to be carried out in further work.

Alternatively, or in addition to identification issues, there may not be enough variation in the data used to estimate the structural regression. By using individual level instruments and fixed effects at both stages of estimation, I am trying to tease out the pure effect of child labour on final educational attainment that is not reflecting within and between household selection bias. In order for the effect to remain significant there should be enough variation in the data to show differences between those who worked more or less, irrespective of household and individual observable and unobservable characteristics. Arguably a dataset containing individuals who are otherwise the same would be necessary to pick up such specific variation; for instance a dataset of twins instead of just siblings.

Both of these reasons are likely to be true to an extent and combined result in the outcomes described above. Overall, conceptually it is difficult to think of an individual level characteristic that only affects schooling decisions through child labour and is not in itself a determinant of schooling. Therefore, attempting to control for within household selection bias in modelling the relationship between child labour and final educational attainment is perhaps an overly ambitious endeavour.

7.2 Age group

Summary statistics presented in Table 2 indicate that trends among the younger individuals differ substantially from those among the older ones. Table 6 shows results of estimating the second stage regression using IV and initial household Fixed Effects with IV separately for those who were between the ages of 7 and 11 during KHDS1 and those between 12 and 15.

The results show that the effect of child labour is weaker among the younger children compared to the older ones. IV results show that an additional hour worked as a child between the ages of 7 and 11 results in a 3 percent decrease in final educational attainment, compared to nearly 5 percent among those between the ages of 12 and 15. The difference is much starker once initial household fixed effects are controlled for. In fact, the negative impact of child labour among younger children looses its significance entirely, while that among the older children increases more than tenfold to 59 percent. However, the high standard error indicates that within a 95 percent confidence interval the fixed effects estimate of the impact could be anywhere between 3 and over 100 percent. This suggests that there is little variation in the child labour variable for the sub-sample of older children, which is likely to be due to weakness of the chosen instruments as predictors of child labour for this sub-group specifically. Consequently, while the fixed effects estimate does indicate that child labour has a significant negative impact on final educational attainment of those in the older sub-group, even given the high standard error, the magnitude of the effect cannot be deduced with any precision.

Table 6: Impact of child labour on final educational attainment, by age group – IV and Household Fixed Effects with IV

	Sample used for estimation consists of individuals who were between the ages of 7 and 11 during KHDS1		Sample used for estimation consists of individuals who were between the ages of 12 and 15 during KHDS1	
	IV (1)	Household FE with IV (2)	IV (3)	Household FE with IV (4)
Dependent variable	Final educational attainment in KHDS2	Final educational attainment in KHDS2	Final educational attainment in KHDS2	Final educational attainment in KHDS2
Specification[†]				
Mean hours worked per week as child (instrumented)	-0.03**	-0.11	-0.05***	-0.59**
	(0.02)	(0.22)	(0.01)	(0.28)
Had attended school at some point by last KHDS1 interview	1.58***	0.80	4.05***	2.67***
	(0.26)	(0.63)	(0.35)	(0.87)
Female	0.08	0.16	-0.13	0.86
	(0.21)	(0.58)	(0.18)	(0.63)
Age in years	-0.48	1.13	-3.81	-1.96
	(1.14)	(2.04)	(2.68)	(4.85)
Age in years squared	0.02	-0.05	0.15	0.11
	(0.06)	(0.12)	(0.10)	(0.17)
Observations	675	675	796	796
Number of KHDS1: Cluster* household		444		482
R-squared	0.27	0.11	0.37	0.23

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
[†]All explanatory variables are from KHDS1

7.3 Education Measures

Two issues are addressed in this subsection. Firstly, an assumption has been made throughout this study that the number of years of schooling attained by KHDS2 is an adequate measure of educational attainment on adulthood. This assumption is justified by the evidence that the great majority of individuals in the sample had finished school by the time of KHDS2. Nevertheless, 8 percent of the sample were not in this category (Table 1). Estimation of the second stage regression using IV and initial household fixed effects with IV excluding those who had not finished school by 2004 shows that the main conclusions do not change. However, the magnitude of the effect of child labour on educational attainment in adulthood tends to decrease. For instance, while an additional hour of child labour per week reduces final educational attainment by 5 percent according to IV estimate using the whole sample (Table 3), exclusion of those who had not finished school by the time of KHDS2 reduces the magnitude of this effect to 3 percent (remaining significant at 1 percent level).

Secondly, the issue of endogeneity of the schooling and working decision already addressed in Section 4 deserves some further attention. A basic discussion in that Section established that the optimal way of coping with potential endogeneity of the schooling and working decisions would be to identify separate instruments for the two variables. In fact, it may be possible to pick such instruments if averaged data from the four baseline rounds is used. However, there is an important difference in these two variables which makes instrumenting both without loss of explanatory power of the child labour instrument difficult, if not impossible. Level of child labour varies from year to year, while school attendance, as measured in this study, is a one-off decision. Therefore, a strong child labour instrument would explain between base-line survey round variation in child labour, while a schooling decision instrument needs to explain variation in schooling attendance at the end of the baseline survey. Instrumenting the average level of child labour would allow to instrument for the schooling decision, however, it would make it impossible to use instruments for child labour that best explain variation in this variable (incidence of transitory shocks, for instance). As child labour is the focus of this study, a decision was made to concentrate on instrumenting this variable in the best way possible and using a carefully selected schooling decision variable[29].

[29] Note also that, as mentioned before, the problem is less serious than may seem since the attendance variable refers to period *t-10* compared to the outcome variable.

7.4 Linearity

There is an assumption of a linear relationship between child labour and final educational attainment inherent to the estimated specification. The summary statistics presented in Table 2 as well as regression results presented in Table 6 indicate that the effect of child labour on final educational attainment becomes stronger with age. The older children also tend to work more on average. These trends suggest that there may be a threshold amount a child can work beyond which child labour begins to have significant adverse effects on educational attainment. The current specification only allows for a constant marginal effect. In order to determine whether there is a non-linear relationship, the structural regression is estimated with the addition of a squared child labour term. This additional term is significant using both IV and Fixed Effects with IV, suggesting that there is some non-linearity in the relationship between child labour and educational attainment; the adverse effect of child labour becomes stronger as more hours are worked.

7.5 Other

Before concluding the robustness section it is necessary to draw attention to one additional potential weakness. As mentioned, previously two different data sets were used to estimate the first and the second stage regressions of the main specification; this precludes the use of some estimation options available in standard statistical packages. Specifically, the “ivreg2” command in STATA, discussed above, allows to not only test over-identifying restrictions but also to correct standard errors in the second stage regression for prediction errors in the first by using non-parametric bootstrapping. Nonparametric bootstrapping re-estimates the prediction using multiple samples which are acquired through excluding a different random set of observations from each individual estimation – re-sampling with replacement. A separate standard error is estimated in each round and then used in the second stage regression. The final results are an average of the different outcomes of the estimations with standard errors that account for both the first and second stage errors. Again, as the command that performs this procedure cannot be used, correcting the standard errors for prediction error would require manual programming of the bootstrap procedure. While the merits of this method are recognized, it is beyond the scope of this study. It is important to note however, that inaccuracy of the standard errors increases the likelihood of Type I and Type II errors in the inference in this study.

8 Conclusion

The aim of this study is to determine whether there is empirical evidence for the commonly made assumption that child labour has a negative impact on human capital accumulation and may, therefore, be perceived as contributing to the persistence of poverty among those forced to use it at inefficiently high levels. In order to answer the question whether and to what extent child labour impacts final educational attainment in rural Tanzania has been explored.

The results suggest that working as a child has a significant negative impact on final educational attainment. Individuals who had worked mean number of hours (18 hours per week) as children during the baseline rounds of the survey, on average attain roughly half the number of years of education than those who had not worked. However, this effect does not appear to operate through displacement of schooling by work, rather through deterring working children from starting school. This is evident from the significant difference in the marginal effects of child labour on those who had commenced school by the end of the baseline survey, compared to those who had not. While an additional hour of child labour among those in the former group reduces final educational attainment by roughly 2 percent, the effect is nearly 6 times as high among those in the latter group, at 11 percent. This trend suggests that those who were combining child labour with schooling during the baseline mainly sacrificed leisure rather than school time in order to work. In contrast, those who had not yet started school were deterred from commencing school by working and attained a significantly lower level of education by adulthood. These results appear to be consistent with the results of Ravallion and Wodon (2000), who do not find that child labour comes at the expense of schooling among those attending school in rural Bangladesh.

This study is limited in a number of aspects. Primarily, better instruments are needed in order to pick up more of the within household variation. For instance, school enrolment may in part be explained by unobservable individual characteristics that are not controlled for in this study. Strong individual level instruments would allow to tease out the impact of child labour more precisely. Further, more work on eliminating risk of endogeneity in the schooling and child labour decisions could also improve precision of the estimated relationships.

Nevertheless, the results are sufficiently robust to suggest that the negative impact of child labour on human capital accumulation assumed in both the theoretical literature and the policy debate around the issue is there. However, it does not work through the commonly assumed channel of displacing schooling. Instead it acts as a deterrent to initial enrolment. This caveat suggests that a shift of focus of the child labour discourse from its interaction with school attendance, to that with school enrolment may improve understanding of the adverse impact and inefficiency of child labour.

Appendix I: Interactive Variables

Taking a simple specification with a two-way interaction such as:
$Y_i = \beta_0 + \beta_1 hours + \beta_2 school + \beta_3 (hours * school)$

The effect of child labour when the school dummy is zero and when it is one can be derived.

$$school = 1 \longrightarrow \frac{\partial E(Y_i)}{\partial hours} = \beta_1 + \beta_3$$
$$school = 0 \longrightarrow \frac{\partial E(Y_i)}{\partial hours} = \beta_1$$

Using the same method an example of a specification with a three-way specification is worked through below to show the effect of child labour on educational attainment for all the possible cases:

$$Y_i = \beta_0 + \beta_1 hours + \beta_2 school + \beta_3 female + \beta_4 (hours * school) + \beta_5 (hours * female)$$
$$+ \beta_6 (school * female) + \beta_7 (hours * school * female)$$
$$female = 1 \,\&\, school = 1 \longrightarrow \frac{\partial E(Y_i)}{\partial hours} = \beta_1 + \beta_4 * school + \beta_5 * female + \beta_7 * school * female$$
$$female = 1 \,\&\, school = 0 \longrightarrow \frac{\partial E(Y_i)}{\partial hours} = \beta_1 + \beta_5$$
$$female = 0 \,\&\, school = 1 \longrightarrow \frac{\partial E(Y_i)}{\partial hours} = \beta_1 + \beta_4$$
$$female = 0 \,\&\, school = 0 \longrightarrow \frac{\partial E(Y_i)}{\partial hours} = \beta_1$$

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