YOUNG LIVES STUDENT PAPER

Heterogeneity in the impact of drought on child human capital – evidence from Ethiopia

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Thesis submitted in part fulfilment of the requirements for the degree of MSc in Economics for Development, University of Oxford.

The data used come from Young Lives, a longitudinal study of childhood poverty that is tracking the lives of 12,000 children in Ethiopia, India (in the states of Andhra Pradesh and Telangana), Peru and Vietnam over a 15-year period. **www.younglives.org.uk**

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HETEROGENEITY IN THE IMPACT OF DROUGHT ON CHILD HUMAN CAPITAL –EVIDENCE FROM ETHIOPIA

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ABSTRACT

Children in the developing world are routinely exposed to drought shocks and other climatic hazards. Such shocks can have lasting effects in adulthood if they affect investments in child human capital. In this study, I investigate the impact of two recent episodes of drought in Ethiopia on two measures of cognitive outcomes: Peabody Picture Vocabulary Test (PPVT) scores and Mathematics Test scores. I use data from the Young Lives study on children followed at ages 8-10 and 12-14. Using both panel data and cross-sectional estimation techniques, I test for differences in drought impact by *cognitive skill* and by *age*. I also explore the channels of drought impact by estimating separate equations for the effect of drought on child anthropometry, enrolment and child's time allocated to different activities. Finally, I test for heterogeneity in drought impacts by investigating variations in shock-coping mechanisms among different demographic groups.

The evidence suggests that drought affects cognitive skills differently – quantitative skills appear to be affected more adversely. However, these differences become less pronounced as children grow older. Broadly, cognitive skills are more likely to be affected adversely at adolescence than at the younger age of 8-10. Adjustments in time spent at school are a major channel affecting cognitive scores; however, evidence on the role of anthropometry and enrolment is much weaker. In terms of heterogeneity, for households specializing predominantly in agriculture, cognitive scores are less adversely affected during drought episodes. Cognitive outcomes are also disproportionately affected for male children, especially first-borns, who fare the worst. On the policy front, failing to take the vulnerability of specific demographic groups into account may translate to deepening poverty traps. Results also suggest that children's aspirations have the potential to play a major role in buffering the impact of drought, however this needs further exploration.

The data used in this publication come from Young Lives, a 15-year study of the changing nature of childhood poverty in Ethiopia, India (Andhra Pradesh), Peru and Vietnam (www.younglives.org.uk). Young Lives is core-funded by UK aid from the Department for International Development (DFID) and co-funded from 2010 to 2014 by the Netherlands Ministry of Foreign Affairs. The views expressed here are those of the author(s). They are not necessarily those of Young Lives, the University of Oxford, DFID or other funders.

1. Introduction

Children in less-developed economies are vulnerable to a host of adverse shocks including environmental, economic and socio-economic shocks. Such shocks not only have short-term consequences for child wellbeing but can also have lasting effects in adulthood, if they lower investments in child human capital (Yamano et al., 2006). Shocks have the potential to generate effects that result in poverty traps and the transmission of poverty across generations (Alderman et al. 2006). Understanding the nature of household shock-coping mechanisms is thus essential to understanding the dynamics of childhood poverty. While the link between short-term and long-term consequences of shocks is unambiguous, evidence on the nature of shock impacts and households coping strategies is more nuanced (Ferriera and Schady, 2009).

This paper analyses the effect of two drought episodes on child cognitive outcomes and the specific channels through which the impact of the drought shock takes place, among a sample of children of school-going age in Ethiopia. Despite economic growth in recent years, Ethiopia ranks poorly on the United Nations Human Development Index. I focus on drought in particular, because it is the most prevalent disaster in Ethiopia and the incidence and severity of drought is expected to rise in the future. In 2009, a large-scale drought affected various parts of the country. This was followed by a more severe but less widespread drought, which affected the southern parts of Ethiopia, including Oromia, Somali and southern Tigray. Both shocks resulted in widespread crop failure, food shortage and rising food prices. These factors generated considerable poverty, with multiple implications for children's wellbeing and human capital.

A wide strand of literature has studied the effects of climatic shocks on children's schooling and health outcomes. I touch upon a few studies briefly, so as to highlight the diverse range of climatic shock impacts[1]. Jensen (2000) reports that school enrolment rates declined considerably for children exposed to drought in Cote d' Ivoire. In the case of drought in Zimbabwe, Alderman et al., (2006) report that households delayed the start

[1] For a systematic review of studies on the impact of climatic shocks see Baez et al., (2009) and Zamand (2016).

of school for children by 3.7 months on average. For Ethiopia, at the time of the 1984 famine, Dercon and Porter, (2014) document that children under the age of three were more likely to drop out of primary school leading to income losses calculated at 3 percent per year. Conversely, Shah and Steinberg (2013) report a rise in schooling attendance in drought years, in parts of rural India. In the nutrition and shock literature evidence is similarly mixed. Dercon, (2003) reports an increased incidence of stunting among children in Ethiopia. Dornan et al., (2014) report similar results for Ethiopia, but report a reduced incidence of stunting among children exposed to drought in India. Hyder and Behrman (2015) also present mixed evidence on the impact of climatic shocks in Ethiopia on both health and cognitive scores – with negative impacts associated with droughts and hailstorm shocks but ambiguous impacts for soil erosion.

Although, this literature yields significant insights into the net effect of climatic shocks on child human capital, there has been scant focus on the nature of mechanisms through which these effects take place and heterogeneity in the impact of shocks on cognitive outcomes. Studies on consumption smoothing at the time of shocks have analysed adjustments in child labour, but the implications of these adjustments for child cognition have received little attention. As Baez and Santos (2009) point out, this owes to the difficultly inherent in disentangling various causal mechanisms. Galab and Outes-Leon (2011) and Novella (2015) explore the implications of shocks for children's time use but do not focus on the implications for child cognition. Galab and Outes-Leon (2011) find that households exposed to drought in India adjust children's time in order to buffer the impact of the shock and the magnitude of these adjustments differ based on childbirth order and whether or not the household has access to irrigation. I follow their systematic approach of investigating the nature of adjustments within the household closely. However my research focuses on coping mechanisms in the overall context of understanding how cognitive scores are affected.

Using panel data from the Young Lives, I investigate the impact of two droughts occurring at different stages of a child's human capital development. I explore whether impacts on cognitive skills are similar across different ages of a child's development by comparing a panel fixed effects specification with separate cross-sectional specification for different rounds of data. I also test for whether impacts differ based on the type of cognitive skills (quantitative skills versus receptive vocabulary) being tested. I also estimate ‘channels’ specification, to study channels through which drought affects child human capital. I estimate the impact of drought on i) child’s health and nutritional status as proxied by anthropometric measures, ii) child’s enrolment in school and iii) child’s time allocation to schooling, work and other activities. Next, I examine heterogeneity in drought impacts based on comparing variations in shock-coping mechanisms among different demographic groups. Lastly, motivated by recent literature on the role of aspirations (Dercon and Krishnan 2009, Dercon et al. 2014), I look into whether children’s educational aspirations affect their ability to buffer the effect of a drought.

The main findings are as follows. Drought affects quantitative skills differently compared to receptive vocabulary; however, these differences become less pronounced as children age. Cognitive skills, in general, are more likely to be affected adversely at adolescence than at the younger age of 8-10. Adjustments in schooling time are the most important channel in explaining differential impacts on child scores. Evidence on the role of anthropometry and enrolment as channels is much weaker. In terms of heterogeneity, older children are more likely to reduce time at school in response to the drought shock, due to greater involvement in the labour market at adolescence. A similar reasoning applies to the finding of comparatively worse cognitive outcomes among male children. Male first-born children fare the worst. They spend marginally less time in school and more time at work, compared to younger siblings. However, such marginal changes translate into significant changes in cognitive outcomes for first-born males.

Interestingly, children in households where agriculture is the primary activity of production exhibit less adverse impacts on cognitive scores, possibly reflecting the reduced opportunity cost of schooling time during times of drought. My analysis also substantiates the role of aspirations in shaping child outcomes – scores are less adversely affected for children with higher educational aspirations. However, this is a preliminary finding and may stem from a link between higher wealth and aspirations.

In sum, the findings of this study contribute to: the small body of literature on the channels explaining climatic shock impacts on child human capital; literature on skill formation at childhood and in adolescence; and literature on the role of child labour in smoothing consumption in poor households.

The remainder of this study is organized as follows. The next section presents the theoretical framework underlying the empirical analysis. Section 1 introduces the Young Lives dataset and provides descriptive statistics. This is followed by Section 4, which discusses identification strategy. Section 5 reports results and discusses key findings. The last section provides concluding remarks.

2. Theoretical Framework

The effect of drought on child cognition is analyzed using Behrman et al.'s (1999) extension of Becker's human capital model. Under Becker's original framework, the decision to invest in schooling depends on private marginal benefits of schooling and private marginal costs. Marginal benefits are measured by the discounted stream of future earnings, from an additional year in school. With each additional year of schooling, marginal benefits decline due to diminishing returns to fixed endowment of ability. Private marginal costs measure direct tuition fee plus the income foregone from alternative uses of schooling time. Marginal costs rise with schooling investments. In this framework, credit constraints can be construed as an additional cost of schooling, in so far as they prevent households form smoothing consumption in order to finance investments in child schooling.

Under this theoretical setting, a drought shock can have two potential impacts. For rural households, in particular, agricultural productivity falls leading to a reduction in household disposable income and an increase in the opportunity cost of schooling for credit constrained households. In the presence of poorly functioning credit markets, poor households may adopt coping strategies such as increasing the number of labour hours (both adult and child) and reducing expenditure on child's nutritional and/or schooling goods. This is the so called 'income effect'. For instance, Dercon (2002) documents that many households experiencing famine in Ethiopia reduced consumption rather than resort to selling their assets. If access to credit were unconstrained households would be able to borrow in order to smooth consumption and no income effect would be observed (Ferriera and Schady, 2009).

At the same time, the drought shock may be associated with a positive substitution effect. Lower agricultural productivity will drive down the demand for child labour and reduce the opportunity costs of school attendance. Moreover, depressed agricultural productivity may also lead to increased parental time investment in child human capital generating activities. The more severe and prolonged the shock is, the greater is the impact on agricultural productivity and the greater is the substation effect.

In sum, the net impact of a drought depends on which of these two effects dominate. Following drought, when the income effect dominates we observe a fall in the equilibrium level of schooling investment. Conversely, when the substitution effect dominates, an improvement in schooling investment is seen. Thus, this theoretical framework allows for the existence of both negative and positive impacts on schooling investment.

Predictions

In light of the above framework, I expect to uncover a strong negative impact of drought shock on cognitive scores in rural Ethiopia - the income effect will be particularly strong given poorly developed credit markets. It is difficult to assess whether the income effect will be strong enough to result in children dropping out from school – recent years have seen an increase in the overall rate of enrolment in Ethiopia. For the health channel, evidence from prior studies suggests that older children are better able to avoid risks to health (Stanke, 2013), thus effects on health are likely to be marginal. I expect reductions in children's schooling and studying time use to emerge as a major coping mechanism, but such adjustments are likely to differ, depending on the age and birth order of the child. The insights from the theoretical framework suggest that the magnitude of income and substitution effects will be larger when children are older, because of greater participation in work activity. Using a similar argument, these effects are likely to be more pronounced for male children as compared to females, higher birth orders and agricultural households. A priori, it is hard to predict which effect will dominate.

3. Data And Descriptive Statistics

The analysis in this study is based on data from the Young Lives study - an on-going panel study of childhood development and poverty in Ethiopia, India, Peru and Vietnam. Commencing in 2002, the project has tracked 3000 children in each of the countries, comprising two different age groups – 2000 children (ages 6-18 months) and 1000 children (ages 7.5-8.5 years). Children were tracked across four different survey rounds: Round 1 (2002), Round 2 (2006), Round 3 (2009) and Round 4 (2013).

The data I use come from the Rounds 3 and 4 household surveys on the younger cohort of children in Ethiopia, ages 8-10 and 12-14 in 2009 and 2013, respectively. In Ethiopia, enrolment typically occurs beyond 7 years of age, thus data from the first two rounds is not suited to the analyses being undertaken. I confine my attention to the younger cohort of children because both ages 8-10 (when a child has just been enrolled into school) and 12-14 (when a child is entering adolescence) present critical phases in a child's development as well as participation in work. Second, my interest lies in exploring drought impacts on human capital, and children in this particular sample are likely to have been exposed to two recent episodes of drought in 2009 and 2011.

Two tests of child cognition were administered to children in this sample and are used as the primary outcome variables in my analysis: i) the Peabody Picture Vocabulary Test (PPVT) score and ii) Mathematics Test score (heretofore Maths score). The main reason behind focusing on child cognitive skills over traditional measures of schooling attainment such as grade-for-age and attendance rates is the reliability of the former as a measure of aptitude. Many children in less-developed countries do not perform well on cognitive tests despite high rates of attendance. Secondly, in the developing country context, grade-for-age measures serve as a reliable guide for basic numeracy and literacy skills, whereas cognitive tests measure a broader array of cognitive skills.

The PPVT is individually administered; the examiner presents a stimulus word orally, and the child selects the picture that best reflects the meaning of the stimulus word (Dunn and Dunn, 1997). The original version of the test was set in English, but the test was adapted to local languages (see Cueto and Leon, 2012 for further details). The test does not measure reading and writing skills directly, but serves as a proxy measure for the child's receptive vocabulary and scholastic aptitude. Higher levels of literacy are associated with richer receptive vocabulary and higher PPVT scores.

The Mathematics tests in both survey rounds were designed using items from national and international testing programmes. Items in the Round 3 tested basic quantitative skills, knowledge of numbers, and ability to perform basic operations with numbers. Items in the Round 4 tested more advanced skills of operations with numbers, data interpretation and geometry etc.

An issue with using PPVT and Mathematics Raw scores in panel-data analysis is the comparability of scores across survey rounds and across different languages. The latter assumes greater importance for PPVT scores, compared to Mathematics scores; see for instance Cueto and Leon (2012)[2]. In the present analysis, I address this issue by converting absolute scores into z-scores (essentially standardizing scores to have a mean of zero and a standard deviation of one, in an attempt to address improvements in ability with age). For the PPVT specification I also add child language and the language in which the PPVT test was administered as additional controls. Recently, Item Response Theory (IRT) scores have been used to produce comparable scores across survey rounds. This involves modeling children's ability as a function of "item difficulty, item discrimination and item guessing" (Leon and Collahua, 2015). However, IRT scores are only comparable within languages. In the Ethiopian sample, there is no single predominant language – Amharric is the most widely spoken language but accounts for 42-percent of the sample only. Thus using IRT scores would involve sacrificing valuable information and efficiency, in order to achieve more reliable comparability of scores across survey rounds.

Of the 1883 children surveyed in 2009, 96 percent took the Mathematics Test and 99 percent took the PPVT test. 1874 children were re-surveyed in 2013, of which around 87 percent took the test. In this paper, I do not model the sample selection process, but highlight it as a cautionary note in interpreting results. 5 children, for whom data on the full set of controls was not available, have also been excluded from my analysis. At this

[2] Questions in the Mathematicss test were read by the fieldworker with the aid of cards, to avoid bias from poor reading skills. Thus Mathematics test scores are less susceptible to the influence of age (and ability).

point, it is also worth mentioning that attrition, in general, does not seem to be a major concern in the Young Lives sample[3].

The main covariate of interest is a binary variable for drought. In both survey rounds, households were asked if they had experienced drought (among a list of other disasters) since the last four years. The binary variable takes on the value of 1 if households reported having experienced drought, and 0 otherwise. Drought was the most widely reported disaster across rounds: 34 percent of the sample reported being affected by the drought in 2014, whereas 13 percent of the sample reported being affected by the drought in 2013. 92 percent of the overall incidence of drought reports can be attributed to households in rural areas.

For the health channels specification, the outcome variables are anthropometric measurements of the child's height-for-age (HFA) and body-mass-index for age (BFA) z-scores. HFA is a cumulative measure of physical growth, whereas BFA captures short-term effects only. Both sets of scores have been standardized to z-scores using World Health Organization (WHO) defined international standards.

In the time use specifications, I exploit information from a separate survey section on time-use, which asked children to log the numbers of hours they spent on various activities: school, studying, paid work, farm work, household chores etc., in a particular week. I combine both time spent in school and studying to estimate the impact of drought on the child's time devoted to both these activities (henceforth this is referred as time-schooling for simplicity). Similarly time spent in paid work and at the farm is treated as one variable. Both variables are expressed as percentage of total hours in a day (24 hours). Certain discrepancies in data are worth noting. First 6 children in the 2013 survey round report non-zero hours of schooling, despite not being reported as enrolled in school, and 3 children report zero hours devoted to schooling, despite being enrolled in school. The discrepancy in the 2009 survey is far more serious and is likely to have serious

[3] Between Round 1 and 4, child deaths account for 4.1 per cent of attrition in the Ethiopian sample. The attrition rate, excluding deaths, was around 2.2 per cent. A technical note on attrition in Round 4 is not available as yet, but careful analysis of attrition from previous rounds suggests that despite following some non-random patterns (e.g. greater number of deaths in lower wealth households) attrition has predominantly been a random phenomenon (Dercon and Outes-Leon, 2008).

implications for the results: 77 children report positive hours of schooling despite reporting non-enrollment. It is unclear whether this discrepancy results from measurement error in the enrolment variable or in the time spent at school variable.

To set the stage for exploring heterogeneity in drought impact, I find it informative to present differences in the means of outcome variables in drought exposed and non-drought exposed households, across demographic and household characteristics, in Table 1. Average cognitive scores are lower in drought-exposed households. Differences in average Maths scores in drought-reporting households versus others are more pronounced for male children, as well as for higher birth orders. Interestingly, differences in both sets of cognitive scores are substantially lower in households, which report agriculture as their primary activity. Paradoxically larger differences in mean scores are also observed for children with relatively educated mothers compared to those with zero years of education.

4. Identification Strategy

4.1. Baseline Specification

In the first specification, my interest is in identifying the impact of drought on cognitive measures of attainment. I estimate a two-way fixed effects model, as in (1):

$$y_{itj} = \delta D_{itj} + X_{itj}\beta' + \mu_j + \alpha_t + u_{itj} \qquad (1)$$

where y_{it} denotes the score of child *i*, at time *t*, in community *j*. D_{it} is the binary choice variable for drought shock. X_{itj} is a vector of observed controls for child and parental characteristics. μ_j are time invariant community fixed effects and α_t is a time fixed effect. u_{ij} is a white-noise error term.

The main coefficient of interest is δ, which can be interpreted as a counterfactual outcome, i.e. outcomes for children in households that were not exposed to the drought serve as a counterfactual for the outcomes of drought-exposed children had the latter not been exposed to the drought shock.

I also estimate separate cross-sectional counterparts to (1) to explore whether there is a difference in drought impacts based on a child's stage in development. In the cross sectional specification, the above model in (1) reduces to estimating the following equation via ordinary least squares (OLS).

$$y_{ij} = \delta D_{ij} + X_{ij}\beta' + \mu_j + u_{ij} \qquad (2)$$

The community-fixed effect approach takes into account all unobserved community characteristics as well as observed time-invariant community wide differences in services, infrastructure, market conditions etc. They also capture the effect of changes in infrastructure in a particular community as well as the introduction of social protection and/or development programs that may have been introduced in a particular community. A fixed effects approach is preferred over a random effects approach because of concern over the correlation of the drought shock with unobserved community characteristics.

An alternative to a *community* fixed effects approach would be to use a fixed effects model, with fixed effects for individual children. I prefer to use the former because it enables a meaningful comparison *across* children in the community, as well as *within* individual children, across time. In this manner I am able to address considerations about the loss of efficiency that are inherent in using individual fixed effects, which ignore all variation between children.

However, a drawback of using community wide fixed effects to identify δ is that they do not capture unobserved child and household heterogeneity. Correlation between these components of the error term and the covariate for drought would violate the strict exogeneity assumption needed to acquire unbiased estimates of the drought coefficient. Arguably, the vector of controls for household characteristics captures most of the unobserved heterogeneity across children and households. This vector of controls includes child's age, child's birth order and gender, gender of household head, highest grade completed by the father and highest grade completed by the mother. I also add controls for two independent climatic shocks that may have an impact on child outcomes: floods and frost reported by the household[4].

As a robustness check, I test for whether including a control for the type of school the child attends has an impact on the drought coefficient. This is likely to affect the coefficient on drought in two ways. First, by introducing schooling quality explicitly, enrolment and non-enrolment are automatically incorporated into the cognitive scores equation. If schooling quality is not observed, this implies that the child is not enrolled in school – in other words enrolment becomes an explicit covariate. Then if drought affects the decision of whether or not to enrol a child in school, introducing enrolment as a covariate would bias the coefficient for drought downward. Second, parents' decision to invest in schooling is a function of household wealth, which, in itself, is adversely affected by drought. The impossibility of disentangling these effects makes explicit inclusion of schooling quality challenging. I tackle the endogeneity of schooling-quality by estimating two separate equations, a full specification with schooling quality as a control and a restricted specification that does not include a control for schooling quality.

Another aspect of unobserved child heterogeneity is inherent ability. I account for differences between child ability by using parental education as a proxy - genetic endowments are likely to be correlated with parental ability. Nonetheless, it is simplistic to think that parental education likely accounts for all variation in ability. In the shock literature, some studies address this by introducing child's lagged test-scores or grades as controls. However, in so far as shocks are correlated over time, the child's past score will be correlated with the current error, violating the exogeneity assumption for unbiased estimates. If the purpose is to understand the role of past ability in shaping current outcomes, then this violation can safely be ignored. But if the purpose is to study the impact of a shock, this can lead to serious biases in the coefficient of interest δ. The same argument also applies to the use of sibling scores as a measure of ability.

[4] The Young Lives questionnaire included questions on having experienced other shocks such as loss of livestock, pest disease, human disease epidemic etc. I do not include these as controls in my specification, because of the possibility that shocks are the result of the initial drought shock, rather than independent and exogenous shocks.

On the household level, it is worth noting that the analysis does not include an explicit control for either household wealth or household composition. This is motivated by the fact that wealth itself is likely to be adversely affected by the drought, and hence the coefficient on wealth will partially capture the effect of drought. Instead, I argue that parental education acts as a valid proxy for wealth, and does not suffer from the same endogeneity concerns. For similar reasons, I do not incorporate explicit controls for the number of adults or children in the household. Migration is prevalent in the subsample of Ethiopian children I study. Between 2009 and 2013, 157 children in my sample reported moving to another location. In addition to the Young Lives child's migration, other household members may also migrate in response to shocks. The variability of family size across rounds reveals this to be the case, indeed. Thus including measures of household or family size, as in previous studies, will capture the effect of drought-induced migration and bias estimates of the drought coefficient.

Another identification concern pertains to the self-reported nature of the drought shock variable. If household reporting is affected by some underlying unobserved factor, then this violates the exogeneity of the drought covariate with respect to the error term and will result in inconsistent estimates. Admittedly, including a set of controls that cater to unobserved household heterogeneity addresses this to a large extent, but it does not dispel endogeneity concerns fully. Ideally, these concerns could be addressed by using either external meteorological data on precipitation or by using an instrumental variable for the propensity of drought. In keeping with maintaining anonymity of the Young Lives children, it is not possible to determine the exact geographic location of Young Lives households and acquire precise meteorological data on the incidence of drought.

The second approach of using drought propensity as an instrument is subject to a serious limitation: a large number of children moved to non-Young-Lives communities, so without any reference community for these children, community wide measures of drought propensity cannot be computed. Children living in non-Young Lives communities account for 345 observations in our panel; the reported incidence of drought was 12 per cent. Excluding these children from analysis is likely to result in more serious bias compared to bias from possible endogeneity of the drought shock.

On a related note, it is important to highlight that children living in non Young Lives communities are modelled as one so-called ‘community’ for the purpose of analysis. Admittedly, in this case the community wise fixed effect will not capture community wide heterogeneity. However, it can be argued that instead the ‘community fixed effect’ captures the common time-invariant attributes of migrant children and households. As mentioned in the preceding paragraph, excluding these children from our sample would result in serious biases. In this case, the resultant bias is likely to be severe and the cure is likely to be worse than the problem at hand.

4.2. Channel Outcomes Specification

In the next stage, I estimate the relationship between drought and different “channel” outcome variables: i) child’s anthropometric status, ii) school enrolment, and iii) percentage of child’s total time allocated to school (and studying), work and domestic activity. If the empirical analysis yields a significant coefficient on the drought coefficient in these specifications, this implies that effect of drought on cognitive outcomes can be explained (in part) by these changes.

To explore whether child health is affected by drought, I use a community fixed effects model as in (1) and (2) above, with HFA z-scores and BMI z-scores as the dependent variable. It is important to note that some empirical studies introduce child health/anthropometry as an explicit determinant of child education (see for instance: Galab and Outes Leon, 2011) either by including current or lagged health as a covariate in the cognitive scores specification. This methodology, which borrows from the production function approach to modelling child scores, suffers from endogeneity. Theoretical literature clearly shows the non-separability of health and cognitive outcomes. Introducing health as an input would introduce endogeneity into the specification and bias estimates. Instead, I consider health in a separate equation to avoid the endogeneity inherent in modelling educational and health outcomes together.

For the enrolment specification, the analysis is conducted for separate cross-sections. This is because children are typically enrolled into formal school between 7-8 years of age in Ethiopia, so enrolment in Round 3 needs to be treated separately from enrolment in Round 4. Formally:

$$\Pr\left(y_{itj} = 1 \middle| D_{itj}, X_{itj,}, \mu_j\right) = \varphi\left(\delta D_{itj} + X_{\square tj}\beta' + \mu_j\right) \qquad (3)$$

where y_{itj} equals 1 if the child is enrolled and is zero otherwise. φ represents the cumulative density function for the normal distribution.

In order to investigate the impact of droughts on alternative uses of a child's time, I utilize separate tobit specifications for i) time spent at school and in studying, ii) time spent in farm work or paid activity and ii) time spent on domestic chores. Tobit models are fairly common in modelling corner solutions - both in cases where data is incompletely observed and where it is fully observed, but takes a non-arbitrary corner value, such as zero. In the latter case, zero time spent on an activity can be thought of as a corner solution to a constrained utility maximization problem. More formally:

$$y_{itj} = 0 \ if \ y^*_{itj} \leq 0$$

$$y_{itj} = y^*_{itj} \ if \ y^*_{itj} > 0$$

$$y^*_{itj} = \delta D_i + X_i\beta' + \varepsilon_i \qquad (4)$$

where y_{itj} measures observed time spent on an activity and y^*_{itj} is a latent variable that is observed for values of time spent on the activity, greater than 0. A major criticism levelled at tobit modelling of corner optimization problems is that it imposes a single mechanism on the "participation" and "amount decision" (Wooldridge, 2009).

Two types of models address the single-mechanism criticism by modelling participation separately, namely Heckman-type sample selection models and two-part models (as in the spirit of Cragg, 1971). However, in this particular case Heckman type models are not suited to estimating child's time use equation, because there is no 'missing data' – time spent in school is actually zero rather than unobserved and missing for children[5]. The second alternative is to use a two-part model. However, estimation routines for such models available in Stata, are fragile and susceptible to convergence issues. Finally, in

[5] Wooldridge (2010) explains this: consider the textbook example of modeling a wage equation, when wages are observed only for individuals who are in the workforce. In this case the wage offer is *not* actually zero, rather we simply do not know what it is. In cases when data is actually zero, rather than unobserved Heckman modeling is not suitable. In the time use context that I consider, the child's time spent in school is actually zero, rather than unobserved.

defense of using a tobit estimation, it can be argued that the assumption that a similar process determines participation in schooling and the amount of time devoted to it, does not seem too unreasonable.

The analysis for time use specifications is conducted for separate cross-sectional data for 2009 and 2013 only. The user developed Stata panel tobit command pantob does not accommodate factor variables, which are of prime interest in my analysis.[6] An alternative would be to use a pooled tobit model as developed by Honoré (1992), but estimates from the model have been shown to be biased and inconsistent.

4.3. Augmented Specification

In the final stage, I augment the cross-sectional specifications above with interactions between the drought covariate and a set of child or household characteristics. This approach allows for exploration of income and substitution effects and heterogeneity in the impact of drought across different households. This is done for the primary outcome variables specification as well as channel variables specifications in (2) and (3). Given discrepancies in the time-use data available for Round 3, I restrict the analysis of augmented specifications to the Round 4 cross-section only.

$$y_{ij} = \theta D_{ij} + \gamma(D_i j \, . \, c_j) + X_{ij}'\beta + \mu_j + u_{ij} \qquad (5)$$

The main coefficient of interest is γ, which provides a difference-in -differences estimate of drought given certain child or household characteristics. The coefficient γ can be interpreted as the difference in the counterfactual and actual outcomes for drought exposed children in particular demographic groups. It is important to keep in mind, that γ must be interpreted alongside the coefficient δ from (2) and (3) respectively, rather than in isolation, to obtain an accurate depiction of the differential impact of drought across groups.

[6] This is true whether factor variables are generated manually or using Stata's inbuilt command.

5. RESULTS AND DISCUSSION

5.1. Results For Baseline Cognitive Outcomes Specification

Table 1 reports results for community wide fixed effects (panel) estimates of (1), for both sets of outcome variables Maths z-scores and PPVT z-scores. Columns (a), (b), (e) and (f) present results for the full specification, with schooling quality as a control. Columns (c), (d), (h) and (g) present results for the restricted specification with baseline controls. Each specification is estimated for the full sample, as well as the rural subsample. In the reduced form equation, with no control added for schooling quality, exposure to drought is associated with a 0.14 standard deviation reduction in Maths z-scores, in the full sample; and a 0.11 standard deviation reduction in scores in the rural sample. As expected, including schooling quality as a control pushes the coefficients on the drought variable downwards. However, the coefficient on drought still retains its significance. The effect of schooling quality control is more pronounced in the full sample. This is as expected, given that differences in school quality are more widespread in urban areas vis-à-vis rural areas.

The panel data estimate of the drought coefficient in the PPVT z-scores equation is suggestive of a negative, but statistically insignificant, relationship. Given the difficulty of comparing PPVT scores across different ages and languages, it is not clear whether this statistical insignificance reflects the actual absence of the impact of drought (and hence is reflective of differential impacts due to different technology of skill formation) or results from issues in score comparability. To test for this formally, I estimate (2) - the cross-sectional counterpart to the fixed effects panel specification.

Results for the cross-sectional specifications for both PPVT and Maths z-scores at ages 8-10 (Round 3) and age 12-14 (Round 4) are presented in Table 4. The coefficient on drought remains insignificant at ages 8-10. However, at 12-14 years, PPVT z-scores are 0.3 standard deviations lower for children exposed to drought. This observed difference in the drought coefficient in the PPVT z-scores specification *across ages* is in tandem with the difference across rounds in the drought coefficient on the Maths z-scores equation. The coefficients retain their significance in the Maths specification, but are much smaller in magnitude; for the full sample the difference in the drought coefficient at adolescence vis-à-vis 8-10 years of age is 0.14 standard deviations. A more detailed exploration of this differential impact is conducted in the next section.

I also investigate whether comparability of PPVT scores *across languages* masks the effect of drought on PPVT z-scores. I test for this by running the cross-sectional specification for PPVT z-scores for the sub-sample of children who speak Amharric, Oromiffa and Tigrian. Results are presented in Table 4. The Amharric sample has an insignificant coefficient on drought, whereas for the Oromiffa sub-sample results show a robust negative association for the adolescent age group for both the full sample and rural sample. The Tigrian sub-sample also shows a significant negative coefficient for drought, for the overall sample. In part, this may be explained by the fact that Afar, Somali and Oromia were the worst hit during the 2009 drought and suffered protracted dry seasons compared to Amhara, which witnessed below normal rainfall (UNICEF Humanitarian Action Report, 2009). However it can be argued that severity (or its lack thereof) would affect Mathematics z-scores equally, and this is not observed in the data.

Thus, on balance, the evidence points towards differences in the technology of skill formation, across i) cognitive skills (quantitative skills appear relatively more vulnerable to drought shock exposure); and ii) different stages of a child's life (cognitive skills are more vulnerable at adolescence than when children start attending school).

5.2. Results For Baseline Channel Specification

In the analysis of the impact of drought on 'channel' variables, I choose to focus on the rural sample, given the smaller incidence of drought in the urban sample. Using the panel fixed effects specification in (1) and the cross-sectional specification in (2), on the rural subsample, I estimate the effect of drought on HFA z-scores and BFA z-scores.

For both variables, columns (a) and (b) report results for the panel specification, columns (c) and (d) report results for children at ages 8-10, whereas columns (e) and (f) report results at ages 12-14. Results provide weak evidence of a negative relationship between child health and drought-exposure. This finding is consistent with recent studies that find limited or no evidence of an association between *current* shocks and anthropometry for older children[7]. In a comprehensive review of the health impacts of drought, Stanke et al., (2013) note that older children are able to circumvent "avoidable risks" and are less vulnerable to the impacts of drought on health status, compared to infants.

Turning to the second potential channel of interest, columns (a) and (e) provides results for the probit equations for enrolment at ages 8-10 and ages 12-14 respectively. The probability of enrolment is unaffected by drought exposure at a younger age, when children are enrolled in school for the first time. However, when children are older exposure to drought reduces the probability of being enrolled in school by 0.05 points. Child's time use behaviour, in drought-exposed households, corresponds to the patterns observed in drought impacts on Maths scores in Table 3 above. Time in school remains unaffected for children when they are young. However, at 12-14 years, children's time in school falls by 37 minutes. In view of the discrepancies in reported time use in Round 3, the results at age 8-10, should be interpreted with caution, and henceforth analysis is conducted for 2013 (Round 4) data only.

The absence of an effect on time use, combined with a smaller effect on cognitive outcomes, for children at 8-10 years of age is less puzzling when one looks at the overall composition of schooling time and working activity across ages. 66 per cent of drought-exposed children at age 8-10 had non-zero working hours, whereas the corresponding figure for adolescents is 81 per cent. This suggests that when children are younger shock-coping and consumption-smoothing through intensifying child labour is weaker. In terms of the theoretical framework, the substitution effect is stronger for younger children.

5.3. Results For Augmented Specifications

In the next stage, I explore heterogeneity in drought impacts in the rural sub-sample of 12-14 year old children (Round 4) by interacting the drought coefficient with different child and household characteristics. Each set of interactions is tested separately to avoid introducing excessive multicollinearity, which can inflate standard errors and sap statistical power. All tables show interaction as well as full effects of drought and child characteristics. For ease of comparison, the estimates of the drought coefficient from the

[7] Empirical studies that do report a link between shocks and anthropometry have focused exclusively on shocks occurring in the first 2-3 years of life (Glewwe et al, 2001) and in utero (Almond and Currie 1992).

baseline regression without interactive terms are included at the bottom of the table.

Table 7 reports results for the augmented specification for farming activity interacted with drought. In column (a) Maths Z-score is the dependent outcome; in columns (b)-(d) time use activities are the dependent outcomes. Agriculture as a primary household activity buffers the impact of drought shock. The theoretical framework would suggest that this is due to a stronger substitution effect in agricultural households – productivity decline in farms leads to children allocating time away form work in the farm to schooling. However, the data shows only a marginal difference in impacts across both demographic groups.

Table 8 provides results for the augmented specifications for drought interacted with gender. The results provide solid evidence that female children are less adversely affected by drought than male children. Overall, girls are more likely to spend time in schooling and less likely to spend time working in the farm or for pay, compared to boys. In response to a drought shock female children's participation in schooling falls less dramatically compared to male gender. Participation in domestic activities increases overall for female children during times of drought. Girls may be taking on more household work as parents and possibly male siblings intensify labour effort. I explore this further in the next specification.

Table 9 shows results for the specification with interactions for birth order and gender[8]. For each outcome variable, two specifications are presented. In the first specification a binary variable for whether or not the child is the eldest boy is interacted with the drought variable (all such specifications are labelled with an " ' "). In the remaining specifications, drought is interacted with each group: elder girl, younger boy and younger girl; elder boy is treated as the reference category. For first-born male children, exposure to drought is associated with over half a standard deviation reduction in Maths scores. This comes about with a reduction in schooling time by 39 minutes[9] and marginal increases in time spent working or tackling domestic chores for elder boys.

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[9] Note: With multiple reference categories, calculating the overall impact for any particular category is a little more involved. The overall impact is computed by adding the differential impact coefficient to the overall impact coefficient from the baseline specification.

So the question arises, what contributes to the marked difference in elder boy's scores, given that reduction in schooling time does not differ markedly for elder boys. If productivity falls more for older boys, bringing with it little or no adjustments in terms of time use to compensate the fall in disposable income, the income effect will dominate and then reduced expenditure on schooling and nutritional investments will lower the elder boy's cognitive scores. Given that elder boys are more likely to be involved in work and on average devote less time to schooling, the effect on their scores is likely to be greater, even in the absence of any marked differences in the time they spend on schooling versus non-first born children. Secondly, the competition between siblings for household resources such as food and parental care may increase risks for higher order children (Bacci et al., 1999). Also, greater household responsibility on boys is likely to contribute to greater stress among elder boys, who may already be working at their full capacity in poor households. I do not explore this fully, and highlight it as a potential area of research.

Results show no differential impact across middle-born male children or female children for cognitive scores. However, the substitution effect is strong for lower birth order children of both sexes; for younger girls, in particular, the substitution effect dominates, leading to increased time spent in schooling among drought exposed younger girls. Younger boys exposed to drought, spend less time in schooling as well as domestic activity. Both findings are suggestive of increased time spent at work, but this is not observed in data. Time at work shows a positive but marginal increase.

Table 10 explores mother's education as a coping mechanism during drought. For each outcome variable, two specifications are presented. In the first specification a binary variable for whether or not the mother has any education is interacted with the drought variable (all such specifications are labelled with an " ' "). In the remaining equations, estimation is based on interacting drought with each mother's education group: primary, middle and higher; no education is treated as the reference category. As expected, results exhibit a positive relationship between mother's education and time spent in schools as well as cognitive outcomes. However, what is perplexing is the significant difference between the drought-coping mechanisms for children of mothers with secondary education and children of mothers with no education. Furthermore, when mother's years of education are broken down into different categories, mother's education beyond grade five is associated with a larger negative impact during times of drought[10]. This differential in cognitive scores is associated with marginal differences in time spent in schooling and study activities between children. Investigating confounders reveals that the finding does not stem from either the mother's leadership of the household or factors such as larger family size.

This finding raises an important concern about the nature of household coping mechanisms – households with educated mothers in my sample are more likely to use child labour to cope with shocks. Skoufias (2007) reports a similarly puzzling finding for children in Mexico when studying health impacts of exposure to rainfall shocks - children with less educated mothers were taller than children with more educated mothers after a negative rainfall shock in 1999. The logical question that arises is what can explain this shock-coping mechanism among households with better-educated mothers. Do preferences shape coping mechanisms in a markedly different manner in these households? Consumption smoothing and asset protection may be perceived as more important versus protecting investments in children, or does the answer lie in decreased parental investment in child care as these mothers intensify labour, in response to the shock. Answering such questions would require delving into an analysis of parental time use, for which data is not available. Nonetheless, I highlight these as important questions for future research.

Table 11 presents results for the aspirations specification. For each outcome variable, two specifications are presented. In the first specification a binary variable, which equals 1 if the child's educational aspirations exceeds grade 12, is interacted with the drought variable (all such specifications are labelled with an " ' "). In the remaining equations, drought is interacted with two aspirations categories: university education and graduate degree; aspirations for education less than 12 years is treated as the reference category. Results reveal that aspirations for higher education are a key-predictor of Maths z-scores. But more interestingly, children who aspire to achieve university education suffer a smaller reduction in their cognitive scores (0.13 standard deviations) during drought compared to children who only aspire to achieve secondary education (0.37 standard

10 The finding is robust to alternative categorizations of mother's education (treating completion of grades 1-8 as primary-middle education, and completion of grades 9 and over as higher-secondary). I avoid using this categorization because of the small number of mothers having completed over 8 years of schooling.

deviations). The differential impact between both groups is 0.24 standard deviations. The results need to be considered more carefully and one needs to ask whether aspirations are endogenous, somehow, for instance are children with higher aspirations better off or have better life circumstances, work smaller hours, have more educated parents, live in households that receive more assistance through development programs. Although, I do not test these links formally, a broad look at data reveals that aspirations are not tightly linked to wealth index and show heterogeneity among different groups.

6. Conclusion

In this paper I investigate the effects of drought on children in Ethiopia and focus on the different channels of impact and heterogeneity among children and households in terms of coping with the shock. I pay particular attention to how coping mechanisms differ between children at 8-10 years and early adolescence: at older ages children are more likely to reduce time in school, in response to a shock. Shocks also affect skills differently. The difference between shock impacts at different ages is more pronounced for quantitative skills. The evidence for broader cognitive skills like receptive vocabulary is conflicting, with a negative association observed across certain languages (and perhaps regions).

The empirical analysis indicates that changes in time allocated to school are the most important channel in explaining differences in child scores; child enrolment and health status appear little affected by drought shock. I proceed to analyze heterogeneity in the effects of drought on cognitive scores and child time allocation across demographic groups. Children in households where agriculture is the primary activity of production suffer less during times of drought. This reflects a strong price effect, which reduces the opportunity cost of schooling investments. A puzzling finding is that children of mothers with middle or secondary education suffer more during times of drought. The small proportion of educated mothers in the rural sample makes it difficult to assess the strength of this finding. It is highlighted as an important area for future research.

There is a clear gender differential in the impact of drought on child cognitive scores – cognitive outcomes suffer more for male children. Among male children, first-borns are the worst affected. When exposed to drought, first-born male children do not appear to spend less time in school or work for longer hours compared to other children. Given the social norm that first born males devote greater time to work and spend less time in school, in the first place, marginal changes in schooling time, as a consequence of drought, can lead to large differences in cognitive outcomes. Finally, my analysis also substantiates recent emphasis on the role of aspirations in improving outcomes for children. This result is a promising area for future empirical work on the role of aspirations in shaping resilience and overcoming poverty traps.

On the policy front, the importance of recognizing heterogeneity in impacts cannot be emphasized enough. Ignoring heterogeneity can lead to seriously underestimating the impact of drought on more vulnerable demographic groups – such as eldest first-borns in the Ethiopian context, and targeting policy inaccurately towards more resilient groups. Failing to take the vulnerability of specific demographic groups into account may result in deepening poverty traps for already vulnerable sections.

7. References

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8. Tables

TABLE 1: DIFFERENCES IN VARIABLE MEANS ACROSS DEMOGRAPHIC GROUPS

	Drought	**No Drought**	**Difference**	**Drought**	**No Drought**	**Difference**
	Male			Female		
Maths-z	-0.44	0.12	0.56	-0.24	0.17	0.41
PPVT-z	-0.52	0.13	0.65	-0.36	0.30	0.66
HFA-z	-1.53	-1.25	0.28	-1.65	-1.25	0.40
BFA-z	-1.63	-1.53	0.10	-1.53	-1.57	-0.04
Enrol	0.77	0.90	0.13	0.75	0.93	0.17
Time-School	0.22	0.29	0.07	0.23	0.29	0.07
	Elder Son			Elder Daughter		
Maths-z	-0.46	0.34	0.80	-0.49	0.38	0.87
PPVT-z	-0.55	0.41	0.97	-0.39	0.42	0.82
HFA-z	-1.40	-1.18	0.22	-1.16	-1.04	0.12
BFA-z	-1.54	-1.46	0.08	-1.68	-1.46	0.23
Enrol	0.77	0.94	0.17	0.78	0.97	0.19
Time-School	0.22	0.30	0.09	0.21	0.32	0.11
	Younger Son			Younger Daughter		
Maths-z	-0.43	0.09	0.52	-0.39	0.01	0.40
PPVT-z	-0.52	0.13	0.65	-0.50	0.03	0.52
HFA-z	-1.62	-1.33	0.28	-1.58	-1.27	0.31
BFA-z	-1.62	-1.55	0.07	-1.63	-1.57	0.06
Enrol	0.74	0.88	0.14	0.79	0.90	0.11
Time-School	0.21	0.28	0.07	0.23	0.28	0.05

TABLE 1: DIFFERENCES IN VARIABLE MEANS ACROSS DEMOGRAPHIC GROUPS CONTD.

	Drought	No Drought	Difference	Drought	No Drought	Difference
	Primary Activity: Agriculture -Household			Primary Activity: Other- Household		
Maths-z	-0.49	-0.43	0.06	-0.24	0.41	0.65
PPVT-z	-0.59	-0.43	0.15	-0.29	0.46	0.75
HFA-z	-1.56	-1.44	0.12	-1.52	-1.16	0.36
BFA-z	-1.65	-1.75	-0.10	-1.56	-1.42	0.14
Enrol	0.76	0.84	0.08	0.78	0.94	0.15
Time-School	0.21	0.24	0.03	0.23	0.31	0.08
	Mother's Grade Completed: Zero			Mother's Grade Completed: Primary-Middle		
Maths-z	-0.45	-0.25	0.20	-0.42	0.31	0.73
PPVT-z	-0.53	-0.24	0.28	-0.38	0.40	0.78
HFA-z	-1.53	-1.52	0.01	-1.56	-1.07	0.49
BFA-z	-1.66	-1.70	-0.04	-1.52	-1.48	0.04
Enrol	0.75	0.84	0.10	0.79	0.95	0.16
Time-School	0.22	0.25	0.04	0.23	0.31	0.08
	Mother's Grade Completed: Secondary					
Maths-z	-0.26	0.55	0.81			
PPVT-z	-0.59	0.61	1.20			
HFA-z	-1.64	-0.98	0.66			
BFA-z	-1.57	-1.31	0.26			
Enrol	0.81	0.96	0.15			
Time-School	0.21	0.33	0.12			

TABLE 2: BASELINE SPECIFICATION - PANEL DATA

	Maths z-score				PPVT z-score			
	Full Controls		Restricted Controls		Full Controls		Restricted Controls	
	Full Sample	Rural Sample	Full Sample	Rural Sample	Full Sample	Rural Sample	Full Sample	Rural Sample
	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
Drought	-0.117*	-0.112**	-0.145*	-0.109**	-0.086	-0.070	-0.104	-0.071
	(0.067)	(0.048)	(0.072)	(0.040)	(0.069)	(0.042)	(0.076)	(0.044)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School-type Control	Yes	Yes	No	No	Yes	Yes	No	No
Constant	-2.330***	-2.422***	-2.499***	-2.734***	-2.935***	-3.001***	-2.951***	-3.140***
	(0.353)	(0.379)	(0.348)	(0.374)	(0.314)	(0.450)	(0.323)	(0.442)
Obs	3,392	1,956	3,392	1,956	3,459	2,012	3,459	2,012
R^2 -Within	0.110	0.078	0.058	0.039	0.104	0.087	0.076	0.056

Note: Significance levels denoted as *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors (SE) in parenthesis, with small cluster T(G-1) degree of freedom correction. Sample size differs for Maths z-score and PPVT z-score specification. Not all children who took the Maths test took the PPVT test and vice versa. Controls include: child's age, child's birth order and gender, gender of household head, highest grade completed by the father and mother respectively, floods and frost shock reported by the household.

TABLE 3: BASELINE SPECIFICATION - SEPARATE CROSS-SECTIONS

	Maths z-score				PPVT z-score			
	Age 8-10 (2009)		Age 12-14 (2013)		Age 8-10 (2009)		Age 12-14 (2013)	
	Full Sample	Rural Sample	Full Sample	Rural Sample	Full Sample	Rural Sample	Full Sample	Rural Sample
	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
Drought	-0.134*	-0.109**	-0.248**	-0.202*	-0.056	-0.063	-0.298**	-0.185*
	(0.078)	(0.042)	(0.104)	(0.099)	(0.065)	(0.048)	(0.131)	(0.094)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-3.225***	-3.010***	-3.095***	-3.366***	-5.266***	-4.720***	-1.662***	-1.360
	(0.503)	(0.655)	(0.647)	(0.719)	(0.376)	(0.404)	(0.584)	(0.977)
Obs	1,780	1,056	1,612	900	1,830	1,098	1,629	909
R^2- Adj	0.446	0.162	0.289	0.075	0.459	0.306	0.497	0.333

Note: Significance levels denoted as *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors (SE) in parenthesis, with small cluster T(G-1) degree of freedom correction. Sample size differs for Maths z-score and PPVT z-score specification. Not all children who took the Maths test took the PPVT test and vice versa. Controls include: child's age, child's birth order and gender, gender of household head, highest grade completed by the father and mother respectively, floods and frost shock reported by the household.

TABLE 4: CROSS-SECTIONAL PPVT SCORES SPECIFICATION - BY LANGUAGE

	Amharric –speaking Sub-sample				Oromiffa speaking sub-sample				Tigrian speaking sub-sample			
	Age 8-10 (2009)		Age 12-14(2013)		Age 12-14(2013)		Age 12-14(2013)		Age 8-10 (2009)		Age 12-14(2013)	
	Full Sample	Rural Sample	Full Sample	Rural Sample	Full Sample	Rural Sample	Full Sample	Rural Sample	Rural Sample	Rural Sample	Full Sample	Rural Sample
	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)	(i)	(j)	(k)	(l)
Drought	-0.231	-0.102	-0.322	-0.047	-0.167*	-0.132	-0.468*	-0.544***	-0.065	-0.038	-.204*	-0.154
	(0.180)	(0.065)	(0.319)	(0.112)	(0.057)	(0.065)	(0.173)	(0.059)	(0.103)	(0.147)	(0.080)	(0.95)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-5.154***	-4.687***	-0.911	0.217	-4.642**	-4.490**	-0.731	0.204	-4.869***	-4.363***	-2.317	-2.389
	(0.559)	(0.755)	(0.616)	(1.882)	(0.861)	(1.007)	(1.478)	(1.557)	(0.786)	(0.494)	(1.616)	(2.062)
Obs	760	296	762	289	348	262	347	259	361	269	377	275
R^2-Adj	0.474	0.212	0.615	0.226	0.186	0.141	0.166	0.134	0.309	0.401	0.486	0.495

Note: Significance levels denoted as *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors (SE) in parenthesis, with small cluster T(G-1) degree of freedom correction. Sample size differs for Maths z-score and PPVT z-score specification, not all children who took the Maths test took the PPVT test and vice versa. Controls include: child's age, child's birth order and gender, gender of household head, highest grade completed by the father and mother respectively, floods and frost shock reported by the household. For the Amharric-speaking sub-sample, 5 proximate communities were treated as a single community; and 2 other proximate communities were treated as a single community, to avoid smaller cluster size and singleton factor variables.

TABLE 5: BASELINE SPECIFICATION - HEALTH CHANNEL

	Panel Specification		Age 8-10 (2009)		Age 12-14 (2013)	
	HFA-Z	BFA-Z	HFA-Z	BFA-Z	HFA-Z	BFA-Z
	(a)	(b)	(c)	(d)	(e)	(f)
Drought	-0.106	-0.0278	-0.154	0.000514	-0.0458	-0.0907
	(0.076)	(0.055)	(0.097)	(0.071)	(0.113)	(0.093)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-2.327**	-0.282	-3.294	-1.550*	-0.642	0.424
	(1.026)	(0.785)	(1.871)	(0.787)	(1.285)	(1.390)
Obs	2,219	2,219	1,121	1,121	1,098	1,098
R^2 -Within	0.043	0.107				
R^2 -Adj			0.064	0.083	0.064	0.084

Note: Significance levels denoted as *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors (SE) in parenthesis, with small cluster T(G-1) degree of freedom correction. 93 observations were not used for estimation of column(a): enrolment was fully observed in community 12. Controls include: child's age, child's birth order and gender, gender of household head, highest grade completed by the father and mother respectively, floods and frost shock reported by the household.

TABLE 6: BASELINE SPECIFICATION - ENROLMENT AND TIME-USE CHANEL

	Age 8-10 (2009)				Age 12-14 (2013)			
	Enrol	Time-Study	Time-Work	Time-Dom	Enrol	Time-Study	Time-Work	Time-Dom
	Probit	Tobit	Tobit	Tobit	Probit	Tobit	Tobit	Tobit
	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
Drought	-0.001	-0.008	0.020*	-0.027	-0.051***	-0.027**	0.025	-0.010
	(0.031)	(0.007)	(0.011)	(0.114)	(0.015)	(0.012)	(0.017)	(0.008)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant		-0.007	-0.157	0.055		0.299***	-0.028	0.001
		(0.094)	(0.115)	(1.330)		(0.089)	(0.113)	(0.059)
Tobit Sigma		0.137***	0.121***	1.620***		0.090***	0.100***	0.049***
		(0.014)	(0.010)	(0.113)		(0.011)	(0.012)	(0.003)
Obs	1,026	1,122	1,122	1,122	1,097	1,097	1,097	1,097
R^2 -Pseudo	0.242				0.199			

Note: Significance levels denoted as *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors (SE) in parenthesis, with small cluster T(G-1) degree of freedom correction. 93 observations were not used for estimation of column (a): enrolment was fully observed in community 12. Controls include: child's age, child's birth order and gender, gender of household head, highest grade completed by the father and mother respectively, floods and frost shock reported by the household. All Tobit equations were tested for robustness. All Tobit equations were tested for robustness against OLS estimates – coefficients retain their significance.

TABLE 7: AUGMENTED SPECIFICATION - IMPACT OF DROUGHT BY PRIMARY ACTIVITY

	Maths-z score	Time-Study	Time-Work	Time-Dom
	OLS	Tobit	Tobit	Tobit
	(a)	(b)	(c)	(d)
Drought	-0.411**	-0.045***	0.045**	-0.013
	(0.171)	(0.007)	(0.020)	(0.010)
APM (Agriculture Primary Activity)	-0.135*	0.010	0.027**	-0.005
	(0.074)	(0.006)	(0.014)	(0.004)
Drought x (APM)	0.274*	0.022	-0.030	0.006
	(0.129)	(0.015)	(0.019)	(0.006)
Constant	-3.283***	0.274***	-0.045	0.015
	(0.719)	(0.091)	(0.117)	0.057
Tobit Sigma		0.090***	0.056***	0.049***
		(0.011)	(0.005)	(0.003)
Obs	900	1,097	1,097	1097
R^2- Adj	0.078			
Baseline Coefficient	-0.191**	-0.029**	0.022	-0.009
	(0.075)	(0.012)	(0.017)	(0.008)

Note: Significance levels denoted as *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors (SE) in parenthesis, with small cluster T(G-1) degree of freedom correction.. Controls include: child's age, child's birth order and gender, gender of household head, highest grade completed by the father and mother respectively, floods and frost shock reported by the household. All tobit equations were tested for robustness against OLS estimates – coefficients retain their significance, although tobit estimates are more conservative.

TABLE 8: AUGMENTED SPECIFICATION - IMPACT OF DROUGHT BY GENDER

	Maths-z score	Time- Study	Time- Work	Time- Dom
	OLS	Tobit	Tobit	Tobit
	(a)	(b)	(c)	(d)
Drought	-0.306**	-0.040**	0.035	-0.025**
	(0.113)	(0.017)	(0.024)	(0.011)
Girl	-0.064	0.015***	-0.117***	0.047***
	(0.062)	(0.004)	(0.019)	(0.008)
Drought x (Girl)	0.236**	0.029*	-0.024	0.032**
	(0.095)	(0.015)	(0.025)	(0.013)
Controls	Yes	Yes	Yes	Yes
Constant	-3.377***	0.278***	-0.019	-0.002
	(0.760)	(0.082)	(0.095)	(0.057)
Tobit Sigma		0.099***	0.054***	0.049***
		(0.012)	(0.006)	(0.003)
Obs	900	1,097	1,097	1097
R^2-Adj	0.010			
Baseline Coefficient	-0.212***	-0.028**	0.025	-0.009
	(0.075)	(0.012)	(0.018)	(0.008)

Note: Significance levels denoted as *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors (SE) in parenthesis, with small cluster T(G-1) degree of freedom correction. Controls include: child's age, child's birth order and gender, gender of household head, highest grade completed by the father and mother respectively, floods and frost shock reported by the household. All tobit equations were tested for robustness against OLS estimates – coefficients retain their significance, although tobit estimates are more conservative.

TABLE 9: AUGMENTED SPECIFICATION - IMPACT OF DROUGHT BY BIRTH-ORDER AND GENDER

	Maths-z score	Maths-z score	Time-Study	Time-Study	Time-Work	Time-Work	Time-Dom	Time-Dom
	OLS	OLS	Tobit	Tobit	Tobit	Tobit	Tobit	Tobit
	(a’)	(b)	(c’)	(d)	(e’)	(f)	(g’)	(h)
Drought	-0.176	-0.487***	-0.026**	-0.044**	0.027	0.036	-0.011	-0.011
	(0.103)	(0.134)	(0.012)	(0.020)	(0.017)	(0.028)	(0.009)	(0.009)
First-born Boy	0.065		0.005		0.050***		-0.024***	
	(0.104)		(0.007)		(0.013)		(0.006)	
Drought x First Born Boy	-0.304***		-0.017		0.007		0.001	
	(0.087)		(0.013)		(0.024)		(0.006)	
First-born Girl		0.091		0.020**		-0.115***		0.047***
		(0.170)		(0.009)		(0.029)		(0.009)
Younger Girl		-0.043		-0.017**		0.015		-0.003
		(0.087)		(0.008)		(0.009)		(0.006)
Younger Girl		-0.133		-0.001		-0.103***		0.044***
		(0.117)		(0.007)		(0.018)		(0.008)
Drought x First-born Girl		0.164		-0.001		-0.004		0.013
		(0.223)		(0.020)		(0.031)		(0.022)
Drought x Younger Boy		0.232**		0.005		-0.004		-0.016**
		(0.082)		(0.014)		(0.020)		(0.007)
Drought x Younger Girl		0.427***		0.038*		-0.029		0.020**
		(0.111)		(0.022)		(0.028)		(0.010)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-3.103***	-3.063***	0.298***	0.275***	0.116***	-0.027	0.056***	0.001
	(0.852)	(0.829)	(0.085)	0.092)	(0.113)	(0.115)	(0.055)	(0.057)
Tobit Sigma			0.090***	0.090***	0.056***	0.056***	0.049***	0.049***
			(0.011)	(0.011)	(0.005)	(0.005)	(0.003)	(0.003)
Obs	900	900	1,097	1,097	1,097	1,097	1,097	1,097
R^2-Adj	0.035	0.056						
Baseline Coefficient	-0.109**	-0.109**	-0.027**	-0.027**	0.025	0.025	-0.010	-0.010
	(0.040)	(0.040)	(0.012)	(0.012)	(0.017)	(0.017)	(0.008)	(0.008)

Note: Significance levels denoted as *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors (SE) in parenthesis, with small cluster T(G-1) degree of freedom correction. Controls include: child’s age, child’s birth order and gender, gender of household head, highest grade completed by the father and mother respectively, floods and frost shock reported by the household. All tobit equations were tested for robustness against OLS estimates – coefficients retain their significance, although tobit estimates are more conservative.

TABLE 10: AUGMENTED SPECIFICATION - IMPACT OF DROUGHT BY MOTHER'S EDUCATION

	Maths-z score		Time-School		Time-Work		Time-Dom	
	OLS	OLS	Tobit	Tobit	Tobit	Tobit	Tobit	Tobit
	(a')	(b)	(c')	(d')	(e)	(f')	(g)	(h')
Drought	-0.196**	-0.171*	-0.022	-0.018	0.020	0.021	-0.005	-0.005
	(0.081)	(0.081)	(0.015)	(0.017)	(0.020)	(0.020)	(0.006)	(0.007)
Mother: No Education (MN)	0.114		0.011		-0.003		0.003	
	(0.102)		(0.007)		(0.009)		(0.004)	
Drought x MN	-0.062		-0.019		0.020		-0.021	
	(0.158)		(0.017)		(0.019)		(0.013)	
Mother- Prim Education (MP)		0.163		0.015**		-0.007		0.003
		(0.103)		(0.007)		(0.010)		(0.005)
Mother: Mid/Sec Education (MS)		0.258*		0.024*		-0.020		-0.001
		(0.137)		(0.013)		(0.014)		(0.003)
Drought x MP		-0.100		-0.025		0.020		-0.020
		(0.154)		(0.020)		(0.021)		(0.013)
Drought x MS		-0.260*		-0.045		-0.018		0.005
		(0.120)		(0.035)		(0.028)		(0.009)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-3.247***	-3.401***	0.266***	0.278***	-0.029	-0.039	0.040	0.015
	(0.714)	(0.785)	(0.082)	(0.084)	(0.113)	(0.012)	(0.061)	(0.003)
Tobit Sigma			0.090***	0.090***	0.100***	0.100***	0.049***	0.049***
			(0.011)	(0.011)	(0.111)	(0.111)	(0.003)	(0.055)
Obs	900	900	1,097	1,097	1,097	1,097	1,097	1,097
R^2-Adj	0.096	0.098						
Baseline Coefficient	-0.109**	-0.027**	0.025	-0.010	-0.211*	-0.027**	0.025	-0.010
	(0.040)	(0.012)	(0.017)	(0.008)	(0.101)	(0.012)	(0.017)	(0.008)

Note: Significance levels denoted as *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors (SE) in parenthesis, with small cluster T(G-1) degree of freedom correction. mother's educational categories are defined differently from baseline specifications: no edu = 0 years of schooling, prim edu = grade 1-5 completed, mid/sec edu= grade 6 or above completed. All tobit equations were tested for robustness against OLS estimates – coefficients retain their significance, although tobit estimates are more conservative.

TABLE 11: AUGMENTED SPECIFICATION - IMPACT OF DROUGHT BY CHILD'S ASPIRATIONS

	Mathz-z score		Time-School		Time -Work		Time-Dom	
	OLS	OLS	Tobit	Tobit	Tobit	Tobit	Tobit	Tobit
	(a')	(b)	(c')	(d)	(e')	(f)	(g)'	(h)
Drought	-0.232***	-0.223***	-0.035*	-0.036	0.023	0.021	-0.014	-0.014
	(0.062)	(0.063)	(0.021)	(0.022)	(0.028)	(0.027)	(0.012)	(0.012)
Asp (Sec/Higher)	0.374***		0.029***		-0.018		0.003	
	(0.058)		(0.010)		(0.012)		(0.004)	
Drought x Asp (Sec/ Higher)	0.142		0.020		0.001		0.009	
	(0.109)		(0.030)		(0.029)		(0.010)	
Asp (Univ)		0.304***		0.029***		-0.023*		0.003
		(0.077)		(0.011)		(0.013)		(0.004)
Asp (Grad)		0.456***		0.034***		-0.015		0.004
		(0.070)		(0.011)		(0.013)		(0.006)
Drought x Asp (Univ)		0.247*		0.018		0.012		0.002
		(0.136)		(0.030)		(0.032)		(0.008)
Drought x Asp (Grad)		0.015		0.018		-0.010		0.016
		(0.123)		(0.032)		(0.027)		(0.014)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Constant	-3.109***	-3.123***	0.315***	0.313***	-0.013	0.003	0.016	0.049***
	(0.664)	(0.622)	(0.091)	(0.092)	(0.108)	(0.100)	(0.003)	(0.053)
Tobit Sigma			0.088***	0.088***	0.098***	0.098***	0.049***	0.049***
			(0.010)	(0.010)	(0.003)	(0.003)	(0.054)	(0.054)
Observations	900	900	1,091	1,091	1,091	1,091	1,091	1,091
R^2-Adj	0.130	0.132						
Basline Coefficient	-0.151**	-0.151**	-0.026**	-0.026**	0.022	0.022	-0.009	-0.009
	(0.071)	(0.071)	(0.011)	(0.011)	(0.017)	(0.017)	(0.008)	(0.008)

Note: Significance levels denoted as *** p<0.01, ** p<0.05, * p<0.1. Clustered standard errors (SE) in parenthesis, with small cluster T(G-1) degree of freedom correction. Controls include: child's age, child's birth order and gender, gender of household head, highest grade completed by the father and mother respectively, floods and frost shock reported by the household. All tobit equations were tested for robustness against OLS estimates – coefficients retain their significance, although tobit estimates are more conservative.