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ABSTRACT

Live aid revisited: long-term impacts of the 1984 Ethiopian famine on children*

In 1984, the world was shocked at the scale of a famine in Ethiopia that caused over half a million deaths, making it one of the worst in recent history. The mortality impacts are clearly significant. But what of the survivors? This paper provides the first estimates the long-term impact of the famine twenty years later, on the height of young adults aged 19-22 years who experienced this severe shock as infants during the crisis. An innovative feature of the analysis is that famine intensity is measured at the household level, while impacts are assessed using a difference-in-differences comparison across siblings, and compared with an IV cross-section, using rainfall as an instrument for the shock. We find that by adulthood, affected children who were aged of 12-36 months at the peak of the crisis are significantly shorter than the older cohort, and their unaffected peers, by at least 5cm. There are no significant effects on those in utero during the crisis, and we cannot rule out that for this cohort, the selection effect dominates scarring. Indicative calculations show that for the affected group such height loss may lead to income losses of around 5% per year over their lifetime. The evidence also suggests that the relief operations at the time made little difference.

JEL Classification: I12, J13, O12 and O15
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1 Introduction

In October 1984 Ethiopia came to the developed world's attention in a dramatic BBC news broadcast from Tigray province in the Northern Highlands. The report showed pictures of starving people on a massive scale and galvanised citizens in Europe and the US into donating millions of pounds to relief agencies, and putting unprecedented pressure on their governments to send humanitarian relief. Up to a million people may have died, and many more were left destitute, making it one of the worst famines in recent history, and on par with the Chinese famine of 1959–61 in terms of mortality as a proportion of the population (O Grada, 2007). This paper examines what has happened to a sample of young people who experienced this extreme shock as infants by following up on their height attainment and other socio-economic outcomes twenty years on from the crisis.

There is a small but growing economics literature documenting the long-term impact of severe shocks and famines on subsequent human development. Several papers investigate impacts of China's Great famine (Chen and Zhou, 2007). Neelson and Stratmann (2010) provide recent evidence on educational impacts of the Greek famine of 1941. Alderman et al. (2007) find negative nutritional and schooling impacts from civil war and drought in Zimbabwe. Examining a positive rather than negative 'shock', Maluccio et al. (2008; 2009) show lasting improvements from an experimental nutrition intervention in Guatemala.

This paper is the first study to quantify the long-term consequences of one of Africa's most severe famines. We have access to longitudinal data of 550 young adults aged 17-27 years old in 2004, collected as part of a rural household panel survey across the country, initially started in 1989 as a study to document the impact of the famine. Our empirical strategy exploits the natural experiment inherent in the drought crisis underlying the famine, by comparing affected and non-affected siblings across cohorts both *in-utero* and in the first few years after birth with their older and younger siblings.

Methodologically, our paper is an innovation on earlier work as we have access to a measure of crisis intensity at the household level, though our sample size is smaller. Other studies of the impact of drought or famine in other countries have relied on shocks specified at the covariate level which, while exogenous, increases measurement error in the measure of the crisis. The standard geographical identification of the shock also limits their ability to isolate the impacts of the drought from other factors. Our measure of the crisis at the household level is then likely to increase precision and offer more convincing causal attribution. However, it is self-reported and could suffer from endogeneity bias for several reasons which we discuss in detail below. In summary, we deal with this in two ways. First, we are also able to examine impacts within households by using data on siblings. Using household fixed effects models allows us to eliminate any endogeneity bias that may have been caused by correlations between measured famine exposure and unobservable fixed household characteristics. Also, famine survivors are typically from the top of the distribution determining survival (Deaton et al., 2008). To the extent that such characteristics are correlated with household fixed effects, the sibling difference model considerably limits this selection bias as well. Second, we use rainfall data at the village level as an instrument for the drought shock at the household level in order to isolate the exogenous variation in household-level drought impacts.

We find that by adulthood, children who were under the age of 36 months at the peak of the crisis are significantly shorter than the older cohort who were at a less vulnerable age, by 5 cm. Besides providing evidence on the impact of a specific famine on height, the paper contributes to the broader empirical literature on the importance of early childhood nutrition for subsequent childhood development (Strauss and Thomas, 2008). Actual height relative to genetic potential height can be seen as a measure of nutritional achievement, and taller people tend to be healthier, do better on cognitive tests, have more schooling and earn more (Deaton and Arora, 2009). We confirm the presence of some of these other socio-economic effects from the famine period: affected children are less likely to have completed primary school, and are more likely to have experienced recent illness. Using a Mincerian framework, we can show that these results are economically important for these young adults, leading to income losses of at least 5% per year over their lifetime.

In the next section we give more background on the 1984 famine, and introduce the data used in this paper. Section 3 offers a conceptual backdrop, and a review of the existing evidence on the impact of serious shocks and famine. Section 4 presents the econometric strategy, and section 5 discusses the results.

2 Background and Data: The 1984 Famine

Ethiopia has a long and troubled history of famines (Pankhurst, 1986a) including prolonged droughts and frequent severe rainfall failure. Since 1984 Ethiopia has appeared frequently in the worldwide media because it was on the verge of famine. The economy has experienced growth in the past decade, but seasonal hunger continues to be an endemic feature of life in many rural areas.

However, even against this difficult backdrop, the 1984 famine is still classed as one of the worst famines ever to have hit Ethiopia, and ranks amongst the worst in recent world history (O Grada (2007)). The impact of the famine in 1984 was deep and broad, though as is often the case in a complex emergency, statistics are sparse and unreliable. There is still no firm consensus on the number of deaths that it caused (see the debate in Pankhurst (1986b), Holcomb and Clay (1987)), though estimates range from half a million to over a million (the upper bound being the most popular media quotation). The main regions affected were Tigray, Eritrea and Wollo in the North of the country, although its effects were felt across the country. Warfare played a key role in causing famine in Tigray even before the drought occurred. Military offensives, aerial bombardment of markets, destruction of cattle and grain stores, burning of crops and tight controls on movements of migrants and traders combined to prevent the normal redistribution of grain and livestock surpluses in Northern Ethiopia, as documented by the Africa Watch Committee (1991). Kidane (1990) calculates mortality based on interviews with resettlers, and suggests a total mortality estimate of 700,000 as reasonable.

He further notes that the famine affected all socio-economic groups equally, a suggestion that is also made by Kumar (1990), who contrasts this with the 1974 famine that had a greater effect on less well-off groups.

Relief in the form of food aid during 1984 was delayed due to political factors: a Marxist regime in power that was hostile to US and EU interests, and viewed the Ethiopia Relief and Rehabilitation Commission (RRC) as a relief agency with a history of overstating crisis statistics. Gill (1986) notes that during a donors' conference in March 1984 (when excess deaths were already apparent), many donors were sceptical of the information provided by the RRC, both on the needs of the population and the amount of available grain reserves. Historical reports by the Ethiopia Relief and Rehabilitation Commission (RRC (1984, 1985)) and other accounts (Gill (1986), Africa Watch Committee (1991), Webb et al. (1992), Jansson, Harris, and Penrose (1987)), further contextualise the development of the crisis. From these sources, we also conclude that 1982 was considered a 'normal year' of production in most regions, though no surveys are available to corroborate this. In April 1983, however the RRC report was alarming, and the main Meher crop[1] season of 1983 showed evidence of widespread crop failure. 1984 was by all accounts a year of severe drought; in almost all regions the rains failed in the Belg (minor crop) season. The drought (from the rainfall data sparsely provided in RRC reports) can be said to have lasted through late 1983 into 1984, and officially ended with the Kiremt rains in 1986.

The focus of the analysis in this paper is on the long-term impact of this serious shock by investigating nutritional and other outcomes 20 years later. We use a sample of 550 young adults (aged 17–27) from the sixth round of the Ethiopian Rural Household Survey (ERHS) in 2004. Their households have been surveyed by the University of Addis Ababa and the Centre for the Study of African Economies (CSAE) at the University of Oxford, as well as the International Food Policy Research Institute (IFPRI) since 1994. It builds on a survey in 1989 conducted by IFPRI to study the impact of the famine (Webb et al. (1992)). The survey covers eighteen villages in fifteen communities[2] from five regions.[3] Within each village, random sampling was used. The households were resurveyed twice in 1994, again in 1995, and subsequently in 1997 and 1999. The sixth round of the survey was completed in 2004. The attrition rate for households is low, at around three per cent per round, reflecting the very low levels of migration in Ethiopia. The rate of attrition for individual household members is somewhat higher, as the survey followed households at a particular location, and not all individuals. Its likely consequences for our estimation are discussed further below. Data on height are from the sixth round of the survey in 2004, twenty years after the famine.[4] We also use data on the heights of these individuals as children, from

[1]The Meher is the main crop of the year, harvested after the main Kiremt rains described above.

[2]These communities are called Woredas, or the equivalent of a county in the UK. They are further divided into what we term villages, officially called Peasant Associations (PAs), the lowest administrative unit.

[3]Although representative, 18 villages is clearly not enough to make strong inference about Ethiopia as a whole.

[4]A seventh round has recently been completed in end 2009, however anthropometric data

the first round of the full ERHS survey in 1994, ten years previous. In 1995, the famine information collected in 1989 was supplemented by a careful recall module on droughts at household level. These data allow us to create a household level variable describing the intensity of the famine, something that is unique in this literature. However, it is self-reported and therefore could potentially be endogenous (see discussion below), so to supplement the household measure of the drought severity we use rainfall data as an instrumental variable.

Segele and Lamb (2005) have analysed rainfall at a regional level over a period of 38 years (1961–99) and find that 1984 is extremely distinctive as the driest overall year – the Kiremt[5] rains started relatively early but then dried up quickly, leading to an impossibly short effective growing season. They cite rainfall deficits of up to 94% in Wollo and the Rift Valley. We compile data on rainfall during the period from three sources: the Ethiopian Meteorological office, the Food and Agricultural Organisation of the United Nations (FAO), and the US National Climactic Data Centre (NCDC) Global Historical Climatology Network (GHCN) database.[6] We use data on the nearest rainfall station to the village which has non-missing data for the relevant period (there is not information on all villages from any one of the time series). Details are outlined in annex (A). As well as an average for the long term (all available years between 1960 and 2004 for each village), we calculate mean rainfall during the period in which the comparison cohort children were young (see below for description of cohorts: 1978 to 1987, omitting 84-85), and the average rainfall in 1984-85, and use the shortfall in 84-85 as our measure of the drought shock. In some specifications we include only older children as the comparison group, so we recalculate mean rainfall, from 1975 to 1982, and the related 84-85 deviation. Table 1 shows the self-reported drought shock averages as well as the rainfall shortages thus calculated. The lowest year on average for which we have all villages is 1984, by quite some margin. There is variation across villages, though no village experienced rainfall above the long term mean. For example, Haresaw in Tigray has rainfall of just 45% of its long-run mean, whereas Yetemen had a shortfall of only 4%.

A further discussion of the data, including the identification of the famine variable, and the child cohorts used in the empirical analysis is included in the econometric strategy.

3 Theoretical framework and existing evidence

Almond and Currie (2011) review recent evidence on how a severe shock in a

were not collected. Note that we use age data from 1994, as there is less heaping when reporting children's ages than adult ages.

[5]The Kiremt rains account for 65–95 per cent of total annual rainfall in Ethiopia and tend to fall between June and September. In some regions (especially south of the Rift Valley) there are two sets of rain, the minor rainy season is the Belg rain in March/April though this varies by region.

[6]FAO data available from `http://geonetwork3.fao.org/climpag/agroclimdb_en.php`, accessed July 17th 2011. Many thanks to Andreas Georgiadis for the alerting us to the latter two updated datasets, and for the use of his NCDC-GCHN data.

critical period of development (usually under the age of five) may lead to persistent lower levels of achievement in human capital, and this is the backdrop to our empirical analysis. Cunha et. al's (2006) review chapter on the economics of human capital formation provides a theoretical framework that echoes the nutrition literature and the focus on 'critical period programming', showing why shocks in childhood may have persistent impacts. A key preoccupation of the economic literature has thus been to try and find exogenous sources of variation in nutrition inputs, and drought is such an example.

The literature in medicine and epidemiology contains a large number of articles on the correlation between childhood characteristics and adult anthropometric outcomes, though they are less successful at documenting causality (Karlberg and Luo (2000), Rona (1981), Ruel et al. (1995) and for a commentary, Gunnell (2002)). The 'foetal origins hypothesis' incorporates a substantial body of epidemiological and biological research that adult outcomes (especially in terms of health) are strongly influenced by experiences in the womb (for an overview see Barker (1992) and Godfrey and Barker (2000)- the latter summarises experimental evidence for rats). There is also evidence that the potential for catching up on lost growth is limited beyond the age of three as summarised by Martorell *et al.* (1994). Medical evidence appears to show that genetic factors play a lesser role than environmental factors or nutrition in explaining height differences across groups (Habicht *et al.* (1974)). The Dutch winter famine of 1944-45 has been studied in the epidemiology literature, with some degree of consensus in the results for example on the long term impact of the shock on coronary heart disease and obesity Roseboom *et al.* and Lumey *et al.*, (2006; 2007) and recently on cognitive function of adults (de Rooij, Wouters, Yonker, Painter, and Roseboom, 2010). Taken together, this literature then suggests that critical ages for a child appear then to be especially up to the age of about 36 months, as catch-up afterwards is limited, providing the basic hypotheses for much of the empirical literature on the long-term impact of shocks.

In the economics literature there is a small but growing body of evidence on the long-term impacts of early childhood shocks. There appears to be some evidence that shocks related to infection and disease have their greatest impact when children are in the womb, whereas nutritional shocks have a critical period in the 12-36 month period. In a seminal study, Almond (2006) found a substantial long-run effect of the 1918 influenza pandemic on US data of those in utero during the crisis, though notes that it is impossible to separate the effect of the illness from other macroeconomic events of the time. Banerjee *et al.* (2007) estimate the long-term impact of disease around the time of birth on adult health outcomes using state level data from 18th century France. They find a significant impact on height, but not on life expectancy or morbidity. Dercon et al. (1996) use longitudinal data on initially non-orphaned children to study the impact of orphanhood in Tanzania in areas seriously affected by HIV-AIDS and find that orphanhood decreases height and educational achievements in adulthood.

Experimental studies can be used investigate the impact of positive nutrition 'shocks'. Several studies on an experimental nutrition intervention in Guatemala have provided supportive evidence for long-term effects (up to 20 years after the intervention). The studies, on child growth rates (Schroeder, Martorell, Rivera, Ruel, and Habicht, 1995), wages earned by men (Hoddinott, Behrman, Flores, and Martorell, 2008) and human capital achievement (Maluccio *et al.* (2009)) find the biggest impacts when children were two years of age during the intervention and insignificant impacts after three years of age.

Extreme negative shocks do not have the luxury of the presence of experimental designs. At best, we can use natural experiments such as those offered by large droughts and other crises. Strauss and Thomas (2008) note that relatively few economic studies have identified a long–term causal impact of an extreme event experienced in childhood, given the high data demands for such an exercise. Chen and Zhou (2007) investigate the China famine of 1959-61 on those born between 1959 and 1962 using a difference-in-differences estimator of birth cohorts using outcome data collected as part of a cross-section survey in 1991, and death rates at the province level as a measure of shock severity. They use individuals born five years before and after as the control group. Their results show a height deficit of just over 3cm for those born in 1959. Luo *et al.* (2006) find increased obesity in later life of the same cohort.

Alderman et al. (2007) use maternal fixed-effects and instrumental variables to study two cohorts of children who were alive during a civil war and drought in Zimbabwe. They examine nutritional and schooling outcomes for children who experienced shocks at a vulnerable age (12–24 months) and find that these children are significantly smaller and complete less schooling; using typical rates of return to education they translate this to a loss in lifetime earnings of seven per cent.

Neelson and Stratmann (2010) find educational attainment was affected by the Greek famine of 1941, especially for those who experienced the crisis as infants. Maccini and Yang (2009) show that even non-extreme variability in rainfall during early life has a significant effect on a large number of future adult outcomes in Indonesia.

While generally persuasive, these and other studies are not without their potential problems (for a review and critique of several of the studies quoted, see Strauss and Thomas (2008)). For example, many rely on variables defined over relatively large geographical entities for distinguishing the affected and non-affected within a particular cohort, contributing to measurement error and attenuation bias, as well as risking that confounding factors cannot be isolated. In our study, we can define the famine shock at the household level rather than relying on a geographically defined shock; in addition we use village level rainfall as an instrumental variable. We can then capture the exogenous variation in this individual shock measure which may be correlated with unobservable household factors. Measurement error, correlated with unobserved household characteristics, could cause further bias.We also control for unobserved household heterogeneity by identifying all impacts using within-household variation. A further issue (Strauss and Thomas, 2008) is that the impact of some of the crises studied above likely lasted longer than the time-period specified (e.g. as infrastructure needed to be rebuilt after a civil war). This is definitely an issue for our study, as the Ethiopian famine took place within the context of a mostly relatively localized but nevertheless intense civil war, so that life in any case did not return to normal immediately after the 'end' of the famine. In any case, this would bias us against finding any results and thus our findings would be a lower bound.

A further issue is mortality and fertility selection. Dyson (1991) identifies some demographic regularities of five large south Asian famines between 1876 and 1975, and finds that fertility is significantly affected by famine, and at an earlier stage of the crisis than mortality, with birth rates well below normal before peak mortality. We discuss the implications of this for our analysis below.

4 Econometric strategy

We aim to test whether the Ethiopian Famine of 1984 affected children who were at a vulnerable age – in utero or newly born up to the age of 36 months – at the time of the drought shock. The empirical analysis uses a reduced form specification, given that we do not have specific information on other inputs in early childhood; our key question is to identify the long-term impact of the famine. Equation 1 provides the basis for our test.

$$H_i = \beta + \sum_{c=2}^{C} \beta_{fc} fam_h \rho_c + \sum_{c=2}^{C} \beta_c \rho_c + \gamma_h + e_i \qquad (1)$$

Our main focus is height H_i for each individual i at adulthood in 2004, but we will use a similar set-up for other outcome indicators, such as education and morbidity. We will compare the height attainment of a number of age cohorts c defined across all the young adults aged 17 to 27 in 2004. In particular, as different age groups are likely to have been differentially affected, we specify a spline function across C cohorts. We consider four cohorts, with the oldest group as the base group. The oldest cohort were over the age of four during the peak of the crisis and therefore beyond the critical stage of development. Two cohorts are likely to have been affected: those aged 2-3 in the crisis period, and those in-utero or born during the crisis. The youngest cohort were conceived after the peak of the crisis, so are less exposed (though it is possible that they were, through prolonged malnutrition of the mother- see discussion below).

We observe a household level famine shock fam_h which is interacted with the age (cohort) of the individual at the time of the shock. β_{fc} will measure the impact of the famine on children of cohort c living in a household directly affected by the drought shock. If the impact of the famine went well beyond cohorts living in families directly affected by the famine, then this would be picked up by values of β_c, defined relative to a base group cohort that was born well before the famine, and past the critical first few years of life. Our identification strategy for the famine effects depends crucially on two elements: a household-specific drought shock and the appropriate identification of cohorts for comparison. We discuss each in turn. We then discuss our two estimating strategies. First, the use of household fixed effects exploiting the differences in birth timings within households, and second an instrumental variable to purge the household drought shock of endogeneity due to correlation with unobservable household characteristics.

4.1 Drought shock definition

As noted in the introduction, the famine peaked in the year lasting from October 1984 to September 1985 (Ethiopian calendar year 1977). Continuing drought meant that many harvests failed towards the end of 1985, and the end of the crisis is seen by most accounts to be towards the end of 1986 when harvests were at almost normal levels. In this section we use the available data to confirm this period as an identifiable drought shock. We identify the drought shock at the household level. In 1995, as part of an in-depth investigation of the 1984–85 famine and other serious shocks, their responses and experiences of relief efforts, households were asked to report whether they experienced serious droughts in the last 20 years.[7]

77% of all households in the sample cite drought as one of the shocks that has affected them in the past 20 years, in a questionnaire that included a variety of potential crises such as; too much rain, pest and diseases, harvest losses in storage, frost and hailstorms. Respondents were asked to cite the three worst crises of the last twenty years. Drought is by far the most common shock. Of those who cite drought, the majority (72%) mentioned the years 1983–85 (1975–77 EC), and 60% cite 1984–85 as their worst year when asked to name the worst three years.

Table 1 column (2) shows the region of each village. The third column (3) shows that over 80% of people mention drought as a problem in approximately two thirds (eleven) of the villages, and the fourth column shows that in these villages 1983–85 is the clear mode. We include the 1984–85 specific drought shock at the household level (column 5). Note that the famine shock variable is collected at the household level, ten years prior to the final height measurement we use as the dependent variable in 2004, and the question referred to the households' experiences, not those of any one child, limiting respondent bias correlated with the height potential of one child relative to another is unlikely. At the same time, despite being based on recall, the underlying question here is nevertheless simple, and refers to one of the most serious crises in these families' life time, rendering measurement error and its implied attenuation bias less likely. Also, even though drought is by its nature a covariate shock, affecting specific geographical areas, it does not affect all individual households in a community in the same way, depending on the specific livelihood, type of crops grown, access to alternative water sources for water harvesting, and, given the mountainous nature of many of the villages and its implications for the micro-climate, the exact geographical location. Only in a few villages, *all* villagers reported to be affected but this is a plausible outcome.[8]

[7]Specifically, the question asked in the third round of the ERHS is "In the last 20 years has the household suffered a substantial loss of harvest through any of the following [list of potential crises]?" The households that responded affirmatively for drought were asked to list the three worst crisis years. Ethiopian calendar year 1977 corresponds to 1984–85 Georgian calendar.

[8]In the village fixed effects estimates, these villages drop out due to lack of variation so we

4.2 Affected and control groups of children

Having carefully identified that the main shock in question occurred in 1984–85, we are particularly concerned to identify the impact on very young children, affected by the famine in utero or in early childhood (i.e. born 1981-85). We take as a comparison group children who are born early enough to be beyond the critical period for early child development (i.e. over 4 years) and those born after the famine. The whole group of children are thus aged 17–27 in 2004. We used the age data collected during the first round of the longitudinal study in 1994. We found there is considerable rounding; for example 46% of children are reported to have a rounded age (e.g. 12 years and zero months) with heaping around age 10 and 12. This will add to measurement error in the identification of the effects of the famine, causing attenuation bias that will reduce the chances of finding an impact of the famine.

We wish to estimate an age fixed effects model as part of the econometric strategy, therefore we take 24 month cohorts of the sample of children born during the drought; just after the drought; and children who are 24 months years older and younger than them - which gives us four cohorts of children aged 7–17 in 1994 or round one of the dataset, ten years after the drought. Table 2 shows the construction of the cohorts, and also notes the key events in the timeline of the crisis. The youngest cohort are born between September 1985 and September 1987, after the peak of the famine had subsided. We expect these children in principle to be largely unaffected, though it is possible that some effect may pass through the mother after a long period of undernutrition just before the pregnancy. They are aged 17–18 at the time of measurement in 2004, and therefore possibly still growing relative to the other cohorts considered.[9] The next cohort are born between September 1983 and September 1985. This is the peak of the famine as seen in the literature review, and these children are either born directly into the midst of the suffering, or are in utero for the suffering periods. They are aged 19–20 years in 2004. Whilst several studies on infectious disease have found significant and large impacts if crises on children exposed in utero (e.g. Almond 2006), the studies reviewed above on nutritional shocks (famine, drought, or nutritional supplementation) have tended to find larger impacts on children aged 12-36 months. This is our second oldest cohort, born September 1981–September 1983, and are therefore aged 13–36 months during the 'peak' famine year of 1984–85. We might *a priori* expect these children to be the most affected for two reasons: firstly, they were at one of the most vulnerable ages to experience a nutrition shock (having probably stopped breastfeeding around the time of the peak crisis), and also, in some of the villages (particularly the three in the North of the country) the food crisis was ongoing from around 1981 due to civil war, therefore they may have lived through a longer period of suffering and nutritional deficit. Further, mortality rates are usually higher for infants during crises (Razzaque *et al.* (1990), Dyson (1991)),

exclude them from the analysis, similarly for the IV estimates.

[9] As adolescents may still have growth spurts up to the age of 18, we will have to be cautious in interpreting the findings, while heterogeneity in the end year of growth will increase the variance of the estimates of the famine impact for this cohort.

so we may expect the selection effect of mortality to be higher for the 'in utero' cohort than the 'young child' cohort, who are aged 21-22 years in 2004. The oldest cohort are born between September 1976 and September 1981.[10] This cohort are thus born before the main famine began, and were at a more 'robust' age when the famine struck (over 36 months): though to the extent that they were at all affected this will reduce our estimate again towards a lower bound. This cohort is aged 23–27 in the 2004 survey, and forms the base group for the analysis.

As well as mortality selection, we must also be aware of fertility selection. In, and just after a crisis fertility may be affected that mean our younger cohorts (born after the event) may be considered as endogenous. For example, richer or less affected families may have more children (either through biological channels, whereby their better nutrition has a positive impact on the probability of a successful pregnancy and childbirth, or, because they choose to have children since their circumstances have not worsened). Conversely, richer households may deliberately plan to have fewer children in response to the adverse environment, whereas poorer households may be less likely to actively plan fertility decisions. Below, we test the relevance of these considerations. We also drop the younger cohort to check robustness of the results to its exclusion.

4.3 Identification strategy: household fixed effects

Our first identification strategy is based on the inclusion of γ_h, in equation 1 capturing all household effects, ensuring that comparisons are done within cohorts of siblings living in the same family.[11] Crucially, it allows us to disentangle famine exposure of a particular cohort from unobserved household heterogeneity correlated with famine exposure. However, and if anything, we still expect that our effects may be biased downwards, giving lower bound estimates of the long-term impact of the famine.

A standard problem in inference on the impact of large crises is positive selection into the sample of those who survived despite being at risk during the critical period. The drought and famine will have had a permanent impact on a large number of children through their early mortality. Stronger, healthier children with better genetic health endowments are more likely to have survived. In addition, these children could have subsequently benefited from reduced cohort sizes due to lower fertility during the famine, or high levels of child mortality at that time. This positive selection would lead to a downward bias in the estimated impact. Given that mortality rates for new-borns and those under 12 months are often higher in crises (as noted earlier), it may well be that this impact is stronger for the second-youngest cohort, born during the famine. It is important, however, to note that evidence from epidemiological studies during famines and other crises in Ethiopia, Guinea-Bissau and Cambodia highlight the strong predictive power

[10] We include five years in this cohort in order to maximise the sample size, but start only in 1976 as another large famine in 1974 may have affected children born before 1976.

[11] In Ethiopia, households are typically organised as nuclear families, with parents living with their children, and married children leaving the homestead. The result is that despite high fertility, households are relatively small, on average below 6 members per household.

of household and parental characteristics such as wealth and parental education in explaining excess mortality during such crises (Kiros and Hogan, 2001; Nielsen, Jensen, Andersen, and Aaby, 2006; Hong, Mishra, and Michael, 2007), so that the use of household fixed effects will reduce the likely bias.

4.4 Identification strategy: Instrumental variable

Several problems remain with HH fixed effects: the differencing may in fact increase the noise to signal ratio, and exacerbate measurement error leading to greater attenuation bias (Bound and Solon, 1999). Further, the self-report of the drought shock may be driven by households' capacity to respond. In this way, there may be 'high capacity' and 'low capacity' households, and if the shock is more likely to happen to the latter, using evaluation language we are capturing a treatment effect on the treated, rather than an average treatment effect - where the impact includes the households' inability to remediate the effect of the shock (and indeed any possible reinforcing of the shock by parents who invest in other siblings). If future famines are likely to affect certain groups then this is not necessarily an estimate with little use - rather it gives *the potential impact on those who are likely to be affected*. A further possibility is that the self-report of the drought shock may be driven by the very fact of having a young child in the household at the time of the crisis- leading to reverse causality.

We check the exogeneity of the drought shock in table 5 below and do not find any evidence of selective reporting based on observable household characteristics. Finally, there is a possibility that differences across siblings are somehow driven by macroeconomic (or village level) factors that are experienced by each cohort.

To allay the concern that the self-reported shock is correlated with unobservables, we use an instrumental variables strategy to isolate the exogenous component of the shock. Rainfall deficits at the village level are used as the exogenous instrument (discussed further in the data section). We estimate a cross-section model separately in order to remove the possibility that sibling differences hich also allows us to rule out confounding the results with any other macroeconomic events or circumstances that may have affected cohorts differently over their life course. Regressing outcomes in 2004 on the famine shock variable in a cross-section means that we leave γ_h unobserved and a potential cause of endogeneity if correlated with fam_h. We therefore instrument fam_h with $rain_v$ in the first stage.

4.5 Outcomes and Descriptive Statistics

To measure the stock of nutritional achievement of the cohorts when they are young adults in 2004 we use their height in centimetres. As other measures of human capital at adulthood in 2004 we use weight measurements to construct a body-mass-index (BMI), and also use a question on recent health. Specifically, the health question asks whether the person has been ill in the past four weeks to the survey (in 2004). Evidence on the impact of nutritional shocks on BMI may be complicated however, recalling the results above that sometimes children or those in utero at the time of the shock actually end up more likely to be overweight (and have high blood pressure) than others, due to physiological changes that make the body more efficient at processing available nutrition.[12] We utilise available information on the highest grade that the cohorts achieved in school- around twenty per cent have never attended school and half have not achieved full primary education or above. Basic summary statistics are reported in table 6. Average heights of each cohort, separated into the affected and unaffected groups is reported in table 3, and we note that we find significant (at 90%) differences only in the oldest group, where the famine affected group are taller. This is the group where children were over the age of 3 when the famine occurred, suggesting that the potential height of children from famine-affected households is higher.

5 Results and Discussion

5.1 Results

We begin with results investigating the exogeneity of the self reported drought shock. This variable has the advantage of measuring impact at the household level, which to our knowledge is unique in the literature. However, the concern is that it may be endogenous in ways that cannot be controlled for by using household fixed effects. One potential issue is that there may be 'high capacity' and 'low capacity' households in terms of their ability to deal with such events, and if the shock is also more likely to happen to the latter group, we may be capturing this phenomenon, which includes an inability to remediate the effect of the shock. A second issue is that the presence of a child who was at a vulnerable age in the household during the crisis period may have induced households to report that they suffered in the famine, and thus cause a problem of reverse causality. To address these, in table 5 we regress the self-reported drought shock on various variables of concern.[13] Column (1) includes predetermined characteristics of the household head such as gender, age, height, schooling as well as village fixed effects. None are significant, and the village fixed effects are almost all significant, and an F-test shows joint significance. In column two we replace the village fixed effects with rainfall deviation in the drought years. This is significant and large in magnitude. A one standard deviation fall from the mean rainfall deviation increases the probability of reporting the drought shock by eight percentage points (the mean number of households reporting the shock is 54 per cent). In this specification the height of the household head is also significant, though its effect is around half that of rainfall - an increase in height of one standard deviation (8cm) reduces the probability of reporting the drought by just under four percent. Note however, that when we include average height of the village in column (3), the individual height impact disappears completely - which may be picking up for example ethnic differences in height, though could also be proxying other unobserved village factors. We include a dummy variable for the presence of a

[12]See Strauss and Thomas (2008) for further discussion and references.

[13]Descriptive statistics at household level for this regression are shown in table 4.

child in each of our study cohorts on the right hand side in all specifications. None of these has a significant coefficient, allaying the concern that having a child in any of the cohorts causes an increase in the probability of households reporting the drought shock. The tests presented are based on observables, so we also report IV regressions later in the results using the rainfall deviation as our instrument, to allay concern about endogeneity caused by correlation between the self reported shock and unobservables.

Table 7 shows the impact of the famine on the 550 young adults aged 17–27 in 2004. Table 6 shows means and standard deviations of the included variables. We include the age cohorts, and their interactions with the shock variable, as well as controls for sex of the child and its birth order. Standard errors are adjusted for cluster-village specific heterogeneity. Column (1) displays OLS (village fixed-effects) estimates. The cohort of children born in 1982-83, who were aged 12-36 months at the peak of the crisis, are 5.3cm shorter than their unaffected cohort peers. Compared to the excluded group of unaffected older children, they are just over 3cm shorter. There are no significant shock impacts on the other cohorts. We would have expected an impact on the next cohort down, who were in utero or infant during the crisis - the coefficient is negative, but insignificant. However, we cannot reject that the coefficient is the same for both groups, given the standard errors. One possible explanation is that selection into the sample is stronger (positive) as mortality rates would have been higher for this more vulnerable group during the crisis. In column (2) we restrict the sample to only those households with 2 or more siblings in the sample and show the household fixed effects estimate, which is slightly higher, though does not appear to be different from OLS given the coefficients and standard errors in both equations, lending some support to the idea that the self-reported drought estimate is exogenous.[14] As discussed above, there is a concern that, given a strong and heterogeneous fertility response to the drought, younger children (i.e. conceived just after the crisis) may be endogenous and therefore an unsuitable comparison group to analyse the impact of the crisis. We therefore re-estimate the OLS village fixed effects model with only those children already conceived prior to the crisis (our oldest three cohorts) in column (3). This leads to a reduction in sample size to 379 individuals. The point estimate goes up by just under one centimetre, though this is not likely to be substantively different from that of the larger sample, given the point estimates and standard errors. In the fourth column we repeat the analysis using household fixed-effects, and the point estimate is also very similar.[15]

[14]Alternatively, it is possible that any downward bias in OLS may be exactly offset by the downward bias potentially caused by the exacerbated measurement error problem in the fixed-effects model.

[15]To further investigate fertility bias, we ran a probit on the three younger birth categories for those who have a child aged 22-27, and included the drought shock, plus village and household characteristics. The drought shock was not significant. Schooling of household head also not significant. Age of the household head and having a child who died that would be in the sample (decreased) increased the probability of having a child in the the youngest group- suggesting that mortality may be playing a role. But from this we do conclude as suggested that we find no evidence of a fertility response of the households in terms of drought experience or socio-economic characteristics. This is consistent with the estimates being effectively the same when

The results in table 5 confirmed no correlation between observable household characteristics and the drought shock. We check the robustness of the results to any endogeneity driven by unobservables by implementing an IV strategy, as outlined above, using the village rainfall deviation in 1985-85 as an instrument for the self-reported drought shock. We present results for each of the three oldest cohorts separately, firstly to remove the possibility of any unobserved macroeconomic events/trends driving the between-cohort results, and implement first an OLS cross-section and then an IV estimate. IV estimation with weak instruments and small sample size could lead to bias in our results towards OLS, and standard errors may be downward biased downwards, (Stock and Yogo, 2002; Murray, 2006). We report the Stock and Yogo (2002) critical values for strength of the instruments, and Fuller IV estimates, a weak-IV robust estimator (Murray, 2006).[16] In the OLS we do not include village fixed effects from the model (as we cannot include these in the following IV estimates, due to having a village level instrument). Columns (1-2) are the youngest cohort (born 1984-85), columns (3-4) are the cohort aged 12-36 months during the crisis (born 1982-83) and the final two columns are the oldest cohort (born prior to 1982). We have a weak IV for the oldest cohort, however for the other cohorts the IV is fairly strong, given the small sample size. The significant impact is again found for the 12-36 month old cohort, confirming the pooled results. The point estimate is slightly higher, which could suggest some downward bias in the fixed effects model, as discussed above.[17] The IV has a Kleibergen-Paap F-statistic of 19.92, which is above the 10% maximal bias in the Stock and Yogo (2005) critical values table.[18] The first stage results are reported in table 9.

In sum, using several different estimation techniques we find a strong and significant effect of the drought and famine shock of 1984-85 on children 2-3 years of age at the time of the famine. In terms of the literature, many studies have found significant impacts of infection or disease on children in the womb (Almond, 2006), though nutrition studies (e.g. Alderman *et al.* (2006) have found impacts on slightly older children, of a similar age to those in our results. This group were just above breastfeeding age, but still not quite at an age to be robust to shocks in the longer term. To further explore exactly the affected age groups, we ran the OLS model with a dummy year-by-year for the cohorts and a famine interaction, and whilst the sample sizes were very small, the pattern confirmed that of the constructed cohort groups: the impacts were negative significant for children born in 1983 and 1984, negative but not significant for those born in 1982, and non-significant for other years.

we restrict the sample to the three older cohorts.

[16] We also compared the results between the Fuller estimates and Limited Information Maximum Likelihood which is an alternative estimator that is also robust in the presence of weak instruments. The results were effectively the same.

[17] Though the standard errors are somewhat larger, and thus we do not claim that the estimate is different.

[18] We did attempt to also estimate across the pooled model. However, instrumenting the interaction of the cohort and the drought shock with the interaction of the cohort and the instrument resulted in weak instruments.

5.2 Robustness and Further Interpretation

We also explored whether the famine had long term impacts on any other human capital outcomes for these cohorts, such as health, BMI or schooling. We ran a probit on whether or not the young person was ill in the four weeks prior to the 2004 survey. The coefficient on the drought interaction term for children born in 1982-83 is significant at 10% but the marginal effects reported in the table were just insignificant (p=0.14). Even this result is quite interesting, given our relatively small sample size, as morbidity measures are generally weak discriminators for health and morbidity is measured 20 years after the event. We also ran regressions on BMI and schooling, but there were no significant effects. For all the specifications, we also explored whether there were any gender-specific effects across all the results reported. We find that there is never a jointly or cohort-specific significantly different effect for girls and boys due to the famine.

The results on height are not driven by opportunistic definition of the relevant period for the famine shock. For example, repeating the analysis by replacing the famine shock as defined here referring to the peak period (in 1984–85) to a broader period 1983–85 (i.e. households that also reported this period as their worst period) made no difference to the results. Qualitative work in some of the communities by Bevan *et al.* (1994) has suggested that the crisis may have started earlier in Tigray, and the communities covered by the survey in these regions, as rains may have failed in 1981–82, and also because this was the specific region most affected by the conflict.[19] The result is that for these communities (Haresaw, Geblen), the use of the older cohort as the base group may not be providing an appropriate counterfactual, potentially underestimating the impact the famine as a result. As a robustness test, we interacted the drought variable with a Tigray dummy and the interaction was insignificant.

Analysing long-term impacts of events decades earlier may be affected by serious attrition problems of which survival bias is only one. As the data in 2004 come from a longitudinal data set, we can pursue the likely impact of any other, post-famine, attrition in the data. For example, even if children survived, differential strength and intellectual ability may have affected migration or marriage of those affected and those not affected differentially. It may be that those not affected by the famine may have more labour market opportunities and have left the village, while those affected and weaker may be in the sample. This could result in further bias, but this time implying that our results may overstate our findings, if among the non-affected the stronger have been able to leave. Here, the first round of the longitudinal survey, 1994, helps to explore this, as this sample collected household membership data on all in the household at that time, with children too young to live away from home.[20]

[19] Intensive qualitative research in the other communities covered by the survey has suggested that the civil war and conflict in Ethiopia was not affecting them until much later, in the late 1980s, contrary to the communities in Tigray.

[20] In 1994, the sample would have been too young to live independently away from home. In recent times, children often live away from home for educational reasons, as secondary schools are only in towns. However, in 1994, few did so in rural Ethiopia. Net primary school enrolment

Relative to 1994, we are faced with considerable attrition for the age-specific sibling sample used in this study. Of the initial sample of the relevant age-cohort in 1994, 21% of the children moved away and about 3 percent died. In our actual analysis, we lose another fifth of the observations, as our identification requires at least 2 observations per household. Missing height measurements of children or of relevant siblings adds further to attrition. However, there seems no evidence that this attrition post-1994 is systematically affecting our results. In particular, we estimated a probit on remaining in the sample using the same correlates as in table 7. We found a significant difference in the probability of being in the sample between cohorts, with those who were younger predictably being more likely to be in the sample, and females less likely to be observed. Crucially, however, for the famine interactions, there were no significant differences either individually or jointly (p=0.35); those affected were not more or less likely to be observed in the sample. We conclude that despite significant attrition, there is nothing systematic that would bias our analysis.

All these long-term impact estimates are net of coping strategies that households may have undertaken in order to mitigate the short-term impact of the shock on household consumption. They are also net of any food aid or other relief that may have taken place. We have some information on whether food aid was received during this period, as well as how the household responded to the crisis. In 1994, detailed data were collected recalling this traumatic period. Contrary to impressions created by the media during famines, few made it to feeding camps (under 5%). Food aid targeting is always difficult, as the general equilibrium effects of a famine mean that all face some problems, such as those linked to rising food prices. Unsurprisingly, in view of what happened, we find relatively imprecise targeting: during 1984-85, when food started being distributed, 41% of those affected by the drought shock obtained some food aid, compared to 25% of unaffected. Still, amounts received per capita were considerably higher for those reporting the shock compared to the others. Median receipts for both groups is zero, and a mean 97kg per affected household is relatively little for a whole year. Imperfect targeting of aid, and the scale of the crisis meant that households had to resort to costly coping strategies. Of those affected by the crisis, 91% cut back on meal sizes, compared to 65% of those not reportedly affected; 61% of those affected ate wild foods they would not normally eat (29% of those unaffected), and 48% sold valuable assets (24% of those unaffected).[21]

Is there any evidence that the food aid reduced the long-term impact of the famine on our cohorts? Non-random placement of famine relief makes this a difficult topic, so at most, we can only provide some indicative evidence. We interacted the cohort and cohort-famine shock variables with a dummy for receiving food aid (there is no separate food aid dummy as this is a household level variable, captured by the household fixed effect). None of the interaction effects were

rates in the sample were only around 20%; and below 10% for secondary education. Even so, these children would have been recorded as in the household.

[21] The relatively high percentages of those not reporting to be directly affected by drought but still using particular coping mechanism is a reflection of the likely general equilibrium effects, making our estimates of impact again more likely to be a lower bound.

significant, neither individually, nor jointly across the specific groups affected by the famine (p=0.67) or across the cohorts (p=0.41). Similarly, using amounts of food aid received, there were no significant interaction effects. We have to be cautious in interpreting these results: it could mean that food aid was irrelevant on average; for example, it was just too little to make any difference for those in those young children at a vulnerable age. By lack of an appropriate counterfactual, it could also mean that it was so well targeted that those receiving food aid would have been worse off than those not receiving it, and that now they have equal opportunities. In view of the evidence on targeting described above, and the key findings of this paper, this would seem rather unlikely. On the basis of our evidence, it would be hard to defend that this was a well-handled successful famine relief operation.

What are the implications of this loss in human capital for the affected cohort? As noted in the literature review, a number of studies have found a positive and significant relationship between height and earnings, in many country contexts. In table 11 we present some simple Mincerian regressions of total annual income, using a sample of household heads from the 2004 round of the ERHS (i.e. the full sample and a larger sample than the young adults we could trace back to the famine). We include village fixed-effects, gender of the head, age of the head and its square, schooling of the head and household size and composition variables.[22] We find that a 1 cm increase in height results in approximately 1% increase in annual income.

6 Conclusion

Nutrition in early childhood is a strong determinant of height at adulthood. Height at adulthood has been shown to be a strong determinant of earnings and other measures of 'success' in life. This paper contributes to a small body of economic evidence on long-term impacts of extreme events experienced in early childhood, by providing the first estimates of the impact of one of the biggest famines to have hit Africa, the case of Ethiopia in 1984.

The use of a family-specific famine shock allows us to distinguish this effect from a more general macro-effect affecting all in particular communities, while the within-household estimator ensures that any household-level heterogeneity correlated to famine exposure is not biasing our results. The effects are large, but could still even be only a lower bound. First, there could still be selection into the sample of probably stronger children, if mortality rates were higher for shorter children. While mortality is considerably determined by family and community

[22]Note that in the ERHS there are very few people who work for wages (approximately 350 with non-missing values), as most people are occupied on the family farm. Whilst this is therefore clearly an incomplete model of income generation, our aim is to provide some basic correlations of the relationship between height of the household head and income generated by the household as a whole. As a robustness check we also estimated the same equation for crop income, and the results were very similar. Table 10 shows descriptive statistics for the sample of household heads.

characteristics, and therefore controlled for by the household fixed effects regressor, within family effects could have led to relatively stronger children surviving. Second, the comparison group is other children who were alive at the time of a severe famine, despite their household not reporting it as the worst year– it is highly likely that every village in Ethiopia was affected by this to a greater or lesser extent, and if so this will make the difference between affected and non-affected children smaller. Third, we have to contend with some heaping of age, and the drought instrument is relatively 'blunt' in the sense that we cannot be more precise (e.g. to the month) about the length of the shock. These measurement issues cause attenuation bias in the estimates, making it less likely that we find any effects.

The results presented show that children who experienced but survived a large scale and severe nutritional shock at a critical period in their development are discernibly smaller than their peers when measured twenty years later. We find that those in the particularly vulnerable age of 12 to 36 months at the height of the famine were about 5 cm shorter due to the famine. We cannot reject that all those in utero and those below the age of 36 months were all similarly and significantly affected. These effects are substantial. The loss can be compared it to the findings summarised in Strauss and Thomas (2008) that developing countries gained an average of 1 cm in height per decade. However the famine impact in Ethiopia is also in line with findings from other serious famines. For example, the results on China quoted in Chen and Zhou (2007) suggest on average a height reduction of 3.03 cm due to the 1959–61 famine, with further effects on labour supply and earnings. We also find some tentative evidence that those vulnerable and affected at the time of the famine may be be more likely to be ill. Indicative calculations show that the observed height deficit could lead to reduced income of around 5% per annum. Our analysis also suggests that famine relief in the form of food aid did not appear to have been effective in reducing impacts on the most vulnerable children, despite massive aid efforts. This study thus adds to a body of knowledge on the long-term impact of severe shocks, and underlines the importance of swift nutritional interventions in complex emergencies, specifically targeting children who are at a critical stage in their development.

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A. Statistical Appendix

Table 1: Famine and Drought, by Village

Peasant association	Region	Drought affected	Drought in 83-5	Worst 84-5	Rain deficit 84-85
Haresaw	Tigray	1.00	1.00	0.77	.414
Geblen	Tigray	1.00	1.00	0.15	-.024
Dinki	Shewa	1.00	1.00	1.00	.032
Yetemen	Gojjam	0.59	0.41	0.41	-.012
Shumsha	Wollo	0.93	0.71	0.64	.289
Adele Keke	Harerghe	0.68	0.54	0.46	.476
Korodegaga	Arssi	1.00	1.00	0.78	.255
Trurufe Ketchema	Shewa	0.87	0.76	0.65	.206
Imdibir	Shewa	0.83	0.83	0.83	.227
Aze Deboa	Shewa	1.00	1.00	1.00	.193
Adado	Sidamo	0.00	0.00	0.00	.335
Gara Godo	Sidamo	1.00	1.00	0.76	.354
Do'oma	Gama Gofa	1.00	0.86	0.61	.176
Debre Berhan-Milki	Shewa	1.00	1.00	1.00	.085
D.B. -Kormargefia	Shewa	0.25	0.25	0.13	.085
D.B. -Karafino	Shewa	0.20	0.20	0.20	.085
D.B. -Bokafia	Shewa	0.00	0.00	0.00	.085

Notes: Sample is the same as in descriptive statistics table below. 1) Specifically, responded 'drought' to the question "In the last 20 years has the household suffered a substantial loss of harvest through any of the following [list of potential crises]?" 2) Households were asked to list the three worst crises, this entry is positive if the household responds EC75, EC76 or EC77 (1983–85) 3) In the list from (2), household ranks EC77 (1984–85) as the worst crisis. 4) Rainfall deficit in 1984-85 compared to the average from 1978-87 (excluding 84-85). A positive number indicates a deficit.

The main data source for the ERHS rainfall is data received from Ethiopian Meteorological Agency, first procured from 1967 to 2000 by World Bank (Luc Christiensen and Stefan Dercon), updated to 2004 by Catherine Porter. We supplement missing data using two other sources: FAO database at `http://geonetwork3.fao.org/climpag/agroclimdb_en.php` and National Climatic Data Center (NCDC)-Global Historical Climatology Network (GHCN) Monthly data (received as stata dataset from Andreas Georgis, University of Oxford). The relevant average is the decade around the birth of the cohort children 1978-1988 (excluding 84 and 85). We compute the rainfall in 84-85 as a proportion of the decade around it. In robustness checks we also compute the total for the years only prior to 1984 when we use the older cohort.
Villages which have imputed data: Haresaw: Mekele (15K) FAO data before 1988. Aze Deboa: Mar, May, Jun 84 missing. We compute the long term average for all years based on the 9 non-missing months for 1984. Debre Birhan: 1980-83 missing. We impute using the percentage change in the next nearest station (Alem Ketema (52Km away and 1000m lower in altitude), using the assumption that whilst levels of rainfall are quite different, the deviations year by year are correlated. Gara Godo: Use FAO data Sodo (31KM). 1983 is missing and there is no rainfall data available on any station within 100KM. Therefore we calculate averages excluding this year. Do'oma: Imputed as for Debre Birhan 1982-85. Do'oma is a resettlement village. We exclude it in some of the robustness checks.

Table 2: Dates of famine and cohort ages

Dates	**Famine related events**	**Cohort birth and age in famine**	**Age 2004 survey**
Sept 78 - Sept 80	Relatively normal harvest	"Born before Famine" (Oldest cohort aged 37-80 months in famine)	23–27
Sept 80 - Sept 81	Relatively normal harvest		
Sept 81 - Sept 82	Bumper rains and harvests in many places during this year	"Just before famine" Age 12-36 months in famine	21–22
Sept 82 - Sept 83	Drought, crop failure, war spread in the north		
Sept 83 - Sept 84	Peak of the famine continues through two years- widespread hunger, death.	"Born during Famine" Born and in-utero during severe shock period	19–20
Sept 84 - Sept 85	Famine peaks in many areas		
Sept 85 - Sept 86	Normal Meher rains (Aug/Sep 86) marked the end of drought	"Born after Famine" (Youngest cohort)	17–18
Sept 86 - Sept 87	Normal year (but some problems in certain villages this year)		

Notes: Dates run from September to September to match the Ethiopian calendar which begins on 11th September. Information on dates is discussed and referenced in the main text.

Table 3: Height by cohort and famine shock

Cohort	**Unaffected**	**Affected**	**T-test diff p-value**	**N**
Youngest group (Born 1986–1987)	155.074	156.1291	0.51 (0.60)	176
In utero-12months in famine (Born 1984–1985)	160.156	159.347	0.49 (0.62)	144
Aged 13-36 months in famine (Born 1982–1983)	163.534	159.643	1.57 (0.11)	110
Aged 36 months + in famine (Born 1976-1981)	163.800	164.404	0.42 (0.67)	162

Notes: Sample is children aged 17–27 years old in 2004, definition of cohorts in table 2, definition of drought shock in table 1. P-value of two-sided t-test in brackets.

Table 4: Summary statistics:Household Level

Variable	**N**	**Mean**	**Std. Dev.**	**Min**	**Max**
Variable	Obs	Mean	Std. Dev.	Min	Max
Household drought shock	985	.544	.498	0	1
Age of household head	985	45.975	15.184	15.875	101
Household head female	985	.229	.421	0	1
Height household head	985	164.726	8.637	80	186
Household head went to school	985	.256	.437	0	1
Have a child born 1986-1987	985	.190	.392	0	1
Have a child born 1984-1985	985	.157	.364	0	1
Have a child born 1982-1983	985	.117	.321	0	1
Have a child born before 1982	985	.461	.499	0	1
Rainfall dev. 1984-85 from 10 year mean	985	.215	.149	-.024	.476
No. infants died who would be in sample	985	.394	.838	0	6
No. infants died in famine	985	.082	.293	0	2

Notes: Sample is household level. Definition of cohorts in table 2, definition of drought shock in table 1.

Table 5: Determinants of self-reported drought shock

	villfe	drght	drght2	drght3
	(1)	(2)	(3)	(4)
Rainfall dev. 1984-85 from 10 year mean		.627	.462	.484
		(.118)***	(.121)***	(.121)***
Age of hh head	.002	.0006	.0005	.0004
	(.001)	(.001)	(.001)	(.001)
Household head female	-.278	-.591	-.619	-.617
	(.824)	(.490)	(.452)	(.456)
Height of HH head	-.002	-.006	-.003	-.003
	(.003)	(.003)**	(.003)	(.003)
Head height*female head	.002	.004	.005	.005
	(.006)	(.006)	(.006)	(.006)
Mean height in village			-.050	-.049
			(.009)***	(.009)***
Have a child born 1986-1987	.048	.002	.004	.006
	(.045)	(.043)	(.044)	(.044)
Have a child born 1984-1985	.017	-.020	-.027	-.019
	(.048)	(.047)	(.047)	(.047)
Have a child born 1982-1983	.046	.038	.037	.032
	(.055)	(.053)	(.054)	(.054)
Have a child before 1982	.020	.031	.029	.038
	(.038)	(.036)	(.037)	(.036)
No. infants died who would be in sample		.035	.039	
		(.022)	(.021)*	
No. infants died in famine				.024
				(.056)
Obs.	985	985	985	985

Notes: Probit estimates. Dependent variable=1 if household self-reports 1984 as drought "worst year" (see table 1 for full definition). All coefficients reported as marginal effects, robust standard errors in brackets. *significant at 90%, ** significant at 95%, *** significant at 99%. Column (1) and (3) include village fixed effects. Included but not reported in column (2, 4 and 5) are average and variance of village rainfall and distance to town.

Table 6: Summary statistics: Child Level

Variable	Mean	Std. Dev.	Min	Max	N
Height (CM) 2004	159.83	11.87	47	186	550
BMI 2004	19.15	2.59	12.35	27.87	550
Any illness or injury?	0.09	0.29	0	1	544
Any schooling	0.73	0.45	0	1	442
Female	0.41	0.49	0	1	550
Birth Order	2.98	1.40	1	5	550
Born 1986-1987	0.31	0.46	0	1	550
Born 1984-1985	0.25	0.43	0	1	550
Born 1982-1983	0.18	0.39	0	1	550
Born before 1982	0.26	0.44	0	1	550
Drought shock	0.59	0.49	0	1	550
Rainfall deviation 1984-85	0.23	0.14	-0.02	0.48	550
Age of hh head	47.48	11.10	25	80	550
Household head female	0.16	0.37	0	1	550
Height of HH head	165.91	8.84	80	186	550
Head ever attend school	0.21	0.41	0	1	547
Number died who would be in sample	0.49	0.89	0	6	547
Number died in famine	0.07	0.26	0	2	547

Notes: Sample is children of the household head aged 17–27 years old in 2004, definition of cohorts in table 2, definition of drought shock in table 1.

Table 7: Drought Impact on 2004 Height: OLS and Fixed-Effect estimates

	villfe	sibfe	olderols	olderfe
	(1)	(2)	(3)	(4)
Drought shock	2.074		1.592	
	(1.492)		(1.426)	
Drought*Born 1986-1987	-.429	-3.811		
	(3.091)	(3.026)		
Drought*Born 1984-1985	-2.454	-2.002	-2.949	-4.167
	(2.336)	(2.892)	(2.193)	(3.188)
Drought*Born 1982-1983	-5.272	-6.287	-5.990	-5.707
	(2.640)**	(2.672)**	(2.547)**	(2.657)**
Born 1986-1987	-7.377	-7.490		
	(2.444)***	(3.092)**		
Born 1984-1985	-2.146	-4.957	-2.321	-5.668
	(1.774)	(2.140)**	(1.695)	(2.532)**
Born 1982-1983	-.643	1.950	-.059	.488
	(2.187)	(1.898)	(2.261)	(1.997)
Age of hh head	.067		-.001	
	(.046)		(.047)	
Household head female	.918		.838	
	(1.743)		(1.993)	
Height of HH head	.187		.141	
	(.097)*		(.103)	
Obs.	550	369	379	199

First column includes village fixed effects. Second restricts the sample to only those with siblings. Third and fourth columns repeat the analysis dropping the youngest cohort. Omitted cohort is the oldest, born 1978-81.

Table 8: IV estimates of height - older cohorts

	ols8485	iv8485	ols8283	iv8283	ols7881	iv7881
	(1)	(2)	(3)	(4)	(5)	(6)
Drought shock	-.938	-1.307	-4.426	-8.017	1.443	2.991
	(1.633)	(5.219)	(2.621)*	(4.689)*	(1.425)	(6.542)
female	-9.360	-9.373	-6.896	-7.102	-7.740	-7.855
	(1.510)***	(1.406)***	(2.286)***	(2.057)***	(1.435)***	(1.476)***
Age of hh head	.014	.013	-.047	-.050	.020	.025
	(.076)	(.077)	(.092)	(.092)	(.064)	(.065)
Household head female	-2.384	-2.337	2.315	2.377	3.702	3.537
	(2.392)	(2.315)	(4.126)	(3.837)	(1.824)**	(1.972)*
Height of HH head	.0003	.001	.387	.367	.260	.270
	(.099)	(.101)	(.193)**	(.196)*	(.103)**	(.106)**
Kleibergen-Paap F-statistic		14.64		19.92		1.94
Obs.	136	136	100	100	143	143

Notes: robust standard errors in brackets. * significant at 90%, ** significant at 95%, *** significant at 99%. Omitted group is those born before 1982. Columns (1, 3, 5) are OLS, for cohorts born in 84-85, 82-83 and pre-1982 (ie from youngest to oldest). Column (2,4,6) are IV Fuller(1) estimates on the same samples.

Table 9: IV estimates : First stage regressions

	born8485	born8283	born7881
	(1)	(2)	(3)
Rainfall dev. 1984-85 from 10 year mean	1.208	1.795	.582
	(.286)***	(.317)***	(.308)*
female	.0004	.040	.098
	(.075)	(.086)	(.081)
Age of hh head	-.004	-.007	-.006
	(.004)	(.005)	(.004)
Household head female	-.036	.0006	-.030
	(.116)	(.147)	(.117)
Height of HH head	.002	-.003	-.007
	(.004)	(.007)	(.006)
Obs.	160	117	169

Notes: robust standard errors in brackets. * significant at 90%, ** significant at 95%, *** significant at 99%. Dependent variable is household self-reported drought shock. Birth order also included but not reported.

Table 10: Descriptive statistics: Household heads, 2004

Variable	Mean	Std. Dev.	N
Height in centimetres	163.83	9.281	1144
Gender (female=2)	1.295	0.456	1144
Age in years	50.557	15.121	1144
Highest school grade attained	3.877	6.283	1144
Dummy: attended school up to primary	0.141	0.348	1144
Dummy: finished primary school (or above)	0.252	0.434	1144
Household Size	5.746	2.534	1144
Female children 5-15	0.921	0.989	1144
Female children under 5	0.339	0.563	1144
Male children under 5	0.32	0.567	1144
Male children 5-15	0.92	0.998	1144
Female elderly	0.167	0.405	1144
Male elderly	0.199	0.49	1144
Log total annual income	7.513	0.974	1144

Descriptive statistics for sample of household heads in 2004 used for the regressions of household income on human capital of household heads.

Table 11: Human capital regressions, 2004

	Income1	Income2
	(1)	(2)
Height, centimetres	.009	.010
	(.003)***	(.003)***
Gender (female=2)	-.162	-.176
	(.074)**	(.075)**
Age, years	-.002	-.006
	(.009)	(.009)
Square of age, years	.00006	.00008
	(.00009)	(.00009)
Highest school grade attained	.075	
	(.019)***	
School squared	-.004	
	(.001)***	
Any school but not complete primary		.078
		(.075)
Completed primary school		.171
		(.059)***
Obs.	1144	1144

Notes: *** Significant at 1%, ** Significant at 5%, * Significant at 10%. Both regressions OLS with village fixed-effects. Dependent variable in both columns is log of household income in 2004.