

Can the Major Public Works Policy Buffer Negative Shocks in Early Childhood?

Evidence from Andhra Pradesh, India

Aparajita Dasgupta



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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 Andhra Pradesh), Peru and Vietnam over a 15-year period.

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Young Lives, Oxford Department of International Development (ODID), University of Oxford,
Queen Elizabeth House, 3 Mansfield Road, Oxford OX1 3TB, UK
Tel: +44 (0)1865 281751 • E-mail: younglives@younglives.org.uk

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Abstract

The study examines the role of the largest public works program in the world—the National Rural Employment Guarantee Scheme (NREGS) - in buffering the negative effects of early childhood exposure to rainfall shocks on long-term health outcomes. Exploiting the spatial and temporal variation in NREGS coverage, the study estimates the extent to which nutritional shocks in early childhood can be offset by access to the policy. The study employs a unique identification strategy by integrating detailed administrative records of drought shock and phase-wise roll-out information of NREGS with a household level panel data—the Young Lives survey—conducted over three waves (2002, 2007 and 2009-10) in the state of Andhra Pradesh, India. Using individual fixed effects estimation the study finds that while the policy does not help correct for long term past health deficiencies it is useful in buffering recent drought shocks, which varies by policy relevant sub-groups.

JEL Classification: I18, J13, O22

Keywords: Child malnutrition, Drought, Height-for-age, NREGS, Safety net

Author

Aparajita Dasgupta is the Fred.H.Bixby Postdoctoral Fellow at the Population Council. She completed PHD in Economics from University of California Riverside. She can be reached at aparajita.dasgupta@email.ucr.edu.

1. Introduction

Exposure to negative shocks in early childhood is known to significantly affect the health and educational outcomes of population, more so in developing countries ([Currie et al. 2012](#)). Increased climatic variability over time poses special challenges for child nutrition especially among subsistence farmers depending on rain-fed agriculture. Additionally, there is no operational practice to forecast drought ([Gore et al. 2010](#)) where such an event may often lead to adverse outcomes such as loss of land rights against debt and declining nutrition levels for the poor majority of population. With a large proportion of households depending on agriculture -a highly volatile source of subsistence- the effects may be worse for the rural poor who often lack access to formal credit markets to smooth consumption.

In such a setup, rainfall shocks can lead to substantial reduction in household income, which can significantly reduce investments in children often compromising their calorie intake. The poor households often resort to sub-optimal coping mechanisms like taking children out of school or deferring healthcare in response to such temporary shocks ([Subbarao et al. 2012](#)). This is a serious concern as the investments in early childhood can have significant impacts on human capital attainment and achievements as adults ([Hoddinott and Kinsey \(2001\)](#); [Maccini and Yang \(2009\)](#)). While the long term consequences of malnutrition during childhood are well established in the literature, little is known about the extent to which individuals are able to mitigate these deficits using social protection programs.

Employment generating programs are expected to support vulnerable households assuring nutrition security during economic downturns. A recent comprehensive [World Bank](#)

[review \(2012\)](#) of public-works program across countries highlights the dearth of systematic evidence on effectiveness of public-works programs in serving as safety nets despite their rapid adoption in diverse country settings. In the context of the major public-works policy in India, few studies have focused on its labor market impacts and self-targeting mechanism as opposed to examining its role in social protection. In this study we systematically examine the causal impact of the National Rural Employment Guarantee Scheme (NREGS) in mitigating effects of negative rainfall shocks on children's long-term health outcome taking evidence from rural Andhra Pradesh, India.

This study integrates a rich panel data from the Young Lives Survey following children across a span of eight years- with detailed administrative records of rainfall shocks and roll out information of the program access. The identification strategy is to compare the trajectory of the height for age scores of children with and without program access interacted with drought shocks between year 2002 and 2010. Since the policy is first targeted to the poorer districts and also involves voluntary participation from households, the potential selection bias in estimates is addressed by including individual fixed effects which account for time invariant unobservable individual heterogeneities. Additionally a host of time varying features that might have an independent effect on the outcome of interest have been controlled for. The estimates indicate that while exposure to drought in past year significantly reduced height-for-age by 0.4 standard deviations, access to the program was able to mitigate the negative impact of drought, as reflected by an increase of 0.26 standard deviations in height-for-age. However the program was not found to be able to correct for long-term past deficiencies (negative impacts incurred by exposure to cumulative drought shocks from the birth year).

This paper contributes to the existing literature on a number of aspects. First, integrating detailed data on weather shocks and policy coverage with the panel data, the study is one of the first to examine the causal impacts of a social protection policy to correct for past deficiencies relevant to child health in the long-run. While there exists a body of literature on the effects of early childhood shocks on human capital outcomes, the issue of how the effect can be mitigated under a public intervention is relatively understudied. Also one requires sufficiently integrated data sets in order to examine this question. Exploiting the anthropometric measures of the child at different stages of life enables to control for the inherent healthiness and comment on the catch up growth. Furthermore, unlike past studies, I collected¹ and used very detailed information of rainfall shocks, program availability, and community level measures of health-infrastructure at the mandal-level (sub-district level). This enabled me to control for a host of factors that influence child health independently, thereby substantially accounting for the selection problem arising from inherent unobservable differences in families who decide to participate in the program. Second, while the existing literature for developing countries mostly focused on a rather extreme health outcome - child mortality, this study was able to focus on malnutrition/child stunting² among survivors. Third, limited attention in the development literature has been given to examine the effectiveness of the huge public-works program in India. The current set up exploits both the timing and intensity of the program- in finding the causal

¹ Compiled the mandal-level information of rainfall and health facility over time from various years of Handbook of Statistics for each sample district in Andhra Pradesh by visiting the Directorate of Economics and Statistics, Government of Andhra Pradesh in Hyderabad.

² Stunting (height for age less than -2 standard deviations) is severely high in developing countries including India - having the highest number of stunted children below the age of 5 in the world ([Unicef 2009](#)).

impact of the program in remediating negative effects of shocks on child growth. Finally, it also examines the differential impact of the mitigation across the demographic features of the child: by age, gender, caste and caregiver's education, which brings out the vulnerability by demographic subgroups, again crucial for policy insights for program design.

The following section discusses the background and implementation of the NREGS in India. This is followed by an outline of the conceptual framework in section 3 that highlights how long-term health evolves under shocks and its scope of mitigation under social protection policy. In section 4 the empirical specification is laid out for the study. The datasets and the relevant descriptive statistics are discussed in section 5. Section 6 presents the main empirical results followed by a brief discussion of the policy insights.

2. The Program: National Rural Employment Guarantee Scheme (NREGS)³

The National Rural Employment Guarantee Scheme (NREGS), which is now the largest public works program in the world ([Azam et al. 2012](#)), came into force in February 2006 under the legislative framework of the National Rural Employment Guarantee Act (2005). By 2010, the National Rural Employment Guarantee Act (NREGA) reached 52 million households across the country. The scheme provided a legal guarantee for 100 days of employment in every financial year to adult members of any rural household willing to do unskilled manual work at the statutory minimum wage of Rs.120⁴ (US\$2.64) per day in 2009 prices. Under this act employment is required to be given within fifteen days of application for work, if it is not then

³ NREGS is now known as MGNREGS (Mahatma Gandhi National Rural Employment Guarantee Scheme).

⁴ In comparison, farm wage typically hovers around of 100-150 rupees depending on agricultural season.

daily unemployment allowance has to be paid (GOI, 2008). Wages are required to be disbursed generally on a weekly basis but it cannot be beyond a fortnight⁵ after the work has taken place. During the financial year 2010–11 Andhra Pradesh provided 274.8 million person days of employment (Galab et al. 2011). The idea is to encourage self-selected participation from those individuals who need it most. Thus one can expect it to act as a safety-net for rural farmers hit by an idiosyncratic shock. What the study does is precisely attempt this question whether the program enables households in safeguarding child nutrition during periods of drought. Several features of the program relevant for the empirical strategy in the study are discussed in the following section.

2.1 Public-works as a safety net

NREGS was introduced in India with an aim of improving the purchasing power of the rural people, primarily providing semi-skilled or unskilled work to people living in rural India, whether or not they were below the poverty line. The purpose of this scheme was to create strong social safety net for the vulnerable groups, increase female labor-force participation, create durable and productive assets⁶ in rural areas that encourage sustainable development and reduce rural-urban migration. The evaluation report from Ministry of Rural Development (2011) finds the policy resulted in reduction in the distress-migration of labor and a rise in expenditure on food and non-food items, which again can strongly influence child's catch up growth.

⁵ Although according to the PACS-CSO survey(2007) , the majority of workers received their wages within 30 days for the aggregate sample of Indian state.

⁶ World Bank report (2011) mentions the policy has only been successful in generating employment but not so in terms of asset creation.

[Berg et al. \(2012\)](#) found the program access boosted the real daily agricultural wage rates by 3.13 per cent with a lag of 6 to 11 months. [Uppal \(2009\)](#) reports positively about the self-targeting mechanism under the NREGS and notes that poorer and schedule caste households were more likely to register for this work which had significantly reduced the likelihood of children in the household being required to work. [Dutta et al.\(2012\)](#) mentions that although NREGS reached the rural poor, backward castes, and encouraged women into the workforce, its targeting performance varied by state. There is evidence that poorer states were unable to meet with the demand for job under this program thereby limiting availability of the scheme where it could benefit the most. However recent evidence ([Subbarao et al.2012](#)) mentions Andhra Pradesh as one of the forerunners in digitizing all the records of transactions across multiple sites and levels of the program and the only state in India to have institutionalized social audits to promote effective program monitoring. [Afridi et al.\(2012\)](#) finds that an increase in mother's share of NREGS workdays raises the educational outcomes of children, particularly girls.

2.2 Gender-sensitive component of NREGS

The scheme promotes women's participation in the labor force through a one-third quota for women in each state and also guarantees equal wages to both men and women workers. According to the official records for NREGS, the share of women workers was found to be greater in Andhra Pradesh than nationally in 2011 (National average share for women being 50.1 %, while in Andhra Pradesh it is 57.5 %). In order to encourage participation of mothers with

very young children, the program made the presence of child care facilities mandatory⁷ at all sites where more than five children under the age of six were present. However about 24% of the sample who did not participate in the scheme mentioned absence of child care centre being the reason, in the third round of the survey when the program has been already universalized. Recent evidence (Khera et al. 2009) also indicates the lack of child care centre in work-site as one of the significant deterrents for participation of women.

Since the prospects are typically worse for women in private casual wage work in India the provision of equal wages should have positive impacts on female participation. As argued by Imbert et al. (2011) NREGS has a sharper impact on female labor force participation⁸ than that of males. Azam et al. (2011) found wages for female casual laborers increased by 8 percent in participating districts as compared to nonparticipating districts. Zimmerman (2012) finds NREGS led to a substantial increase in the private-sector casual wage for women, the effects being concentrated in the main agricultural season. Hence if we believe on average women's participation in labor market has increased because of the program this has important associations⁹ for child wellbeing.

⁷ In spite of this provision the program has only 8.74% of registered respondents reported the availability of on- site child-care center in the second round (Galab, 2008).

⁸ Khera et al. (2009) points that NREGA wages implied a substantial jump in the earning potential for women at the national level.

⁹ Women's independent income benefit household nutrition and child health, both through increase in household income as well as through an increase in women's status, autonomy and decision-making power specially those relating to nutrition, immunization and feeding practices (Smith, 2001).

2.3 Implementation of NREGS

The Government has implemented NREGS in phase-wise manner making use of a ‘backwardness index’ developed by the Planning Commission -comprising of agricultural productivity per worker, agricultural wage rate, and Scheduled Caste/Scheduled Tribe population. For Andhra Pradesh the program roll out expansion¹⁰ across all its districts is shown in [Figure 1.1](#). Importantly for our identification strategy, four of the Young Lives sample districts (comprising of 11 mandal sites having 66% of the sample) were covered by the NREGS in the first phase of implementation in 2005-06 (Anantapur, Mahaboobnagar, Cuddapah, Karimnagar), with the addition of one more sample district -Srikakulam (comprising of 4 mandal sites) - in 2007, coinciding with second phase of implementation, and lastly the district of West Godavari (with two mandal sites)was included in 2008- coinciding with the third phase of the program expansion. Two out of six rural districts covered by Young Lives fell within the second and third phases, and in these two districts a large proportion of the Scheduled Tribe households were covered. The current study utilizes this variation in timing and intensity of the program across the mandals.

3. Conceptual Framework: Shocks, Child Vulnerability and Remediation

In order to discuss the potential impacts of the employment guarantee scheme on the anthropometric outcome of the child in a simple analytical framework, the underlying hypothesis examined in this study is that direct positive income from wages earned from public work can

¹⁰ Phase I included 13 districts in 2005, then to further 6 districts in 2007 and three more districts in 2008, to cover all 22 districts in the state.

feed into child investments in an otherwise situation of crises protecting the long-term health status. Stunting, or low height-for-age, is a measure of chronic malnutrition and is generally considered a long-term indicator for health status. Earlier studies have pointed that stunting might be permanent when nutritional deficits begin early and are prolonged.

[Hoddinott et al. \(2001\)](#) finds that droughts in rural Zimbabwe occurring between the ages of 0 and 12 months lead to significant reductions in child height when measured 12 months later. [Maccini et al. \(2009\)](#) finds a strong relationship between rainfall in the birth year and adults' health and socio-economic outcome for women but not for men in Indonesia. [Almond et al.\(2011\)](#) points that even relatively mild prenatal exposure can result in lower birth weight, which can have persistent effects.

Despite the prevailing view that height deficits are hard to correct for after the first two years of a child's life, catch-up growth has in fact been documented in several studies from developing countries until the age of 12 years([Cameron et al. 2005](#)). There is evidence that undernourished children from poor families who were adopted, by age five, into middle-class families reflected accelerated growth rates in adolescence ([Allen et al. 2001](#)). These results suggest that there is some potential for catch-up growth in children into the preadolescent years. Additionally the medical literature in this regard points that there exists biological potential for catch-up in response to clinical interventions, which is explored in some studies focusing on catch-up growth ([Deolalikar, 1996](#); [Fedorov et al, 2005](#); [Alderman et al, 2006](#); [Mani, 2012](#)). Although stunting might be permanent when nutritional deficits begin early, nutritional remediation can still take place as long as the critical period for growth remains open ([Outes et al. 2013](#)).

[Martorell et al. \(1994\)](#) surveys from medical literature and found evidence of catch up growth when living conditions were improved, especially for younger children. Few studies in this regard point the potential for early nutritional intervention in accelerating growth. [Schroeder et al. \(1995\)](#) find that nutritional supplementation has a significant impact on growth for kids under 3 year olds in Guatemala. [Yamano et al. \(2005\)](#) highlights in the context of rural Ethiopia- food aid can compensate the negative effects of early shocks, but that inflexible targeting, endemic poverty and low maternal education often keep stunting at high levels despite such interventions. In Mexico, [de Janvry et al., \(2006\)](#) found that conditional cash transfer protects education, particularly that of girls, and thus fosters the formation of human capital, offsetting shocks such as parental unemployment or illness. [Duflo \(2003\)](#) provides some suggestive evidence that the old-age pension had very different effects on child health¹¹ depending on whether it was received by a woman or by a man. For the case of public works component of Ethiopia's Productive Safety Net Programme (PSNP) [Gilligan et al. \(2009\)](#) found modest but positive effects on food security (improved by 0.40 months) and livestock holdings.

In terms of the evidence base of social protection policies, a recent systematic review of [Hagen-Zanker, et al. \(2011\)](#) points out that there were significantly more studies available on cash transfers compared to employment guarantee programs, indicating further need for systematic evidence on the impacts of the latter. In the particular context a recent review ([Dercon \(2011\)](#)) discussing the impacts of social safety net indicated there is no evidence till date on the effectiveness of NREGS in safeguarding nutritional outcomes in rural poor households.

¹¹ The study found that for girls, living with an eligible household member was associated with an increase of 0.68 standard deviation in height for age.

In this context, it is very important to identify the extent to which individuals are able to compensate deficits in child nutrition and offset these negative effects when a social safety net is in place. Furthermore this exercise also calls for examining carefully how the mitigation varies by policy-relevant demographic subgroups.

4. Empirical Specification and Identification

The study examines whether access to the program is able to protect the households during shocks from the irreversible damage induced by the different sub-optimal coping mechanisms that worsen long term child outcomes. Utilizing the temporal and spatial variation on program roll out time we compare the child nutritional outcomes across mandals with and without the program interacted with the exogenous drought variable.

The main outcome variable in our analysis is height-for-age z-score¹² which is a standardized measure of health status and is a well-established long run indicator of individual health status especially among children (Martorell, 1999). It shows the height of the child relative to an international reference group of healthy children. Since height is a stock variable that reflects all past inputs into child health including the impact from past shocks along with the effect of child level unobservables, it gives a cumulative picture of the child's overall growth

¹² This analysis uses height-for-age z-score as an indicator of catch-up growth following the rationale pointed by Cameron et al.(2005). First, they note the correlation between baseline and follow-up height is dependent on the ratio of height standard deviations of the two measurements, which in contrast, z-scores are not subject to, as they already take into consideration reference groups of equal age and sex. The second justification is that demonstration of catch-up growth needs to be compared with growth in a control group, which z-score measurement fulfills but a single height measurement does not. Third, the authors note that by using z-score measurements, catch-up growth may be separated from correlations predicted by regression to the mean.

status. The study further uses average stunting¹³ percent in mandal as another outcome variable of interest to get an aggregate picture of program impact on the health outcome at the community level.

In estimating the effect of the employment guarantee scheme there can be a potential serious problem of selection that arises at two levels, first from the targeted roll-out of the program and second from the self-selection mechanism¹⁴ by which the scheme operates giving rise to potential econometric issues. The issue of self-selection cannot be simply done away by using administrative records of roll-out as the phases were also determined according to the backwardness index of the district. Hence, within a particular mandal if poorer households -with worse-off outcomes to begin with- self-select themselves into the scheme, then simple OLS regression estimates would likely be downward biased. In contrast if the more informed and well-connected households (among the poor ones) take advantage of the scheme first, then estimates without fixed effects might be biased upwards in measuring the impacts of the scheme.

Furthermore, investment decisions about the amount of inputs to use may depend on, among other things, the health endowment of the child. It might be that a weak child may attract more attention and inputs from parents in an attempt to ensure her survival. Additionally, the overall level and mix of inputs depends on the parental preferences for health, which if not controlled can result in biased estimates. Here, the fixed effects approach helps explore the

¹³ A child is considered to be stunted if the height-for-age is less than minus two standard deviations of the reference group. In Andhra Pradesh, according to National Family Health Survey (NFHS-3, 2006) prevalence of malnutrition among children (0-59 months) is very high (32.5% underweight 42.7 % stunted and 12.2% wasted).

¹⁴ Uppal (2009) finds that households hit by drought were 10.7% more likely to register for the NREGS than other households.

dynamics related to the persistence of shocks across individuals controlling unobserved heterogeneity between families that influences health outcomes like height.

Using the fixed effects in the estimation enables to tackle some of these concerns. Besides genetic factors, the fixed effects approach also neutralizes additive effects of other unobserved heterogeneity between families, like heterogeneity in terms of disadvantages associated with a location, family structure, traditions, values norms, habits, wealth and household practices that can influence height. However accounting for time varying characteristics across households is more challenging. By using individual-fixed effects estimation we reasonably reduce these individual-specific but time invariant unobservable heterogeneities.

Apart from the endogeneity concern at the individual level there is also another such potential concern at the geographic level. It is possible that mandals having better unobservable attributes (like local political connection, leadership quality) have better outcomes of interest and program coverage. For example, in case where there is rationing of employment on NREGS, work allotment can be related to the local political economy and social network. Further, it is possible that even in the absence of such rationing of employment, take up of work under this program is driven by the demand for work in the locality that may be picking up the work-ability of the households- which again may relate with their nutritional status. Some of these concerns are addressed by defining program access from the administrative records as opposed to self-reported participation. Further, recent evidence ([Dutta et al. 2012](#); [Afridi et al. 2012](#)) indicates that the program is typically driven by the supply of projects at the district and mandal level rather than demand and hence it is unlikely that the availability of the program is picking up the

ability to work or the nutritional status of the households. Furthermore, the interaction of the exogenous drought variable with the program access provides the most plausible source of exogeneity enabling us to causally interpret its impact when exposed to drought.

We model the determinants of child health (as reflected by height-for-age z-scores) status as follows:

$$(1) \quad H_{ijt} = \beta_1 \text{Drought}_{jt-1} + \beta_2 \text{Coverage}_{jt} + \beta_3 (\text{Drought}_{jt-1} * \text{Coverage}_{jt}) + \sum \beta_k X_{ijk t} + \alpha_i + \varepsilon_{ijkt}$$

where t=survey year (2002;2007,2009-10)

H_{ijt} is the height-for-age z-score of the i^{th} child from the j^{th} mandal measured in survey year t . α_i represents the individual fixed effects. Drought_{jt-1} ¹⁵ represents a measure of negative rainfall shock in mandal j in the year previous to the survey ($t-1$). Coverage is a measure of access to NREGS¹⁶ that also varies by mandal and time. X_k 's are time-varying regressors which include individual controls like age of the child in months and community level controls like health facilities. We saturate equation (1) with all the relevant controls which can change over time and have independent influence on health status like community health infrastructure. The time-invariant regressors like sex of the child, mother's schooling, ethnicity of the household get absorbed in the individual fixed effects specification.

While we do not focus on the independent impact of coverage on households, the key

¹⁵ The drought variable is defined in reference to the last monsoon season, which predates the timing of the program information; in other words, program is not contemporaneously defined with drought. The households in the second round were all interviewed in 2007 (Jan- May) and were merged with the administrative records of policy coverage lagged about 6 months and drought records of the last financial year.

¹⁶ We primarily identify coverage from administrative records rather than self-reported measures of participation, hence the analysis is primarily based on the treatment that the households were intended to receive with few follow up robustness checks using actual participation information.

parameter of interest in our analysis is that on the interaction term (β_3) which permits us to analyze the effectiveness of the program in buffering households who were exposed to the drought shocks, for whom we expect it to be all the more beneficial. Precisely, a positive and statistically significant β_3 would indicate that the negative effect of drought exposure on child health status is mitigated by the policy access.

In order to minimize the potential concern of unobserved geographical heterogeneity (for example mandals with better political connectivity may be in a position to generate more work) we include mandal-fixed effects specification in equation (2). Here the outcome variable is average stunting percent (S_{jt}) at the mandal(j) level in year t. μ_j represents mandal fixed effects.

$$(2) \quad S_{jt} = \delta_1 \text{Drought}_{jt-1} + \delta_2 \text{Coverage}_{jt} + \delta_3 (\text{Drought}_{jt-1} * \text{Coverage}_{jt}) + \sum \delta_k X_{jkt} + \mu_j + \varepsilon_{jkt}$$

The standard errors are clustered at the mandal level to control for intra-mandal correlations. Clustering is done at the mandal level because the variation for drought and program exposure varies at this level. In order to tackle the problem of low numbers of cluster units the robust boot strapped¹⁷ standard errors clustered at the level of mandal (treatment level) were used.

While there is agreement that the make-up of health is highest in early childhood, estimates of mitigation can differ widely by a number of factors, such as severity, duration of the shock exposure, stage of development of the child at the start of malnutrition, gender of the child, household level demographics like education of the mother/caregiver, caste of the household. Thus in order to find heterogeneous impact we further estimate equation (1)

¹⁷ This method aims at improving inference in cases with a small number of clusters as proposed by Cameron et al. (2008).

separately by policy relevant sub-groups.

5. Data and Descriptive Statistics

The current study uses a household panel data set: Young Lives Survey from Andhra Pradesh, India- which is a longitudinal data set collected through household surveys conducted over three waves (2002, 2007 and 2009-10). For our study we use the longitudinal information of children who were aged 6 to 18 months in year 2002. The sample comprises of 20 sub-districts or mandals, the unit of variation in treatment for the current study. The sampling strategy was based on randomly selecting 150 children within 20 clusters or mandals spread across Andhra Pradesh¹⁸. The sample consisted of 7 districts (including 103 villages) from the state to represent the different regions¹⁹ and income levels within the state. Overall attrition by the third round was 2.2%²⁰ (with attrition rate of 2.3 per cent for the younger cohort) over the eight-year period.

For identifying the variation in access and intensity of NREGS we primarily use the detailed administrative records at the mandal (month-wise mandal-wise records of the average number of days of employment provided, fraction of years the program has been running in an administrative division etc.). We define ‘Coverage’ variable which measures the average number of work days under NREGS per household for a particular mandal in the last financial year. We also have self-reported measures for participation in the program at the household level and on whether the household had a job card under the scheme. We use a second definition of coverage-

¹⁸ Andhra Pradesh is divided into 23 administrative districts that are further subdivided into 1,125 mandals and 27,000 villages.

¹⁹ Andhra Pradesh has three distinct agro-climatic regions: Coastal Andhra, Rayalseema and Telangana. The sampling scheme adopted for Young Lives was designed to identify inter-regional variations with a uniform distribution of sample districts across the three regions to ensure full representation.

²⁰ Attrition in the Young Lives sample is low in the international comparison with other longitudinal study (Outes and Dercon, 2008)

to construct variable ‘NREGS’ which is the first definition but corrected for very low participation (obtained from the household survey data). We declare it to be zero where participation in a mandal was found to be less than 5 percent. This is our preferred measure as it not only captures participation information that are directly contingent to household needs it also implicitly captures the intensity of the program variable (For instance for how long the program has been in place can matter if we believe that the program delivery has improved with time), as typically the average days available under the scheme have increased with time.

While there is no unique, universally-accepted measure of “deficient rainfall”, droughts in most contexts refer to drier-than-average rainfall conditions compared to the long term average of 50 years (IMD, 2002). The drought shock is defined as receiving lower rainfall than the corresponding long term average for a mandal²¹. For the state of Andhra Pradesh-where over 80 per cent of the population depends on agriculture even mild deviation from the expected rainfall during the months of June-September²² can have adverse impacts on the food grain production. Unlike some previous studies which identify drought shocks in YL sample by self-reported incidence (Dercon et. al 2011) or constructed from district-wise rainfall, this uses the disaggregated annual rainfall records at mandal level²³, which can be expected to have less measurement error.

Four of the Young Lives sample districts comprising of 11 sub-districts/mandal sites

²¹ We check our results with an alternative measure of drought capturing the fraction of years exposed to drought from birth year till the point in the survey. The estimates on the drought coefficient using the current definition will perhaps give the lower bound of the impact as we did not separate severe droughts, where one can expect the impact would have been even greater.

²² Three-quarters of rainfall is received by the country annually at this time (PACS, 2008).

²³ Obtained from the Directorate of Economics and Statistics, Government of Andhra Pradesh.

were covered by NREGS in the first phase of implementation in 2005-06, with the addition of four mandal sites in 2007, coinciding with second phase of implementation, and lastly two more mandal sites were included in 2008- coinciding with phase-III of the program expansion. So, essentially, as per our definition of program coverage, in round two of the survey only phase-I districts were ‘treated’ while both phase-II and III sites were not covered. By the third round, all the sample districts were covered, but there exists variation in the program intensity as number of employment days available by mandal was different, which we also include as a further source of variation in the program variable.

We restrict the sample to 4289 observations keeping households that are present in all the survey rounds with complete information on all control variables and exclude potential outlier cases with height-for-age z score beyond the $[-5, +5]$ range. Since, the employment guarantee policy is only relevant²⁴ for the rural sector we focus on rural sample comprising 17 mandals and use the urban sample for falsification test.

We include the following time varying observables that can be controlled- the exact age of the child in months at the time of interview, community health infrastructure²⁵ (Health Facilities) captured by the number of health care units (both government and private hospitals) present in the community (mandal-level). We also check whether inclusion of factors like access to external food supplement as captured by whether child has been a part of supplemental food

²⁴ It might have some indirect /spillover effects on the urban sample which we include as a falsification test.

²⁵ There exists variation in terms of health infrastructure across communities which might be related with health outcomes of child or approximating the health awareness factor in a community.

program in ICDS²⁶ centre/mid-day meal²⁷ that has independent influence on health status makes any difference to our findings.

While identifying drought at the mandal level from administrative records (rather than measuring drought exposure reported at the household-level), we have mitigated the reporting bias and some selection bias (from family-specific unobservables related with exposure variables) we have also introduced a source of measurement error and caused a potential attenuation bias in the estimates. Even though droughts are categorized as covariate shocks which simultaneously affect households over large geographical areas (in spite of the fact that we do have very disaggregated rainfall data at the mandal-level), they are unlikely to affect all households equally in a given community. Precisely the household-level impact of a drought will depend on the occupation type among household members, availability of alternative irrigation sources, availability of alternative livelihood, access to safety nets, etc.

For approximating household education we construct the variable ‘Primary’ measuring whether the caregiver has completed primary schooling. The ‘Food Supplement’ is a binary variable constructed from self-reported measures that takes value 1 if the child received food under the ICDS scheme between round 1 and round 2 or if the child availed mid-day meal scheme between round 2 and round 3 (i.e. when the kids in our sample were of school going age). However we do not primarily include this variable (it might be endogenous) in our main specifications but include it to see if the inclusion changes our result.

²⁶ Launched in year 1975, Integrated Child Development Scheme (ICDS) supplementary feeding is supposed to provide support to all children 0-6 years old for 300 days in a year (25 days a month).

²⁷ The Midday Meal Scheme is a school meal program in India universalized by 2002. Both of these food supplement programs were universalized across the country much ahead of the NREGS policy implementation and were not associated with the availability of the employment guarantee scheme in a mandal.

We show the descriptive statistics of the key variables in [Table 1](#) by phase-wise sites (phase II and III sites have been clubbed together as none of these received the program by the second round of the YL survey). We find that the anthropometric status of children – as measured by height-for-age – deteriorates between the time of birth and 5 years of age for all phases-wise locations ([Figure 1.2](#)). We have 66 % of our total rural sample from the phase I locations, which were the only ones to receive the program by the second round of the survey.²⁸

We present the mean height for age score by drought exposure and coverage access in [Table 2.1](#). We find that the mean height for age score is statistically different by exposure to drought among those individuals who did not have coverage (column 1). However the difference is not statistically significant by drought exposure for those who *had access* to coverage (column 2). On average we see the height-for-age z-scores declined sharply from round 1 to round 2 for all the locations ([Figure 1.2](#)). Now, this is more or less a typical behavior in height-for-age in the case of developing countries where the z-score declines in the first few years and then stabilizes. Noteworthy is the fact that we get some action in the trajectory even between round 2 and round 3, i.e., after the first five years of age of the child. In round 1 of the survey the mean height-for-age z-score in phase I mandals was -1.20 (statistically different from that in phase II and III mandals) which substantially went down to -1.84 in round 2 and improved to -1.81 in the third round. For phase-II²⁹, the mean height-for-age z score went down from -1.50 to -1.70, which

²⁸ The phase I mandals got access to coverage by April 2006, phase II mandals by April 2007, and Phase III mandals by April 2008.

²⁹ It should be noted that the urban locations from all the districts were dropped from the current analysis, however the calculation of backwardness index on the basis of which coverage was rolled out in a particular district included these locations. Thus, it is not surprising, in spite of being slightly higher in rank in the backwardness index as a district, for the remaining rural sample locations under phase II, the average height-for age was lower.

again went up to -1.66 in the third round. Noteworthy is the fact that compared with other two phases, for phase III sites, which although being higher on the development index, had worse outcomes to start with witnessed decline in mean height-for-age z score between *all* the three rounds (from -1.55 to -1.74 between the first two rounds and then to -1.84 in the third round). These location sites in phase III were the last ones to get the coverage.

Although the difference in mean height-for-age z scores between phase I and the rest of the sites was statistically different in round one of the survey, when we restrict the sample to those who suffered from drought in birth year this difference in mean z-scores between the phases is no longer statistically significant (Figure 1.3). Interestingly, when we split the phase I sample³⁰ by drought exposure in birth year (Figure 1.4) we find a very stark difference in mean height for age score between the two groups.

The average height for age z score of the children (aged around 1 year) was found to be around 0.4 standard deviations less for the children who were exposed to drought in the year of birth and this difference is statistically significant in the first round. Now this difference is not conditional on the program hence we cannot comment whether it would have been any different in the presence of the policy. However the height for age drops for both of these groups between the first two rounds and the difference between the outcomes of the two groups are no longer statistically significant from second round onwards. We try to examine if there is any difference based on participation of the program. Around 32% of the households in phase 1 report to have not participated in the program in round 2. We utilize this variation and compare the child outcomes of the participants with the non-participants (both being exposed to drought in their

³⁰ The other two phase sites faced drought in birth year, hence we just restrict the sample to phase I sites for analyzing this variation in Figure1.3.

birth year). So, for phase I locations we restrict the sample to those who were exposed to drought in birth year ([Figure 1.5](#)) and try to see if any difference in mean z score exists by participation in the program. We find on average, households that availed the program by the second round (i.e. already had the program for about one year) had higher average height-for-age score than those who did not participate. However the difference is not statistically significant.

In [Figure 1.6](#) we add the trajectory of mean height for age for sample of households who did not face drought in birth year in addition to the graph in [Figure 1.5](#). We find although difference in the average z score (between the sample exposed to drought in birth year versus those who were not) is statistically significant in round 1 it is not so in round 2. Further, the mean z-score for the unexposed group was almost the same as the score for individuals exposed to drought in birth year but who participated in the program.

[Figure 1.7](#), [Figure 1.8](#) and [Figure 1.9](#) captures the mean z scores by education of the caregiver, gender of the child and caste groups respectively. We find a statistical significant difference in means of the outcomes by education of the caregiver (where the height-for-age scores of children with caregiver's education below primary schooling was found to be always significantly lower compared to the reference group) and by caste groups (z scores of children from lower caste households were significantly lower for all the rounds) as expected from our intuition. We discuss the regression estimation results in the next section.

6. Discussion of the Findings

All regression specifications with height-for-age as outcome variable includes individual fixed effects, and regressions with average stunting percent at the mandal level include mandal

fixed effects. We use robust boot strapped standard errors clustered at the level of mandal (treatment level).

Table 2.2 shows the regression estimates of drought shock, program access and their interaction on Height-for-Age for individual-fixed effects specifications³¹. We find while exposure to drought significantly reduces the height-for-age by around 0.373 standard deviations, the significant and positive coefficient of the interaction term indicates that program serves as a significant buffer against these shocks. In order to interpret the magnitude of the effect, we find that for one standard deviation increase in program day increases the height-for-age z-score by around 0.264³² standard deviations for those who suffered from drought³³, thereby mitigating some of its negative impact.

Based on this estimation we plot the predicted marginal effect of drought exposure (capturing the interaction effects) on height for age score for each level of average number of days available under NREGS in Figure 1.10. We find that as the number of days increase under NREGS the marginal impact of drought exposure increases the height for age z score. This provides additional evidence that number of days matter in the buffering role of the program in the mitigating drought impacts.

We present the results of robustness check using the alternative definitions of the program variable in Table 2.3. All the four specifications include the individual fixed effects

³¹ For a falsification test we include the urban sample in specification (2) the idea being that the availability of the program in the rural will not have an impact on urban households. However we do not find any statistically significant impact of drought shock on urban and hence cannot check the buffering effect of program.

³² Since Coverage is the average number of days we find the magnitude by multiplying the interaction coefficient by the standard deviation of the NREGS days $(.012 * 22) = .264$ standard deviation increase in height for age .

³³ By not distinguishing the different degrees of severity in the drought measure perhaps the estimated impacts are biased downwards. Noteworthy is the fact that 34% of the drought in 2002 were severe droughts, and restricting to that definition would have yielded much higher negative impacts.

apart from the usual controls of age, health facilities along with the drought exposure, program variable and their corresponding interaction term. Specification (1) uses the average number of days provided (Coverage); specification (2) uses the coverage variable (NREGS) corrected for very low participation; specification (3) includes the coverage intensity (Program intensity) - measuring the fraction of years the mandal received the program; specification (4) includes self-reported participation variable (NREGA).

We find the interaction effect using the self-reported participation to be the highest among all of these. Precisely, a one standard deviation increase in program intensity leads to 0.33 standard deviations increase in height for age. From specification (4) we see participation in the program is associated with an increase in the z-score by 0.48 standard deviations for those who exposed to drought. However as stated earlier self-reported participation is more endogenous than the administrative coverage variable hence we mainly focus on the first three specifications.

In [Table 2.4](#) we carry out a similar exercise with the outcome variable of average stunting percentage at the mandal level. We include 'Coverage' in specification (1) and (3) and 'NREGS' in specification (2). We find that the average level of stunting increases by around 8% with exposure to drought. The result does not change and is robust to alternative measures used for defining coverage³⁴. We find that a one standard deviation increase in average program days leads to reduction in stunting by 6%³⁵ for locations exposed to drought. In specification (3) we estimate the same equation as that in specification (1) but for urban sample. Again, we do not find any significant impact of the drought or the program in the urban sample.

³⁴ We also check by including the intensity of the program captured by the fraction of years it has been in place (not reported here).

³⁵ Magnitude is obtained by multiplying the standard deviation with the interaction coefficient (0.003*22).

We now examine if the effects of mitigation vary by demographic characteristics of the household. In [Table 2.5](#) we simply disaggregate the regression results by gender to examine if there is asymmetric burden of shocks on female child. We do not find any significant difference in the estimates by gender.

In [Table 2.6](#) we examine the impacts by ethnic/caste groups, as one might expect that the backward caste households faces most of the brunt of shocks. While there is a greater negative impact of drought exposure (reduction in height for age by 0.38 standard deviations) for the backward caste children we find the availability of program is significant in serving as buffer for this group only.

In [Table 2.7](#) disaggregating the results by education level of the caregiver we find a strong significant negative impact of drought exposure on children when the caregiver's education level is below primary level (reduction in height for age by 0.36 standard deviations). The impact of drought although negative is not found to be statistically significant for those kids whose caregivers have finished the primary education. However we find significant mitigating impact of the program across both of these groups.

The findings highlight the extreme vulnerability faced by the rural poor households particularly by education level and ethnicity which further underscores the importance of social protection scheme for these households to counter the negative shocks. Also, when we include the food supplement variable we find a strong positive and significant impact of the food variable³⁶.

³⁶ The estimations including food supplements although shows positive and significant impacts were not reported here as it might be endogenous and also interact with the program variable. However inclusion of food supplement does not alter our main findings.

The coefficient of program variable across the specifications although statistically insignificant has a negative sign indicating the possibility of negative selection for participation in the program. It is quite plausible that people who lost jobs/ had a decline in household income joined the program. Also, notable is the fact that when we exclude the fixed effects the OLS results (not reported here) understates the impact of both drought and the mitigation. Although, we find the health facility variable to be positive and significant in the OLS specifications, we find it insignificant with the fixed effects. The estimated coefficient on 'Age' is always negative and significant across all specifications in rural sites signifying worsening of z-score with the age, which is often the case in developing countries ([Hoddinott 2011](#)) where the height for age score typically declines in the first three years and then stabilizes. In our result a one year increase in age decreases height-for-age z-scores by around 0.09 standard deviations in the fixed effects estimation.

While there is no significant difference of the program impact by the gender of the child there is significant difference by the caste and education level of the household members. Hence there is much room which the policy can address by working on ensuring food security issues of these households. Thus to conclude we find evidence that the program helps mitigate recent exposure to shocks, especially for the case of lower educated households and scheduled castes, who are presumably more vulnerable in the face of climatic variability.

Now, in order to examine whether the policy can help mitigate past shocks that has accumulated over the years which is particularly relevant for height measure (height being a stock variable reflecting all past inputs into child health including the impact from past shocks) we check the robustness of the current results defining drought in a cumulative manner

(specifically capturing the fraction of years the child was exposed to drought from birth till the point in survey).

[Table 2.8](#) and [Table 2.9](#) present the results for height-for age and average stunting level with the cumulative drought measure. Both the results confirm that cumulative drought has significantly strong negative effect on health outcomes. The interaction term of program and drought in [Table 2.8](#) although positive (suggestive of mitigation) is not statistically significant. This implies the program availability is not able to serve as a buffer for correcting past deficits that has been induced by cumulative drought exposures over the years. The result has crucial implications for insuring vulnerable rural poor households from unforeseen weather shocks given that negative effects of these shocks in early life prove to be irreversible even when a social safety net is available later on in life. It does only prove to be effective for buffering recent shocks. Hence, taking evidence from our findings it is important to note here that social safety nets available later on life cannot mitigate past deficiencies that carry forward later on life.

7. Policy Insights

To discuss our findings in the light of policy insights we find while there is long-run impact of early-life conditions on health several years later, access to coverage helps tackle only for recent shocks but not correct for longer-term past deficiencies. This reconfirms the fact that there is little scope of remediation of correcting past deficiencies which highlights the importance of insuring households against such unforeseen shocks. This has crucial implications in the light of the recent literature that reinstates earlier deficiencies in human capital is very likely to be transferred across generation. A recent study by [Hoddinott et al. \(2011\)](#) finds that

individuals who did not suffer growth failure in the first three years of life complete more schooling, score higher on tests of cognitive skill in adulthood, have better outcomes in the marriage market, earn higher wages and were more likely to be employed in higher-paying jobs. So, as we recognize the critical role of early life conditions prove to have influence on human capital outcomes in long run, there is much room that policies can address in this regard.

Firstly, it has important implications for program design -we find that an increase in 22 working days per household increases height-for-age by around 0.26 standard deviations for those always exposed to recent drought. In terms of commenting on how big is this effect, we can say it is quite substantial: bridging about half the rural-urban gap. Now given that Andhra Pradesh has been one of the better performers in implementation of this program one has to be careful in generalizing this result for other states. While the availability of this longitudinal data was critical to the measurement of program impact over time it would be a difficult to undertake the same exercise for other states due to lack of similar data. Combining with the recent evidence ([Dutta et al. 2012](#)) that highlights the extent of unmet demand in the poorer states were higher, special attention needs to be given on correcting that aspect. Noteworthy is the fact that this increase in height for age is quite significant given the children were all around five year old when the program came in place. The mitigation could have been perhaps higher had it come much early on their lives, given the importance of adequate resources that would guarantee nutrition in the very early stages of life.

Secondly, there needs to be special focus on correcting for the lack of child care centre at the worksites that can play a key role in encouraging women participation. This will also potentially help reduce the negative effects of reduced child care time of the mother/caregiver

foregone in her increased work time for the earned income. [Dreze et al.\(2007\)](#) mention that although the main stated objective of the NREGA is not tied to improving child nutrition, it can reduce childhood malnutrition in a much effective³⁷ manner through the convergence of nutritional programs with the provision of crèches³⁸. [World Bank report \(2011\)](#) indicates the program has failed in building assets valuable to the community often due to lack of community participation and absence of sensitization of women's concerns in the project design. All these findings call for correcting the loopholes in program implementation and a rigorous cost-benefit analysis to generate rigorous evidence on the proposed convergence of ICDS with the program. It would be important to consider the synergies between the various programs that can benefit the vulnerable households in this regard. Specifically, if participation in the program leads to reduced time for child care, its adverse impacts can be thus accounted for by having functional childcare facilities in the worksites.

³⁷ Allen et al. (2001) mentions combining and converging the services of improved infant feeding, better household access to food, and improved and more accessible sanitation would be a cost effective way in combating undernutrition, (where food, health and care are all problems) than any of these measures taken alone.

³⁸ Few studies including ([Khera et al.2009](#)) mention the lack of-almost non-existent- child-care facilities as one of the most important difficulties for women to participate especially those with breastfeeding babies

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Table 1 Descriptive Statistics

Variable	Phase I		Phase II and III	
	Mean	Std. Dev.	Mean	Std. Dev.
<i>Outcome Variables</i>				
Height-for-age	-1.62	1.24	-1.63	1.09
Average Stunting	0.38	0.12	0.36	0.10
<i>Measures for NREGS</i>				
Coverage (Average Days)	26.77	22.41	13.99	20.92
Participation Percent	0.44	0.33	0.22	0.31
NREGS	26.77	22.41	13.26	21.04
<i>Measures for Drought Shocks</i>				
Drought	0.56	0.50	0.67	0.47
Cumulated Drought	0.45	0.25	0.65	0.17
<i>Child Level Variables</i>				
Food Supplement	0.43	0.49	0.62	0.49
Age	4.82	2.88	4.84	2.90
Male	0.53	0.50	0.52	0.50
<i>Household Characteristics</i>				
Primary Education of Household Head	0.25	0.43	0.54	0.49
Caste	0.16	0.37	0.10	0.30
<i>Community Characteristics</i>				
Health Facilities	1.88	1.18	3.63	1.23
Observations N=4289	2831		1458	

Note:

(i) Coverage represents the average number of days available for the mandal in the last financial year, obtained from administrative records.

(ii) Participation percent is constructed from the self reported measure of participation in the program.

(iii) 'NREGS' represents the Coverage variable corrected for low participation using self-reports.

(iv) Drought is dummy of receiving less than the long term average rainfall at mandal in the year prior to survey.

(v) 'Cumulated Drought' is the fraction of years of having 'Drought' cumulated from birth year.

(vi) Food Supplement is dummy for whether child has been a part of supplemental food program in ICDS centre/mid-day meal

(vii) Health Facilities are the number of health care units (both government and private hospitals) present in the community (mandal-level)

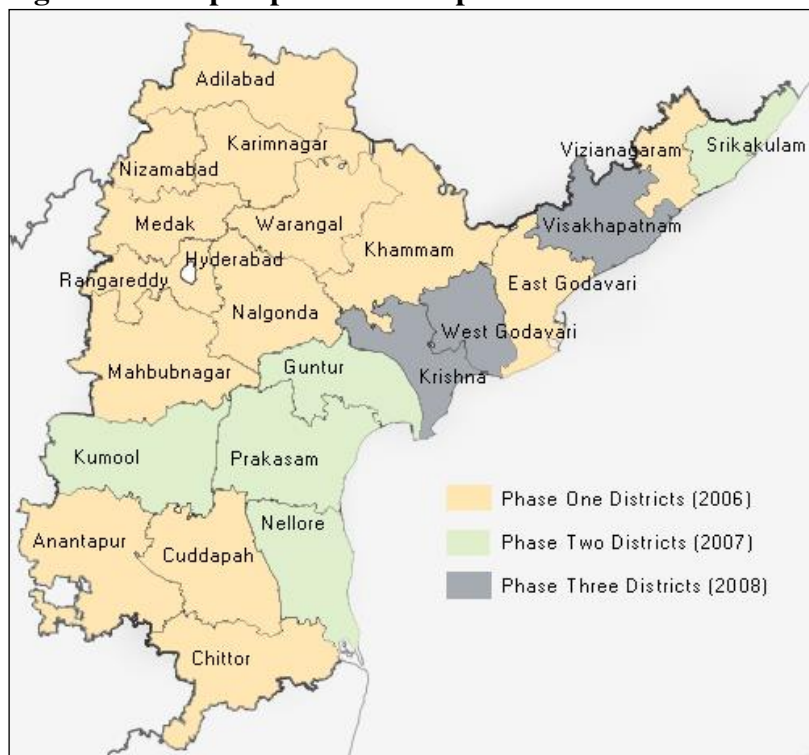
Table 2.1 Comparison of mean Height for age scores by program access and Drought

	Coverage=0	Coverage =1
Drought=0	-1.237 (0.050)	-1.849 (0.032)
Drought =1	-1.487 (0.042)	-1.778 (0.024)
Difference	0.2499	-0.070
p-value	0.0001	0.9591
N	1803	2486

Note: Standard errors in parentheses

- (i) Coverage is a dummy for program availability obtained from administrative records.
- (ii) Drought is dummy of receiving less than the long term average rainfall at mandal.

Figure 1.1: Map of phase-wise expansion of NREGS across Young Lives Sample



Index for Figure 1.1: Phase-wise Coverage across sample districts in Andhra Pradesh

Phase – I	Phase - II	Phase – III
VIZIANAGRAM	EAST GODAVARI	WEST GODAVARI
CHITTOOR	GUNTUR	KRISHNA
CUDAPPAH	KURNOOL	VISHAKHAPATNAM
ANANTPUR	NELLORE	
MAHBUBNAGAR	PRAKASAM	
MEDAK	SRIKAKULAM	
RANGA REDDY		
NIZAMABAD		
WARRANGAL		
ADILABAD		
KARIMNAGAR		
KHAMMAM		
NALGONDA		

*The colored districts are the sample ones from the current survey.

Figure 1.2: Average Height-for-Age by Round and Phase

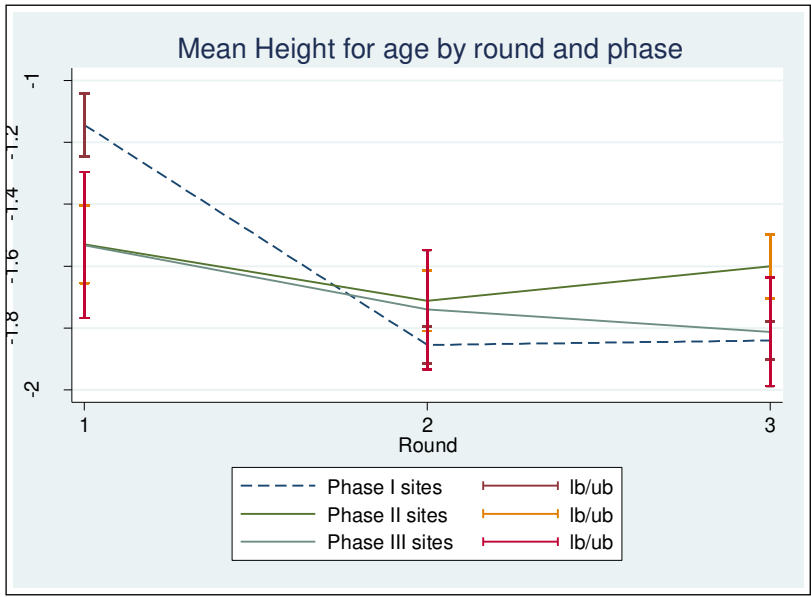


Figure 1.3: Average Height-for-Age by Round and Phase for exposed to drought in birth year

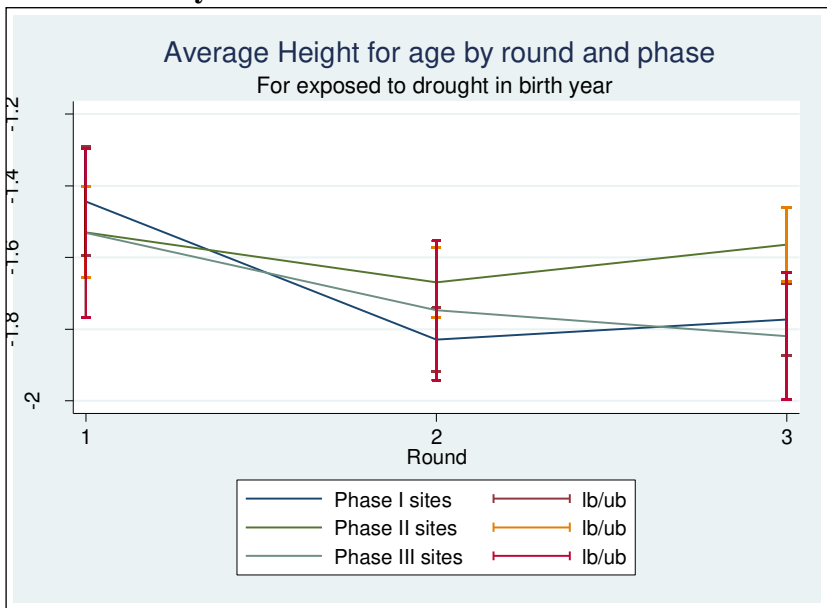


Figure 1.4: Average Height-for-Age by Round and exposure to drought in birth year

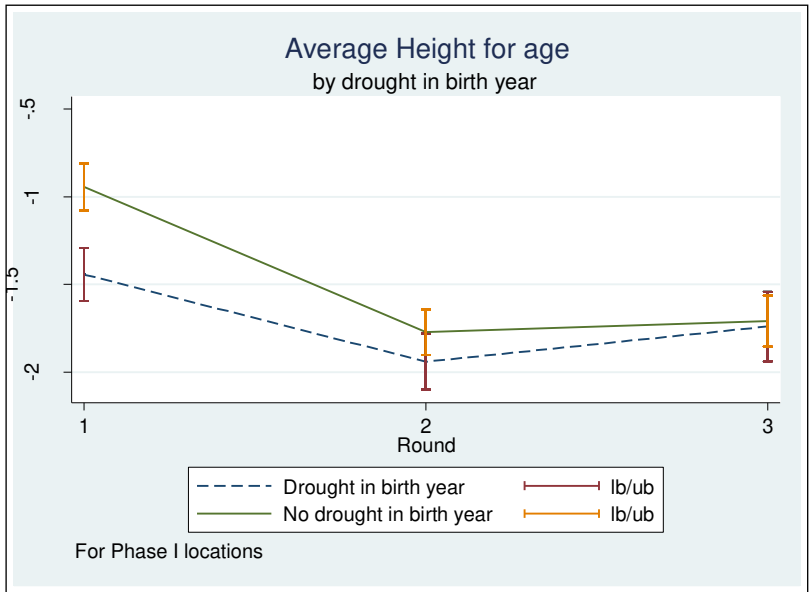


Figure 1.5: Average Height-for-Age by Program participation for exposed to drought in birth year

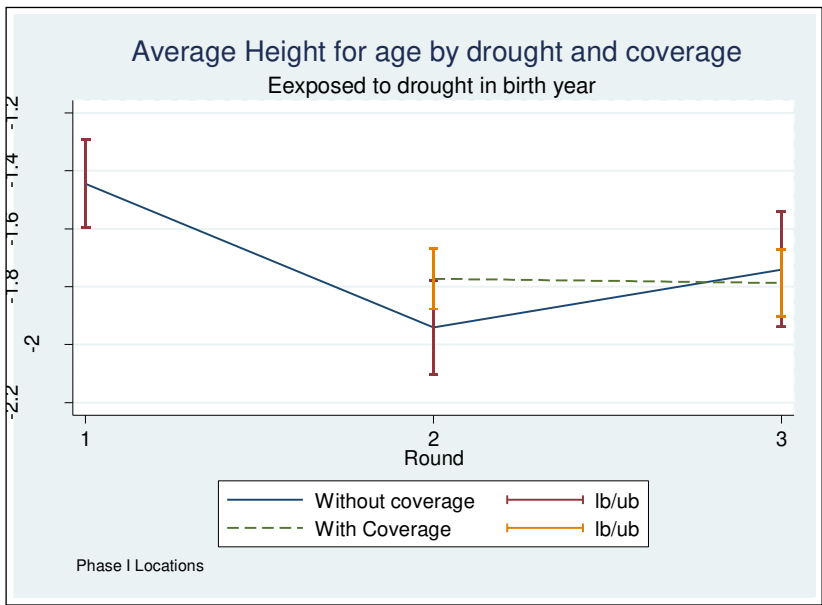


Figure 1.6: Average Height-for-Age by Program participation and exposure to drought in birth year (Phase I sites)

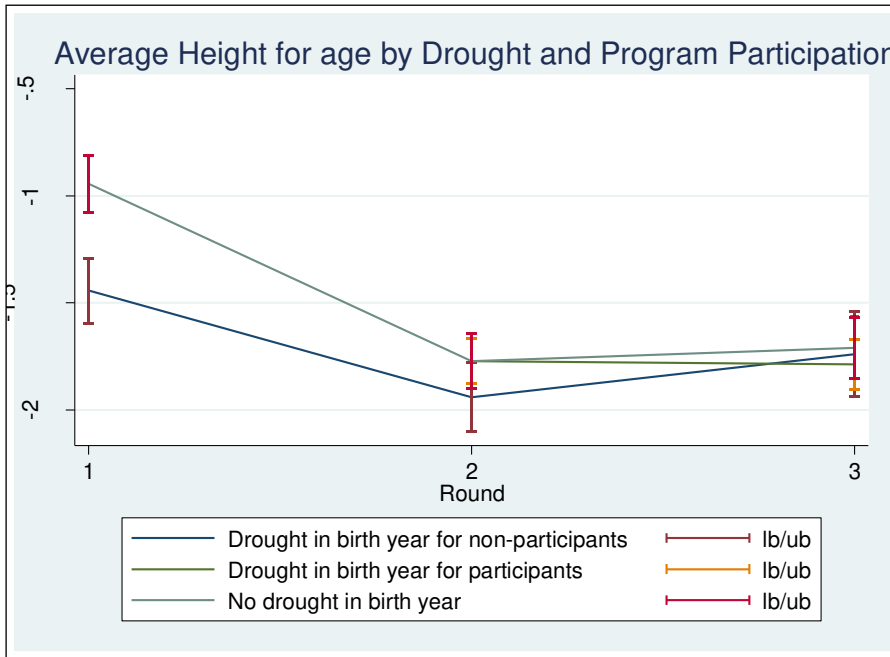


Figure 1.7: Average Height-for-Age by Caregiver's Education

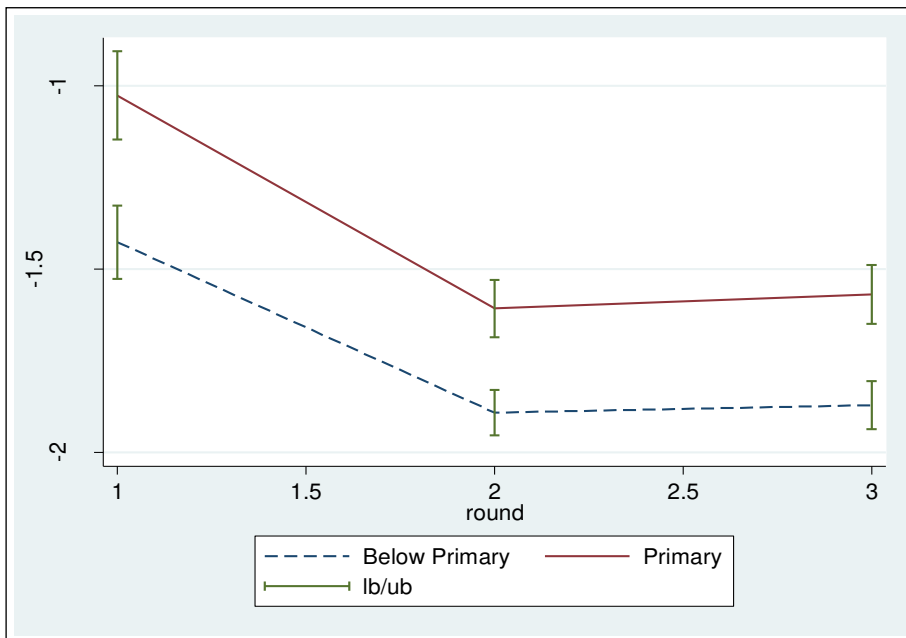


Figure 1.8: Average Height-for-Age by Gender

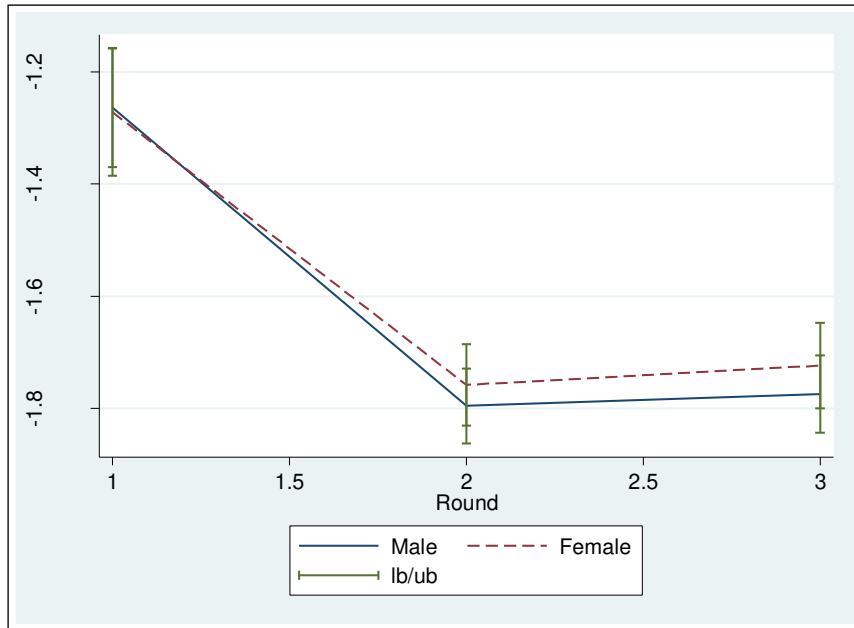


Figure 1.9: Average Height-for-Age by Caste

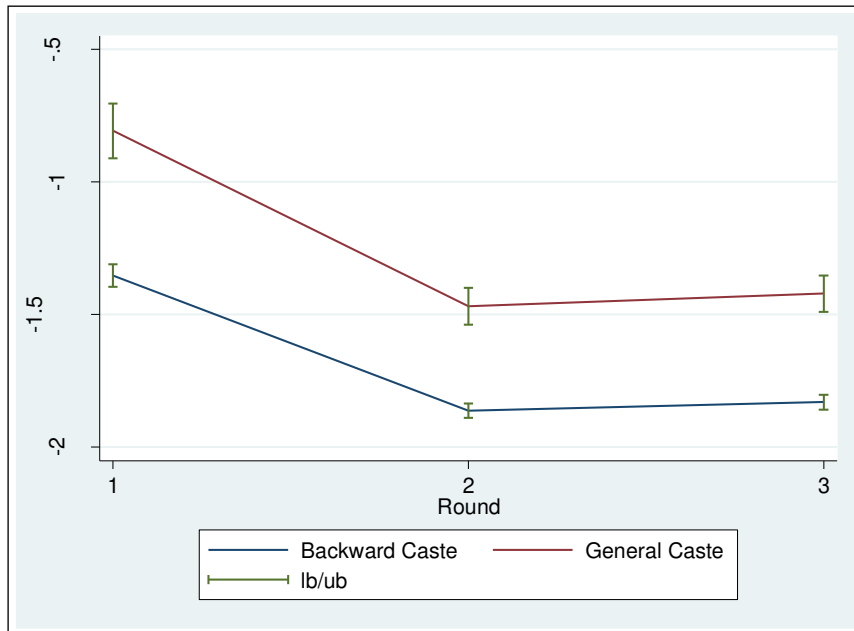


Table 2.2 Estimation of Height For Age by Coverage and Drought
Dependent Variable: Height For Age

	(1)	(2)
	Rural	Urban
Drought	-0.373*** (0.132)	0.0322 (0.257)
Coverage	-0.00696 (0.00586)	-0.00593 (0.00887)
Drought*Coverage	0.0127*** (0.00467)	0.00468 (0.00782)
Health Facility	0.0692 (0.0873)	0.106 (0.154)
Age	-0.0851*** (0.0255)	-0.0201 (0.0306)
Observations	4289	1376

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses:

* p < 0.10, ** p < 0.05, *** p < 0.01

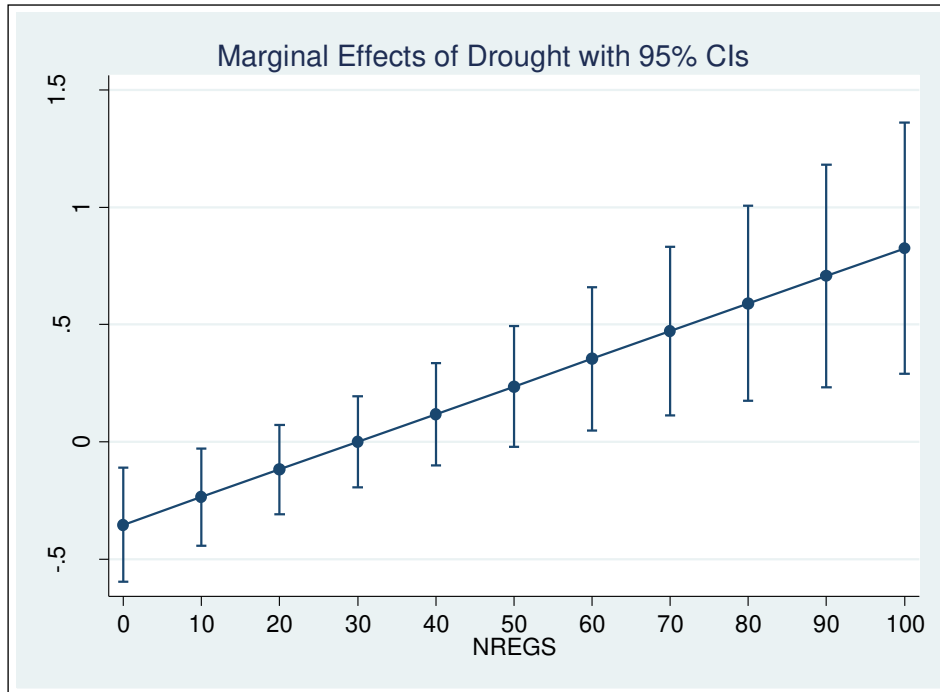
a) HAZ indicates Height for Age, adjusted for both Age and Sex

b) Coverage is average number of days available under the program in the mandal

c) Drought is defined as receiving less rainfall than the long term average at mandal in the year prior to survey

d) All specifications include individual fixed effects

Figure 1.10: Marginal impact of drought shock on height-for-age by NREGS days



Note: (i) NREGS is average number of days available under the program in the mandal
(ii) Linear prediction based on estimation results from specification (1) in Table 2.2

Table 2.3 Estimation of Interaction effects

Dependent Variable: Height For Age

	(1)	(2)	(3)	(4)
Interaction variables	NREGS (Avg days)	Corrected CNREGS	Program Intensity	NREGA (participation)
Drought	-0.373 ^{***}	-0.369 ^{***}	-0.452 ^{***}	-0.245 ^{**}
	(0.144)	(0.134)	(0.111)	(0.115)
Drought*NREGS	0.0122 ^{***}			
	(0.00470)			
Drought*CNREGS		0.0120 ^{***}		
		(0.00442)		
Drought *Prog.Inten			1.121 ^{***}	
			(0.260)	
Drought*NREGA				0.480 ^{***}
				(0.154)
Observations	4289	4289	4289	4289

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses:

* p < 0.10, ** p < 0.05, *** p < 0.01

a) Drought is receiving less than the long term average rainfall at mandal in the year prior to survey

b) Specifications include individual fixed effects

c) CNREGS is Coverage (Average days per household under the program) corrected for low participation

d) Program Intensity measures the fraction of years the mandal received the program

e)NREGA is a dummy constructed from self-reported participation.

f) All specifications include individual fixed effects, respective program variable, age & health facility

Table 2.4 Estimation of Stunting by Drought and Coverage
Dependent Variable: Stunting

	(1) Rural	(2) Rural	(3) Urban
Drought	0.0821** (0.0349)	0.0785** (0.0352)	-0.0292 (0.0439)
Coverage	0.00238** (0.00117)		0.000236 (0.00155)
Drought*Coverage	-0.00341*** (0.000843)		0.0000705 (0.00128)
Health Facility	-0.0116 (0.0284)	-0.0113 (0.0315)	-0.0222 (0.0258)
Age	0.0166** (0.00777)	0.0166** (0.00697)	0.00546 (0.00714)
NREGS		0.00233** (0.000921)	
Drought*NREGS		-0.00333*** (0.000810)	
Observations	4289	4289	1376

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses:

* p < 0.10, ** p < 0.05, *** p < 0.01

a) Coverage is Average number of Days available under NREGA in the mandal

b) Drought is receiving less than the long term average rainfall at mandal in the year prior to survey

c) Specifications include mandal(sub-district) fixed effects

d) NREGS is Coverage (Average days per household under the program)corrected for participation

e) All specifications include mandal fixed effects

Table 2.5 Dependent Variable: Height For Age (Results by Gender)

	(1) Male	(2) Female
Drought	-0.384*** (0.137)	-0.353*** (0.121)
NREGS	-0.00876* (0.00453)	-0.00428 (0.00368)
Drought*NREGS	0.0133*** (0.00358)	0.0114*** (0.00382)
Health Facility	0.0707 (0.115)	0.0689 (0.0928)
Age	-0.0807*** (0.0297)	-0.0969*** (0.0233)
Observations	2272	2017

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses

Note: * p < 0.10, ** p < 0.05, *** p < 0.01

a) HAZ indicates Height for Age, adjusted for both Age and Sex

b) Drought is receiving less than the long term average rainfall at mandal in the year prior to survey

c) All specifications include child fixed effects

d) NREGS is Coverage (Average days per household under the program)corrected for participation

e) All specifications include individual fixed effects

Table 2.6 Dependent Variable: Height For Age (Results by Caste)

	(1) General Caste	(2) Backward Caste
Drought	-0.277* (0.156)	-0.380*** (0.145)
NREGS	-0.00517 (0.00790)	-0.00679 (0.00519)
Drought*NREGS	0.00960 (0.00639)	0.0127*** (0.00390)
Health Facility	0.126 (0.148)	0.0678 (0.0692)
Age	-0.0957*** (0.0336)	-0.0871*** (0.0250)
Observations	600	3689

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses

Note: * p < 0.10, ** p < 0.05, *** p < 0.01

a) HAZ indicates Height for Age, adjusted for both Age and Sex

b) Drought is receiving less than the long term average rainfall at mandal in the year prior to survey

c) All specifications include child fixed effects

d) NREGS is Coverage (Average days per household under the program)corrected for participation

e) All specifications include individual fixed effects

Table 2.7 Dependent Variable: Height For Age (Results by Caregiver's Education Level)

	(1) Primary & Above	(2) Below Primary
Drought	-0.372 (0.242)	-0.365 ^{***} (0.132)
Coverage	-0.0118 (0.00808)	-0.00566 (0.00510)
Drought*Coverage	0.0177 ^{**} (0.00726)	0.0110 ^{***} (0.00350)
Health Facility	0.0434 (0.114)	0.0863 (0.0940)
Age	-0.0756 ^{**} (0.0368)	-0.0911 ^{***} (0.0274)
Observations	1229	3057

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses

Note: * p < 0.10, ** p < 0.05, *** p < 0.01

a) HAZ indicates Height for Age, adjusted for both Age and Sex

b) Drought is receiving less than the long term average rainfall at mandal in the year prior to survey

c) All specifications include child fixed effects

d) NREGS is Coverage (Average days per household under the program)corrected for participation

e) All specifications include individual fixed effects

Table 2.8 Estimation of Height For Age by Coverage and Cumulative Drought

Dependent Variable: Height For Age	
CD	-0.975*** (0.323)
Coverage	-0.00700 (0.0113)
CD*Coverage	0.0118 (0.0208)
Health Facility	0.00765 (0.159)
Age	-0.0627*** (0.0224)
Observations	4289

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses

Note: * p < 0.10, ** p < 0.05, *** p < 0.01

a) HAZ indicates Height for Age, adjusted for both Age and Sex

b) Coverage is Average number of Days available under NREGA in the mandal

c) CD =Cumulated Drought is a fraction of years having drought at mandal level cumulated from birth year

d) All specifications include individual fixed effects

Table 2.9 Estimation of Stunting by Cumulative Drought and Coverage

Dependent Variable: Stunting		
	(1)	(2)
CD	0.246 ^{***} (0.0775)	
CD*Coverage	-0.00422 (0.00430)	
Drought		0.0780 ^{**} (0.0319)
Drought*Coverage		-0.00325^{***} (0.000913)
Observations	4289	4289

Robust boot-strapped Standard errors (clustered at the mandal) in parentheses

* p < 0.10, ** p < 0.05, *** p < 0.01

a) Coverage is Average number of Days available under NREGA in the mandal

b) Drought is receiving less than the long term average rainfall at mandal in the year prior to survey

c) CD=Cumulated Drought is a fraction of years receiving less than the long term average rainfall at mandal cumulated from birth

d) All specifications include mandal fixed effects

Can the Major Public Works Policy Buffer Negative Shocks in Early Childhood? Evidence from Andhra Pradesh, India

The study examines the role of the largest public works program in the world-the National Rural Employment Guarantee Scheme (NREGS) - in buffering the negative effects of early childhood exposure to rainfall shocks on long-term health outcomes. Exploiting the spatial and temporal variation in NREGS coverage, the study estimates the extent to which nutritional shocks in early childhood can be offset by access to the policy. The study employs a unique identification strategy by integrating detailed administrative records of drought shock and phase-wise roll-out information of NREGS with a household level panel data-the Young Lives survey- conducted over three waves (2002, 2007 and 2009-10) in the state of Andhra Pradesh, India. Using individual fixed effects estimation the study finds that while the policy does not help correct for long term past health deficiencies it is useful in buffering recent drought shocks, which varies by policy relevant sub-groups. We find that an increase in 22 working days per household increases height-for-age by around 0.26 standard deviations which is bridging about half the rural-urban gap in average height for age score. We find the program is most effective for the case of lower educated households and scheduled castes, who are presumably more vulnerable in the face of climatic variability. Hence there is much room to reap in the indirect benefits of the program by ensuring food security issues of these households.



About Young Lives

Young Lives is an international study of childhood poverty, involving 12,000 children in 4 countries over 15 years. It is led by a team in the Department of International Development at the University of Oxford in association with research and policy partners in the 4 study countries: Ethiopia, India, Peru and Vietnam.

Through researching different aspects of children's lives, we seek to improve policies and programmes for children.

Young Lives Partners

Young Lives is coordinated by a small team based at the University of Oxford, led by Professor Jo Boyden.

- *Ethiopian Development Research Institute, Ethiopia*
- *Pankhurst Development Research and Consulting plc*
- *Save the Children (Ethiopia programme)*
- *Centre for Economic and Social Sciences, Andhra Pradesh, India*
- *Save the Children India*
- *Sri Padmavathi Mahila Visvavidyalayam (Women's University), Andhra Pradesh, India*
- *Grupo de Análisis para el Desarrollo (GRADE), Peru*
- *Instituto de Investigación Nutricional, Peru*
- *Centre for Analysis and Forecasting, Vietnamese Academy of Social Sciences, Vietnam*
- *General Statistics Office, Vietnam*
- *University of Oxford, UK*

Contact:

Young Lives
Oxford Department of
International Development,
University of Oxford,
3 Mansfield Road,
Oxford OX1 3TB, UK
Tel: +44 (0)1865 281751
Email: younglives@younglives.org.uk
Website: www.younglives.org.uk