Income Shocks, Public Works and Child Nutrition

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Abstract

There has been a renewed interest in public works programs in the recent past. While their causal impact on wages, employment and consumption expenditures has been well researched, there is limited evidence on how these programs affect children. In this paper, I analyze the functioning of the National Rural Employment Guarantee Scheme (NREGS) in India as a nutritional safety net for children. Income shocks have detrimental consequences on child health. Studying the role of the NREGS in mitigating the impacts of such shocks on children thus becomes important. I first develop a simple model of intra-household time allocation and child health. This model is then empirically tested to determine the program effects. The main finding that emerges is that the NREGS does indeed serve as a buffer against income shocks, particularly for boys. Paradoxically, richer households seem to benefit more, suggesting significant rent seeking. However those with small land holdings also seem to do better. Thus, while on the whole the NREGS is quite effective in altering the situation of the poor during crises, there are strong heterogeneous impacts.

1 Introduction

It is well known that poor households in developing countries are limited in their ability to smooth consumption in the face of income shocks. This generates large variations in consumption which can have adverse consequences for the well-being of these households. The impact of such shocks on children has been well researched (see e.g Jensen, 2000, Alderman et al., 2009 and Alderman et al., 2006). Early life exposure to economic shocks has important implications not only for subsequent adult health and other socio-economic outcomes but also for perpetuating intergenerational poverty (Maccini and Yang, 2009). In such an event, social protection schemes such as the National Rural Employment Guarantee Scheme (NREGS) in India may have the potential to mitigate the adverse impacts of an income shock on child health outcomes by serving as a nutritional safety net.

Envisaged as being both a safety net and as providing alternative employment, the NREGS[1] is the largest program of its kind in the world with annual expenditures equaling about 1 percent of India's GDP[2]. While the use of public work programs in developed countries has declined, there has been a resurgence of such programs in developing countries[3]. They have evolved into long term anti-poverty measures rather than merely serving to reduce temporary unemployment (Zimmermann, 2014). Using data from Andhra Pradesh[4] in India, this paper seeks to understand the extent to which a safety net like the NREGS buffers the impact of income shocks on investments in children.

While there is a growing literature on the impact of the NREGS on the labour market (e.g. Berg et al., 2012, Azam, 2012, Imbert et al., 2012) there has been a lack of focus on its role as a buffer for poor households in the event of a shock and the consequent impact on children[5]. To address this gap in the literature and using pre and post-intervention data from the Young Lives Panel study in India, I use a quasi-experimental approach to examine the extent to which the NREGS buffers the impact of income fluctuations on child health. That is, I examine the intent-to-treat effects of the program. I also assess the differential impact of the NREGS across wealth quartiles, land ownership and gender. It is important to remember that since the NREGS was implemented in a phased manner with the poorest districts getting it first, selection bias is a serious issue. By assuming that program placement is additive and time invariant, this study corrects for selection bias by using a difference-in-difference framework.

[1]The NREGS offers a legal guarantee to every rural household: about 68 percent of the population.

[2]About Rs 33000 crores ($5500 million) in 2013-14.

[3]Subbarao et al. (2012) provide an extensive review of public works programs - their design, implementation issues, poverty impacts and cost effectiveness - in developing countries.

[4]On June 2, 2014 Andhra Pradesh was split into two states: Telangana and Andhra Pradesh. This however doesn't impact my analysis since the data used in this paper is from 2002 to 2009.

[5]Zimmermann (2014) and Dasgupta et al. (2013) are notable exceptions.

There are a number of important findings that emerge. First, the NREGS has large and positive mitigating effects on child health in the aftermath of a shock. There is a cumulative improvement of about 0.08 standard deviations in height-for-age z-scores for those who had access to the NREGS in the event of a shock. Second, the program has strong differential impacts. Those belonging to higher wealth quartiles seem to benefit more from the NREGS after a shock suggesting significant rent seeking. Moreover, there is an asymmetric burden of shocks on girls with boys benefiting more from the safety net feature of the NREGS. These results are robust to alternative specifications.

These findings are potentially very relevant from a policy perspective because transitory shocks such as variable rainfall can induce path dependence[6] and generate long term losses in health and education. While households in developing countries face both idiosyncratic[7] and covariate shocks, covariate risks such as droughts and floods are harder to cope with since everyone in a community is impacted by the shock to some degree. Ferreira and Schady (2009) show that the long term impact on children of such covariate shocks can be significant. A social safety net like the NREGS is expected to support poor households by acting as a basic insurance mechanism after a shock. By ensuring income in times of economic downturn it prevents distress reactions such as asset sales which harm long run productive possibilities. Further it also enables poor risk averse households to lengthen their planning horizons and invest in high return-high risk activities such as newer technologies (Dasgupta et al., 2013, Ravi and Engler, 2009).

There are multiple channels through which the NREGS could impact child health in the event of a shock. First, the NREGS will result in changes in the labour supply of the household and as long as the income effect outweighs the substitution effect, child health will not be negatively affected in the event of a shock. Second, given that the NREGS leads to the strengthening of community level infrastructure such as water supplies and roads, child health is positively impacted as a result of this improved community infrastructure. Third, as a result of the NREGS, village economies become functional again and this leads to an increase in the income/per capita consumption of households which can again impact child welfare (Mani et al., 2014).

The remainder of the paper proceeds as follows. Section 2 provides a brief review of the previous developments in the literature. It also provides a description of the NREGS. Section 3 provides a basic theoretical framework linking intra-household time allocation to child health. Section 4 discusses the data. Section 5 describes the econometric model and related methods. Section 6 presents the results while Section 7 concludes.

[6]Transitory shocks can have permanent consequences.

[7]Idiosyncratic risks include events such as death or illness of a family member. These are usually uncorrelated among members of a community. There is some evidence in the literature that households have managed to offset the damages caused by idiosyncratic risks through various mechanisms such as informal risk sharing (e.g. Townsend, 1995, Mobarak and Rosenzweig, 2013).

2 Background

2.1 Previous Work

Rural households in developing countries often operate in risky environments[8]. Vulnerability to shocks is an important cause of deprivation (Dercon, 2001). This is compounded by the presence of weak financial instruments and the adoption of other sub-optimal coping mechanisms such as asset sales, migration and child labour (Rosenzweig and Wolpin, 1993, Morduch, 1994 and Dercon, 2005). There is now growing evidence on the permanence of income shocks on human capital formation, nutrition and incomes. For instance, Foster (1995) finds that child growth (measured by weight) is highly responsive to fluctuations in income and prices and the effect is greater for credit constrained households. Alderman et al. (2006) have looked at the impact of a drought in Zimbabwe on child nutrition outcomes. They find that children aged 1-2 years lost 1.5 to 2 cm of height attainment after the drought and catch up was very slow even four years after the drought. Using DHS data from Peru, Paxson and Schady (2005) find that there was an increase of 2.5 percentage points in infant mortality rates for children born during the Peruvian economic crisis of the 1980s.

One of the more notable interventions used to alleviate the impacts of droughts on child growth has been food aid. This has been motivated, among other things, for its notable beneficial impact on child malnutrition (Yamano et al., 2005). Quisumbing (2003) has shown that food aid and food-for-work interventions have a positive direct impact on weight-for-height z-scores of Ethiopian children in the aftermath of a shock. Yamano et al. (2005) also estimate that food aid positively offset the negative effects of shocks in communities that received the aid.

While direct interventions such as food aid and their impacts have been relatively well studied, the role of larger safety nets like large scale public works programs has been comparatively less researched. While their design has received much attention[9] there is a dearth of evidence on the effectiveness of such social protection programs in assuring nutrition for households that are exposed to economic crises. Given the importance of such public works in mitigating the effects of a contraction in incomes due to a shock as pointed out by Ravallion (1991), this lack of evidence seems even starker. Moreover, as Alderman (2010) points out while short term climate shocks have long term impacts on children that persist into adulthood, safety nets whether

[8]For instance the average coefficient of variation of household income of farmers is almost 40 percent in the villages of the ICRISAT survey (Walker and Ryan, 1990).

[9]Some of the studies that revolve around the poverty alleviation aspect of employment guarantee schemes are: (a) the incentive argument of public works programs, specically, the self-targeting and screening potential of these programs (Besley and Coate, 1992), (b) the use of means-tested and universal schemes in the alleviation of poverty (Besley, 1990), (c) schemes aiming at wide coverage at low wages and restricted coverage at higher wages (Ravallion, 1991).

in the form of income transfers or targeted nutrition interventions, can go a long way in mitigating the impacts of these shocks.

2.2 The National Rural Employment Guarantee Scheme

Enacted into law by the Government of India in 2005, the National Rural Employment Guarantee Scheme (NREGS)[10] guarantees a minimum of 100 days of unskilled wage-employment in a financial year to rural households on productive public works at state prescribed minimum wages. Currently available to 56 million households, it is the largest safety net scheme in the world (Subbarao et al., 2012). It differs from other previous schemes in that it promises employment as an entitlement and there are no eligibility requirements (Azam, 2012). The Act also stipulates that one-third of all beneficiaries be women (Mani et al., 2014).

The NREGS was rolled out in three phases beginning in 2005. Districts in India were ranked based on a Backwardness Index designed by the Planning Commission. Based on this index the 200 poorest districts were covered in the first phase of the program between September 2005 and February 2006. The second phase commenced in May 2007 and covered 130 districts while in April 2008 all the remaining districts were covered.

The NREGS applies only to rural areas. The Act provides adult members of a household casual manual labour at the statutory minimum wage which is about Rs. 120 (2 USD)[11] per day (Azam, 2012)[12]. During 2010-11 Andhra Pradesh provided 274.8 million person days of employment (Galab et al., 2008). Given the susceptibility of rural households in India to periodic weather shocks and seasonal variations, the NREGS has been tailored to meet the objective of livelihood security by reducing the dependence on agricultural wages (Subbarao et al., 2012).

The NREGS was designed to be a program based on self-selection. Work carried out on identifying participation in the NREGS has found that holders of Antodaya cards (below poverty line cards issued to the poorest) are 20 percent more likely to register for the program (Shariff, 2009 and Uppal, 2009). Likewise, Jha et al. (2009) find that targeting of the program has been satisfactory with there being wide participation from traditionally disadvantaged groups such as scheduled castes and scheduled tribes. Thus the NREGS seems to be encouraging participation from those who need it the most.

Moreover, the effectiveness of the NREGS as an anti-poverty tool is becoming increasingly apparent. Using

[10] The scheme was renamed the Mahatma Gandhi National Rural Employment Guarantee Scheme in 2009. Academic literature still widely uses the original name.

[11] Farm wages in comparison are usually about 100-150 rupees, varying somewhat depending on the agricultural season (Dasgupta et al., 2013).

[12] The statutory minimum wages varies across states.

data from 2000-11 from 19 Indian states, Berg et al. (2012) find that on average, the NREGS boosts real agricultural wages by 5.3 percent. Imbert et al. (2012) supplement this by showing that the gains from this rise in wages accrues disproportionately more to the poor. Similarly, using data from 1000 households in Andhra Pradesh, Ravi and Engler (2009) find an increase in monthly per capita consumption for households participating in the NREGS to the tune of 6 percent. The differential impact of the program is also emerging. Afridi et al. (2012) find that increased female labour force participation in the NREGS increases school attendance and grade attainment of children, especially girls.

3 Theoretical Framework

This section presents a simple model on the impact of intra-household time allocation on child growth. I use this framework to show the mechanisms through which the NREGS mitigates the impact of an income shock on child health.

3.1 Basics

An income shock affects household income adversely and thus impacts consumption and consequently child health. Implicit in this is the assumption that households are unable to insure their consumption fully from such income shocks. This is plausible because my data includes only rural households who for the most part have few, if any formal coping mechanisms for such shocks (see e.g. Dercon, 2001 for a review).

An employment guarantee program such as the NREGS can have two effects in the event of an income shock. First, it is expected to have a positive impact on child growth because it supplements household income during a shock and thus enhances the households ability to purchase nutrition enhancing items (Yamano et al., 2005)[13]. This can be directly through actual wages paid on the NREGS works or indirectly through strengthening of village infrastructure, increased resilience to shocks etc. Second, unlike food aid, under the NREGS households supply labour on public works. This means that it could take away from the time that would have been spent on child care. Moreover for about 57 percent of my sample the child's mother has participated in the NREGS. Assuming that the mother is the primary caregiver especially for younger children this 'substitution' effect could negatively impact child growth in the event of a shock[14].

[13]However as Debela et al. (2014) point out the degree to which an increase in income boosts nutrition depends on the marginal propensity to consume health and nutrition goods out of money income. Moreover the effect of the NREGS on child growth further depends not only on intra-household allocation but also on the gender and age preferences of the parents.

[14]Most rural households in India have large families and thus it is true that even if the parents are out on the NREGS works

3.2 Wage Implications of a Transitory Shock

The NREGS functions both as an alternative source of employment ex-ante and as an ex-post coping mechanism after an income shock. Assuming there are no constraints on off-farm labour supply, a household will work either in the private off-farm sector or in ex-ante NREGS employment and will work wherever the wage is higher. This is because both these forms of employment are perfect substitutes and contribute in the same manner to household utility. Exogenous income shocks however, change the ex-post wage in the private off-farm sector. This is because in the event of an income shock, when labour productivity is low, demand for labour (whether own or hired) on one's own farm decreases. This leads to an increase in off-farm labour supply and a reduction in wages. Since the off-farm wage is now contingent on the weather it is less useful as a risk mitigation tool. In the event of a shock not only are households adversely impacted because of direct losses in farm income/profits because of the shock itself, but also from the reduction (and increased variability) in off-farm wages. This can be easily seen in a simple one-period model.

Assume that each household in the economy has k units of land. Before the introduction of the NREGS, households allocate time, T, between working in the off-farm sector[15], l and working on their own family-farm, f. The period ends and total income which is a combination of the wages earned working off-farm, w_o and profits from the family farm, y is realized. Household utility, u is a function of total income earned from the two activities and $u' > 0$, $u'' < 0$[16].

Production on the farm is Cobb-Douglas in land and labour:

$$y\,(k, d) = \tilde{A} d^{\beta} k^{1-\beta} \tag{1}$$

where $\beta \in (0,\,1)$, $\tilde{A}$ is total factor productivity, and d is labour demand. Let this be stochastic and of the following form:

$$\tilde{A} = \begin{cases} A_H \text{ with probability } \frac{1}{2} \\ A_L \text{ with probability } \frac{1}{2} \end{cases}$$

with $A_H > A_L$. Thus during a good year total factor productivity is high (A_H) and during a drought it is low (A_L).

any other adult member of the family can take care of the child. Moreover, the NREGS also has provisions for child care outfits to be set up on the work site so that the mother need not leave the child unattended at home. Both of these imply that the impact of the negative substitution effect may not be very large.

[15]This could include working as an agricultural labourer for a landowner.

[16]Thus I assume that households are risk averse.

Labour demand and supply decisions are made ex-post, once the shock, whether high or low, has been realized. Assuming that the labour demand decision on the family farm is separable from other choices, the labour demand decision is given by equating the marginal product of labour to the agricultural wage (w_f):

$$\frac{\partial y}{\partial d} = \tilde{A}\beta \left(\frac{k}{d}\right)^{1-\beta} = w_f$$

$$d^* = k \left(\frac{\tilde{A}\beta}{w}\right)^{\frac{1}{1-\beta}} \tag{2}$$

From (2) it is clear that labour demand is lower in periods of low productivity (A_L) such as when a drought occurs, than in periods of high productivity/in good years (A_H).

Given this reduction in demand for labour because of low agricultural productivity, households supply less labour on the farm and instead increase their off-farm labour supply[17]. The interior solution to the labour supply decision of the household is given by:

$$\max_{l} \quad u((T-l)y + lw_o) \tag{3}$$

The first order condition for off-farm labour supply is:

$$\phi(l^*) = u'((T-l)y + lw_o)(w_o - y) = 0 \tag{4}$$

Equation (4) pins down the optimal off-farm labour supply, l^* and by extension f^*.

Thus when the weather is more variable and total factor productivity is low, labour demand on the farm is also low and there is a tendency to shift away from employment on one's own farm and towards the private casual sector. Off-farm labour supply increases in the event of an exogenous shock. This excess supply of labour drives down the off-farm wage further exacerbating the adverse impacts of the shock[18].

The introduction of the NREGS however changes the situation in a fundamental way. In the event of an income shock households can now work on the NREGS works and are thus *guaranteed* employment after a shock. The wages paid on the NREGS are fixed and thus there are no fluctuations in wages. Thus while

[17] This implicitly assumes that households are unable to borrow and/or draw on their savings to smooth consumption and have to resort to looking for a job in the off-farm sector.

[18] Jayachandran (2006) has found that productivity shocks causes large swings in wages and impacts rural households adversely. Contrary to what is assumed here, she also finds that poorer households have fairly inelastic labour supply and thus are not easily able to switch labour from the farm to the off-farm sector.

households are still worse off compared to the situation without any shock[19], in the event of a shock, they are better off with the NREGS than without it. This is because the NREGS absorbs all the excess workforce *without* impacting the wage, which is fixed. Thus not only is total household income higher in places where the NREGS is available, it is also less variable. This is the particularly powerful role of the NREGS - as a safety net.

3.3 Implications of NREGS Access for Child Health

A child's height[20] at time t, h_t is a function of height last period, h_{t-1}, overall income Y_t (where $Y = fy + lw_o$), labour allocated to child rearing activities[21], L^H, observable household and community characteristics, X_t, unobservable individual characteristics, ϵ_t and unobservable household and community characteristics, u_t:

$$h_t = f(h_{t-1}, Y_t, L^H, X_t, \epsilon_t, u_t) \tag{5}$$

Overall income, Y_t in turn is a function of exogenous shocks such as drought or pest damage, S_t, transfers in the form of income earned on the NREGS works, F_t, observable household and community characteristics, X_t and unobservable household and community characteristics, u_t:

$$Y_t = f(S_t, F_t, X_t, u_t) \tag{6}$$

Substituting (6) in (5) we get:

$$h_t = f(h_{t-1}, S_t, F_t, L^H, X_t, \epsilon_t, u_t) \tag{7}$$

An income shock results in the familiar substitution effect where time spent at home decreases and time spent on the NREGS works increases. However, the income effect is positive because not only does the NREGS lead to wage payments in the event of actual participation, it also indirectly enhances income through improved community infrastructure, increased resilience to shocks etc. Thus if the increase in income compensates for the 'cost' of reallocation of labour from activities at home, then the overall impact on health outcomes could be positive. The critical point is that these wage payments are both higher and less variable (in a relative sense) than they would be in the absence of the NREGS. That is, were the NREGS not available to buffer

[19]This is because shocks such as droughts lower agricultural labour productivity and thus reduce real agricultural wages.

[20]I model child growth by the widely used measure of height. In the empirical section this is measured using height-for-age z-scores.

[21]As Ferreira and Schady (2009) point out these time intensive activities at home can be very important for children. These include antenatal checkups for pregnant women or preventive health checkups for children, cooking healthy meals or ensuring clean water.

the impact of the shock, the income effect would be negative and this combined with the substitution effect would unambiguously lead to a negative impact on child health.

The net impact of the NREGS on health outcomes in the event of a shock depends on the reallocation of labour, the size of the wage payment received and the marginal impact of this payment on child health. Based on the theoretical framework this study poses the following hypothesis: children from households that have access to the NREGS in the event of an income shock are likely to have better health outcomes. This is because the opportunity cost of reallocating labour to the NREGS is most likely low and total household income in the presence of the NREGS is most likely higher than in the absence of it.

4 Data

4.1 Data

The data used in this paper comes from the Young Lives Longitudinal Study. The Young Lives dataset is a rich panel that tracks two cohorts of children (younger cohort born in 2001-02 and older cohort born in 1994-95) from 2002 to 2016. The study is tracking approximately 12,000 children in four countries: Peru, Vietnam, Ethiopia and India. In India the study has been conducted in the state of Andhra Pradesh[22].

The sites chosen in India cover all three agro-climatic zones in Andhra Pradesh[23]. The survey covered the six districts: Cuddapah, Anantapur, Mahbubnagar, Karimnagar, West Godavari, and Srikakulam and also the capital city of Hyderabad. Since our main focus here is on the causal impact of the NREGS in mitigating the impact of shocks I focus only on rural areas because the NREGS was implemented only in rural areas. Thus I exclude Hyderabad from the sample.

Since the NREGS was rolled out only starting in 2005, none of the districts in our sample had the NREGS in 2002. By the second round of the Young Lives survey in 2007, four districts (Cuddapah, Anantapur, Mahbubnagar, Karimnagar) had been covered under the scheme (these will be referred to as the early phase-in/early treatment districts). By the third round in 2009-10 the remaining two districts (West Godavari, and Srikakulam) were also covered (these will be referred to as the late phase-in/late treatment districts).

[22] The data is thus not nationally representative. However, by comparing certain basic attributes and characteristics Outes-Leon and Dercon (2008) have found the data to be representative of the broader region.

[23] 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 (Dasgupta et al., 2013).

In this paper the latest available data from the first three waves (mid-2002, early 2007 and mid-2009) is used. For the purpose of this study the younger cohort consisting of approximately 2011 children is used[24]. The overall rate of sample attrition is low with only about 4 percent of children lost over a seven year period. While constructing the final data set the following exclusion rules have been used: one, only children living in rural areas in all three periods have been included. Two, for econometric reasons I include only those children that are present in all three rounds of the survey. Three, I exclude children with missing information on the dependent variable of interest: height-for-age z-score. Finally I exclude children with height-for-age z-scores outside the [-6, +6] range. My dataset after these exclusions contains data on 4018 children across three years in 6 districts. I use the older cohort for a falsification test.

4.2 Variables of interest

The World Health Organization (WHO) outlines three important indicators for child nutrition: height-for-age (this is a long term indicator of chronic malnutrition), weight-for-height (this is an indicator of acute malnutrition and is being unable to gain weight) and weight-for-age (this is a combination of the above two and is used to give an overall indicator of malnutrition).

In most survey data these indicators are standardized with respect to a reference population and are presented as z-scores. In this paper I focus my attention on the first indicator: height-for-age. This is because when analyzing the impact of shocks, height-for-age is the only accurate indicator of long term impact. Weight-for-height and weight-for-age are short term indicators and children can make up lost weight easily. They would thus give an inaccurate representation of the actual impact of the shocks.

Since my sample consists of rural households it is reasonable to assume that one of the main income shocks they face is variations in rainfall[25]. To construct the income shock variable, the long term average (1951-2009) for each district is used. Standard deviation for the same period is also calculated at the district level. Then rainfall shock is defined as the deviation of actual rainfall last year from the long term average divided by the standard deviation. The shock variable is thus normalized. I use prior rainfall deviations in order to give rainfall shocks some time to feed through and for them to influence household decision making[26].

[24] Attrition in the Young Lives sample is low in the international comparison with other longitudinal studies (Outes-Leon and Dercon, 2008)

[25] Over 70 percent of the population in the state of Andhra Pradesh is engaged in agriculture.

[26] Rainfall data has been obtained from the Indian Meteorological Department.

I also include child and household level controls: age, household size, wealth quartiles[27,28], land owned in acres, dummies for if head of the household has completed primary education, gender, Hindu, SC/ST[29], OBC[30] and debts[31]. There is a significant amount of literature that points to the current health status being determined by attainment till the previous period (Strauss and Thomas, 2007). This can be captured by including a lagged dependent variable as a regressor. While I don't include this in my main specification I do re-estimate the above regressions using the Arellano-Bond GMM estimator and use lagged height-for-age z-score in period *t-2* as an instrument (Arellano and Bond, 1991).

I also include a dummy variable for if the child received food under the ICDS[32] scheme between rounds 1 and 2 or availed of the mid-day meal scheme[33] between rounds 2 and 3. However since this variable is potentially endogenous, I do not include this in my main specification but only to see if it changes my main results.

4.3 Descriptive Statistics

Tables 2 & 3 describe the relevant summary statistics for all three years. The height-for-age z-scores deteriorates from the time of birth (round 1) to when the children are five years (round 2) improving slightly by round 3. As Dasgupta et al. (2013) points out this is more or less consistent with the case of developing countries where height-for-age z-scores decline in the first few years and then stabilize. In round 1 of the survey the mean height-for-age z-scores for those in the early phase-in districts was -1.30 which goes down substantially to -1.82 in round 2 before recovering slightly to -1.63 in round 3. The districts in the late phase-in group follow a similar pattern of declining z-scores from rounds 1 to 2 with some improvement being seen between rounds 2 and 3. What is also interesting is that the late phase-in districts (which includes those districts that got the NREGS later which means that they were higher up on the development index) had worse height-for-age

[27] Wealth quartiles were generated from a wealth index which was constructed as a weighted average of a housing quality index (based on the number of rooms per person and the materials used to construct the house), a consumer durables index (based on ownership of assets by the household, viz. television set, radio, car, motorbike, bicycle, telephone, mobile phone, refrigerator, fan, electric oven, table and chair, sofa and bedstead) and a services index (based on if the household has access to key resources such as drinking water, electricity and toilets).

[28] Including the wealth index implies that I don't include income explicitly as an explanatory variable not only because it would be endogenous but also because the wealth index captures all the impact that income would have captured because the index is a far more comprehensive measure.

[29] Scheduled Caste/Scheduled Tribe

[30] Other Backward Caste

[31] In this data, gender is the only variable that is time invariant. Caste and religion do change slightly over the three waves for some households. It is difficult but not uncommon in India for people to change castes and/or change their religion. Thus dummies for SC/ST, OBC and Hindu are not time invariant.

[32] The Integrated Child Development Scheme (ICDS) was launched in 1975 in India and is a supplementary feeding program targeted at children aged 0-6 years. Thus since the children in my sample were 0-1.5 years months in 2002 and 5-6.5 years in 2007 I consider these two rounds.

[33] The Mid-Day Meal scheme was launched in different states at separate points in time but the scheme was universalized in 2001-02. Designed to improve nutritional status, it provides free lunches to children in Primary and Upper Primary classes in government run schools. Since the children in my sample were between 6.5-9 years between rounds 2 and 3 of the survey I consider these two rounds.

z-scores than the early phase-in districts in 2002 before the NREGS was in force.

There are no significant differences between the two groups in terms of age, gender and religion. However, there are significant differences across the groups in terms of caste, schooling of household head, household size, debt, land owned and access to supplementary feeding programs. This is to be expected because the NREGS was not a randomly assigned program. The poorest districts received the program first. This implies that the early phase-in districts and late phase-in districts are markedly different on a number of indicators. This however, as detailed in the next section, is resolved by using a difference-in-difference methodology and assuming that the placement bias is additive and time invariant.

Examining the other control variables I find that on average 53 percent of the sample is male while the average age in 2002 and 2009-10 is approximately one year and 7.5 years respectively. The average child has approximately four other members co-residing with her. The children are primarily Hindu (92 percent) and a majority belong to backward castes, SC/ST or OBC. Not surprisingly, the proportion of the sample with caregivers having completed primary education is low (about 30 percent). The wealth index takes a value between 0 and 1. The average value of 0.19 suggests that households have only about 20 percent of all of these assets suggesting that the children in the sample come from very poor households.

5 Empirical Strategy

The impact of the NREGS on child growth in the event of an income shock can be multi-dimensional. First, the NREGS leads to wage payments and this income effect may alone lead to higher expenditures on child health[34]. Even if it doesn't lead to increased expenditures it is reasonable to assume that given this 'insurance' in the event of a shock, expenditures on child health would potentially not be reduced. Second, the NREGS will lead to increased labour supply by the parents in the aftermath of the shock and this may lead to a substitution effect away from time spent at home on activities beneficial for child health. Third, the NREGS leads to the creation of village infrastructure such as roads and this may increase the risk bearing capabilities of households after an income shock[35] without necessitating an increase in labour force participation. Fourth as Mani et al. (2014) point out, for about a quarter of the sample of children from the Young Lives data, the NREGS workers from the household are not biological parents. This means that considering only parent's labour force participation could bias the true impact of the NREGS. This is because if none of the parents

[34] This does depend on the marginal propensity to consume health goods out of cash income.

[35] Skoufias (2003) points out that publicly provided mechanisms such as sound infrastructure play an important role in reducing the risk for poor households vulnerable to shocks.

participate then only the income effect remains and thus we would underestimate the true impact of the program if we focused only on parental participation. Finally, the NREGS also reduces the ex-ante risk that communities face as a result of covariate shocks regardless of actual participation in the program. This leads to a rise in overall village income and consequently better child outcomes. The study therefore estimates the intent-to-treat effects of the program which includes both the income and substitution effects[36].

5.1 Measuring overall impact

One of the main challenges in this study is that there is no untreated counterfactual group since all six districts received the program at some point in time. This identification issue can be potentially serious because before and after evaluations can be biased if there is no comparison to untreated groups (Clemens and Demombynes, 2010). Thus, it is necessary to evaluate benefits by comparing beneficiaries to other beneficiaries and exploiting the phase-wise implementation of the NREGS (Hanson et al., 2013).

First, in order to eliminate the program placement bias, the main specification I run is a fixed effects model given by the following equation:

$$H_{idt} = \beta_0 + \beta_1 NREGS_{d,t} + \beta_2 Shock_{d,t-1} + \beta_3 [NREGS * Shock]_{d,t} + \epsilon_i + \epsilon_d + \epsilon_h + \epsilon_{it}$$

The dependent variable H_{idt} refers to the height-for-age z-score for child i in district d at time t. The variable $NREGS$ measures access to the NREGS and varies by district and time. $Shock_{d,t-1}$ is the income shock at the district level in the *previous* period. The coefficient on the interaction term, β_3 is the parameter of interest and captures the mitigating effects of the NREGS in the event of a shock[37]. This specification includes year dummies.

5.2 Measuring multi-year impacts

The above specification however does not allow me to disentangle the multi-year impacts of the program. In order to do so, I examine two groups: the early phase-in districts and the late phase-in districts. The NREGS was rolled out in two waves with four districts (Cuddapah, Anantapur, Mahbubnagar, Karimnagar) getting the program in 2007 and the last two districts (West Godavri and Srikakulam) getting it in 2009. Thus in the

[36]Using this approach however means that I am unable to disentangle the income and substitution effects since the intent-to-treat effects estimate the total impact of the program.

[37]The impact of the shock in the absence of the NREGS is given by β_2 while in the presence of the NREGS the corresponding value is given by $\beta_3 - \beta_2$

final year the differences between the two groups are caused only by differential access to the program. Thus I examine two phases of treatment (2007 and 2009) in addition to one pre-intervention year (2002).

Thus, in order to disentangle the multi-year impacts of the program I run the following difference-in-difference model:

$$H_{idt} = \beta_0 + \beta_1 Shock_{d,t-1} + \beta_2 Time1_{d,t} + \beta_3[Time1 * Shock]_{d,t} + \beta_4 Time2_{d,t} + \beta_5[Time2 * Shock]_{d,t} + \epsilon_i + \epsilon_d + \epsilon_h + \epsilon_{it}$$

As before H_{idt} refers to the height-for-age z-score for child i in district d at time t. $Time1_{d,t}$ and $Time2_{d,t}$ are indicator variables for NREGS assignment to the first and second years of exposure respectively. They vary by district and time (e.g. $Time1_{d,t}$ takes a value of 1 for the first four districts in 2007 and for the last two districts in 2009. Similarly, $Time2_{d,t}$ takes a value of 1 for the first four districts in 2009 since it was the second year of the program for these districts. Table 1 outlines the implementation framework.). In this framework, β_3 captures the average mitigating effect of the first year of exposure to the NREGS on child health in the event of a shock and β_5 is the analogous buffering effect of the second year of exposure to the NREGS[38]. This specification also includes year dummies.

Table 1: NREGS Implementation Framework

Group	Districts Covered
First year of treatment	1, 2, 3, 4 in 2007 & 5, 6 in 2009
Second year of treatment	1, 2, 3, 4 in 2009
Control	1, 2, 3, 4, 5, 6 in 2002 & 5, 6 in 2007

Both specifications also include a set of observable individual and household level controls. Including these variables helps control for pre-treatment differences that were present between the early and late phase-in districts. While these are observable, there may be unobservables at the geographic (district) level, household level and even some child level unobserved heterogeneity. My specification thus includes time invariant child (ϵ_i), household (ϵ_h) and district (ϵ_d) characteristics.

Intent-to-treat (ITT) estimates the average effect of the treatment on the outcome variable of interest for all eligible individuals irrespective of actual participation in the program. While it is desirable to estimate the impact of actually participating in the program (Treatment Effect on the Treated) both because that would be more realistic given my theoretical model and from the perspective of the NREGS literature, the ITT

[38] Like above, the impact of the shock in the absence of the NREGS is given by β_1 while the impact of the shock with NREGS access for one year is given by $\beta_3 + \beta_2 - \beta_1$. The impact for two years of NREGS exposure is $\beta_5 + \beta_4 - \beta_1$

estimate is useful from the viewpoint of policy. As Azam (2012) and Yamano et al. (2005) point out, the ITT parameter is useful for policymakers designing similar policies for the same population because policymakers have little influence over actual participation by individuals.

5.3 Challenges

There are a number of challenges that arise while estimating the causal impact of the NREGS. The first is that of selection bias. There are two sources of selection bias that need to be considered. One, as mentioned before, assignment to NREGS was non-random. The poorest districts were covered first under the program. However as outlined in other studies dealing with the NREGS (Mani et al., 2014 and Azam, 2012) if we assume that program placement is correlated with time invariant individual, household and community level characteristics which enter additively into my specification then using a fixed effects model should resolve the problem. This is because a fixed effects specification will eliminate the program placement effect leaving only the causal effect of the NREGS in buffering the impact of an income shock on child nutrition. The second source of selection bias is that participants in the NREGS are not randomly assigned. It is possible that people less able to cope with shocks are more likely to participate in program thus biasing simple OLS estimates of the causal impact. This however, as mentioned above can be dealt with by examining the overall intent-to-treat effects of the program rather than the treatment effect on the treated which depends on actual participation.

The other main challenge with my empirical analysis is that the number of districts (for which I have data) over which the NREGS was implemented is small (six districts). This means that the usual methods which are employed to correct for clustering often lead to an over-rejection of the null hypothesis of no effect when the number of clusters is small (Cameron et al., 2008). Following Cameron et al. (2008) the standard practice is to report the robust bootstrapped standard errors clustered at the district level. However since these were about the same as the robust clustered standard errors, I report the latter (where the number of clusters/districts is six).

6 Discussion of findings

6.1 Overall impact on child nutrition

Tables 4 & 5 show the results for running both the above mentioned specifications. As seen from column 2 in Table 4 being exposed to an income shock significantly reduces the height-for-age z-score. For those without access to the NREGS, a one standard deviation change in rainfall from its mean results in loss of height-for-age z-score by about 0.50 standard deviations. On the other hand, for those who did have access to the NREGS, the impact of the shock is about 0.08 standard deviations[39]. That is, the impact of the shock is differential based on NREGS access. Having access to the NREGS mitigates the adverse impact of the shock on child health.

6.2 Multi-year program impacts

Table 5 reports the multi-year impacts of the program. The coefficients for each year of NREGS exposure and the mitigating role of the NREGS are cumulative and not marginal. The impact of exposure to the NREGS in all years is significantly different from zero. The mitigating role of the NREGS in the event of a shock is clear. In the absence of the program, an income shock would have led to a decline of height-for-age z-scores by 0.61 standard deviations. Exposure to one year of the NREGS in the event of a shock, led to a positive and statistically significant differential impact of 0.32 standard deviations in height-for-age z-scores[40]. Thus the shock fails to adversely impact children if they have access to the NREGS.

Being exposed to two years of the program led to a similar positive and statistically significant improvement of 0.50 standard deviations for those who had access to the NREGS during a shock. The joint test that both coefficients are zero is rejected. However height-for-age increases by less in the second year[41]. Moreover, the coefficients for the first and second years of exposure are significantly different than each other.

Since the impact of the second year is lower than that of the first year, this suggests that there is a decay in the short run effects. That is, the late phase-in districts tend to catch up to the early phase-in districts and there are no long term persistent effects of the program in buffering children against income shocks. This

[39] The magnitude of the impact is found by subtracting the impact of the shock from the impact due to the NREGS (0.58-0.50)=0.08 standard deviation increase in height for age.

[40] The magnitude of the impact is found by subtracting the impact of the shock from the cumulative impact due to the NREGS (0.871 + 0.058 - 0.610)=0.32 standard deviation increase in height for age.

[41] Impact of second year = Cumulative impact - impact of first year = 0.50 - 0.32 = 0.18 standard deviations increase in height-for-age.

however ties in with what one would expect given the dual nature of the NREGS in enhancing capabilities and serving as a safety net. Since I'm analyzing the same cohort of children and their families it is possible that over time the NREGS has enabled these households to become better equipped in dealing with shocks. This can be either through the enhanced income that the NREGS provides or its implicit role as an insurance mechanism which makes households less risk averse and more able to cope with shocks. It is possible thus that for the same household, utility of the NREGS as a safety net may diminish over time as they enhance their capabilities. Thus there is no differential impact of the shock because of NREGS access in the long run.

Moreover, improvements in height-for-age are most significant when children are younger and therefore between 2007 and 2009-10 (the time frame of the medium run effects) when the children in my sample are between 5 and 7 years, there might not be significant improvements in height. This is even more conceivable because in both my specifications, the coefficient on age is negative which means the older the child the lower is the increase in height-for-age.

Columns (3) and (2) in Tables 4 & 5 detail the regression results for both specifications after including the food supplement variable. As is evident my results don't change much and the mitigating effect of the NREGS is still positive and statistically significant. However, the supplement variable is also positive and statistically significant implying that access to supplementary feeding programs like the ICDS and the Mid-Day Meal Scheme contribute significantly to children's height attainment.

Within the described controls, I perform a series of F-tests of the null hypothesis that the coefficients of these variables are jointly equal to each other. I reject the null hypothesis that the coefficients of the time dummies are jointly zero (p-value=0.000). This implies that there are significant time effects. I am unable to reject the null hypothesis that all the individual level controls are jointly equal to zero (p-value=0.3775). I also fail to reject the null hypothesis that all the household level controls are jointly zero (p-value=0.2474). Finally, I reject the null hypothesis that the two variables of interest, access to NREGS and the income shock are jointly zero (p-value=0.000).

These results validate those that have been found in similar studies. For instance Dasgupta et al. (2013) finds an improvement of 0.27 standard deviations in height-for-age as a result of the NREGS in India. Quisumbing (2003) finds that food-for-work and food aid have positive impacts on weight-for-age for children between 0-5 years from low-asset households in Ethiopia. Similarly Yamano et al. (2005) also find that food aid significantly helps in mitigating the impacts of income shocks on height-for-age z-scores in rural Ethiopia.

6.3 Heterogeneous impacts —By wealth quartiles, land ownership and gender

The role of the NREGS in helping mitigate the impacts of a shock may hide large heterogeneity of impacts across households belonging to different socio-economic groups. In order to address this I run both my specifications by an indicator of household wealth, gender and land ownership. I construct sub-samples of children who belong to households with less than the median and more than the median wealth level index (the median is the sum of the first two wealth quartiles and is based on the pooled sample which includes all three years).

The results are given in Tables 6, 7 & 8. The mitigating impact of the NREGS on children's height after an income shock is positive and statistically significant. Ideally one would expect that the NREGS should be most beneficial for the poorest. However, contrary to expectations, the mitigating impact of the NREGS is higher for households with wealth levels higher than the median level. That is, for richer households, the differential impact of the shock for those who have NREGS access is 0.08 standard deviations while for the poorer households the improvement is only 0.07 standard deviations. This can be explained by a number of factors. One, public works in developing countries are plagued by implementation problems like corruption and underpayment of wages. If true, then this attenuates the mitigating impact that the NREGS has in the event of a shock, especially for the poor who are more dependent on the NREGS after a shock relative to richer households. Two, an audit by the Indian government in 2012 reveals that awareness (on a relative basis) about the NREGS and its entitlements is still very low. Richer households with better connections (political and social) might be more aware of the program and thus more able to access it in the event of a shock as opposed to poorer households. Three, using nationally representative data Dutta et al. (2012) have found that the demand for NREGS far outstrips the supply which leads to rationing of projects. This leads to rent seeking and consequently richer households are better placed to find work than poorer households after a shock. Thus, the overall results I find above are significantly driven by the sub-sample of those children who belonged to richer households[42] for whom the impact of the NREGS contributes more to improvements in child health than the total sample.

I find a similar differential impact when I disaggregate my sample by gender. Columns (3) & (4) of Table 6 detail the results of my specifications. The mitigating impact of the NREGS is higher and statistically significant for boys relative to girls. This implies there is an asymmetric burden of shocks on the girl child. That is, for those who had access to the NREGS after a shock, there is an improvement of 0.12 standard deviations in height-for-age z-scores for boys compared to 0.04 for girls. This ties in with our expectations

[42]It is important to remember that here 'richer' is used in a relative sense because my entire sample consists of the rural poor.

about the male preference bias in Indian households. While because of data constraints I don't examine it here, but it would be interesting to see if the increase in female labour force participation that the NREGS has seen leads to an increase in the bargaining power of women and an improvement in female child health.

Table 7 highlights multi-year heterogeneous impacts. I find similar results to those found above. The impact of the shock for those who were exposed to the NREGS in the first year is 0.39 for those with wealth less than the median wealth level and 0.49 for those with wealth more than the median level.

The results from decomposing the sample by land ownership are given in Table 8. The mitigating effect of the NREGS is significant for households both above and below median land ownership. For those with land less than the median level, the impact of the shock for those with access to the NREGS is 0.15 standard deviations. The analogous figure for those with land more than the median level is -0.02 standard deviations. Thus, households with smaller plots of land seem to do better during a shock than those with larger plots of land. There are several explanations for this. One, assuming land quality is similar across various plots of land[43], those with smaller plots of lands are poorer in a relative sense. When agricultural wages fall because of a productivity shock, the income effect leads workers to supply more labour while the substitution effect makes them supply less of it. For poorer households the income effect outweighs the substitution effect. The presence of the NREGS thus benefits them disproportionately more. Two, those with more land are going to be worse off in the case of an income shock such as highly variable rainfall. This is because, households with smaller plots of land may be less affected by a rainfall shock as they depend less on agriculture to meet their subsistence requirements. They are consequently less impacted by a shock that lowers agricultural productivity and hence overall profits.

6.4 Accounting for lagged height

In line with my theoretical model I also consider including the lagged dependent height-for-age z-score as an explanatory variable. This is because height in period *t+1* is a function of height attained till a period before. Thus the health production function from Section 3 is dynamic because it includes lagged health as an explanatory variable for current health (Yamano et al., 2005, Quisumbing, 2003 and Alderman et al., 2006). Thus following Quisumbing (2003) I use the Arellano-Bond estimator[44]. Since I have only three years

[43] While this is not a reasonable assumption to make in most cases, since my sample is from one state in India it is reasonable to assume that there is less heterogeneity in terms of land quality. However, ideally one would want to control for land quality before any robust conclusions can be drawn.

[44] The Arellano and Bond (1991) dynamic panel data estimator addresses the problem of correlation of the lagged dependent variable with the error term by first differencing to remove the individual-specific random effects, and then using lagged levels of the dependent variable and predetermined variables and differences of the strictly exogenous variables as instruments.

of data, I use the Anderson-Hsiao/Difference-GMM and use height-for-age in period *t-2* as an instrument for the lagged endogenous variable, H_{it-1}. The results of running this model are given in Column (3) in Table 5. The impact of the shock is negative for those without access to the NREGS. This however is exactly offset by having access to the NREGS in the first year. The net impact on height-for-age z-scores is hence zero.

6.5 Robustness Checks

In order to test that the treatment effects identified above are consistent and unbiased, I perform robustness checks in this section. Thus I estimate the above analysis using a placebo group (instead of the actual treatment group) comprising of the older cohort of children from the Young Lives study. Table 8 reports the results of running the fixed effects model with the 'fake' treatment group. Given that the treatment group now consists of older children, one would expect that there are no mitigating impacts of the NREGS as older children are not affected by nutrition as much. In line with expectations, the buffering impact of the NREGS is not statistically significant. Since the treatment group in this case consisted of older children an income shock does not have a statistically significant impact on their health. Therefore the mitigating effect of the NREGS is insignificant as well.

6.6 Testing Parallel Trends

The key assumption with the fixed effects specification is that of parallel trends. That is, in the absence of the NREGS the treatment and control districts would have had parallel time trends and it is the introduction of the NREGS that introduced a deviation in that trend. I test the parallel trend assumption both using the pre intervention data from the Young Lives data (first wave of the survey 2002) and using the Indian Human Development Survey (IHDS), 2005. The IHDS was administered between November 2004 and October 2005, and the first phase of the NREGS was rolled out in September 2005. So the IHDS is suitable to test for pre-intervention time trends. Out of the six districts in my sample the IHDS was administered in five of those excluding Srikakulam. I plot the mean height[45] for children from the early phase-in and late phase-in districts in Figure 1. Figure 2 plots the height-for-age z-scores for the early and late phase-in districts using Round 1 (pre-NREGS) of the Young Lives data. Both figures, for the most part, highlight parallel trends between the two groups. Given these, it is reasonable to assume in this context that pre-intervention outcomes follow parallel trends.

[45]The IHDS recorded anthropometric outcomes differently than the Young Lives data. The data does not contain z-scores but only has absolute numbers measured in centimeters.

7 Policy Implications

Public works programs have a long history. More recently they have evolved into policy instruments for generating employment and alleviating long term poverty. Well designed programs provide income transfers to poor households in periods of critical need. Evidence on their use as safety nets, however is low. In this regard, this paper tests the effectiveness of one such program. Using longitudinal data from the 2002, 2007 and 2009-10 waves of the Young Lives Survey in Andhra Pradesh, India I assess the effectiveness of the National Rural Employment Guarantee Scheme (NREGS) in mitigating the adverse impacts of income shocks on child health.

The main finding in this paper is that the NREGS has large and positive mitigating effects on child health in the aftermath of a shock. This has important policy implications. First, while the NREGS wasn't designed to be a program that helps in tackling child malnutrition, its ability to function effectively as a safety net means that it can prevent large fluctuations in child health. If this were to be combined with the proper functioning of on-site child care facilities then this would further enhance the ability of the NREGS to buffer rural households. Second, from my empirical analysis richer households benefit more from the mitigating impact of the NREGS than poorer households. This suggests rent seeking and indicates that there are implementation and institutional problems with the program. Given that the poorest are the least able to cope with shocks it is important that the program should benefit them positively as well. This differential impact suggests that the way in which public programs are designed is of vital importance and has consequences beyond the immediate aims of these programs. Third, while the results derived in this paper rest on the unitary household assumption, the NREGS in practice leads to increased female labour supply. This implies that the impact of the NREGS could be stronger given that an increase in labour supply could enhance the bargaining power of females and thus lead to improved child outcomes. Finally, given that the early phase-in districts see large impacts of the NREGS it therefore implies that the timing of these programs is critical. The gains from receiving the program early on are significant and policymakers need to be cognizant of this.

8 Appendix

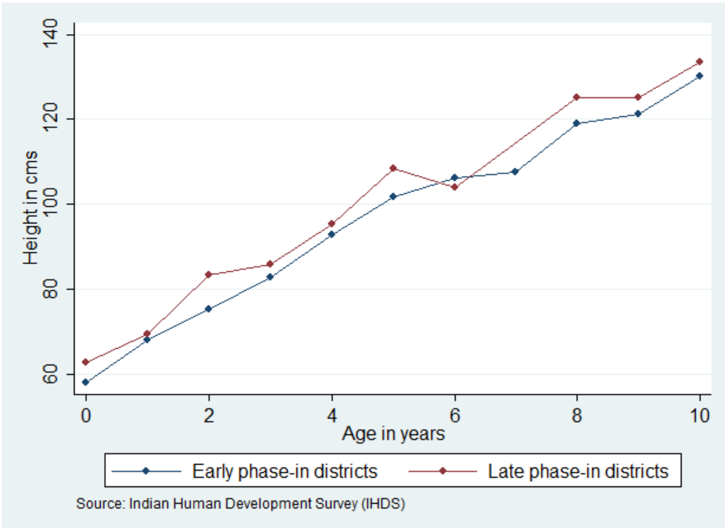

Figure 1: Pre-intervention (2004) Height in cms by Age in Years

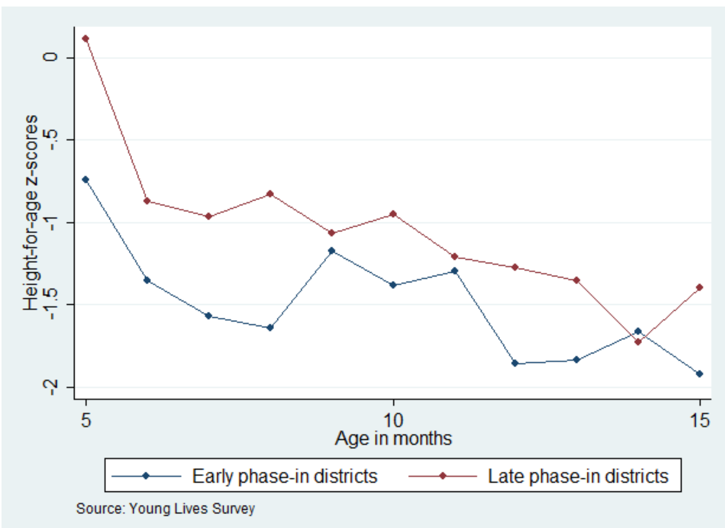

Figure 2: Pre-intervention (2002) Height-for-age by Age in months

Table 2: Summary statistics

Variables	2002 Mean (S.D.)	2007 Mean (S.D.)	2009-10 Mean (S.D.)
Height-for-age z-score	-1.43	-1.76	-1.58
	(1.65)	(1.33)	(1.19)
Gender	0.53	0.53	0.53
	(0.50)	(0.50)	(0.50)
Age (in months)	11.83	64.29	95.15
	(3.53)	(3.78)	(3.76)
SC/ST dummy	0.39	0.37	0.37
	(0.49)	(0.48)	(0.48)
OBC dummy	0.48	0.49	0.51
	(0.50)	(0.50)	(0.50)
Religion dummy	0.92	0.92	0.97
	(0.26)	(0.16)	(0.16)
Schooling Head dummy	0.30	0.50	0.50
	(0.46)	(0.50)	(0.50)
Household size	5.65	5.67	5.68
	(2.47)	(2.33)	(2.34)
Debt dummy	0.61	0.75	0.38
	(0.49)	(4.19)	(0.48)
Land owned (acres)	2.73	1.91	3.04
	(2.87)	(2.68)	(3.97)
Supplement dummy	0.42	0.44	0.44
	(0.49)	(0.50)	(0.50)
Wealth Index	0.14	0.17	0.27
	(0.15)	(0.16)	(0.17)
Rainshock	-0.12	0.57	0.59
	(0.76)	(0.83)	(0.93)

Table 3: Summary statistics contd.

Variable	Early Phase-in Districts	Late Phase-in Districts	t[a]
Height-for-age (in 2002)	-1.30	-1.61	-3.97*
Height-for-age (in 2007)	-1.82	-1.63	2.84*
Height-for-age (in 2009-10)	-1.63	-1.48	2.21*
Height-for-age if caregiver completed primary	-1.50	-1.37	2.22*
Height-for-age if caregiver not completed primary	-1.66	-1.73	-1.34
Height-for-age (Females)	-1.51	-1.48	0.44
Height-for-age (Males)	-1.65	-1.69	-0.64
Gender	0.53	0.52	-1.12
Age (in months)	56.07	56.33	0.21
SC/ST dummy	0.34	0.42	4.77**
OBC dummy	0.49	0.49	-0.17
Religion dummy	0.96	0.95	-1.70
Schooling Head dummy	0.45	0.39	-3.96*
Household Size	5.84	5.30	-6.79**
Debt dummy	0.66	0.41	-3.12*
Land owned (acres)	2.89	1.55	-11.31***
Supplement dummy	0.37	0.56	11.31***
Wealth Index	0.39	0.38	-2.72*

[a] Test of equality of means, *Significant at the 5 percent level

Table 4: Regression Results: Specification I
Dependent Variable: Height-for-age z-score

Variables	(1) OLS	(2) FE	(3) FE
NREGS	-0.003	0.093	0.081
	(0.144)	(0.187)	(0.183)
Shock	-0.477**	-0.496**	-0.507***
	(0.170)	(0.133)	(0.122)
NREGS*Shock	0.428**	0.576***	0.583***
	(0.141)	(0.079)	(0.069)
Debt	-0.007**	0.008**	0.008**
	(0.002)	(0.002)	(0.002)
Age	-0.024*	-0.009	-0.008
	(0.011)	(0.022)	(0.023)
Wealth Quartile 1	-0.404***	-0.174	-0.172
	(0.077)	(0.091)	(0.092)
Wealth Quartile 2	-0.191	-0.108	-0.111
	(0.110)	(0.091)	(0.091)
Wealth Quartile 3	-0.147**	-0.056	-0.053
	(0.050)	(0.033)	(0.036)
SC/ST	-0.221**	0.047	0.026
	(0.075)	(0.088)	(0.079)
OBC	-0.339**	0.024	0.019
	(0.090)	(0.064)	(0.060)
Gender	-0.161**		
	(0.061)		
Geligion	-0.009	-0.036	-0.040
	(0.233)	(0.110)	(0.107)
Schooling Head	0.221**	0.049	0.052
	(0.062)	(0.068)	(0.060)
Household Size	-0.019*	-0.001	-0.003
	(0.007)	(0.010)	(0.011)
Land Owned (acres)	0.010	-0.006	-0.006
	(0.009)	(0.005)	(0.005)
Food Supplement			0.139
			(0.083)
Constant	-0.533	-1.206***	-1.257***
	(0.325)	(0.240)	(0.255)
Year Dummies	Yes	Yes	Yes
Observations	3,232	3,232	3,232
R-squared	0.115	0.145	0.149

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Regression Results: Specification II
Dependent Variable: Height-for-age z-score

Variables	(1) Diff-in-diff	(2) Diff-in-diff	(3) Difference-GMM
Shock	-0.610**	-0.606***	-0.116**
	(0.167)	(0.149)	(0.052)
Time1	0.058	0.013	-0.153***
	(0.103)	(0.100)	(0.028)
Time2	0.416	0.311	
	(0.251)	(0.210)	
Time1*Shock	0.871**	0.859***	0.261***
	(0.228)	(0.203)	(0.089)
Time2*Shock	0.593***	0.584***	0.018
	(0.113)	(0.098)	(0.042)
Food Supplement		0.139	
		(0.080)	
Debt	0.008**	0.009**	0.001
	(0.002)	(0.002)	(0.002)
Age	-0.016	-0.015	-0.035
	(0.024)	(0.025)	(0.025)
Wealth Quartile 1	-0.177	-0.176	-0.079
	(0.092)	(0.094)	(0.059)
Wealth Quartile 2	-0.113	-0.117	-0.084
	(0.092)	(0.092)	(0.071)
Wealth Quartile 3	-0.060	-0.056	-0.045
	(0.033)	(0.035)	(0.036)
SC/ST	0.033	0.010	-0.240***
	(0.088)	(0.082)	(0.045)
OBC	0.013	0.006	-0.114***
	(0.064)	(0.060)	(0.043)
Religion	-0.013	-0.022	
	(0.128)	(0.123)	
Schooling Head	0.042	0.048	
	(0.072)	(0.063)	
Household Size	-0.001	-0.003	0.015
	(0.009)	(0.010)	(0.186)
Land Owned (acres)	-0.006	-0.005	0.001
	(0.005)	(0.005)	(0.006)
Lagged Height-for-age			0.063**
			(0.029)
Constant	-1.150***	-1.190***	1.291
	(0.266)	(0.284)	(0.788)
Year Dummies	Yes	Yes	Yes
Observations	3,232	3,232	887
R-squared	0.150	0.154	

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Regression Results: Specification I (by Wealth Quartiles and Gender)
Dependent Variable: Height-for-age z-score

Variables	(1) FE (Wealth ≤ Median)	(2) FE (Wealth > Median)	(3) FE (Female)	(4) FE (Male)
NREGS	0.141	0.142	0.243	-0.054
	(0.161)	(0.261)	(0.195)	(0.186)
Shock	-0.549***	-0.469*	-0.458**	-0.530***
	(0.108)	(0.184)	(0.144)	(0.123)
NREGS*Shock	0.616***	0.545***	0.499***	0.647***
	(0.072)	(0.129)	(0.097)	(0.066)
Debt	0.001	0.010**	0.007	0.008*
	(0.003)	(0.004)	(0.006)	(0.003)
Age	-0.030	-0.005	0.001	-0.013
	(0.020)	(0.031)	(0.022)	(0.033)
Wealth Quartile 2	0.062		-0.083	-0.134
	(0.042)		(0.093)	(0.091)
SC/ST	0.200**	-0.446	-0.112	0.230
	(0.064)	(0.298)	(0.352)	(0.150)
OBC	0.087	0.098	-0.037	0.051
	(0.176)	(0.095)	(0.089)	(0.130)
Religion	-0.022	-0.127	-0.123	0.101
	(0.059)	(0.260)	(0.219)	(0.184)
Schooling Head	-0.065	-0.003	0.205*	-0.098
	(0.110)	(0.120)	(0.098)	(0.097)
Household Size	-0.070*	0.021	-0.002	0.005
	(0.032)	(0.025)	(0.018)	(0.022)
Land Owned (acres)	-0.004	-0.001	-0.026	0.006
	(0.018)	(0.004)	(0.022)	(0.014)
Wealth Quartile 3		-0.071**	-0.101*	-0.024
		(0.025)	(0.047)	(0.053)
Wealth Quartile 1			-0.214*	-0.132
			(0.101)	(0.081)
Constant	-0.880	-1.178***	-0.995*	-1.522***
	(0.442)	(0.180)	(0.423)	(0.335)
Year Dummies	Yes	Yes	Yes	Yes
Observations	1,654	1,578	1,497	1,735
R-squared	0.141	0.161	0.165	0.150

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Regression Results: Specification II (by Wealth Quartiles and Gender)
Dependent Variable: Height-for-age z-score

Variables	(1) DID (Wealth ≤ Median)	(2) DID (Wealth > Median)	(3) DID (Female)	(4) DID (Male)
Time1*Shock	0.931***	0.927**	0.820**	0.918***
	(0.160)	(0.312)	(0.229)	(0.219)
Time2*Shock	0.619***	0.581**	0.512***	0.651***
	(0.088)	(0.167)	(0.120)	(0.106)
Time1	0.141	0.056	0.202	-0.121
	(0.090)	(0.176)	(0.111)	(0.111)
Time2	0.509**	0.532*	0.571**	0.183
	(0.176)	(0.261)	(0.162)	(0.305)
Shock	-0.687***	-0.597**	-0.582**	-0.627**
	(0.134)	(0.211)	(0.164)	(0.165)
Debt	0.003	0.010**	0.007	0.008*
	(0.004)	(0.003)	(0.006)	(0.004)
Age	-0.042	-0.009	-0.010	-0.017
	(0.024)	(0.032)	(0.021)	(0.034)
Wealth Quartile 2	0.062		-0.093	-0.136
	(0.046)		(0.097)	(0.091)
SC/ST	0.160*	-0.465	-0.133	0.219
	(0.072)	(0.291)	(0.356)	(0.148)
OBC	0.035	0.094	-0.047	0.038
	(0.167)	(0.088)	(0.080)	(0.142)
Religion	0.014	-0.096	-0.087	0.104
	(0.065)	(0.282)	(0.242)	(0.190)
Schooling Head	-0.075	-0.008	0.197*	-0.102
	(0.111)	(0.124)	(0.097)	(0.102)
Household Size	-0.069*	0.022	-0.003	0.006
	(0.032)	(0.025)	(0.018)	(0.022)
Land Owned (acres)	-0.002	-0.001	-0.025	0.006
	(0.018)	(0.004)	(0.023)	(0.014)
Wealth Quartile 3		-0.069**	-0.111*	-0.021
		(0.026)	(0.045)	(0.053)
Wealth Quartile 1			-0.229*	-0.127
			(0.106)	(0.086)
Constant	-0.742	-1.185***	-0.889*	-1.480***
	(0.439)	(0.212)	(0.409)	(0.349)
Year Dummies	Yes	Yes	Yes	Yes
Observations	1,654	1,578	1,497	1,735
R-squared	0.147	0.169	0.172	0.154

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8: Regression Results (by Land Ownership)
Dependent Variable: Height-for-age z-score

Variables	(1) FE (Land $\leq$ Median)	(2) FE (Land > Median)
NREGS	0.073	0.129
	(0.142)	(0.343)
Shock	-0.563***	-0.515*
	(0.109)	(0.206)
NREGS*Shock	0.715***	0.494**
	(0.062)	(0.136)
Debt	-0.005	0.010**
	(0.003)	(0.003)
Age	-0.003	-0.042
	(0.021)	(0.026)
Wealth Quartile 1	-0.055	-0.158
	(0.066)	(0.119)
Wealth Quartile 2	-0.045	-0.072
	(0.081)	(0.158)
Wealth Quartile 3	0.045	-0.109
	(0.036)	(0.073)
SC/ST	-0.211**	0.401**
	(0.082)	(0.151)
OBC	-0.074	-0.045
	(0.146)	(0.147)
Religion	-0.197	0.017
	(0.172)	(0.119)
Schooling Head	0.085	0.076
	(0.073)	(0.084)
Household Size	0.014	0.001
	(0.016)	(0.011)
Land Owned (acres)	-0.062	0.002
	(0.063)	(0.003)
Constant	-1.132**	-0.964*
	(0.303)	(0.387)
Year Dummies	Yes	
Observations	2,104	1,128
R-squared	0.135	0.235

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Regression Results: Robustness Check
Dependent Variable: Height-for-age z-score

	(1)
Variables	FE
NREGS	0.160*
	(0.047)
Shock	0.028
	(0.027)
NREGS*Shock	0.002
	(0.055)
Debt	0.047**
	(0.017)
Age	-0.005***
	(0.001)
Wealth Quartile 1	-0.116
	(0.117)
Wealth Quartile 2	-0.048
	(0.062)
Wealth Quartile 3	-0.056*
	(0.025)
SC/ST	-0.496***
	(0.044)
OBC	-0.240***
	(0.058)
Gender	0.085
	(0.073)
Religion	-0.017
	(0.094)
Schooling Head	0.182***
	(0.018)
Household Size	0.024*
	(0.010)
Land Owned (acres)	0.001*
	(0.000)
Constant	-1.253***
	(0.194)
Year Dummies	Yes
Observations	2,097
Number of childid	704
R-squared	0.069

Clustered robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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