YOUNG LIVES STUDENT PAPER

Is the NREGS a Safety Net for Children?

Studying the access to the National Rural Employment Guarantee Scheme for the Young Lives families and its impact on child outcomes in Andhra Pradesh

Vinayak Uppal

May 2009

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

The data used in this paper comes from Young Lives, a longitudinal study investigating the changing nature of childhood poverty in Ethiopia, India (Andhra Pradesh), Peru and Vietnam over 15 years. For further details, visit: www.younglives.org.uk.

Young Lives is core-funded by the Department for International Development (DFID), with sub-studies funded by IDRC (in Ethiopia), UNICEF (India), the Bernard van Leer Foundation (in India and Peru), and Irish Aid (in Vietnam).

Young Lives
An International Study of Childhood Poverty

Is the NREGS a Safety Net for Children?

Studying the access to the National Rural Employment Guarantee Scheme for Young Lives families, and its impact on child outcomes in Andhra Pradesh

Thesis submitted in partial fulfillment of the requirements for the Degree of Master of Science in Economics for Development at the University of Oxford.

Vinayak Uppal

May 2009

Acknowledgements

This paper has benefited from the help and support of numerous people. I would like to thank Prof Stefan Dercon for his patience and guidance in supervising this essay. I would like to thank Dr. Francis Teal for his many comments and suggestions and Dr. Christopher Adam for his support as my MSc. supervisor. A big thanks to Abhijeet who helped at various stages of this paper and Arvind for all the support and help.

All errors are my own. I would appreciate comments and feedback at:
vinayakuppal@gmail.com

Abstract

The NREGS is an ambitious public works program intended to provide a basic safety net to the rural poor in India. Institutionalised in 2005, it is currently the largest public works program in the world. Despite its scale and the political importance attached to the program, its success in targeting and its impact on participating households has not been well studied. This paper attempts to study two aspects of the program's functioning using data from the Young Lives longitudinal Study conducted in Andhra Pradesh. Firstly it looks at the targeting of the program and the characteristics of those who self select into it. We find that poorer and lower caste households are more likely to register as are those affected by drought. We also find that having more than 5 influential relatives increases the probability of registration by 10.3 percentage points. We next attempt to estimate the impact of program participation on the children in participating households, looking specifically at anthropometric scores as indicators of health outcomes, and the incidence of child labour. While there seems to be a positive correlation between program participation and health outcomes, this does not remain robust across specifications. On the other hand we find that program registration reduces the probability of a boy entering child labour by 13.4% points and program take up reduces it for girls by 8.19% points.

We find that the targeting efficiency of the program seems to be largely effective and it seems to offer a viable security net for households with variable employment opportunities. It also seems to have an important effect on children, further strengthening the program's significance.

Table of Contents

1. Introduction

Public Works Programs (PWPs) have been on the policy menu in India for millennia. The 'Arthashastra', written by Chanakya in 4th century B.C. mentions welfare measures that a ruler needs to undertake in times of famine and duress. (Dreze and Sen, 1989) In more recent times, the Famine Code of 1880 advocated the use of work camps, and the putting in place of famine 'tests' which only those most in need would pass. Post independence there have been numerous similar attempts to ensure a minimum level of security of livelihoods and to prevent against famines and other adverse shocks. The most successful of these was the Maharashtra Employment Guarantee Scheme (MEGS)[1]. The most ambitious program yet, The National Rural Employment Guarantee scheme (NREGS), was launched with great fanfare in 2005, and is seen as a revolutionary step in creating a safety net and securing livelihoods for India's poorest households.

The largest public works program in the world, the scheme is truly gigantic in scale, having reportedly provided employment to over 43.5 million households, generating employment of over 2.04 billion person-days and over 2.5 million works taken up. Politically, it is the current United Progressive Alliance (UPA) government's flagship rural development and poverty alleviation program, and has been touted as one of the primary reasons for its recent reelection.

While the program has generated considerable debate in policy discussions about poverty in India and elsewhere, it has not been adequately analysed, and almost no quantitative studies exist. The goal of this paper is to look at two primary questions regarding the program. The first is to try and estimate access to the scheme. Who are the people who actually self select into the program, and is it being targeted to those who most need it? The second is a more conventional treatment evaluation but with a specific focus, namely children.

[1] This is discussed in greater detail in section 2

Participation is not a simple matter in a self-targeting scheme of this kind, since past cases (see Lanjow and Ravallion, 1998 and Ravallion, 1991) have seen issues like elite capture, corruption and rationing arise. This paper attempts to look at what characteristics determine program participation including possible effects that arise from network variables. With specific regards to the NREGS, Jha, et al (2008) attempt to study how the self targeting objective is faring but do so with an extremely small sample of three villages in Rajasthan while Shariff (2008) attempts something similar with a larger sample from seven states across the country. Neither however have a baseline study, and neither looks into the potential impacts of the program focusing solely on the participation aspect.

In terms of the impact of the program on children, we look specifically at the effect on health outcomes as well as on the incidence of child labour. Both of these could have ambiguous effects as laid out in models by Brown et al (1994) and Ravallion, M and Q Wodon (2000). This paper attempts to assess empirically what these effects are.

This paper uses the Young Lives Data set, an extensive longitudinal study that offers a baseline and post intervention survey with a variety of household and child specific details. The paper makes important contributions as it uses this to look at the characteristics of those who self select into the program. More importantly it also focuses on the impact program participation has on children in the household, something which has largely been ignored in the existing literature, and could have potentially important long-term benefits.

The paper is structured as follows: the next section gives an outline of the program structure and background, section three discusses the relevant literature; section four describes the data, discusses econometric issues and presents the results along with a brief discussion; the last section concludes.

2. The Program and Background

Public works programs have been used in countries of varying income levels, and with numerous objectives including short term income generation, asset creation, protection from negative shocks (natural disasters, droughts, macro- economic etc.) and poverty alleviation. (Ninno et al, 2009) India has, post independence, used public works programs extensively[2], often to mitigate adverse shocks like drought that can otherwise have significant negative consequences. (Gilligan and Hoddinott 2006) The NREGS, the largest and most ambitious such program till date, came into force in February 2006 under the legislative backing of the National Rural Employment Guarantee Act (2005), whose preamble states that it is an "An Act to provide for the enhancement of livelihood security of the households in rural areas of the country by providing at least one hundred days of guaranteed wage employment in every financial year to every household whose adult members volunteer to do unskilled manual work"(GOI, 2005)

The act was seen as a landmark in ensuring a minimal level of stability and security to incomes in rural households, especially in lean agricultural seasons, and was the outcome of a long and sustained effort by activists, academics and political leaders from across the country. The scheme has consciously attempted to counter weaknesses of earlier programs through several features in its design. It introduced a rights-based framework with legislative backing. It also incorporated time bound action to fulfill guarantee of work within 15 days of demand for work and a disincentive for non- performance (Mehrotra 2008, Ambasta et al 2008).

The program attempts to target numerous different objectives, as the Comptroller and Auditor General's report (2007) points out "The basic objective of the Act is to enhance livelihood security in rural areas …This work guarantee can also serve other objectives: generating productive assets, protecting the environment, empowering rural women, reducing rural-urban migration and fostering social equity, among others."

The scheme was initially rolled out in 200 of the poorest districts in early 2006, making use of a backwardness index developed by the Planning Commission. It was then expanded to an additional 130 districts in 2007, and finally expanded to cover the remaining 274 districts in 2008.

Once the program is available in a district there are no fixed eligibility criterion. To take advantage of the scheme a household has to register with its local *gram panchayat* (village council), stating the names of all adult household members who are willing to work under

[2] See Dreaze and Sen, 1989 and Ravallion, 1991 for good surveys

the scheme. The household is then issued with a unique identifying number that entitles it to apply for work. When any household wishes to take up work under the scheme, it needs to make an application to the Gram Panchayat stating its intent to work and the requested number of days it wishes to undertake, which the Gram Panchayat is then legally bound to provide.

There is therefore a selection problem that arises here at two levels, first from the targeted rollout of the program and secondly from the self-selection mechanism by which the scheme operates giving rise to potential econometric issues. These are discussed in greater detail in section four.

The NREGS has unfortunately not yet been the subject of adequate rigorous empirical study so there is limited literature regarding its targeting efficiency or impacts. Existing studies on the NREGS have tended to primarily focus on the administrative and operational aspects like the selection of worksites, physical asset creation, payment of wages on time and minimum stipulated amounts, social audits and transparency and anti corruption measures. They have also largely been conducted on a small scale and case study basis, and are thus unsuitable for an econometric study. The Comptroller and Auditor General of India, which is the primary audit institution in the country, carried out a comprehensive and detailed audit of the scheme in 2007 (CAG, 2007) but focused almost solely on its operational aspects. Scattered reports have begun to emerge of singular instances of the NREGS providing benefits in one or the other village, such as preventing migration or increasing crop under irrigation. (PACS, 2008)

Murgai and Ravallion (2005b) recognise that, in all likelihood, the only effective way to achieve a more-or-less binding minimum wage rate for the poorest in the developing world is for the government to act as the "employer of last resort." The government commits to employ the entire excess supply of unskilled labor at the stipulated wage rate. Murgai and Ravallion (2005) attempt to study the potential cost and poverty reduction impact an employment guarantee scheme would have in rural India.

While adequate work has not yet been carried out on the NREGS there is a fair amount of literature in existence on its predecessor, the Maharashtra Employment Guarantee Scheme

(MEGS). Originally introduced in 1965, when it was designed as a state-level response to adverse economic and demographic trends in rural Maharashtra, the MEGS was given statutory support by the passing of the Maharashtra Employment Guarantee Act in 1977.

The MEGS received much attention in the work of Dreze and Sen (1989) who comment extensively on the role of the MEGS in preventing starvation and famine in India in recent times, highlighting the crucial role to be played by such assured public works employment as a security net. Other studies have looked at the potential behavioural changes and net income gains caused by the program, and the issue of the dilution of the 'guarantee' aspect of the employment program (see Datt and Ravallion 1994 and Ravallion et al 1993)

The questions that we are interested in addressing – the targeting efficiency and effect of the NREGS on child outcomes – have only been partially analyzed in the literature, with impact on child outcomes being entirely ignored. In addition there have also been virtually no econometric studies of the NREGS carried out with large data sets, especially ones with baseline pre-program surveys. It is these gaps in our understanding of the scheme that this paper seeks to fill.

3. Relevant Research and Literature Review

Turning to the first research question of self-targeting, the scheme, in its initial phases, used geographical area as a targeting device. Certain districts were classified as 'poor' using an algorithm incorporating various factors, and then everyone within the area was treated identically and had the *opportunity* to participate in the program. Within the area self-targeting is achieved by making use of a workfare scheme. As Besley and Kanbur (1993) point out, this kind of scheme works by making a claimant of poor relief give up labour time in exchange for an income transfer. Ravallion (1991) notes that labour intensive rural public works projects have the potential to both screen and protect the poor, with the evidence suggesting few non-poor want to participate, while the direct and indirect transfer and insurance benefits to the poor can be sizeable.

Since the scheme depends on self-selection it is an interesting question in itself to analyse what the characteristics of the households who take advantage of the scheme are, and as a corollary, its Targeting efficiency. Studies on the MEGS (See Ravallion 1991 for a survey) suggest that participants were on average less well off than non participants, with certain studies suggesting that as many as 90 percent of participants came from below the poverty line. Similar work carried out on the NREGS, also suggests that the self targeting objective is being met. Shariff (2008) finds that holders of *Antyodaya* cards (issued to the poorest of the poor) are nearly 20% more likely to register for the program. Similarly, Jha et al (2008) find that the targeting of the program is far from dismal with disadvantaged groups such as the scheduled castes, scheduled tribes and landless households widely participating. These findings are however to be treated with a degree of caution since they were both carried out on preliminary and fairly limited cross sectional surveys.

On a national level it seems like the self-targeting objective is broadly working: While the share of scheduled castes (SCs) in India's population is 14 per cent, their share in households who received employment under the NREG is 27 per cent. In fact, while the share of scheduled tribes (STs) in the total population is only 8 per cent, they constituted 32 per cent of the total employed under the NREG. This is of particular significance since National Sample Survey (NSS) data for 2004-05 shows that 80 per cent of the poor in India are SCs, STs or Other Backward Classes (OBCs)(Mehrotra, 2008).

Other studies (Lanjow and Ravallion, 1998 and Caeyers and Dercon, 2008) have shown how local social and political processes impact on who gets access to transfer programs. Lanjow and Ravallion (1998), looking specifically at anti poverty and school participation programs in India show how there exists evidence of early capture of these programs by the non-poor. They conclude on this basis that expansion of these schemes would more than proportionally benefit the poor.

Coming now to the question of potential impacts, one can, in a simple analytical framework, make the connections between labour intensive public works to the health nutritional status of participating households using the idea of opposing income and subtitution effects. There is a direct positive income effect that occurs from wages/food earned from labour work. At the same time there is a potentially very serious negative substitution effect that occurs due to the labour time conditionality put on the earned income. One of the inputs into child health is time; directly in terms of child care as well as a complementary input to both food and nonfood nutrition inputs. In low-income situations it often becomes imperative for greater number of members to participate in income generating activities, especially women and children. (Brown et al 1994) This can have negative impacts on the children of participating households and further perpetuate impoverishment. It must be mentioned that the program design does try to counter this by assuring the presence of child care facilities at all sites where more than five children under the age of six are present, a move especially important given that the NREGS has a condition of atleast 33% of beneficiaries being women. (GOI, 2008) This does not however seem to be reflected in the data with only 8.74% of registered respondents reporting the availability of child-care centres on site.

Braun et al (1992) make a case for large public works in assuring food security in Africa. They argue that program design must ensure that women's employment does not adversely affect household health outcomes, and that overall increased incomes will have favourable effects on food consumption. The empirical evidence regarding the relationship between household calorie availability and external female employment is not very clear with studies in varied environments such as Ghana and Canada (Higgins and Alderan 1997, Brown at al 1994,Campbell and Horton (1991)) finding that female employment does seem to have a negative effect on the women's nutritional status and calorie availability. With specific regards to children, Brown et al (1994) looking at public works in Niger find that increasing the share of public works employment for women has positive impacts on child nutrition over and above its impact of increasing calorie availability; despite potential reductions in direct time inputs into child care.

The final impact that we are assessing in this paper is one of the most perverse, and possibly destructive indicators of poverty, child labour. An issue that is prevalent across the developing world, (Lieten, 2006) it has the potential of ensuring that a child is caught in a vicious cycle where a lack of education and work from a young age ensures future poverty.

The issue of child labour is a fairly pervasive one in India and is prevalent across regions. Studies suggest that work carried out by children is considered essential to maintaining the economic level of households, either in the form of work for wages, of help in household enterprises or of household chores in order to free adult household members for economic activity elsewhere. (Mehra-Kerpelman 1996) Official estimates put the number of children involved in manual work at 12.66 million under the age of fourteen (Census, 2001) while other agencies and NGOs put the figure much higher. (Global March Against Child Labor 2006) The state of Andhra Pradesh reportedly has 1.36 million child workers, which is just under 8 percent of the age group population in the state. The Young Lives data also has a high reporting of some child work, with 20.30 percent of the children in the older cohort reporting some form of renumerative work.

The effect of the NREGS on child labour in the region is not obvious with effects, as in the case of the health outcomes, which could go either way. On the one hand, the increased income and availability of work could ensure that children are not required to work to ensure the household's economic well-being. On the other, it could be argued that the requirement that the adult members expend labour time at a worksite could force them to substitute their time by increasing their children's participation in work on the household farm or other chores.

Ravallion and Wodon (2000) argue, in the context of a targeted enrollment subsidy in Bangladesh, that it is unclear on theoretical grounds whether a reduction in the price of schooling generated by a higher stipend will reduce child labor since the extra time spent at school may well come out of children's leisure. Their empirical findings reassert this, with a small stipend being enough to assure nearly full school attendance amongst participants. There was however no corresponding reduction in the incidence of child labor, though some evidence of this was also found. Parents are clearly substituting other uses of their children's time, so as to secure the current income gain from access to the program with modest impact on earnings from their children's work.

Anecdotal evidence from Rajasthan (Burra, 2006a) suggests a 20 per cent reduction in the incidence of migration amongst children and a corresponding increase in school enrolment and retention by 25 percent, but there is still a lack of clear empirical evidence either way with regards to the impact of the NREGS on child labour.

4. Empirical Section

4.1. Data and Descriptive Statistics

The Young Lives Study follows approximately 3000 children and their households in the state of Andhra Pradesh in Southern India. The study tracked 1008 children born in 1994-95 and 2011 children born in 2001-02 in two rounds in 2002 and 2006-07 respectively. Of these, 1950 children of the younger cohort and 994 children of the older cohort could be traced and resurveyed in the second round implying a low rate of attrition. Outes et. al (2008) show that the attrition in the Young Lives sample is, to a limited extent, non-random but when tested on empirical models is unlikely to generate significant attrition bias. In both rounds, an extensive household questionnaire was administered to capture various household characteristics. Regarding the NREGS, households were asked whether anybody in the household had registered for the NREGS, how many days were they employed for under the NREGS in the past 12 months, the wage rate they were paid for the work, whether they benefited from unemployment allowance, and whether they benefited from childcare facilities at the worksite, and from the worksite being available in their village.

The data was collected in six districts of Andhra Pradesh, chosen to represent the different regions and income levels within the state while households were chosen randomly amongst those which had children born in the stipulated years. Importantly, four of the six districts were covered by the NREGS by the first phase of implementation in 2005-06 itself. (Cuddapah, Karimnagar, Anantapur, Mahaboobnagar in Phase I, with the addition of Srikakulam in Phase II) Since the second round of the survey was carried out in 2006-07, the data predates the expansion of the scheme under Phase II and III that took place in 2007 and 2008. This paper therefore is looking specifically at the access to and impact of the scheme in its first phase in Andhra Pradesh.[3]

[3] There is the problem of a few households self-reporting registration in the scheme, even though it wasn't available in their cluster. To ensure robustness and to safeguard against measurement error, these observations are not considered in the sample.

While not collected for the specific study of the NREGS, the data have several strengths for our purposes: they cover just the right period with the introduction of the NREGS in between the two rounds; the extensive breadth of the surveys provides us adequate controls in estimation; and finally, no comparable panel data exist, to my knowledge, using which better estimates can be arrived at, making these data very important. More importantly the detailed information that this data provides regarding the educational, health, social, and other characteristics of the children are invaluable when attempting to study how a program of this kind impacts children. Children are one group that have been largely ignored in the literature, with few studies assessing possible effetcs. At the same time there is a growing literature that suggests that childhood health has a strong correlation with later socio economic status (see Currie and Moretti 2007, Black et al 2007, Behrman 1996), suggesting that any positive impact could give rise to crucial long term gains.

Sample selection bias may be an issue if the characteristics of households that had either 1 year old or 8 year old children in 2002 differed systematically, in a manner relevant for the evaluation of the NREGS, from those who did not. This could potentially be an issue since we would expect that parents (and thereby households) with children of a particular age would belong to a rather tight age-range themselves and therefore, to a particular stage of the life cycle. This may well affect labour decisions, as also decisions on NREGS participation. This would however mean only that our results should not be generalized to the entire population without very careful inspection and thought. They still however, remain valid for the age range we observe these households in and *especially for this cohort of children* since, within this age range, we are unable to see any other channels of potential bias. Given this paper's emphasis on the impact on children and the absence of other, possibly more suitable data, the use of Young Lives data for estimating program impact is justified.

Descriptive Statistics

Table 1: Household Characteristics and Program Availability and Registration[4] (Both Cohorts)

	Total Sample	EGS Available	EGS Not Available	t -statisitic of difference	EGS Registered	Not Registered	t -statisitic of difference
Male⁺	51.73	51.26	52.19	-0.50	51.52	51.86	0.1714
Wealth Index^	0.330	0.240	0.416	-28.19***	0.222	0.399	-23.28***
Scheduled Caste⁺	18.84	26.92	11.04	11.19***	32.69	11.8	14.081***
Scheduled Tribe⁺	11.85	6.97	16.57	-8.10***	6.92	14.39	-5.921***
Other Backward Caste⁺	48.20	48.68	47.74	0.50	49.39	47.67	0.8772
Other Castes⁺	20.93	17.43	24.31	-4.58***	11.00	25.88	-9.485***
Household Size^	5.41	5.70	5.13	7.29***	5.72	5.52	5.729***
Mother's Education^	4.27	3.87	4.66	-3.37***	4.02	4.40	-1.485*
Father's Education^	5.92	5.67	6.15	-2.06**	5.5	6.12	-2.572***
Casual⁺	30.42	36.47	24.58	7.04***	41.75	24.69	9.607***
Agriculture⁺	24.91	34.59	15.56	12.18***	35.23	19.72	9.279***
Observations	2919	1434	1485		982	1937	

Table 2: Health Status and Program Availability and Registration (Younger Cohort)

	Total Sample	EGS Available	EGS Not Available	t -statisitic of difference	EGS Registered	Not Registered	t -statisitic of difference
Stunting⁺	27.09	31.65	22.72	4.4361***	31.69	24.74	3.253***
Δ Height for age^	-0.3238	-0.5276	-0.1282	-7.0706***	-0.5293	-0.2218	-5.113***
Δ Weight for age^	-0.0235	-0.0697	0.0206	-2.2553**	-0.083	0.004	-2.053**
Observations	1934	948	986		650	1284	

Table 3: Child Labour and School Drop out Rates and Registration (Older Cohort)

	Total Sample	EGS Available	EGS Not Available	t -statisitic of difference	Egs Registered	Not Registered	t -statisitic of difference
Child Labour⁺	20.30	29.83	11.02	7.54***	30.72	14.90	5.9368***
Drop out ⁺	10.16	12.37	8.02	2.264**	12.68	8.91	1.8526**
Observations	985	486	499		332	653	

Note: *** p<0.1, ** p<0.05, * p<0.1. +- Percentages. ^- Averages

[4] EGS Available is defined as clusters where the program was operational from Phase I while EGS Not available is all other clusters. EGS Registered includes all households, which have registered for the NREGS, while Not Registered includes all households that have not, including in areas where the scheme was not available.

The above statistics immediately reveal some aspects of the program. The first is the seemingly successful geographical targeting of the scheme in its first phase. There is a clear indication in the descriptive stats above, that the families who lived in areas where the scheme was made available, were on average worse off in terms of the wealth index, educational attainments and health outcomes. They also had a higher concentration of scheduled castes, child labour and dropouts, and were more dependant on agricultural and casual labour for their livelihoods. Similarly the self-targeting mechanism of the scheme also seems to, at first sight be functioning. In terms of registration, the same indicators seem significant as in the case of program availability, though this could simply be reflecting the effectiveness of the geographical rargeting.

4.2 Econometric issues

There are various econometric issues that arise in this study, which must be countered before any of the results can be seen as credible. We use a variety of specifications and models to ensure robustness and to, within the given constraints, counter the potential issues that arise.

The issue of self-selection is probably the most important to deal with, and at the same time the most challenging. The primary source of selection bias arises from the fact that participating in the scheme is not a random occurrence but something that comes about due to selection on numerous levels.

By its very design, the scheme sets up two stages of potential selection. The first arises due to the targeted nature of the initial roll out of the scheme. It was first implemented in the 200 poorest districts of the country, and it is primarily individuals from this first batch of districts that makes up our sample of scheme participants. This can, and is, controlled for using cluster fixed effects. By estimating within cluster variation, we hope to be able to control for all local community level effects that might be playing a role.

To ensure that we can control for unobserved fixed effects we estimate a difference in differences equation in our estimation of the impact on health outcomes. In a two period panel, first differences (diff in diff) is computationally equivalent to individual and household fixed effects. By first differencing therefore we are able to remove all the bias that could potentially arise from time invariant child and household fixed effects, along with all time invariant unobservables at the cluster level. This implies that the equation used in the nutrition estimates can, as long as any time varying unobservables are uncorrelated to the regressors, give an unbiased and consistent estimate.[5]

Given the program design and data constraints, it is very difficult to use the traditional methods of instrumental variables or heckman correction. It is difficult to instrument for participation in the program since it was rolled out in a completely non-randomized manner, and any variable correlated with program participation, will also tend to be correlated with outcome variables of interest. Similarly, applying the heckman correction becomes problematic since it is unlikely to be possible to find a credible exclusion restriction. The two period panel also means that GMM methods cannot be applied to try and solve potential endogeneity issues. Given the data constraints this leaves us with only the option of controlling extensively for observable characteristics and cluster effects.

Leaving out the lagged health status of the child (round 1 z score) implies that the variable has a coefficient value of zero and that the child's health status starts afresh every period, with no relation to status in the previous period. Since this does not seem to be the case (Strauss and Thomas 2008, Singh 2008 and Kirchberger 2008) this omission leads to a dynamic misspecification. However, simple addition of the lagged health status variable (round 1 z scores), in turn leads to another econometric issue, namely endogeneity. Instrumenting the lagged dependent variable can potentially solve this problem; in this case instrumenting the child's anthropometric score from Round 1, by using the caregiver's perception of birth size.[6]

Omitted variable bias is another potential concern. This has been restricted through the

[5] This does however assume that the time-invariant unobservable characteristics have no trend effects, which could potentially be a source of bias.

[6] While birth weight would have been a more robust instrument than the perception of size, the quality and self-selection in the data means that it is not very viable. Less than half of the younger cohort report birth weight, which is a reflection of the fact that many of the births took place at home, and not in medical centres. Thus using birth weight would have meant an implicit selection of those born in medical centres which could be highly correlated with factors such as health awareness, connections etc.

usage of a broad and exhaustive range of controls, made possible by the breadth of the available data set. This includes time unvarying community and household characteristics, time varying characteristics and initial characteristics. There is also the possibility of heterogeneity in impacts. This would occur if the program had different impacts, depending on specific characteristics of the household. For example it could be possible that drought affected or lower caste households are impacted differently by the program. To control for this, various interactions are used in the specifications but are not found to have a substantial impact and are not reported due to space constraints[7].

4.3 Access and Take Up

4.3.1 Conceptual Framework and Econometric Specification

Using a similar strategy to Caeyers and Dercon (2008) we try to estimate the factors that predict registration in the NREGS, as well as the second step of actually undertaking work within the scheme. We estimate a conditional correlation equation expressing the probability of registering as a function of household characteristics based on a vector of household characteristics, a shock variable (drought) and a network variable (influential relatives) [8].

The estimation equation therefore is:

$$EGS\ Registration = \alpha + \beta_1 Drought + \beta_2 HH + \beta_3 infrel5 + \beta_4 Z + \mu \qquad (1)$$

Where *EGS Registration* is a dummy for registering in the scheme, *Drought* is a dummy for whether the household has suffered a drought in the past four years, *HH* is a vector of household characteristics, infrel5 is a dummy that is equal to 1 if members of the household have more than 5 influential relatives, and 0 otherwise. *Z* is a community level control, allowing us to capture within cluster variation and μ is an error term. The *HH* vector includes variables like caste, household size, literacy of parents and a wealth index. An interesting variable to note here is *EGSVillage*, which is whether there is a worksite in

---

[7] These include the interaction of program and drought variables, caste and drought variables, caste and program variables, caste and influential relatives variables, caste and wealth variables.

[8] The question asked in the survey is how many of the relatives that live in this community, are influential in the community?

the village. This variable is not included in this specification since it turns out to be a *perfect* predictor of registration, thus giving no within cluster variation. This immediately shows that one way of improving access to the scheme is having a local work site and improving local awareness and information about the program.

Since individuals in our sample (after the dropping of outliers) are registered only in areas where the scheme is available, the probit estimator drops all individuals in clusters where the scheme is not available, as that becomes a perfect predictor of non-participation. What this means implicitly is that we are estimating the characteristics that determine registration, given that the scheme was available. This is suitable since it means that it is controlling for availability and taking into account the fact that the scheme was targeted initially at the most backward districts.

The next step in the scheme, namely the taking up of paid work is also estimated in a similar fashion. We estimate a dummy (*EGS Work*), which is one if the household has worked under the scheme and zero if not. We use a similar specification to the previous one, but include the *EGSVillage* variable, while continuing to control for cluster fixed effects as well as a broad range of household and individual characteristics. We run this specification on two samples, one being that of all households where the scheme was available, and another on the sub sample of those who have registered, since take up can only take place after registration.

The model being estimated is therefore:

$$EGS\ Work = \alpha + \beta_1 Drought + \beta_2 HH + \beta_3 infrel5 + \beta_4 EGSVillage + \beta_5 Z + \mu \qquad (2)$$

4.3.2 Results and Discussion

The following results immediately throw up some interesting findings. We first run a linear probability model on the same model, to try and estimate the various parameters.[9] This seems to show results in the expected direction. Availability, drought shock, household size

[9] Not reported here due to space constraints.

backward castes, influential relatives and female literacy all seem to have positive and significant effects, while wealth in the first round has a strongly significant negative effect.

Table 4: Probit on Program Participation (Marginal Effects)

	(1)	(2)	(3)	(4)
	Registration	**Registration**	**Work**	**Work on Registered Sub Sample**
Drought	0.105***	0.107***	0.0565**	0.0646*
	(0.0302)	(0.0302)	(0.0273)	(0.0384)
Infrel5		0.103**	0.0583	0.0287
		(0.0487)	(0.0586)	(0.0733)
EGS Village			0.151***	-0.0241
			(0.0268)	(0.0353)
Round 1 Wealth Index	-0.476***	-0.481***	-0.115	0.0250
	(0.112)	(0.112)	(0.101)	(0.146)
Scheduled Caste	0.284***	0.287***	0.0774*	0.00205
	(0.0290)	(0.0289)	(0.0446)	(0.0598)
Scheduled Tribe	0.0186	0.0226	-0.0107	-0.0126
	(0.0642)	(0.0637)	(0.0579)	(0.0863)
Other Backward	0.198***	0.201***	0.0373	-0.0309
Caste	(0.0353)	(0.0354)	(0.0373)	(0.0558)
Casual	0.150***	0.151***	0.0195	-0.0138
	(0.0304)	(0.0304)	(0.0309)	(0.0432)
Agriculture	0.131***	0.127***	-0.0157	-0.0788*
	(0.0300)	(0.0301)	(0.0306)	(0.0432)
Household size	0.00957*	0.00901	0.00960*	0.0133*
	(0.00581)	(0.00587)	(0.00492)	(0.00712)
Observations	1422	1422	1422	976
Pseudo R-squared	0.1885	0.1905	0.1170	0.0823
Observed P	.6863572	.6863572	.2419128	.352459
Predicted P	.7226411	.7230107	.2141039	.3410297

Note: *** p<0.1, ** p<0.05, * p<0.1. Standard Errors in parenthesis. SC-Scheduled Caste, ST- Scheduled Tribe, BC-Other Backward Caste and wi- Wealth Index. Base category- Other caste, not drought affected, 5 or less influential relatives and non EGS worksite villages. Coefficients of cluster dummies, adult education levels not reported.

The more appropriate Probit specification seems to reaffirms these findings. Looking at specifications (1) and (2) some interesting trends are noticeable; the coefficient on the wealth index in round 1 is strongly significant and negative. We find that a 1% point increase in the wealth index reduces the chance of registering by 0.48% points. This clearly seems to point to the fact that the self-targeting mechanism is functioning and it is largely the poor who seem to be taking advantage of the scheme.

The positive and significant coefficients on the casual and agriculture variables suggests that households who have variable incomes and are prone to seasonal shocks and lean agricultural periods seem to be more likely to register for the scheme. Households whose primary occupation is agriculture are 12.7% more likely to register, while those whose primary occupation is casual work are 15.1% more likely to register. In connection with this are the significant and positive coefficients on the Scheduled Caste and Other Backward Caste variables, with SC's being 28.7% more likely to register, and OBCs 20.1% more likely to register. This again suggests that the self-targeting mechanism is working fairly well and there does not seem to be any evidence of strong program discrimination against the disadvantaged castes. The Scheduled Tribe variable is not statistically significant but is also positive.

This is no small step as earlier studies have documented how caste discrimination has a big role to play in large government program. Thorat and Lee (2005) look at caste specifically in the Mid Day meal scheme and the Public Distribution System, finding evidence for favoritism and discrimination along caste lines, across the country. Interestingly they find that of the states they survey, Andhra Pradesh has the least degree of explicit discrimination along caste lines.

The other interesting outcome is the coefficient on the drought shock variable which shows that households impcted by drought are 10.7% more likely to register, suggesting that the scheme seems to be satisfying one of its primary objectives; that of risk coping. One of the design features of the scheme is that it is largely demand driven, and thus able to aid households that have suffered a temporary negative shock (in this case drought) by providing a local insurance mechanism, in the form of paid wage labour. Krishnamurty (2006) highlights this aspect of the scheme and the potential it has in mitigating and coping with crises.

Looking specifically at the network variable, influential relatives, the results indicate that those who are better connected (have more than 5 influential relatives) are 10.3% more likely to register for the program. This may be seen as evidence that the process may be biased towards the more influential and powerful in the community. It may also be explained by greater awareness and knowledge arising from the better connections. It should also be noted that none of the coefficients on the other variables of interest change significantly with the inclusion of the *infrel5* variable.

In the next step of taking up work (Specification (3)) we find that the coefficients are largely insignificant, though in the expected directions. Drought remains a significant factor in determining whether households take up work under the scheme. Interestingly household size has a significant, though small, positive impact on whether households choose to work on the scheme. Since the scheme is implemented on the household level, this finding would fit in well with a surplus labour model, where the households with many adult members would be able to take greater advantage of such a scheme. The caste variables again have positive coefficients, though small in magnitude, and with only the Scheduled Caste variable significant at the 10% level.

The factor that seems to play the strongest role in determining whether households actually take up work is the EGS work site being in the village. This shows that when local sites are available, households are much more likely to take advantage of the scheme. This also links in with the other evidence regarding scheme registration, where we know a local work site is a perfect predictor of a household registering for the scheme.

Once we control for registration, that is looking at the registered sub sample in Specification (4), these trends become less apparent. Drought remains a positive factor, but is significant only at the 10% level. Similarly household size remains a small but positive factor in determining the taking up of work. The interesting variable in this specification is the large and significant negative coefficient on agriculture. Those who are dependant on agriculture as their primary source of occupation, are 12.7% more likely to register, but once registered are 7.88% *less* likely to take up work. This might suggest that these households recognize the potential insurance capability of the program thus registering, while have possibly not required to actually make use of the income transfer in the surveyed period.

4.4 Child Health Outcomes

4.4.1 Conceptual Framework and Econometric Specification

Following the framework laid out in Strauss and Thomas (2008) we assume that an individual's welfare depends on labor supply, L, and consumption of purchased goods, C. Individuals in households then maximize a utility function, U, which is assumed to depend on health outputs, H, as well as observed characteristics such as socio-demographic characteristics, A, non-health human capital (including schooling) and family background, BU:

$$U=U\ (C, L;H\ , A, BU,\xi) \qquad (1)$$

In turn, health can be identified as a static production function of the form:

$$H=H\ (N; A, B_H, D, \mu) \qquad (2)$$

Where H represents an array of measured health outcomes. They depend on a vector of health inputs and behaviors, N, which are under the control of the individual. The technology, or shape of the underlying health production function is likely to vary over the life course and so varies with age and, possibly, with other socio-demographic characteristics, A, such as gender. The technology may also vary with dimensions of family background that affect health, B_H, such as parental health and genetic endowment. Technology will likely vary with environmental factors, D, such as the disease environment, public health infrastructure and treatment practices or standards of care. Finally, μ represents the unobserved characteristics in the health production function.

The approach towards estimating health outcomes shall be based on anthropometric measures. This approach rests on the presumption that people's physical appearance reflects their nutrition (and health) status. (Svedberg 2000) While a number of measures have been used in the past (including head and arm circumference, triceps, etc) we shall focus on height-for-age and weight-for-age z-scores which are widely recognized as jointly reflecting the status of a child's health. These anthropometric indicators are calculated by subtracting the median height (weight) of a reference population from a child's height (weight) and dividing by the standard deviation (WHO, 1995). Children whose height-for-age (weight-for-age) z score is less than two standard deviations below the median are classified as stunted (underweight). The two variables indicate different aspects of childhood health. Height-for-age is used to monitor chronic under nutrition or repeated diseases. Weight-for-age is a composite indicator, intended to capture aspects of stunting as well as wasting (low weight-for-height) (Svedberg 2000) We do not use the third indicator that is often seen in the literature, Weight-for-height, since it is available only for children up to 60 months old and much of our sample is beyond that age.

In this section on health outcomes we focus exclusively on the younger cohort, aged five to six at the time of survey. This is because we are interested in seeing the impact of the intervention on the children's health status. Boersma and Wit (1997) note that while there is great scope for catch up growth, in later childhood outcomes are far less vulnerable to shocks and correspondingly the responsiveness of child health outcomes to interventions decrease with age.

The estimation strategy is to estimate a within cluster difference in differences equation. We start with a level equation of the form:

$$Z = \alpha + \beta_1 Reg + \beta_2 Work + \beta_3 Drought + \beta_4 C.t + \beta_5 z + \gamma t + \mu + \varepsilon^{10} \quad (3)$$

where Z is the height for age and weight for age z scores which we shall take as proxies for the child's health status. On the right hand side it is important to note here that we split the impact of the program into two separate variables to better understand the channels through which it can potentially work. To do this we define *Reg* as a dummy variable for someone who has registered but not worked, while *Work* is someone who as done both, registered and taken up work. Drought is a dummy for whether the household has been affected by drought in the four years between the two rounds, and C and z are vectors of the child's, parent's and household's characteristics. Since it is possible for some time unvarying characteristics to have an effect on growth we include a time dummy variable, t, and its interaction with the C vector of characteristics. These include age, gender, caste, round 1 wealth index, parent's education endowment, mother's height (as a proxy for parent's health endowment) and occupation. μ is a sum of errors that arise from time invariant unobservables that arise at the child, household and cluster level, and ε is an idiosyncratic error. This simple levels specification is not however ideal as it is prone to biases that arise from the presence of time unvarying unobservables, which could be

[10] The time dummies are included to allow for the time unvarying characteristics to have trend effects in the differenced equation, as otherwise they would be absent from the equation and give rise to a potential bias.

correlated with the explanatory variables. To counter this we then use a differencing strategy, thus estimating a difference in difference estimator of the form:

$$\Delta Z = \alpha + \beta_1 Reg + \beta_2 Work + \beta_3 Drought + \beta_4 C + \beta_5 \Delta G + \gamma + \Delta\varepsilon \qquad (4)$$

This specification has the advantage of removing all time unvarying unobservables at the child, household and cluster levels.

Finally, we arrive at the dynamic specification by including the lagged z score:

$$\Delta Z = \alpha + \beta_1 \mathrm{R}eg + \beta_2 Work + \beta_3 Drought + \beta_4 C + \beta_5 \Delta G + \beta_5 Z_{t-1} + \gamma + \Delta\varepsilon \quad (5)$$

4.4.2 Results and Discussion

Table 6: First Differences

	Static OLS		Dynamic OLS		Dynamic IV	
	(1) Change in Height for Age	(2) Change in Weight for Age	(3) Change in Height for Age	(4) Change in Weight for Age	(5) Change in Height for Age	(6) Change in Weight for Age
EGS Reg	0.108	-0.00293	0.0550	0.0135	0.0811	0.00665
	(0.0970)	(0.0696)	(0.0675)	(0.0606)	(0.0763)	(0.0623)
EGS Work	0.171	0.0807	0.139*	0.0976	0.155*	0.0906
	(0.112)	(0.0801)	(0.0777)	(0.0697)	(0.0873)	(0.0717)
Δ Age	-0.219***	-0.160***	-0.119***	-0.108***	-0.168***	-0.130***
	(0.0152)	(0.0109)	(0.0108)	(0.00973)	(0.0218)	(0.0132)
Round 2 Age	0.0986***	0.0759***	0.0375***	0.0349***	0.0675***	0.0520***
	(0.00759)	(0.00545)	(0.00546)	(0.00504)	(0.0127)	(0.00865)
Drought	-0.0115	-0.0373	-0.0128	-0.0225	-0.0122	-0.0287
	(0.0760)	(0.0545)	(0.0529)	(0.0475)	(0.0593)	(0.0488)
Male	-0.00590	0.0854**	-0.0689*	0.0270	-0.0379	0.0513
	(0.0524)	(0.0376)	(0.0365)	(0.0328)	(0.0425)	(0.0352)
Round 1 Wealth Index	0.0151	0.0663	0.463***	0.328***	0.243	0.219*
	(0.191)	(0.137)	(0.134)	(0.120)	(0.171)	(0.131)
Rural	0.131	0.147	0.131	0.161*	0.131	0.155
	(0.153)	(0.109)	(0.106)	(0.0953)	(0.119)	(0.0979)
Scheduled Caste	-0.0403	0.0947	-0.0604	0.00537	-0.0505	0.0425
	(0.0944)	(0.0678)	(0.0657)	(0.0591)	(0.0737)	(0.0626)
Scheduled Tribe	0.201	0.229**	0.0109	0.0823	0.104	0.143*
	(0.128)	(0.0921)	(0.0894)	(0.0804)	(0.106)	(0.0862)
Casual	-0.0839	-0.0315	-0.0815*	-0.0442	-0.0827	-0.0389
	(0.0703)	(0.0504)	(0.0489)	(0.0439)	(0.0548)	(0.0452)
Agriculture	-0.0566	-0.0948*	-0.0472	-0.0933*	-0.0518	-0.0939*
	(0.0773)	(0.0555)	(0.0538)	(0.0483)	(0.0603)	(0.0496)
Household Size	0.0231*	0.0218**	0.0145*	0.0175**	0.0187*	0.0193**
	(0.0125)	(0.00897)	(0.00870)	(0.00781)	(0.00988)	(0.00806)
Mother's Education	0.00430	0.000306	0.00762**	0.00415	0.00599	0.00255
	(0.00520)	(0.00373)	(0.00362)	(0.00325)	(0.00410)	(0.00340)
Father's Education	0.000841	-0.00297	0.00443	5.69e-05	0.00267	-0.00120
	(0.00494)	(0.00354)	(0.00344)	(0.00309)	(0.00391)	(0.00321)
Mother's height	-0.00463	-0.00165	0.0216***	0.00842***	0.00872	0.00423
	(0.00414)	(0.00297)	(0.00294)	(0.00262)	(0.00580)	(0.00319)
Round 1 Height for age			-0.627***		-0.319***	
			(0.0143)		(0.115)	
Round 1 Weight for age				-0.433***		-0.253***
				(0.0181)		(0.0756)
Constant	6.316***	3.766***	0.0627	1.245	3.135**	2.293**
	(1.299)	(0.932)	(0.915)	(0.818)	(1.532)	(0.943)
Observations	1836	1836	1836	1836	1836	1836
R-squared	0.226	0.183	0.626	0.382	0.529	0.347
F-Stat					21.21	12.57
Anderson LM Statistic					35.714	110.657

Note: *** p<0.1, ** p<0.05, * p<0.1. Standard Errors in parenthesis. Base category- Other caste, not drought affected, 5 or less influential relatives, urban and other occupations. Coeffiecients on cluster dummies, construction, and influential relatives not reported.

The above table shows the results on the various specifications used to test the impact of the program on child health outcomes. The findings are interesting as we find that the coefficients on both the program variables are consistently positive, and the take up of work variable is significant at the 10% level in both the dynamic specifications for the height for age scores.

We find that in the case of the Dynamic OLS, take up of work increases the height for age z score by 0.13 standard deviations, while the coefficient increases to .155 standard deviations in the Dynamic IV specification. This is a significant finding since it shows that the program seems to be having a significant positive effect on the health outcomes of these children in participating households. The IV regressions, which use caregiver perception of birth size as an instrument of first round health status, are both exactly identified, and do not have a weak instrument problem.

We control for any variations in health status caused by the variation in ages by including both the change in age and age in round 2 variables. These are both found to be significant albeit in different directions.

Taking into account the different specifications and methods used, we see a positive impact of the program on child anthropometry but which is only sometimes statistically significant. We also find that while both the program variables have positive effects, it is the actual take up of work that seems to be having an impact. This suggests that it is not simply registration in the program that has an impact, but the income transfer resulting from take up, which is important. It is possible that there are positive and negative impacts on the children, as established in the background section, which are cancelling each other out. We can quite safely say however that there is no net negative effect.

4.5 Child Labour

4.5.1 Conceptual Framework and Econometric Specification

Ravallion and Wodon (2000) develop a theoretical framework to try and model the decision making process leading to child labour. Similarly, we assume that parents are free to determine how their children's time is allocated. In making that choice, let parents' utility be:

$$U = U(C, S, H; Z) \qquad (1)$$

where the household's current consumption is C, S is the child's school attendance, and H is the child's leisure. We assume that U is strictly quasi-concave in C, S and H. The variable Z represents a vector of exogenous household and local geographic variables. The child's total time available (T) can be devoted to schooling, leisure (H), or wage labor (L):

$$S + H + L = T \qquad (2)$$

In addition to income from child labor or the NREGS, the household obtains an income Y from other sources, which we assume to also be a function of Z. (The latter will include the parents' education and landholding.) So the budget constraint is:

$$C = wL + b + Y(Z) \qquad (3)$$

Where w is the wage rate for child labor, and b is the value of the total wages received under the NREGS.

Using this we can see that maximising utility, subject to (2) and (3) could lead to child labour increasing or decreasing, depending on the impact that participation in the program has on the parent's time allocation and whether the child's time is required o act as a substitute for the adult in certain forms of work.

The empirical strategy therefore is to estimate a probit on the observed binary variable, CL (taking the value one if the child reports working for monetary compensation, and zero otherwise), with the program registration and participation as possible explanatory variables. We adopt the same method as in the section on health where the *Reg* term specifically indicates registered but not taking up work, while *Work* specifies having done both. We run three specifications, one with both the variables (*EGS Reg* and *EGS Work*) and then one each with only work (*EGS Work*) and Registration (*All Reg*). We also incorporate parent's own expectations of their children by including variables that indicate the desired level of education they would like for their children, and whether they expect them to reach it. We focus in this section on the older cohort, since the younger cohort is still too young to be actively participating in the work force.

The specification being estimated is therefore:

$$CL= \alpha+\beta_1 Reg + \beta_2 Work +\beta_3 Drought +\beta_4 X +\beta_5 Exp+\mu \quad (4)$$

To further estimate which of the program variables is having an impact, we estimate the same specification with only one of the program variables:

$$Z= \alpha+\beta_1 AllReg +\beta_2 Drought +\beta_3 X +\beta_4 Exp+\mu \quad (5)$$

and

$$Z= \alpha+\beta_1 Work +\beta_2 Drought +\beta_3 X + \beta_4 Exp+\mu \quad (6)$$

4.5.2 Results and Discussion

Table 7: Probits on Child Labour (Marginal Effects)

	Both Genders			Only Male			Only Female		
	(1) CL	(2) CL	(3) CL	(4) CL	(5) CL	(6) CL	(7) CL	(8) CL	(9) CL
Reg	-0.00529			-0.149**			0.0985		
	(0.0380)			(0.0664)			(0.0750)		
EGS Work	-0.0505		-0.0480	-0.0792		-0.00352	-0.0330		-0.0819**
	(0.0342)		(0.0295)	(0.0739)		(0.0869)	(0.0580)		(0.0337)
All Reg		-0.0220			-0.134*			0.0546	
		(0.0356)			(0.0709)			(0.0593)	
Round 1 Wealth Index	-0.0230	-0.0275	-0.0225	0.0175	0.0147	0.0112	0.0498	0.0337	0.0230
	(0.0908)	(0.0909)	(0.0907)	(0.217)	(0.217)	(0.218)	(0.129)	(0.130)	(0.132)
Male	0.0635**	0.0628**	0.0641**						
	(0.0268)	(0.0269)	(0.0265)						
Rural	0.0344	0.0357	0.0340	0.0203	0.0186	-0.000805	0.0983*	0.101*	0.101*
	(0.0872)	(0.0868)	(0.0874)	(0.189)	(0.189)	(0.199)	(0.0512)	(0.0524)	(0.0558)
Drought	0.0798**	0.0786**	0.0794**	0.149*	0.148*	0.130*	0.0469	0.0412	0.0464
	(0.0366)	(0.0365)	(0.0364)	(0.0771)	(0.0773)	(0.0758)	(0.0500)	(0.0494)	(0.0511)
Scheduled Caste	0.0605	0.0576	0.0589	0.119	0.123	0.0679	0.151	0.140	0.171
	(0.0626)	(0.0624)	(0.0612)	(0.126)	(0.125)	(0.114)	(0.135)	(0.130)	(0.136)
Scheduled Tribe	0.155	0.150	0.154	0.226	0.226	0.209	0.270	0.224	0.280
	(0.103)	(0.102)	(0.102)	(0.182)	(0.181)	(0.177)	(0.205)	(0.193)	(0.202)
Other Backward Caste	0.0536	0.0556	0.0527	0.00396	0.00591	-0.0194	0.164**	0.170**	0.170**
	(0.0447)	(0.0450)	(0.0443)	(0.0918)	(0.0911)	(0.0890)	(0.0770)	(0.0769)	(0.0759)
Casual	0.0952**	0.0956**	0.0943**	0.121	0.119	0.0802	0.106*	0.103*	0.110*
	(0.0415)	(0.0415)	(0.0410)	(0.0839)	(0.0839)	(0.0791)	(0.0585)	(0.0580)	(0.0597)
Agriculture	0.0391	0.0426	0.0381	0.0843	0.0814	0.0474	0.0237	0.0319	0.0303
	(0.0393)	(0.0396)	(0.0387)	(0.0883)	(0.0881)	(0.0832)	(0.0527)	(0.0541)	(0.0545)
Mother's Education	-0.00369	-0.00368	-0.00369	-0.00280	-0.00257	-0.00279	-0.00610*	-0.00563*	-0.00623*
	(0.00241)	(0.00241)	(0.00241)	(0.00506)	(0.00508)	(0.00512)	(0.00332)	(0.00331)	(0.00341)
Level of desired	-0.0217***	-0.0219***	-0.0218***	-0.0361***	-0.0367***	-0.0373***	-0.0181**	-0.0206***	-0.0165**
education	(0.00525)	(0.00526)	(0.00524)	(0.0130)	(0.0129)	(0.0132)	(0.00757)	(0.00762)	(0.00749)
Expectation of achieving	-0.418***	-0.413***	-0.418***	-0.509***	-0.514***	-0.499***	-0.483***	-0.471***	-0.480***
desired grade	(0.0895)	(0.0890)	(0.0895)	(0.132)	(0.131)	(0.132)	(0.140)	(0.138)	(0.138)
Observations	826	826	826	340	340	340	365	365	365
Pseudo R-squared	0.4620	0.46	0.4619	0.4795	0.4779	0.4696	0.4798	0.4656	0.4740
Observed P	0.214	0.214	0.214	0.285	0.285	0.285	0.219	0.219	0.219
Predicted P	O.107	0.107	0.107	0.179	0.179	0.184	0.093	0.096	0.098

Note: *** p<0.1, ** p<0.05, * p<0.1. Standard Errors in parenthesis.Base category- Other caste, not drought affected, 5 or less influential relatives, other occupations.Coeffiecients on cluster dummies, father's education, influential relatives dummy and household size not reported.

The findings presented above are worthy of note and suggest some positive impact of the program. In the first three specifications, the various program variables are consistently negative (suggesting a decrease in the likelihood of child labour) but are not significant. However when we separate the sample along the lines of gender we seem to find a clearer impact. The variables seem to suggest different processes determining the incidence of child labour in males and females.

Looking at males first, we find that a drought seems to increase the likelihood of work by 14.8%, while registration has an almost identical negative effect, reducing the likelihood of work by 13.4%. In other words the increased insurance or safety perceived by registering for the program seems to counter the detrimental effect of drought on increasing child labour. In the case of women, the process seems to be slightly different. For girls, being in a rural area has a significant impact, increasing the chances of child labour by 10.1%. Here the program seems to have a positive impact through the income transfer that it generates, with households taking up work being 8.19% less likely to participate in child labour, again almost completely countering the detrimental impact of being in a rural area. While there seems to be some gender profiling taking place in the incidence of child labour, both sexes appear to be benefiting from the program, albeit apparently through different channels. The fact that different variables seem to matter (drought for boys and rural for girls) is a point to note, as is the fact that different aspects of the program seem to be playing a role in the two cases.

4.6. Discussion

Having presented the different results, it would be useful to briefly outline the possible explanations and also the implications it has for the program. The first set of results is fairly reassuring. They suggest that unlike what initial reports had suggested, those participating in the program are relatively worse off and disadvantaged. In other words they suggest that broadly the self-targeting strategy seems to be working. There is however some cause for worry in the form of the important role having influential relatives seems to play. This could be elite capture at work or simply an indication of those who are better connected and informed in the local environ taking greater advantage of the program. It is very important thereby to further improve access by ensuring awareness of the program, as well as active support for potential participants from both grass roots organisations as well as the government. Here we have controlled for the geographical targeting of the program by looking only at households residing in the areas where it was implemented in phase I. There are also a wide variety of cluster, household and individual level controls that attempt to incorporate all the observable characteristics that could determine program participation. There remains the potential problem of Omitted Variables or unobservables, correlated with the included characteristics, biasing the above results.

When looking at the impact that the program has had on the children of participating households we run into a variety of econometric problems. The most important of these, potential endogeneity and self-selection, are countered through a variety of measures. We use a broad range of observable characteristics to control for program participations, we estimate within cluster variation, to control for program placement and finally we use a difference in difference approach to control for time unvarying unobservables at the various levels. These regressions do implicitly make the assumption that once we take first differences, and control for a wide variety of observable characteristics that could drive participation, both *Reg* and *Work* are conditionally exogenous.

In the case of the health status it does not seem like we can give a conclusive answer as to the impact. While there seem to be some positive correlation, it does not remain robust across specifications and is not highly statistically significant. There is the question of there being findings of greater magnitude and significance in the height for age z score variable in comparison to the weight for age score. This could be a sign that the program is having a significant impact in reducing chronic deficiency of food, which is what height for age tends to capture. It is also a reflection of the possibility of 'catch- up', something which various studies have expressed. (see Boersma and Wit 1997, Tanner 1981, and Golden 1994) One additional point that should be taken into consideration is the lack of childcare facilities at sites. Less than 10% of those who register for the program report the presence of childcare at work sites in our data. If these are set up and run in the form of an *anganwadi*[11] for under six year olds, as they are meant to be, the positive impact could be greater.

While improving the health status of children is not the primary objective of the program, any impact can have important and long-term implications. Numerous studies have now shown that a child's health status is a good predictor of various long-term variables like earnings. It is thus very important if the program can help in reduce any disadvantages at childhood.

[11] A government supported integrated health and child-care facility for mothers and young children.

The findings on the incidence of child labour are far more promising, indicating an 8 to 15% reduction in the likelihood of child labour due to program participation, depending on gender. It is also interesting to note the different channels through which child labour works in the different genders, which is also reflected in the significance of different program variables in the two. Grootaert and Kanbur (1995) talk of how the process whereby children enter into child labour are not the same across gender, which often has a prominent role to play in determining whether a child works and under what circumstances a child is pushed (or pulled) into work.

An additional caveat that must be kept in mind is the variation in implementation of the NREGS across the country. Most reports suggest that Andhra Pradesh is one of the front-runners in this respect. A survey conducted by the Centre for Budget and Governance Accountability (CBGA) indicated an awareness level of the scheme at 98 per cent in Andhra Pradesh compared to only 29 per cent in Jharkhand. (Dreze, 2006) In addition it has been the first state to implement the entire scheme making explicit use of IT services while also incorporating more traditional techniques like social audits and performance checks to increase transparency and accountability. This point is critical when trying to generalise any results from this data, while at the same time useful in showing the magnitude of effects a well run and efficiently functioning scheme can have.

5. Conclusion

The largest public works program in the world, the NREGS has often been a topic of heated debate, with both proponents and opponents often adopting hard stands. The debates often played out in the public arena and media have unfortunately not been able to substantiate themselves with quantitative analysis due to the lack of data and econometric studies. We use the Young Lives Data set, one of the only ones available, which offers both a baseline and post intervention survey, to study the access to the scheme and assess the impact specifically on children. While there are econometric challenges and the specific nature of our data does not allow for easy generalisations, there seem to be some findings worth noting.

In terms of initial geographical targeting, and the subsequent self-targeting, the results seem to be fairly encouraging with strong evidence for targeting efficiency. The presence of the network variable is potentially worrying.

The program, being first geographically targeted and then based on self-selection with no set out inclusion criterion, sets out numerous potential econometric pitfalls when attempting to assess impacts. These are countered as best as possible by using a variety of methods and specifications. These issues notwithstanding, there seems to be some discernible positive impacts from the presence of the scheme. It is to be remembered that while health impacts are not robust across specifications, they remain consistently positive, and exposure to the program had been for just about a year when surveyed.

In terms of child labour, the results seem to be more robust. We find that program participation seems to reduce the likelihood of households resorting to child labour. There is also the interesting finding of different impacts on gender lines, with boys being more likely to work in case of a drought, an effect which is almost entirely countered by program registration. Girls on the other hand are more likely to work in case of living in rural areas, again an effect which is almost entirely countered by program take up.

The NREGS seems to offer the safety net and employment that many rural families require due to the seasonal and variable nature of employment opportunities. It also seems to have an important effect on the well being of children, something which can have important long term benefits and potentially help counter the troubling current scenario where nearly half the country's children are malnourished. This all, points to an important role for the NREGS in future policymaking discussions. It must however, be kept in mind that for the program to have a meaningful and sustained impact, it must be run fairly and efficiently, ensuring adequate monitoring and delivery mechanisms, while also retaining its people driven demand approach.

6. References

Altonji, Elder, and Taber (2005) “Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools.” *Journal Of Political Economy* 113:1,151-184.

Ambasta, P, Vijay Shankar and Mihir Shah (2008) “Two Years of NREGA: The Road Ahead” Economic an Political Weekly. February 23.

Beegle, Weerdt and Dercon (2008) Orphanhood and Human Capital Destruction: Is there Persistence into Adulthood?

Behrman, J (1996) The impact of health and nutrition on education. The World Bank Research Observer, 1996 - World Bank

Besley, Tim and Ravi Kanbur (1993) “The Principles of Targeting” in Including the Poor. Edited by Michael Lipton and Jacques Van Der Gaag.

Black, S, PJ Devereux and KG Salvanes From the Cradle to the Labor Market? The Effect of Birth Weight on Adult Outcomes. The Quarterly Journal of Economics. MIT Press

Boersma B & JM Wit (1997) Catch-up growth. Endocr. Rev. 18. 646-661

Braun, J Von, T Teklu and P Webb (1992) Labour-intensive public works for food security in Africa: Past experience and future potential. International Labour Review.Vol 131 No. 1

Braun, J. (1995) Employment for poverty reduction and food security. Washington, D.C.: IFPR

Brown, Lynn R., Yisehac Yohannes and Patrick Webb (1994) Rural Labor-Intensive Public Works: Impacts of Participation on Preschooler Nutrition: Evidence from Niger. American Journal of Agricultural Economics, Vol. 76, No. 5, Proceedings Issue Dec. pp. 1213-1218.

Burra, N (2006a) NREGA and its impact on child labour: field notes from Dungarpur. www.levy.org

Burra, N (2006b) Transparency and accountability in employment programmes: the case of NREGA in Andhra Pradesh. www.levy.org

Campbell, C., and S. Horton. (1991)"Wife's Employment, Food Expenditures, and Apparent Nutrient Intake: Evidence from Canada." American Journal of Agricultural Economics. 73:784-94.

Caeyers, B and Dercon, S (2008) Political Connections and Social Networks in Targeted Transfer Programs: Evidence from Rural Ethiopia The Centre for the Study of African Economies Working Paper. CSAE WPS/2008-33

Currie, J and E Moretti (2007) Biology as Destiny? Short-and Long-Run Determinants of Intergenerational Transmission of Birth Weight. Journal of Labor Economics U Chicago

Press

Datt and Ravallion (1994) "Transfer Benefits From Public-Works Employment: Evidence for Rural India." The Economic Journal, Vol. 104, No. 427, (Nov., 1994), pp. 1346-1369

Dreze (2006) National employment guarantee inaction. The Hindu. 12th September

Drèze, J and AK Sen, (1989) "Hunger and Public Action." Oxford University Press

Gilligan and Hoddinott (2006) Is There Persistence in the Impact of Emergency Food Aid? Evidence on Consumption, Food Security, and Assets in Rural Ethiopia FCND Discussion Paper 209. IFPRI.

Global March Against Child Labor (2006) Review of Child Labour: Education and Poverty Agenda. India Country Report.

Golden MH. (1994) Is complete catch-up possible for stunted malnourished children? European Journal of Clinical Nutrition 1994 Feb;48Suppl 1:S58-70; discussion S71

Gopalan (1992) Undernutrition: Measurement and Implications in S.R. Osmani (ed.) Nutrition and Poverty, Clarendon Press, Oxford

Government of India (2008) 'The National Employment Guarantee Act 2005 Operational Guidelines. 3rd Edition. Ministry of Rural Development.

Government of India (2005) 'The National Employment Guarantee Act', The Gazette of India. 7th September.

Grootaert and Kanbur (1995) Child labour: an economic perspective. International Labour Review

Human Rights Watch. 1996. The Small Hands of Slavery - Bonded Child Labor in India. New York: Human Rights Watch

Higgins, P and H. Alderman (1997) Labor and Women's Nutrition: The Impact of Work Effort and Fertility on Nutritional Status in Ghana. The Journal of Human Resources, Vol. 32, No. 3 (Summer, 1997), pp. 577-595

Hinrichs, P (2007) "Essays in the Economics of Education" Massachusetts Institute of Technology mimeo.

Hirano Keisuke, Guido Imbens, and GeertRidder, 2003. "Efficient Estimation of Average Treatment Effects Using the Estimated Propensity Score." *Econometrica* 71(4): 1161-1189

Hirano Keisuke and Guido Imbens. 2001. "Estimation of Causal Effects using Propensity Score Weighting: An application to Data on Right Heart Catheterization." *Health Services & Outcomes Research and Methodology* 2: 259-278.

Jha, Gaiha and Sankar (2008) Reviewing the national Rural Employment guarantee Program. Economic and Political Weekly 15 March.

Kirchberger, M (2008) "Do water and sanitation really matter for child health?" University of Oxford, mimeo.

Krishnamurty, J (2006) "Employment Guarantee and Crisis Response" Economic and Political Weekly. March 4.

Mehra-Kerpelman, K. 1996. Children at work: How many and where? World of Work 15:8-9.

Mehrotra, S (2008) "NREG Two Years On: Where Do We Go from Here?" Economic and Political Weekly. August 08

Murgai, Rinku and Martin Ravallion (2005) "Employment Guarantee in Rural India" Economics and Political Weekly. July 30.

Murgai, Rinku and Martin Ravallion (2005b) 'Is a Guaranteed Living Wage a Good Anti-Poverty Policy?' mimeo, Development Research Group, World Bank and World Bank Resident Mission, New Delhi.

Ninno, Carlo del, Kalanidhi Subbarao and Annamaria Milazzo (2009) How to Make Public Works Work: A review of the experiences. Unpublished Mimeo. January 29th version.

Official Website of the NREGA: http://nrega.nic.in

Official Website of the NREGA in AP: http://nrega.ap.gov.in/Nregs/Home_eng.jsp#

Official Website of the MEGS: http://mahaegs.nic.in/index.html

Outes-Leon, I., & Dercon, S. (2008). Survey Attrition and Attrition Bias in Young Lives. *Young Lives Technical Note No. 5* .

Poorest Areas civil society Program (2008) Two years of the NREGS: A critical review-I and II. Accessed on http://www.empowerpoor.com/backgrounder.asp?report=763

Ravallion, M (1991) Reaching the Poor through Rural Public Employment: Arguments, Evidence and Lessons from South Asia. The World Bank Research Observer.

Ravallion, M,Datt and Chaudhuri (1993) "Does Maharashtra's Employment Guarantee Scheme Guarantee Employment? Effects of the 1988 Wage Increase." Economic Development and Cultural Change, Vol. 41, No. 2, (Jan., 1993), pp. 251-275

Ravallion, M and Q Wodon (2000) Does child labour displace schooling? Evidence on behavioural responses to an enrollment subsidy. Economic Journal

Singh, A (2008) "Do School Meals Work? Treatment Evaluation of the Midday Meal Scheme in India." Young Lives Student Paper

Shariff, Abusaleh (2008) Growth and Safety Net in Tandem: A Case of India's National Employment Guarantee Scheme. IFPRI (Asia)

Strauss, J., and Thomas, D. (2008) Health over the life course, in T.P. Schultz and J. Strauss (eds.), Handbook of Development Economics, Volume 4, Amsterdam: North Holland Press.

Svedberg, Peter (2000) Poverty and Undernutrition: Theory, Measurement, and Policy, Oxford University Press

Tanner, J. (1981) - Catch-up growth in man. Brit. Med. Bull. 37: 233–238.

Thorat, S and J Lee (2005) Caste Discrimination and Food Security Programmes. Economic and Political Weekly, September 24

World Health Organisation. (1995) Physical Status: The Use and Interpretation of Anthropometry, WHO Technical Report No. 854, Geneva

Wray, R (2006) "Lessons from Argentina's Employment Guarantee" Economic and Political Weekly. June 10.