Impacts of India's Rural Workfare Program on Child Development

Anushah Hossain[1]
Department of Economics
University of California, Berkeley
Thesis Advisor: Aprajit Mahajan
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Abstract: Nearly 30% of India's rural population currently lives in poverty. In recent years, program such as the Mahatma Gandhi National Rural Guarantee Act (NREGA) have sought to improve rural livelihoods by offering pay for work. In this paper, I examine the impacts of NREGA on child development outcomes spanning health and cognitive ability. I use a differences-in-differences estimation strategy to compare outcomes in early and late phase-in districts and find that intent-to-treat program effects are largely insignificant. This suggests that household gains from NREGA may not trickle down to children through either increases in income or time availability, though these findings are subject to the limitations of the purposive sampling of the data set and rely on the fulfillment of the parallel trends assumption between districts.

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Impacts of India's Rural Workfare Program on Child Development

Nearly 30% of India's rural population currently lives in poverty. In recent years, government initiatives have sought to address poor economic conditions through loose rights-based programs that guarantee access to food, water, and sanitation, amongst others.[2] Chief among these rights-based programs is India's employment guarantee scheme, the Mahatma Gandhi National Rural Employment Guarantee Act (NREGA), which offers adults from rural households one hundred days of wage labor each year to curb unemployment and boost rural incomes. Current documentation of NREGA suggests that there are positive economic and social outcomes associated with uptake, despite variable quality of administration (Klonner and Oldiges 2014). However, the majority of these studies examine effects on proximate outcomes such as labor market participation, wage levels, and poverty incidence. As administration of NREGA improves, it becomes increasingly possible and important to study the broader impacts of the employment guarantee to justify government investment and ensure program efficacy.

In this paper, I examine the impacts of NREGA on child development outcomes spanning health and cognitive ability. I assess changes in outcomes using a cohort study of nearly 3000 children from six rural districts in the state of Andhra Pradesh. This analysis allows us to evaluate NREGA's deeper impact on rural society through the focus on other household members — children — and on performance metrics rather than participation or uptake levels. I use a differences-in-differences estimation strategy to compare outcomes in early and late phase-in districts and find that intent-to-treat program effects are largely insignificant. This suggests that household gains from NREGA may not trickle down to children through either increases in income or time availability, though these findings are subject to the limitations of the purposive sampling of the data set and rely on the fulfillment of the parallel trends assumption between districts.

I. The Mahatma Gandhi National Rural Employment Guarantee Act

NREGA participates in a centuries-old tradition of *workfare* - the supplying of wages and social support in return for labor. In India, this practice dates as far back as the British Poor Law of 1834. The present incarnation of the program, started in 2005, builds on Maharashtra's state level employment guarantee program of 1972, the first to set a predefined minimum wage within the program rules, and expands it into the largest active public works program in the world. Similar social safety nets that position the government as the 'employer-of-last-resort' currently exist in Argentina and are being considered in France (Samson et al 2001). Familiar analogs of workfare programs from American history are the Public Works Administration and the Tennessee Valley Authority, amongst other New Deal initiatives.

Unlike other limited programs, however, NREGA is designed to be universally accessible to all rural households. Adult members of a rural household may apply for a job card and, upon approval, request 100 days of manual unskilled labor assignments to be supplied within fifteen days of application. Wages must be paid at the state minimum or more and be distributed within a week of assignment completion. NREGA's immediate goals are to lower the incidence of poverty and unemployment, but its secondary goals are to strengthen rural infrastructure, build administrative capacity, and curb rural-to-urban migration. Field reports suggest the spillover effects of construction and capacity building may take time to appear (Indian Institute of Science 2013).

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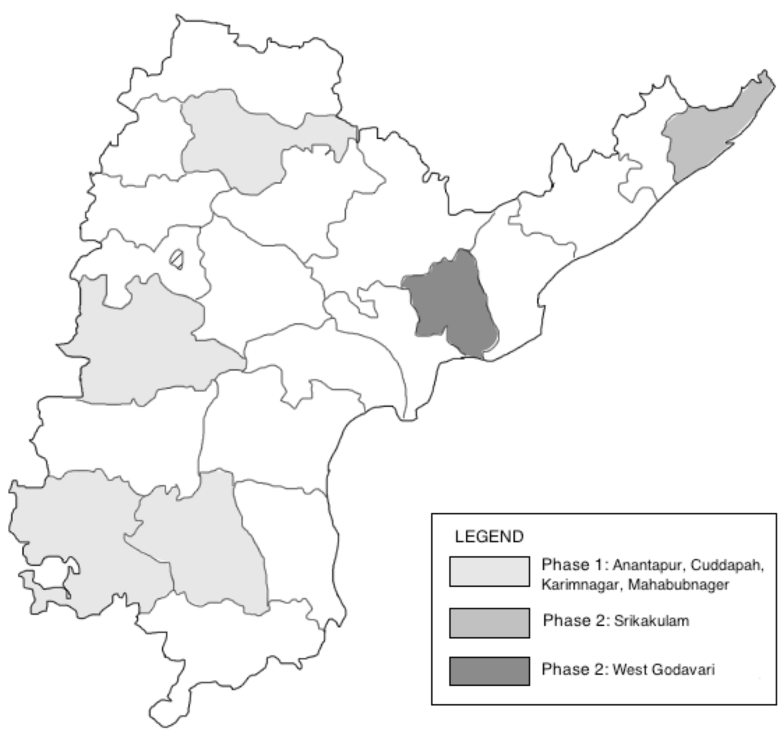
[2] See the Mid-day Meal Scheme, the Public Distribution System, and the Total Sanitation Campaign (leads behavioral campaigns and provides subsidies for both water and sanitation). Many state governments have also taken to distributing free consumer durables such as televisions and laptops to those without, through programs such as ELCOT in Tamil Nadu.

The conditions of the program implicitly target the most marginal members of rural society — scheduled castes, tribes, and women — through program conditions and prioritized implementation. The ultra-poor are thought to be more willing to partake in the manual unskilled tasks. Women are incentivized to participate due to the guaranteed minimum wage, allowing them to somewhat overcome agricultural labor market discrimination. NREGA further ensures female participation beyond the inherent self-selection by requiring at least one third of the workforce be women and that women participate in the monitoring and management of the scheme. All worksites are intended to have childcare facilities to lessen the work burden for mothers, however field documentation notes that most projects have yet to add childcare components (Narayan 2008). As a result, fewer mothers may participate in NREGA and the intent-to-treat effects of NREGA upon children may be moderated. In the Young Lives sample data used in this study, only 1 in 4 households enlisted in NREGA and surveyed in 2009 report having child care facilities at their worksites. Of the families with NREGA job cards and children younger than five, 1 in 4 report that the women do not participate in NREGA because of the lack of childcare facilities.

How does NREGA work?

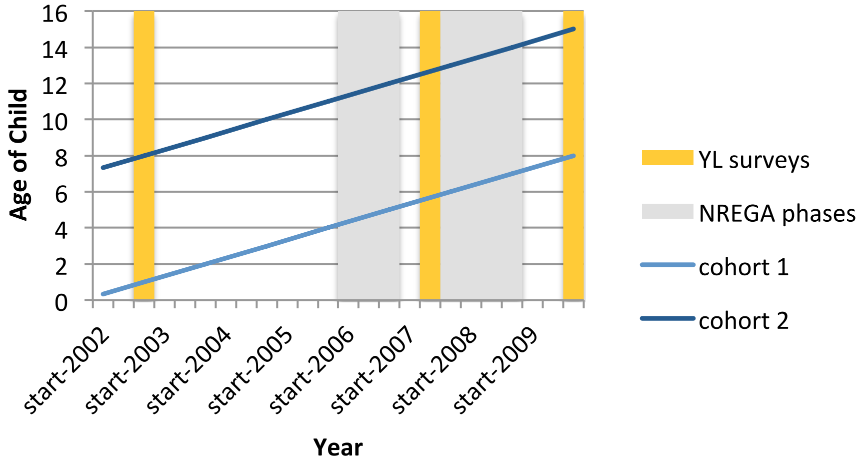
NREGA was rolled out in three phases across rural districts (only 100% urban districts were exempt). The order of district implementation was determined by a 'backwardness index' previously calculated by the Indian National Planning Commission in 2003. Mani et al (2014) have found the backwardness ranking — based upon proportion of scheduled tribes and castes, agricultural wages, and output per agricultural worker (4) — to be an appropriate representation of poverty in districts.

Figure 1 – NREGA Program Phases and Young Lives Sample Districts[3]



[3] From the Andhra Pradesh NREGA district website: http://nrega.nic.in/mnrega_dist.pdf

Figure 2 – Timeline of NREGA phases, Young Lives surveys, and Child Ages



The rollout of the program lends itself to this analysis because of the temporal and geographic variation. Though districts were not randomly chosen, differencing their outcomes over time allows us to control for time-invariant factors (used to determine program implementation order) and compare between early and late program-treated districts.[4] Furthermore, the timing is particularly amenable to study using the Young Lives data set, as the program periods fall nicely between survey rounds and allows for clean identification of treatment. Figures 1 and 2 above show the program districts from the Young Lives sample, and the time line of NREGA implementation phases and Young Lives surveys.

NREGA in Andhra Pradesh

NREGA covers thirteen districts (655 blocks) in Andhra Pradesh. As of 2015, there are 76.73 lakh active workers, of whom about 20% are scheduled caste members, and 8% scheduled tribe members. Women make up 54% of the workforce (Ministry of Rural Development 2010). In comparison, the World Bank reports national proportions of 31% scheduled caste, 25% scheduled tribe, and 50% women (Satish 2013). Other reports show that female participants tend to compose a larger chunk of the work hours: in 2009/10, women made up 58% of the total work days in Andhra Pradesh, compared to 48% India-wide (Sudarshan 2011).

Andhra Pradesh has been identified as one of the highest performing states since the inception of NREGA in 2006. Khera (2011) ranks Andhra Pradesh, Madhya Pradesh, Rajasthan, Tamil Nadu, and Chhattisgarh as star performers, despite lower performance levels than guaranteed by the act. Imbert and Papp's (2012) study on the distributional labor impacts of NREGA further confirm that these states generate significantly more employment under the act relative to other states in India. Sudarshan (2011) also finds that Andhra Pradesh has the second-highest number of total workdays under NREGA.

NREGA's apparent effectiveness and high participation rate in Andhra Pradesh make it a suitable region of analysis for this study. As several papers have already found positive first-order results related to participation, wages, and consumption in Andhra Pradesh, the focus here on second order impacts,

[4] This differencing strategy in accompaniment with difference-in-differences is used by researchers such as Imbert and Papp (2012) and Mani et al. (2014).

particularly those on children's health and educational performance, is appropriate (see Afridi et al. 2012; Johnson 2009; Dasgupta 2013; Ravi and Engler 2009; Das and Singh 2013; Liu and Deininger 2010).

II. Literature Review

Few workfare programs have been evaluated to compare with NREGA. Conditional cash transfers provide the closest analog, though they induce both income and behavioral effects on participants. Mexico's Progresa/Oportunidades program in particular has been noted for being associated with improved height-for-age z-scores and cognitive development among children. De Janvry et al (2005) found that the program was also able to mitigate negative income shocks and ensure sustained child school enrolment among treatment households. More recently, Fernald et al. (2008) isolated the cash transfer component of Progresa, as opposed to the conditionality, and found positive and significant health and cognitive improvements among children, suggesting that the income effect may suffice to achieve the desired child outcomes.

Other studies have examined the effect of boosted income on child development through other policy channels. Studies in developed countries such as the United States, Canada, and the United Kingdom have generally found that short term increases in income have negligible effects on child health and test scores, but that long term policy-driven increase, such as changes in tax policy, have moderate impacts (Dahl and Lochner 2012; Milligan and Stabile 2011; Blau 1999; Burgess et al 2004). Similar studies in developing countries tend to find higher magnitude improvements in child health and cognitive ability compared to Western contexts, particularly when the income increase is channeled through women (Duflo 2000; Afridi et al 2012).

Because of the particular benefits accrued to women in NREGA, the program has received increasing attention from researchers interested in examining the gender dimensions of social security programs. Azam (2012) examined changes in labor force participation and real wages, finding sharp impacts on women and only marginal improvements for men. These impacts were studied in the context of the entire population of NREGA-treated districts, rather than solely among program participants. Das and Singh (2013) have also noted the direct seasonal benefits to women due to their usual gendered roles in the harvest cycle. NREGA's on-demand policy is intended to allow employment substitution during droughts and dry seasons, especially for women.

Qualitative assessments of NREGA have further documented improvements in women's intra-household decision-making power. Narayan (2008) finds female participation in NREGA to correspond to higher investment in child welfare and faster pay-off of debts. A secondary strand of literature builds upon the idea of improved bargaining power by considering women's NREGA participation as the mechanism for improvements in child educational attainment. Results of such studies are mixed however, often varying upon the region of study. For example, Afridi et al (2012) find higher grade attainment and time spent in school in Andhra Pradesh, but Das and Singh (2012) find no statistical significance of female NREGA participation along the same metrics when measured nationwide.

Broader studies on the impact of NREGA on child development have had modest findings. Dasgupta (2014) examines changes in Height-for-Age using Young Lives data from Andhra Pradesh, but finds no significance of program uptake alone. Instead, she finds that children who experienced early-life drought benefit somewhat from NREGA. Mani et al (2013) also consider changes in cognitive ability due to NREGA using the Young Lives surveys, restricted to the older cohort, and find minimal to no significant impacts. This study builds upon these previous works by extending the range of outcomes and observations to try and understand the disaggregated effects on different age groups and the channels through which they act.

III. Conceptual Basis

In conceptualizing how NREGA may impact child development, we can think of child health and test performance (a proxy for cognitive ability) as a function of several relevant inputs: access and use of resources, time availability, and supportive environment. Under resources, we can consider factors such as nutrition and food availability, health resources such as doctor's visits or medicine, and school textbooks and other learning tools. These are related directly to income, and are plausibly the primary channel through which NREGA participation may impact child development.

A second channel through which NREGA may impact child development is through time availability. Increased leisure or study time may lead to improved test scores. As well, more discretionary time may indicate less time spent in child labor. Islam and Sivasankaran (2015) found that child labor dropped throughout India due to NREGA, though the effect was felt differently by age group. Younger children tended to spend more time on education-related activities (attending school, studying) if their parents participated in NREGA, while older children often picked up more work outside the household in response to higher wages driven by NREGA programming. Similarly, Afridi et al (2012) found that time availability actually decreased for older children following female NREGA participation, as they became responsible for greater household duties. As the literature thus far suggests the direction of NREGA's time impacts varies across age cohort, I anticipate a moderate negative effect of NREGA on child development for older cohorts. A positive treatment coefficient may indicate, however, that the income effects overcome any negative time effect, or vice versa.

Finally, we can imagine a positive impact of NREGA on child development through an improved home environment. Many have documented NREGA's ability to cushion households against external shocks and smooth consumption (Ravi and Engler 2009; Bhupal and Sam 2014). Less stress within the household may have a particular impact on child test performance, and may also affect general health and well-being. As field documentation suggests that NREGA has yet to make large infrastructural improvements in communities through public works projects, I assume children are not yet reaping benefits from community development.

Given the largely positive theoretical impacts of guaranteed employment and boosted incomes for children, I hypothesize that the treatment effects of NREGA on child health and cognitive ability will be correspondingly positive for the younger cohort, at minimum. Outcomes for the older cohort may differ based on changes in the labor market and wage rates.

IV. Data & Summary Statistics

I use data from the Young Lives panel data set, which tracks 3000 children in Andhra Pradesh over three rounds from 2002 to 2009. The Young Lives initiative seeks to incentivize studies of child poverty across the world, and has thus far constructed panel data sets for Ethiopia, Peru, and Vietnam, in addition to Andhra Pradesh in India. The panel data has a negligible attrition rate and provides data on child health, education, socio-demographics, parental employment, and community resources. It also contains some of the only anthropometric data available over time from India, and is the only cohort study tracking the same individuals until the prospective release of the second wave of the Indian Human Development

Survey in late 2015.[5] The cohort study allows us to difference across individual-level characteristics and isolate program effects. Finally, as mentioned in the previous section, the timing of the surveys corresponds well with NREGA program implementation, as the second survey round falls perfectly between phases 1 and 2 of NREGA roll-out.

It is worth noting, however, that the Young Lives initiative actively sought to include a high proportion of poor children in its sample, implying that the findings of this study many not be externally valid even within the state of Andhra Pradesh (Young Lives 2011). Districts were selected based on a relative development index, matching one poor to each non-poor district within each agro-climate. Mandals were further selected based on a development index, from which villages and individuals were randomly selected. Perhaps as a result of the poor to non-poor district matching strategy, sample households ended up being slightly wealthier when compared to those from the more nationally- and state-representative 1998/9 Demographic and Health Survey (DHS) (Kumra 2008).

Table 1. Pre-Intervention Descriptive Statistics

	Early districts	Late districts	T-statistic
Scheduled Caste/Tribe	8.74%	31.92%	-23.18***
Other Backward Caste	48.33%	46.36%	1.97%
Hindu	97.62%	99.28%	-1.66***
Muslim	2.02%	0.43%	1.59***
Other Religion	3.61%	2.89%	0.72%
Average Household Size	5.83	5.22	0.60***
Father's Education - highest level completed	3.54	3.51	0.03
Mother's Education - highest level completed	1.54	2.35	-0.82***
Female Child	47.89%	49.40%	-1.51%
Mean Wealth Index	0.34	0.31	0.03***
School Enrolment[6]	33.53%	32.56%	0.96%
% HHs with NREGA job card[7]	69.81%	48.40%	21.41***
% HHs participating in NREGA within last year[7]	59.00%	44.65%	14.35***

The Young Lives data tracks child characteristics for two age cohorts, the first aged one in 2002, and the second aged eight in 2002. Their respective ages at the times of surveying and NREGA implementation are tracked in Figure 2 in the previous section. As I discuss later, several studies have noted differential effects of social programs by age cohort, so I examine the results of NREGA on both the aggregate and age-disaggregated samples.

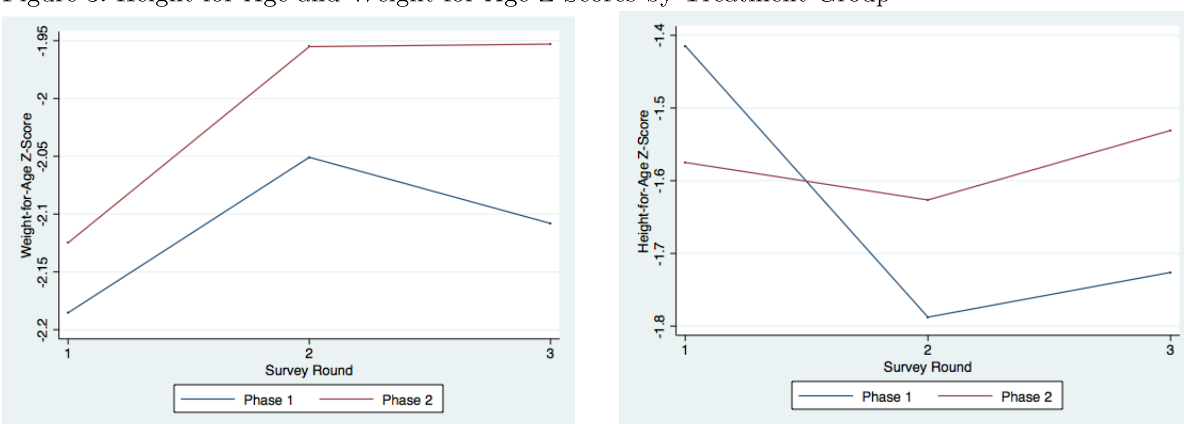
While the Young Lives surveys from 2007 and 2009 directly ask household members whether they participate in NREGA and how many work days they've completed, I do not use household participation levels in my analysis due to the risk of endogeneity, as households taking up NREGA may simultaneously make other investments in child health and intellectual capital. These figures are detailed above in Table 1, however, to provide a sense of NREGA uptake within the sample. As shown, by 2009, nearly 60% of

[5] See news article on IDHS-II accessible here:
http://www.popcenter.umd.edu/research/selected_research/research_1288898821143_desai
[6] Statistics are for older cohort only, as younger cohort not yet of school age. School enrollment rises across districts to approximately 90% by Round 2 and remains at that level for round 3.
[7] From 2009 Round 3 data (NREGA already administered in all districts)

early phase-in districts are actively participating in NREGA.

Table 1 above also presents a range of descriptive statistics on the early and late phase-in districts. As expected, there is a sizeable backward caste population within the sample. Somewhat surprisingly, we see that the late NREGA phase-in districts, which are designed to be wealthier, have a lower wealth index on average than the early phase-in districts in the sample. This is likely due to the larger lower-caste population in the late districts. As health and test performance also tend to be related to economic status, the imbalance presented in this sample appears somewhat in the anthropometric summary statistics presented in Figure 3 below. As shown, early treatment districts actually begin with higher height-for-age z-scores than late phase-in districts, though the confidence intervals are wide.

Figure 3. Height-for-Age and Weight-for-Age Z-Scores by Treatment Group



V. Empirical Strategy

To measure the intent-to-treat effects of NREGA, I use a difference-in-differences estimation strategy to exploit differences in timing and geography between early and late treatment districts in Andhra Pradesh. The Young Lives sample spans six rural districts, of which four received NREGA programming between April 2006 and March 2007. The remaining two districts did not receive programming until April 2007, after the second Young Lives survey, allowing for clean identification of program treatment status in the data. The Phase I districts compose the treatment group in our study, while the Phase II and III districts compose the control group. Given the three rounds of survey data from Young Lives, I am able to measure both the short and longer-term effects of NREGA treatment on districts. The basic formulations are modeled using the regression equations below:

$$Y_{it} = \beta_0 + \beta_1 NREGA1_i + \beta_2 Round2_t + \beta_3(NREGA1_i * Round2_t) + e_{it} \quad (1)$$

$$Y_{it} = \beta_0 + \beta_1 NREGA1_i + \beta_2 Round3_t + \beta_3(NREGA1_i * Round3_t) + e_{it} \quad (2)$$

$$Y_{it} = \beta_0 + \beta_1 NREGA1_i + \beta_2 Round2_t + \beta_3 Round3_t + \beta_4(NREGA1_i * Round2_t) + \beta_5(NREGA1_i * Round3_t) + e_{it} \quad (3)$$

In the equations above, Y_{it} refers to health and cognitive outcomes for child *i* in time period *t*. Model 1 estimates the short-term treatment effects of NREGA by conducting a difference-in-differences using only the first two rounds of data. NREGA1 takes on a value of 1 if the child is in an early treatment district (the treatment group), and 0 otherwise. Round2 takes on a 1 for the second round observations from the sample. The interaction term presents the main coefficient of interest, as it indicates post-intervention observations from the treatment group and captures the effect of NREGA programming. In this sample, the phase I districts will have received treatment for approximately twelve to eighteen months by round 2 of the Young Lives survey, compared to no treatment for the phase II and III districts.

In model 2, I look at the longer-term effects of NREGA between rounds 1 and 3 of the Young Lives data. The variables are structured similarly to model 1, with β_3 again representing the coefficient of interest capturing the effect of receiving treatment for three to four years, compared to about two years among the control districts. Model 3 is the combined regression I use to simultaneously estimate both short- and long-term treatment effects. The error term contains factors that may be related to child development such as caste, age, religion, parental education, wealth, and community resources, which I control for in the initial specifications reported in section VI. For each health and cognitive ability regression, I also add pre-intervention characteristics from round 1 to control for early-childhood differences that may persist. However, as there may still be unobservable child-specific factors that affect health and cognitive ability, I also add child-fixed effects (X_{ni} in model 4) in my secondary specifications that cause any unobserved characteristics and the time invariant variables in X_i and X_m (individual/household level and mandal level characteristics, respectively) to drop out.

$$e_{it} = X_i + X_m + X_{ni} + u_{it} \qquad (4)$$

As NREGA was not administered randomly, but rather in order of a calculated backwardness index (based off backward caste populations and community resources), it is important to include the controls or first-differencing in the regressions to avoid biasing the effects of NREGA.

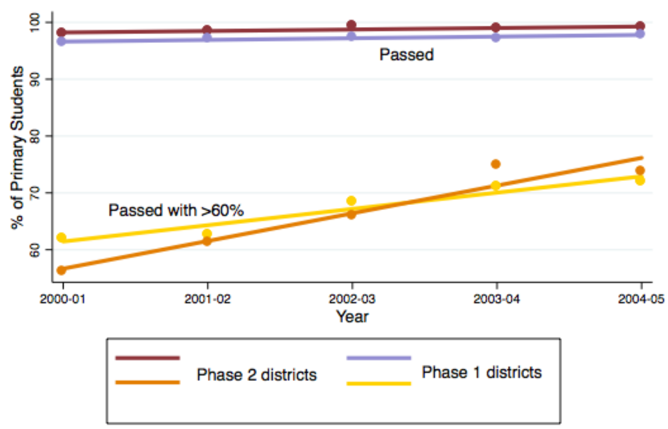
Using model 3 with and without child fixed effects, I test three child development measures spanning health and cognitive ability. To measure health, I use height-for-age and weight-for-age z-scores, calculated using growth standards provided by the World Health Organization (WHO) and the Center for Disease Control (CDC) respectively. The WHO currently only provides continuous growth curves for height-for-age and BMI-for-age for children aged 2-20. As BMI is calculated directly from child height, I instead use the weight standards provided by the CDC for children and young adults to capture more short-term variations in health, supplementing the longer-term variation captured in height-for-age. It is worth noting, however, that health professionals in India and other developing countries have found both the WHO and CDC growth charts to over-diagnose stunting and underweight in their child populations (Khadilkar 2011). Future work might consider using the 2007 Affluent Indian Growth Charts that are said to be more suitable for Indian child health studies (Khadilkar 2011).

I also use the Peabody Picture Vocabulary Test (PPVT) administered in rounds 2 and 3 of the Young Lives survey to measure cognitive ability. The PPVT is widely used as rough measures of receptive vocabulary and scholastic aptitude. I estimate these regressions using model 2 and the second two rounds of Young Lives data.

Parallel Trends Assumption

The difference in differences strategy relies on the assumption that control and treatment groups would follow similar trajectories in the absence of treatment. While this is difficult to ascertain without rich pre-intervention data, I use pass-fail statistics from the District Information System for Education (DISE) from 2000-2005 to produce rough plots of the treatment and control group trajectories.[8] As shown in Figure 4, the percentage of children passing their examinations in primary school is roughly parallel between groups leading up to NREGA implementation. The lower trend lines in Figure 4, however, reflect the percentage of children scoring over 60% on their examinations during this same period, and suggest that children in the late treatment districts may have been improving their test performance leading up to program inception. As specific grading standards remain widely inconsistent across districts, the broader definition of 'pass' reflected in the upper trend lines may be a more reliable metric of performance in school (Kidwai 2013). Since each yearly mean is also calculated from a set of two to four data points, we should also be wary of these trend lines due to the wide confidence intervals.

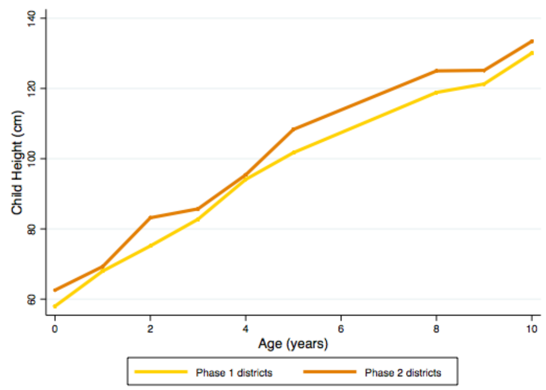
Figure 4. Primary School Pass-fail Trends in Sample Districts, 2001-5

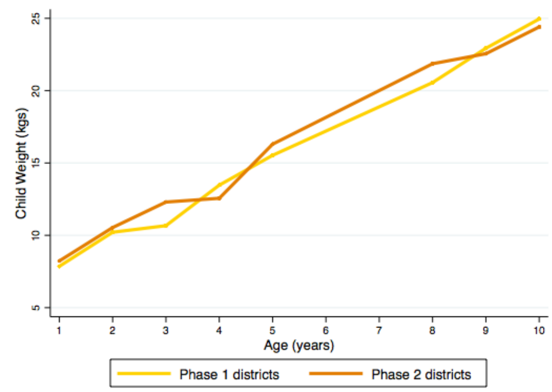


Little anthropometric data is available on children in India. In the absence of time series data to test the parallel trends, I plot child height and weight by age from the 2005 Indian Human Development Survey (IHDS), as shown in Figure 5 below. These figures portray the mean height and weight for children in rural districts of Andhra Pradesh from the IDHS, which was conducted between November 2004 and October 2005, ending a month after NREGA began being implemented (Desai 2005). Though we cannot confirm child growth trajectories during the pre-intervention period, we see that in the time preceding treatment, both groups have similar growth levels across age groups. It seems plausible to assume that parallel trends hold for the health levels for the sample districts, though education trends may diverge given the lower set of trend lines in Figure 4 above.

Figure 5. Average Child Height and Weight in Sample Districts, 2005

[8] DISE provides data on school achievement, infrastructure, and demographics at the district, mandal and school level through their online database, accessible at: http://schoolreportcards.in/SRC-New/RawData/RawData.aspx





VI. Results

I begin identifying program effects by estimating model 3. Prior to this, I restrict the sample to only rural communities and drop observations from children who migrate between rounds, as we do not have information on their new communities and cannot tell when they received NREGA treatment. For the anthropometric regressions, I also discard observations that appear to be outliers (Height-for-Age z-scores above magnitude 6; Weight-for-Age z-scores above magnitude 5). Following these procedures, I only retain children from whom I have complete data for all three rounds of the Young Lives survey. After restricting to the rural sub-sample, the subsequent restrictions only limit the remaining observations by 5%.

Attrition within the Young Lives is also inconsequential, as less than 3% of the children do not appear in all three rounds of the survey. Given the low attrition rate, I assume there is no selective attrition biasing the remaining sample.

Table 2 presents the results from the initial difference-in-difference regressions. We see that for height-for-age, the point estimates are significant and negative for the 'round' variables, matching the curves we had seen in Figure 3. It seems that children seem to drop about 0.15 standard deviations below the WHO-calculated mean between rounds 1 and 2, and approximately 0.4 standard deviations below the mean between rounds 1 and 3. This seems to suggest children in this sample tend to worsen along long-term measures of health such as Height-for-Age. This finding is supported by literature on child anthropometrics in developing countries that find that divergences in health widen between wealthy and poor populations over time (Guntupalli 2007; Liu et al 2013).

As expected, we also see that wealth has a large positive and statistically significant impact on height-for-age, weight-for-age, and PPVT scores. Notably, the wealth coefficient has a larger magnitude for height-for-age than weight-for-age, the former being a 'long term measure of depravation. Interestingly, I also find that girls from this sample tend to have better weight-for-age z-scores than boys, though the magnitude of the coefficient is small. Girls tend to be about 0.08 standard deviations above boys for their weight-for-age z-scores. Pre-intervention health levels also largely explain current health, as shown in the round 1 health term coefficients. School enrollment also has a strong effect on PPVT scores; being enrolled in school relates to scoring about 27 points higher on the PPVT.

For all three outcome variables, however, we see that the interaction variables capturing the treatment effects are statistically insignificant. A potential explanation may be the clustering of errors at the district level, rather than a lower unit such as mandal or village (as Mani et al. 2013 chose to do with the Young Lives data set) as there are only six rural districts in the Young Lives data, compared to 85 mandals or groupings of villages. According to Cameron and Miller (2013), errors should be clustered according to the perceived group structure within the sample; intuitively, mandals make sense as administrative units through which NREGA is administered. However, as I only have data on treatment timing by district, I report district-wise clustered errors to present a conservative estimate of statistical significance.

Table 2. Difference-in-Difference regressions

	Height-for-Age Z-Score (1)	Weight-for-Age Z-Score (2)	PPVT Raw Score (3)
NREGA1	0.0328 (0.0660)	-0.0235 (0.0273)	-5.4341 (4.5421)
Round 2	-0.1498 (0.0343)***	0.0778 (0.0530)	
Round 3	-0.4174 (0.0926)***	0.0010 (0.0534)	-24.2444 (2.5088)***
NREGA1*R2	-0.2970 (0.1644)	-0.0605 (0.0870)	
NREGA1*R3	-0.2624 (0.1482)	-0.0858 (0.0612)	8.5775 (6.7827)
Age (months)	0.0014 (0.0006)*	0.0016 (0.0003)***	0.8573 (0.0419)***
Mother's education	0.0014 (0.0032)	0.0031 (0.0039)	1.1038 (0.1843)***
Father's education	0.0005 (0.0028)	0.0016 (0.0034)	0.6318 (0.1400)***
Wealth Index	0.6300 (0.0484)***	0.2416 (0.0609)***	17.8572 (3.6482)***
Household size	-0.0095 (0.0032)**	-0.0019 (0.0041)	-0.2979 (0.2207)
Scheduled caste/tribe	-0.0248 (0.0839)	-0.0303 (0.0251)	4.1764 (3.5249)
Other backward caste	-0.0617 (0.0233)**	-0.0953 (0.0248)***	-3.4634 (1.6596)*
Hindu	-0.2141 (0.1728)	0.0062 (0.1736)	-5.1221 (3.9356)
Muslim	-0.2603 (0.2058)	0.0134 (0.2102)	-5.7770 (6.3907)
Female	-0.0466 (0.0266)	0.0754 (0.0205)***	-5.4187 (0.9150)***
Round 1 HfA	0.6166 (0.0273)***		
Round 1 WfA		0.6692 (0.0134)***	
Enrolled in School			27.0057 (1.9748)***
Constant	-0.4954 (0.2244)*	-0.8233 (0.0904)***	-31.4723 (7.9412)***
Observations	5675	5673	3636
R-squared	0.4598	0.5573	0.6642

Note: *p<0.1; **p<0.05; ***p<0.01 Robust standard errors clustered by district (N=6)

The direction of the treatment coefficient for PPVT scores makes sense; receiving NREGA treatment for one to two more years as a phase 1 district, compared to being in a phase 2 district, corresponds approximately to an 8-point increase in test scores. It is worth noting the negative point estimates of the height-for-age and weight-for-age treatment variables (current p-value of approximately 13%; become significant when errors clustered at mandal). The estimates suggest that, in the absence of NREGA, the phase 1 districts would have 0.3 standard deviations higher z-scores for height for age, and 0.06-0.8 standard deviations higher z-scores for weight-for-age. Conceptually, this makes little sense as we would expect positive, if any, effects from NREGA. The only other child health evaluation of NREGA conducted by Dasgupta (2013) also shows negative, but insignificant, point estimates on height-for-age. We see in the child-fixed effects regressions in Table 3 a similar pattern in which the treatment point estimates remain negative but with p-values closer to 50%, suggesting the coefficients will remain statistically insignificant regardless of error clustering specifications. These findings refute the hypothesis that NREGA has positive program impacts on child health, as measured through anthropometrics. The implications and possible limits of these findings are further discussed in the following section.

Table 3. Difference-in-differences Regressions with Child Fixed Effects

	Height-for-Age Z-Score (1)	Weight-for-Age Z-Score (2)	PPVT Raw Score (3)
Round 2	-0.8931 (1.9296)	0.3351 (0.5147)	
Round 3	-1.5956 (3.1083)	0.4145 (0.7809)	-6.2968 (115.1653)
NREGA1*R2	-0.2943 (0.2184)	-0.0676 (0.1028)	
NREGA1*R3	-0.2622 (0.1991)	-0.0896 (0.0698)	9.7708 (9.6405)
Age (months)	0.0155 (0.0365)	-0.0030 (0.0085)	0.7784 (3.5558)
Mother's education	-0.0109 (0.0102)	0.0314 (0.0024)***	
Father's education	0.5258 (0.0223)***	0.6590 (0.0299)***	
Wealth index	0.5920 (0.1598)***	-0.0008 (0.1385)	6.0417 (10.3331)
Household size	-0.0247 (0.0174)	-0.0011 (0.0093)	0.4144 (0.6811)
School enrollment			10.2400 (3.2976)**
Constant	-3.9081 (1.5806)**	-4.4569 (0.4740)***	-31.5637 (332.3991)
Observations	5675	5674	4030
R-squared	0.6642	0.755	0.8894
Adjusted R-squared	0.4798	0.6205	0.7712

Note: *p<0.1; **p<0.05; ***p<0.01 Robust standard errors clustered by district (N=6)

As there may still be unobserved sources of bias due to child characteristics, Table 3 presents regression estimates with child fixed effects that eliminate any time-invariant factors. This is particularly important for studying NREGA as program roll-out followed a backwardness ranking rather than random selection;

differencing out the selection factors allows us to compare more evenly between control and treatment districts. Even with the child-fixed effects here, we see again that the program treatment effects remain insignificant, with larger p-values. Though NREGA does not seem to have an impact, changes in parental education, particularly the father's education, lead to improvements in child health, likely through an income effects channel. Another year of parental education corresponds to a rise of 0.5 to 0.67 standard deviations in height-for-age and weight-for-age z-scores. We also see that income remains a significant determinant of health, with a point-increase in the wealth index corresponding to a 0.6 standard deviation rise in height-for-age. As with previous studies that examined the effects of boosts in income on child development, household characteristics and decision-making seem to have the most significant impacts on health (see Blau 1999; Burgess et al 2004).

As none of the parents in the sample add levels of education between rounds 2 and 3 of the data collection, we cannot look at the impacts of education on child test scores. As expected, however, being enrolled in school does relate to higher performance on the PPVT. Mani et al (2013) run similar specifications using the Young Lives data but cluster their errors at the mandal level to find NREGA program effects to be statistically significant. They also limit their study to observations from the older Young Lives cohort, but as I find in the following table, the younger cohort drives most of the results across the outcome variables.

Table 4. Difference-in-Differences Regressions by Age Cohort with Child Fixed Effects

	Height-for-Age Z-Score		Weight-for-Age Z-Score		PPVT Raw Score	
	Older Cohort	Younger Cohort	Older Cohort	Younger Cohort	Older Cohort	Younger Cohort
Round 2	-0.122	-2.208	1.584	0.033		
	(0.04)	(1.28)	(1.30)	(0.04)		
Round 3	-0.400	-3.678	2.472	-0.079	2.538	2.363
	(0.09)	(1.32)	(1.27)	(0.06)	(0.02)	(0.02)
NREGA1*R2	0.011	-0.453	0.145	-0.147		
	(0.07)	(1.35)	(0.90)	(1.40)		
NREGA1*R3	0.069	-0.434	-0.016	-0.145	5.074	12.096
	(0.49)	(1.39)	(0.17)	(1.83)	(0.60)	(1.16)
Child Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.61	0.68	0.71	0.76	0.83	0.75
N	2,118	4,075	2,118	4,075	1,381	2,649

Note: *p<0.1; **p<0.05; ***p<0.01 Robust standard errors clustered by district (N=6)

In these regressions, the program variables remain statistically insignificant when errors are clustered at the district level. It is interesting to consider possible explanations for the negative point estimates for the health outcomes for solely the younger cohort. As the results do not relate to theory, a likely explanation is incongruent trend lines in the absence of treatment for the younger cohort. As the NREGA districts were chosen based on a backwardness index, it may be the case that more 'backward' districts developed differently than later program districts. This development may have disproportionately affected younger children who are more sensitive to their surroundings and available resources. This scenario accords with the point estimates for the older cohort, which are largely positive and grow in magnitude over the rounds. For the height-for-age, NREGA has a small but increasing impact on older children, as shown the interacted variable coefficients in column 1. There is a larger positive short-term impact of NREGA on weight-for-age for the older cohort, as shown in column 3, but this figure turns negative when examining the long-term effects. Intuitively, this seems reasonable as weight-for-age, itself a measure of more variable health in the short term, may receive a boost within the first 1 to 1.5 year period, but no significant boost over a longer stretch of time. This would explain the near-zero point estimate in the NREGA-R3 interaction variable. Regardless of these possible narratives, the present estimates remain statistically insignificant. I consider further statistical and theoretical explanations for these results in the following section.

VII. Discussion

The findings from both sets of difference-in-difference regressions ran contrary to the expectations of NREGA's impacts on child development. Treatment coefficients were largely insignificant, though these corroborate previous findings related to NREGA's child impacts as documented by Das and Singh (2012) and Dasgupta (2014). Possible explanations for these findings are documented below.

Firstly, we consider the statistical limitations of this study. Using the difference-in-difference estimation strategy, I assumed that the treatment and control groups – early and late NREGA phase-in districts respectively – would have behaved similarly in the absence of program implementation. I was only able to check for parallel trends for the cognitive ability child outcomes, and even so found some evidence of differences in the trajectories between districts. As with many studies related to child development, limited data availability make it difficult to track children's outcomes thoroughly over a period of time. Others such as Imbert and Papp (2012) have applied the difference-in-difference strategy to NREGA districts but examined the entire range of districts in India, rather than a restricted sample as I have here. As the Young Lives survey intended to reach poorer rural populations, the results derived from analyzing this unrepresentative sample likely have low external validity. There is also always the possibility that trends diverged between control and treatment groups during the intervention period – a counterfactual we can never truly know.

Other statistical problems may relate to the lack of precision accorded by our district-level intent-to-treat analysis. Firstly, the intent-to-treat analysis is useful for maintaining pseudo-randomization across the sample, but may produce modest results if the rate of uptake in the sample is moderate, as is the case here (~50%). We are able to analyze the effect of policy broadly, rather than the specific impacts of income boosts in a household, for example. Furthermore, in this study, I only had data for three periods, rather than yearly or monthly data that would allow me to consider variations in timing between mandals rather than entire districts. As the treatment timings were aggregated by district, I essentially had six district data points that I was tracking. These aggregations may not accurately represent local program impacts as field researchers have documented significant mandal-level variation in administration and coverage of NREGA (regarding project availability and wages). A richer data set and mandal-level identifiers would permit future research to examine the exact treatment effects of NREGA uptake and may provide different results.

Accepting these possible statistical limitations, these results suggest that for this sample, NREGA's program effects on child health and cognitive ability were negligible. This may indicate that increases in household income do not correspond to increased household spending on resources that affect child development, such as food items or school resources. It may also be the case that the child may not receive time savings from NREGA implementation, or that time savings may not correspond to improvements in child development.

VIII. Conclusion

The Mahatma Gandhi National Rural Employment Scheme is one of the largest government undertakings worldwide that seeks to improve living conditions for the poor. While it acts primarily through boosting incomes of households, program conditions and spillover effects may also affect household's time availability and decision-making environment. Few evaluations of these broader effects have been studied or documented, partially due to want for better administrated programming. In Andhra Pradesh, however, reports suggest that the NREGA performs sufficiently well to study second-order impacts. In this study of child development – particularly health and cognitive ability – I find that NREGA has yet to make a significant impact on children of treatment districts. Receiving treatment for a range of time from one year to four does not seem to largely influence the direction of child growth in height, weight, or cognition. These findings are necessarily subject to the limits of the Young Lives data set, an unrepresentative sample of rural children in Andhra Pradesh. The difference-in-differences framework used is also contingent upon the parallel trends assumption holding for the sample treatment and control groups, for which I found limited evidence.

Future work may employ richer data to analyze differences in impact by child sub-samples, as other studies have documented differential effects felt by different age and gender groups. In this data sample, I found reasonable positive effects for older children, whose sub-sample seem more likely to fit the parallel trends assumption. The literature suggests, however, that older children are most likely to feel negative impacts on their development outcomes from NREGA and similar programs, due to higher payoffs on the labor market rather than in school or the home (Afridi et al 2012). The literature also documents different impacts on boys and girls due to intra-household dynamics and prioritization of male over female outcomes (Jayachandran and Pande 2015). In this data, gender-disaggregated regressions returned no statistical significance and high p-values remained over the statistical significance thresholds with smaller error clusters (see Appendix A). Qualitatively, these results were similar to those of the aggregated sample.

While child outcomes are only a secondary goal of social welfare programs like NREGA, it is in the interest of policy makers to improve outcomes for other household members in NREGA treatment districts. As aspects of the program that focus on children, such as onsite child-care facilities, become more prevalent, further attention should be paid on evaluating child outcomes and developing a holistic analysis of program efficacy.

Appendix A

Table 5. Difference-in-Differences Regressions by Gender with Child-Fixed Effects

	Height-for-Age Z-Score		Weight-for-Age Z-Score		PPVT Raw Score	
	Male	Female	Male	Female	Male	Female
Round 2	0.516	-3.220	1.019	0.105		
	(0.26)	(1.11)	(0.85)	(0.13)		
Round 3	0.590	-5.242	1.469	0.120	7.354	7.381
	(0.19)	(1.13)	(0.77)	(0.10)	(0.07)	(0.06)
NREGA1*R2	-0.294	-0.300	-0.100	0.015		
	(1.74)	(1.12)	(1.97)	(0.09)		
NREGA1*R3	-0.297	-0.240	-0.082	-0.118	8.638	10.304
	(1.85)	(1.12)	(1.18)	(1.55)	(0.76)	(1.26)
Child Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.69	0.61	0.76	0.72	0.89	0.89
N	3,202	2,991	3,201	2,992	2,080	1,950

Note: *p<0.1; **p<0.05; ***p<0.01 Robust standard errors clustered by district (N=6)

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