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Impact of the NREGS on Schooling and Intellectual Human Capital

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Keywords
National Rural Employment Guarantee Scheme, NREGS, Schooling, Test Scores, Panel Data, India, Reading, Writing, Math, Gender, Enrollment, Placement, Young Live Panel Study, Grade Progression

Disciplines
Education | Educational Assessment, Evaluation, and Research | Educational Sociology | Growth and Development | Other Social and Behavioral Sciences | Rural Sociology | Social and Behavioral Sciences | Sociology

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Impact of the NREGS on Schooling and Intellectual Human Capital[a]

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Abstract: This paper uses a quasi-experimental framework to analyze the impact of India's largest public works program, the National Rural Employment Guarantee Scheme (NREGS), on schooling enrollment, grade progression, reading comprehension test scores, writing test scores, math test scores and Peabody Picture Vocabulary Test (PPVT) scores. The availability of pre and two rounds of post-intervention initiation data from the three rounds of the Young Lives Panel Study allow us to measure both the short- and medium-run intent-to-treat effects of the program. We find that the program has no effect on enrollment but has strong positive effects on grade progression, reading comprehension test scores, math test scores and PPVT scores. The average effect size computed over several outcomes is similar to the effects of conditional cash transfer programs implemented in Latin America. These short-run impact estimates all increased in the medium run, that is, there is no decaying of impact but instead medium-run augmentation of the estimated short-run effects. The findings reported here are robust to attrition bias, endogenous program placement, type I errors and type II errors.

Keywords: NREGS, Schooling, Test Scores, Panel Data, India
JEL Classification: I 25, I38, J 13

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1. Introduction

During the fiscal year 2013-2014, the government of India allocated over US$ 5.5 billion for its largest public works program and flagship social protection program, the National Rural Employment Guarantee Scheme (NREGS).[5] The NREGS provides 100 days of unskilled wage employment to any household residing in poor rural areas whose adult members chose to work in the program. Employment opportunities made available in the NREGS are closely tied to the construction and maintenance of public goods in the community. Despite public works programs offering a promising solution for some dimensions of poverty eradication, critics of these programs have expressed skepticism about their usefulness.

Existing evaluation studies of the NREGS suggest positive impacts on employment, consumption expenditure and income. Ravi and Engler (2009) use both cross-sectional and panel data from Medak district in Andhra Pradesh and find that per capita expenditure on food and certain categories of non-food consumables and the probability of savings are all higher among households that participate in the NREGS. Liu and Deininger (2010) use panel data from five districts in Andhra Pradesh and find that participating households have higher per capita consumption expenditure and aggregate calorie and protein intakes in comparison to non-participating households. Azam (2011) expands the analysis of the program using nationally representative data from multiple rounds of the National Sample Survey and finds that the implementation of the NREGS is positively related to increases in male and female labor force participation rates and real wages for men and women with the effects being stronger and larger for women. Evaluation studies using data from other states of India conducted with varying degrees of rigor also report positive benefits associated with access and participation in the scheme [Tiwari et. al 2011, Nayak and Khera 2009]. All the studies reviewed in Subbarao (2003) and Betcherman et. al (2000) on other public works programs than the NREGS focus on household-level outcomes and individual-level labor market outcomes and in general find positive effects of public works program on labor force participation and consumption expenditure.

Anti-poverty programs like the NREGS, however, are often argued to serve as policy instruments for multidimensional poverty alleviation, not just consumption expenditure and therefore likely to improve educational outcomes for children in multiple ways. First, the NREGS is likely to directly result in changes in parents' labor supply decisions,

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[5] http://www.hindustantimes.com/India-Budget-2013/Chunk-HT-UI-IndiaBudget2013-Economy/NREGA-losing-sheen-Allocation-unchanged/SP-Article1-1019269.aspx

which are positively (negatively) related to improvements in children's schooling if the income (substitution) effect outweighs the substitution (income) effect.[6] Second, the NREGS can increase schooling outcomes through improvements in community-level public good provision such as improvements in roads, and water supplies. Third, the NREGS can result in improvements in household income/per capita consumption that are not led by increased labor force participation but instead by improvements in community-level infrastructure targeted at flood control, land development and maintenance of irrigation system and canals.[7] Since, there are multiple channels through which the NREGS may affect children's outcomes, attempting to capture these effects through parents' labor force participation alone, as has been the focus in most of the literature on effects of the NREGS to date, can therefore result in misleading findings about the net effect of the NREGS on measures of intellectual human capital.

While the large literature summarized above examines the effectiveness of NREGS on employment, consumption expenditure and income, there is very limited direct evidence on how such programs benefit children. Three exceptions are Uppal, 2009; Afridi et. al 2012 and Dasgupta 2013, but none of these papers have examined the impact of the NREGS on grade progression and measures of cognitive skills, which in recent studies have been found to be more strongly related to wage earnings than schooling attainment.[8]

To close this important gap in the literature, we use a quasi-experimental approach to examine the net effect of the NREGS on children's enrollment, grade progression, reading comprehension test scores, writing test scores, math test scores and PPVT scores, using data from three rounds of the Young Lives Panel Study in India. We combine pre- and post- intervention initiation data in an experimental framework to measure the short- and medium-run effects of the NREGS on intellectual human capital.

The NREGS was rolled out in a phased-in manner, the first phase of the program was rolled out between the 2002 (round 1) and 2007 (round 2) waves of the Young Lives Panel Study and targeted approximately 200 poorest rural districts of India. By the end of the third round of the Young Lives Panel Study in 2009-10 (and Phase II and III of the nation wide program rollout), the NREGS was placed in all remaining rural districts in India. We combine the availability of pre- and two rounds of post intervention initiation

[6] See Behrman and Knowles (1999) for a review on the relationship between household consumption and schooling investments in children.

[7] Dasgupta (2013) finds that it is only children exposed to drought shocks in the past that benefit from the NREGS.

[8] For instance, Behrman et. al (2009), using data from Guatemala and treating all measures of human capital as endogenous, show that, controlling for height and fat-free body mass, improvements in reading comprehension test scores rather than schooling attainment are related to increases in earnings.

data in a quasi-experimental framework to examine the intent-to-treat effects of the NREGS on measures of children's intellectual human capital.

To our knowledge, this paper is the first to quantify the short- and medium-run intent-to-treat effects of having access to the NREGS on schooling and intellectual human capital. A number of important findings emerge from our analysis. First, we find that NREGS was primarily placed initially in poor communities. We find that communities that have higher levels of pre-intervention population, and fewer hospitals and health centers were more likely to receive the program first. Therefore controlling for program placement is important for our investigation, which we do through first-differencing. Second, the NREGS has no effect on schooling enrollment, a relatively short-run measure of investment in intellectual human capital. Third, access to the program has large and positive effects on children's performance on reading comprehension, math and PPVT scores, relatively longer-run measures of intellectual human capital. The average effect size in the short run suggests a 0.08 standard deviation improvement in schooling and cognitive outcomes for children assigned to the early phase-in NREGS districts in comparison to children not assigned to the program early on. Fourth, short-run effects of the program are all sustained in the medium run, that is, there is no decaying of observed treatment effects but instead augmentation of short-run treatment effects. The medium-run average effect size increases to 0.15 standard deviations indicating a persistent difference in cognitive outcomes between children living in the early phase-in districts and late phase-in districts. Finally, our impact estimates are robust to a number of concerns – attrition bias, type I errors, type II errors, and endogenous program placement.

The rest of the paper is organized as follows. A complete description of the National Rural Employment Guarantee Scheme is provided in section 2. Data description and summary statistics are provided in section 3. The conceptual framework is outlined in section 4. Findings are reported in section 5. Finally, concluding remarks follow in section 6.

2. The program: National Rural Employment Guarantee Scheme

NREGS came into effect in September 2005 and is effective in all states of India since then except for the state of Jammu and Kashmir, where it came into effect in December 2007. On 2nd October 2009 the National Rural Employment Guarantee act was renamed as the Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA).[9] The

[9] We use the term National Rural Employment Guarantee Scheme (NREGS) throughout the paper even though the name was changed to Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS) in 2009 because NREGS was the name for the period studied.

act guarantees at least 100 days of unskilled wage employment to any household residing in rural areas whose adult members (18 years and older) chose to work in the program. The Act also sets aside a special quota for women; at least one-third of all beneficiaries in the program must be women. It also requires that a 60:40 wage-to-material ratio be maintained in all public works projects.

The NREGS focuses on the construction of community-wide assets targeted to improve water conservation and rain water collection, rural connectivity, flood control, irrigation canals, drought proofing, and land development. The adult household member interested in seeking unskilled wage employment as part of this scheme has to submit an application to the Gram Panchayat (village council). This application is further sent to the Mandal Computer Centre (MCC) for the creation of a job card. This job card is used to seek unskilled employment in the village and, at least per the act, the entire application process has to be completed within 15 days of submitting the application. If employment is not provided to the job seeker within 15 days of the application, then the state is required to compensate the job seeker for delays by paying an unemployment allowance.

The NREGS was rolled out in three phases. During phase I, between September 2005 and February 2006, the scheme was targeted to the 200 poorest districts in India identified using the backwardness index developed by the Sharma Committee in the Planning Commission. By May 2007 the second phase of the program was rolled out and covered an additional 130 districts and finally by April 2008 the program reached out all remaining rural districts in India. This is India's largest public works program aimed at eradicating rural poverty.

The Ministry of Rural Development, Government of India (2006-2007 annual report) shows that more than 200 million households, of which over 10% resided in the state of Andhra Pradesh alone, were employed under the NREGS during 2006-2007. Given the large-scale nature of this public works program, it has generated great interest among policy makers and academicians to understand the effectiveness and benefits of this anti-poverty program. However, as noted earlier in the introduction, to date there is no evidence on how the NREGS affects children's grade progression and cognitive development. In this paper our focus is to provide evidence on the impact of the NREGS on grade progression and cognitive development, which in the long run are strongly related to economic and social well-being.

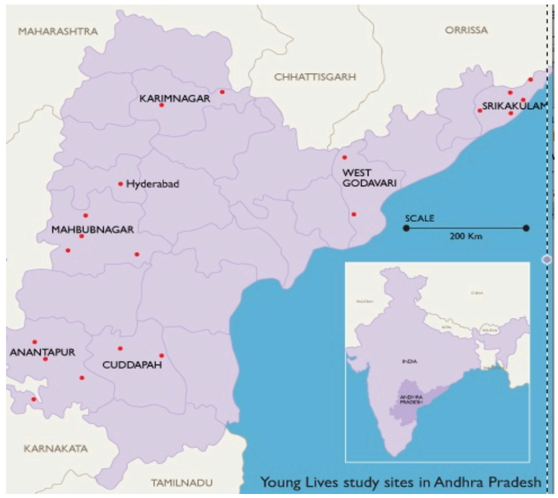
3. Data

3.1 Young Lives Panel Study

The data used in this paper comes from the Young Lives Panel Study – a panel survey that collects a rich set of data on children's human capital (health, schooling, cognition, illness), parents' human capital (completed grades of schooling and height), household assets, household per capita consumption expenditure, and incidence of shocks from children in four different countries – Ethiopia, India (only includes the state of Andhra Pradesh), Peru and Vietnam. The Young Lives sample in Andhra Pradesh was selected from 20 sentinel sites and within each site, 100 households with a 1-year-old child (younger cohort) and 50 households with an 8-year-old child (older cohort) were randomly selected for survey purposes in 2002 (round 1). The younger cohort (1-year-old in 2002) and the older cohort (7-8-years-old in 2002) were subsequently re-surveyed during the 2007 (round 2), and the 2009-10 (round 3) waves of the survey. Our analysis sample is restricted to only include the older cohort since the majority of the pre-intervention data collected in 2002 on grade progression, reading comprehension, and writing ability are only available for this cohort. The sentinel sites were chosen to represent all three agro-climatic (Coastal Andhra Pradesh, Rayalseema, and Telangana) regions of Andhra Pradesh. See Kumra (2008) for further details on the sampling approach adopted by the Young Lives Panel Study in Andhra Pradesh.

The Young Lives Panel Study covers the following six districts – Cuddapah, Anantapur, Mahbubnagar, Karimnagar, West Godavari, Srikakulam and the city of Hyderabad in Andhra Pradesh (see Figure 1). Since the primary objective of this paper is examine the impact of the NREGS, which is only implemented in rural areas, we restrict our analysis sample to only include rural areas. The National Rural Employment Guarantee Act was enacted in 2005 and by 2007 (round 2) implemented in four (Cuddapah, Anantapur, Mahbubnagar, Karimnagar) of the six Young Lives districts in Andhra Pradesh. By the second round of Young Lives data collection in January-June 2007, 70% of the sample residing in the NREGS districts were registered in the NREGS, suggesting sufficient coverage of the program to have had effects on schooling outcomes during this period. By the third round of the Young Lives Panel Study in 2009-10, the NREGS was phased-in all remaining rural districts in India including the remaining two districts (West Godavari and Srikakulam) in the Young Lives Panel Study in Andhra Pradesh. By this time, approximately 81% of the households residing in the early phase-in districts and 75% of the households residing in the late phase-in districts were registered in the NREGS. We combine this cross-sectional and temporal variation in the introduction of the NREGS program across the Young Lives sample to examine the short- and medium-run effect of access to this scheme on measures of schooling and cognitive development.

Figure 1: Young Lives sites in Andhra Pradesh



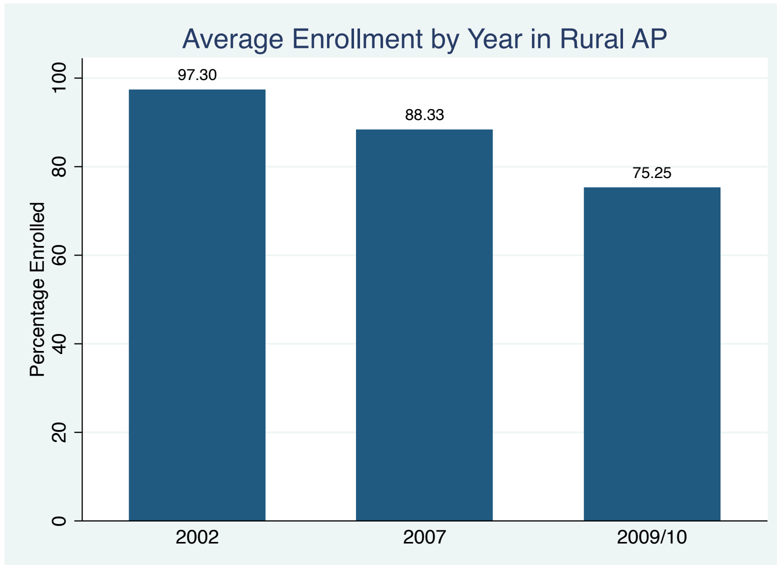
3.2 Key Variables

We focus on six outcome variables of interest – enrollment, a measure for short-run investments in intellectual human capital, and five other longer-run measures of intellectual human capital including grade progression, reading comprehension test scores, writing test scores, math test scores and Peabody Picture Vocabulary Test (PPVT) scores. These longer-run measures of cognitive development are strongly related to wage earnings [see Hanushek and Woessmann (2008) for a review of studies from both developed and developing nations].

Enrollment here is defined as a dummy variable that takes a value 1 if currently enrolled in school and zero otherwise. Figure 2 below shows that 97% of children are enrolled in school at the time of the survey in round 1 and this number decreases by almost 10 percentage points by round 2 when the children are 12 years old and further decreases to 75% when the average age of the child in the sample increases to 15 years. It is not surprising that enrollment decreases with age in this context.[10] In the results section we will further examine if the decrease in enrollment is smaller among children who had earlier access to the NREGS compared to children who had later exposure to the scheme.

[10] We compute enrollment rates for school-age children using data on five of the six Young Lives Districts covered by the 2005 Indian Human Development Survey to find similar patterns in the relationship between age and enrollment. See Figure A1 in the appendix.

Figure 2: Enrollment by Year in Rural AP



Grade progression here is defined as completed grades of schooling divided by the potential grades where potential grades are calculated as total number of grades accumulated had the individual completed one grade of schooling by age 6 and continued to accumulate an additional grade of schooling in each subsequent year. Grade progression has the following advantages over the popularly used completed grades of schooling – (a) children with the same completed grades of schooling, some of whom still in school, are treated differently depending upon their age, except if the actual completed grade is zero[11] and (b) observations on completed grades of schooling are right-censored for children currently enrolled in school resulting in censoring that biases the estimated effect of the NREGS program downwards (Tansel, 1997). We could follow the censored ordered probit specification used by King and Lillard (1983, 1987). However, this approach relies on the strong assumption that children who belong to the uncensored category (not enrolled) in any one period do not re-enter schools. We find that this assumption is frequently violated in our sample: approximately 70% of children not enrolled in school during 2002 have re-entered school by 2007 and 7% of children not enrolled in school during 2007 have re-entered school by 2009-10. Grade progression does not suffer from some of the shortcomings outlined above and hence is preferred over completed grades of schooling, the more popular measure of schooling.

[11] In our sample, approximately 1% of the children have zero grades of schooling.

We also use test score data from reading comprehension tests and writing ability tests that were administered to children in rounds 1 and 2. The reading comprehension test scores are coded on a scale of 0 to 3 [0=cannot read anything, 1=read letters, 2=read words, 3=read sentences]. The writing ability test scores are coded on a scale of 0 to 2 [0=nothing, 1=yes with difficulties, 2= yes without difficulties]. These tests are similar to the reading comprehension and writing tests administered by the Annual Status of Education Report (2012), which releases India's largest report card on schooling to give up-to-date information on children's schooling performance all over India.

During rounds 2 and 3, the Peabody Picture Vocabulary Test (PPVT), a widely-used measure of receptive vocabulary, and math tests were administered to all children. To capture relative performance and also to make the test results comparable over time, we construct percentile ranks for the math and PVVT scores in each round. Cueto and Leon (2012) show that there is significant positive correlation between PVVT scores collected in rounds 2 and 3. They also show that the levels of item difficulty in PPVT are not identical across waves and hence it is recommended to compute the relative position of the child's performance in each round, as we do using the percentile ranks here. Changes in item difficulty are captured by the variation in raw test scores over time across the math and vocabulary tests. The distribution of the raw test scores and the test scores in percentile ranks (adjusted for relative performance) are reported in Appendix Figures A2-A7.

Summary statistics on all outcome variables are reported below in Table 1, Panel A. Enrollment decreases from 97% in round 1 to 75% in round 3. Grade progression increases between the first and second rounds of data collection but decreases marginally by round 3, which coincides with the rollout of the NREGS into all remaining rural districts. Reading comprehension and writing ability both improved between waves 1 and 2. The raw scores on the math test and PPVT have also improved over time. In Table 1, Panel B we also provide the means and standard deviations on all pre-intervention right-side variables included in the final regressions.

Table 1: Summary Statistics

Panel A: Outcome variables	**2002 Mean (s.d)**	**2007 Mean (s.d)**	**2009-10 Mean (s.d)**
Enrollment	0.97	0.88	0.75
	(0.16)	(0.32)	(0.43)
Grade progression	0.85	0.89	0.88

	(0.35)	(0.18)	(0.18)
Reading comprehension test scores	1.99	2.64	NA
	(1.05)	(0.81)	
Writing test scores	1.21	1.60	NA
	(0.80)	(0.60)	
Raw math test scores	NA	5.57	8.89
		(2.27)	(5.90)
Raw PPVT scores	NA	87.36	127.44
		(24.59)	(40.27)
Math test scores (in percentile ranks)	NA	50	50
		(28.46)	(28.83)
PPVT scores (in percentile ranks)	NA	50	50
		(28.88)	(28.88)

Panel B: Control Variables	**2002**
Male dummy (=1 if male, 0 = female)	0.48
	(0.50)
Age in months	96.31
	(3.95)
Household size	5.58
	(2.06)
Number of school-age children	1.40
	(1.03)
Wealth index	0.33
	(0.16)
Raven's test scores	22.63
	(5.20)
SC/ST dummy (=1 if Scheduled caste/Scheduled	0.38
tribe, 0 otherwise)	(0.48)
OBC dummy (=1 if other backward class, 0	0.47
otherwise)	(0.49)
Religion dummy (=1 if Hindu, 0 otherwise)	0.91
	(0.28)
Mother's schooling (completed grades)	1.79
	(3.07)
Father's schooling (completed grades)	3.55
	(4.43)
Sample size	703

Notes: The sample covers rural areas only.

On average 48% of the children in our sample are male and approximately 64% of the sample is 7 years old in 2002 and the remaining 36% is 8 years old. The average household of a Young Lives child has four other members co-residing of whom there is on average at least one school age sibling of the child. The children are primarily Hindu

(91%) and around 85% of the sample belongs to backward castes (SC/ST = 38% and OBC = 47%). Average completed grades of schooling among parents are, not surprisingly, low in our sample. Mothers have completed on average less than 2 grades and fathers less than 4 grades of schooling. The wealth index is computed as a weighted average of a housing quality index (based on the number of rooms per person and the materials used for construction of the house), a consumer durables index (based on ownership of assets) and a services index (based on whether or not the household has access to key resources including but not limited to drinking water, electricity, and toilets) where each of the sub-categories is weighted equally in the index.[12] The wealth index takes a value between 0 and 1. The average value of 0.33 suggests that on average a household has only one-third of all possible resources (assets, services and durables), which suggest that children in our sample reside in extremely poor households. The Young Lives Panel Study also administered Raven's progression matrices to children in round 1 only; these scores are additionally controlled in the right-side of the first-difference specification to account for pre-intervention differences in analytical reasoning ability. The average child scores approximately 61% on the pre-intervention Raven's test administered at age 8.

3.3 Sample Attrition

The older cohort includes approximately 1000 children surveyed during September-December 2002 of which 757 resided in rural areas. During round 2, implemented during January-April 2007, the Young Lives Panel Study was able to trace approximately 97% (N=731) of the round 1 rural respondents and in round 3, implemented during August 2009-January 2010, the study was able to trace approximately 95% (N=719) of the rural children surveyed in rounds 1 and 2.[13] The overall rate of sample attrition is very low, with only around 5% of the children lost over a 7-year period, and results in an attrition rate of less than 1% per annum. We restrict our

[12] See UK Data Archive Study Number 5307 - Young Lives: an International Study of Childhood Poverty: Round 1, 2002 for further details on the construction of the wealth index.

[13] The usual school year in India begins in April and ends in March. Children surveyed during the 2002 and 2009-10 waves of the Young Lives study all belong to the same school year except for 20% of the children surveyed during round 2 in April and May 2007 (next school year). This would bias our estimates only if the children surveyed during the later months were systematically higher in the treatment districts in comparison to the control districts. We find that in treatment districts, 24% of the children were surveyed later and a similar proportion, 22% of children were surveyed later in the control districts in 2007. This difference is not statistically significant at conventional levels of significance [p-value = 0.46]. Further, the timing of the survey will only affect the computation of one variable, grade progression. We re-estimate our preferred estimates for grade progression adjusting for the difference in the timing of the survey in Appendix Table A2. We find no difference between the treatment effects reported in column 2 Panel B Table 4 for grade progression and Appendix Table A2. We can further account for changes in performance associated with the timing of the survey with the inclusion of a dummy variable which takes a value 1 if you were surveyed later in 2007, 0 otherwise. We re-estimate our preferred estimates reported in Panel B Table 4 with this additional control variable and find no difference in the treatment effects reported in Table 4. These results are available from the authors upon request.

analysis sample further to include only children who resided in the same rural district over all three rounds to be able to disentangle migration effects from the program effects, which are correlated both spatially and temporally. We lose only about 16 observations by imposing this restriction. Our final analysis sample includes 703 children residing in rural Andhra Pradesh who are followed through all three rounds of the Young Lives Panel Study.

The identification strategy to be outlined in section 4 depends on the assumption that there is no selective attrition between NREGS (early phase-in) and late-NREGS (late phase-in) districts. To test this assumption, we estimate a linear probability model of attrition of the following kind:

$$Attrit_i = \beta_0 + \beta_1 \, NREGS_i + \beta_2 \, Y_{io} + \beta_3 \, NREGS_i \text{ X } Y_{io} + \beta_4 X_{io} + \varepsilon_i \qquad (1)$$

Where Attrit (=1 if dropped out of the sample, 0 otherwise) is regressed on baseline outcome variables (Y_{io}), socioeconomic characteristics from baseline (X_{io}), NREGS (=1 if assigned to the NREGS between rounds 1 and 2, 0 otherwise), and interaction between the NREGS and the pre-intervention outcome variables ($NREGS_i$ X Y_{io}). We also examine the determinants of attrition controlling for baseline numeracy test scores, which are likely to be correlated with post-intervention initiation math test scores that are unavailable at baseline. The results are presented below in columns 1-5, Table 2.

There are three important findings that emerge from Table 2. First, assignment to the NREGS is not related to attrition, the coefficient estimate on the NREGS is not statistically significant at even the 10% significance level, ruling out selective attrition between NREGS (early phase-in) and late-NREGS (late phase-in) districts. Second, pre-intervention enrollment, grade progression, Raven's test scores and writing test scores are all unrelated to sample attrition; the reading comprehension test scores are negatively related to attrition but only marginally significant. Baseline numeracy test scores are positively related to sample attrition suggesting negative selection into the sample as more-able children are likely to drop out of the sample over time. Third, positive/negative selection on baseline test scores would bias treatment effects only if this selection is correlated with the treatment indicator. The interaction terms between NREGS and pre-intervention outcome variables are all statistically insignificant, that is, the null that there is no selective attrition based on differences in pre-intervention outcomes between NREGS (treatment/early phase-in) and late-NREGS (control/late phase-in) districts cannot be rejected at even the 10% significance level, thus ruling out attrition associated selection related to the critical baseline observables for this study.

Table 2: Linear Probability Model of Attrition

	(1) Attrit	(2) Attrit	(3) Attrit	(4) Attrit	(5) Attrit
NREGS	-0.084	0.039	0.008	0.042	0.03
	(0.15)	(0.04)	(0.04)	(0.03)	(0.03)
Enrollment	-0.113				
	(0.14)				
NREGS*enrollment	0.134				
	(0.15)				
Grade progression		-0.010			
		(0.02)			
NREGS*grade progression		0.008			
		(0.04)			
Reading comprehension			-0.022*		
test scores			(0.013)		
NREGS*reading			0.017		
comprehension test scores			(0.02)		
Writing test scores				-0.005	
				(0.013)	
NREGS*writing test scores				0.004	
				(0.022)	
Numeracy test scores					0.04**
					(0.02)
NREGS*numeracy test					0.02
scores					(0.034)
Household size	0.013**	0.013**	0.013**	0.013**	0.014**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Number of school age	-0.032***	-0.032***	-0.032***	-0.032***	-0.032***
children	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Raven's test scores	0.001	0.001	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Wealth index	-0.018	-0.023	-0.012	-0.022	-0.034
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Male dummy	0.44	0.44	0.45	0.44	0.42
	(0.43)	(0.44)	(0.44)	(0.44)	(0.44)
Age in months	0.003	0.003	0.004	0.003	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Male dummy*age in	-0.005	-0.005	-0.005	-0.004	-0.005
months	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)

SC/ST dummy	-0.085*	-0.086*	-0.088*	-0.086*	-0.085*
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
OBC dummy	-0.097**	-0.098**	-0.10**	-0.098**	-0.095**
	(0.047)	(0.047)	(0.048)	(0.048)	(0.047)
Religion dummy	0.018	0.019	0.022	0.020	0.015
	(0.031)	(0.031)	(0.031)	(0.031)	(0.032)
Mother's schooling	-0.002	-0.002	-0.002	-0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Father's schooling	-0.002	-0.002	-0.002	-0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Constant	-0.13	-0.23	-0.23	-0.24	-0.26
	(0.39)	(0.35)	(0.35)	(0.34)	(0.33)
Sample size	757	757	757	757	757
R-squared	0.041	0.039	0.041	0.039	0.044

Notes: *** p<0.01, ** p<0.05, * p<0.10. Robust standard errors (in parentheses) clustered at the community level. The sample covers rural areas only.

4. Conceptual framework

The NREGS can impact children's schooling outcomes through multiple channels, as noted above. An important effect of the scheme on children as noted earlier may be through improvements in parents' labor force participation. There are several other channels as well, for instance, the public works projects implemented in the village can improve agricultural output for the household even without increasing labor force participation rates among parents. In addition, not all participants from the household registered under the NREGS are parents of the children in the sample. For instance, in 2009-10 the Young Lives Panel Study obtains detailed information on the relationship of the NREGS worker to the young lives children. More than 25% of the NREGS workers were not biological parents of the sample child. Therefore, identifying the impact of the NREGS only through improvements in parents' labor force participation rates would bias the estimated effect of the program. The direction of the bias is downward if important channels through which the program influences schooling have positive effects (and vice versa). Our aim here is to therefore estimate the intent-to-treat, that is, the total effect of the program on schooling outcomes and intellectual human capital.

Our first two outcome variables of interest are enrollment and grade progression for which consistent data are available from all three rounds of the Young Lives Panel Study, which allows us to estimate the short-run (between rounds 1 and 2), medium-run (between rounds 2 and 3) and long-run (between rounds 1 and 3) effects of the early phase-in/exposure to the program between rounds 1 and 2. We combine spatial and temporal variation in access to the program to compute the intent-to-treat effects of the program as specified by equation (2).

$$Y_{it} = \beta_0 + \beta_1 \text{ NREGS}_i + \beta_2 \text{ Time1}_t + \beta_3 \text{ NREGS x Time1}_{it} + \beta_4 \text{ Time2}_t + \beta_5 \text{ NREGS x Time2}_{it} + \varepsilon_i + \varepsilon_c + \varepsilon_h + \varepsilon_{it} \quad (2)$$

NREGS is the usual treatment indicator, a dummy variable that takes a value 1 if the child lives in a district that received early exposure (between rounds 1 and 2) to the employment guarantee scheme, 0 otherwise. The coefficient estimate on NREGS, β_1 captures all pre-existing differences between early exposure districts (treatment) and late exposure districts (control). Time1 is a dummy variable that takes a value 1 if the year is 2007 (post-intervention), 0 otherwise. The coefficient estimate on Time1, β_2 captures common time-effects for both treatment and control districts. The coefficient estimate on the interaction term (NREGS x Time1), β_3 captures the short-run intent-to-treat effect of the NREGS, the relative increase in enrollment and grade progression between the pre- and post-intervention periods for children who lived in areas that were exposed to the NREGS between 2002 and 2007 (rounds 1 and rounds 2 of the Young Lives Panel Study) compared with those who were exposed to the program later between 2007 and 2009-10 (rounds 2 and 3 of the Young Lives Panel Study). Time2 is a dummy variable that takes a value 1 if the year is 2009-10, 0 otherwise. The coefficient estimate on Time2, β_4 once again captures common time-effects. The coefficient estimate on the interaction term (NREGS x Time2), β_5 captures long-run effect of the program between rounds 1 and 3 of having early exposure to the program relative to late exposure. The difference between β_5 and β_3 captures the additional/medium-run effect gained between rounds 2 and 3. If the medium-run effect is negative then we know that the control group tends to catch-up at least somewhat (entirely if the absolute magnitude of the medium-run effect is the same as the short-run effect) to the treatment group (early exposure districts) and if the additional effect remains positive then the effect of receiving early exposure to the program will have long-run persistent effects on schooling enrollment and grade progression that are greater than the short-run effects.

The disturbance term in equation (2) includes four components. The first three (ε_i, ε_c, ε_h) are time invariant unobserved characteristics of the individual child, community and household, respectively. If these are not controlled in the estimation and if they are correlated with early phase-in of NRGES as seems likely given the effort to first target poorer areas, their presence will then bias the estimated NRGES effects downwards. The fourth component (ε_{it}) is a random shock for the i^{th} child in the t^{th} period.

The other outcome variables of interest in this paper are reading comprehension test scores, writing test scores, math test scores and PPVT scores. All these measures are only available from two waves of the Young Lives Panel Study making it possible to only estimate either the short- or the medium-run effects of the program. The reading comprehension test scores and writing test scores are available from the 2002 and 2007 waves of the Young Lives Panel Study making it possible to only estimate the short-run intent-to-treat effects of the program using a simple difference-in-difference specification (3).

$$Y_{it} = \beta_0 + \beta_1 \text{ NREGS}_i + \beta_2 \text{ Time1}_t + \beta_3 \text{ NREGS x Time1}_{it} + \varepsilon_i + \varepsilon_c + \varepsilon_h + \varepsilon_{it} \qquad (3)$$

As defined earlier, NREGS takes a value 1 if assigned to an early phase-in district (treatment) and 0 if assigned to a late phase-in district (control). The coefficient estimate on the NREGS dummy, β_1 here again captures all pre-existing differences between children residing in the treatment and control districts. Time1 is a dummy variable that again takes a value 1 if year is 2007 (post-intervention period), 0 otherwise. The coefficient estimate on the interaction term (NREGS x Time1), β_3 again captures the short-run intent-to-treat effect of the NREGS. It captures the relative increase in reading comprehension test scores and writing test scores between the pre- and post-intervention periods for children who live in treatment districts compared to children residing in the control districts.

We make use of the two rounds of post-intervention data from the 2007 and 2009-10 waves available on PPVT and math test scores to compute the medium-run/additional effect of early exposure to the NREGS. The NREGS dummy in equation (4) still takes a value 1 if assigned to an early phase-in district (treatment) and 0 if assigned to a late phase-in district (control). Time is a dummy variable that takes a value 1 if year is 2009-10, 0 otherwise.

$$Y_{it} = \beta_0 + \beta_1 \text{ NREGS}_i + \beta_2 \text{ Time}_t + \beta_3 \text{ NREGS x Time}_{it} + \varepsilon_i + \varepsilon_c + \varepsilon_h + \varepsilon_{it} \qquad (4)$$

The coefficient estimate on the interaction term (NREGS x Time) β_3 captures the additional/medium-run effect of the program beyond the short-run effect. A positive coefficient on the interaction term suggests that changes in test scores between rounds 2 and 3 for the treatment group will be greater than observed changes in test scores between rounds 2 and 3 for the control districts that received the program later. This suggests that the effect of earlier exposure to the program is augmented in the long-run even after the control group starts receiving access to the program. On the contrary a negative coefficient on the interaction term suggests that the control group tends to catch-up to the treatment group.

We know that the assignment to the NREGS program was not random; instead the program was purposely placed first in areas that have high concentrations of poor families (also see section 5.1). However, if we assume that program placement is correlated with individual (ε_i), household (ε_h), and or community (ε_c) specific time-invariant unobservables that are additive in nature, then first-differencing the specification as done in the results section will sweep out all program placement effects allowing us to estimate the casual effect of the program on schooling outcomes and measures of intellectual human capital conditional on this specification. To obtain unbiased and consistent treatment effects, we still need to assume that– (a) there is no differential attrition between the treatment (early phase-in) and control (late phase-in) districts, we have shown this in section 3.3, and (b) treatment and control districts would have had parallel time trends in the absence of NREGS (see below). It is always a challenge to test the latter assumption since it requires several years of pre-intervention data on the outcome variables.

To test the assumption of parallel time trends in the absence of the NREGS, we use data from the 2005 Indian Human Development Survey (Desai et. al 2005). The Indian Human Development Survey (IHDS) was administered between November 2004 and October 2005 and the first phase of the NREGS was rolled out only starting in September 2005. The timing of the IHDS makes it suitable for examination of pre-intervention time trends. The IHDS has collected data on 5 (except Srikakulam) of the 6 Young Lives districts in rural Andhra Pradesh. In Appendix Figure A8 we plot years of schooling for children aged 8, 9, 10, 11 and 12 years for the treatment (early phase-in districts) group and the control (late phase-in districts) group. Notice that the trends in years of schooling at different ages during the pre-intervention period are linear and parallel between the treatment and control districts. We also examine in Appendix Figure A9 years of schooling for different age groups (15-30 years, 30-40 years, 40-50 years, 50-75) years) in the treatment and control districts. Once again, we find parallel trends in accumulation of schooling in the treatment and control districts. Appendix Figures A8 and A9 both suggest that it is reasonable in our context at least to assume that the pre-intervention outcomes follow parallel trends between the treatment and control districts.[14]

[14] We also examine pre-intervention reading comprehension test score and writing test score from the Indian Human Development Survey available only for a small sub-sample, 8-11 year old children. We find no difference in pre-intervention reading comprehension test scores [p-value = 80] and the writing test scores [p-value = 0.17] between the treatment districts and control districts. The sample is too small to present disaggregated averages using plots.

5. Results

5.1 Endogenous Program Placement

In the absence of random assignment of districts to the NREGS, OLS estimation of equations (2)-(4) will not result in unbiased and consistent program effects since unobserved time-invariant individual (ε_i), household (ε_h) and community (ε_c) specific unobservables, as noted above, are likely to be correlated with assignment of districts to the NREGS between rounds 1 and 2. Rosenzweig and Wolpin (1986, 1988), Frankenberg and Thomas (2001), and Duflo (2001), among others, in different contexts have shown that true program effects are likely to be substantially biased in the presence of time-invariant unobservables. To further understand the observed sources of placement we use data from round 1 (2002) to estimate the following placement equation:

$$\text{NREGS}_c = \beta_0 + \beta_1 \, Y_{c0} + \sum_{j=2}^{R} \beta_j \, X_{jc} + \varepsilon_c \qquad (5)$$

Since there are only six districts in our sample we cannot estimate the placement equation at the district level; instead we estimate equation (5) using data from 82 communities spread across the six Young Lives districts in Andhra Pradesh. The dependent variable in equation (5) takes a value 1 if assigned to receive the NREGS between rounds 1 and 2, 0 otherwise. This is regressed upon a set of pre-intervention community level resources (Xs) and pre-intervention outcome variables (Y_{c0}) averaged at the community-level. There is no pre-intervention data available on math and PPVT scores and therefore we include data from a pre-intervention numeracy test and Raven's test administered in round 1 that are possibly closely related to the post-intervention initiation math and PPVTs administered in rounds 2 and 3. The regression results are reported in columns 1-6, Table 3. Program placement here is negatively related to availability of electricity in the community, percentage of the population with secondary schooling or more and availability of community hospital and health center indicating that placement is fairly pro-poor and related to community level resources at baseline. Program placement is also positively related to community population. Pre-intervention outcome variables are rarely related to program placement except for grade progression, which is negatively related to early phase-in of the NREGS. Treatment assignment here is significantly related to observed variables that generally indicate pro-poor placement. This is further suggestive that unobserved variables are also likely to be associated with pro-poor placement. If so, then failure to control for these unobserved variables would probably result in downward biases in estimated program effects.[15]

Table 3: Determinants of Program Placement

	(1) NREGS	(2) NREGS	(3) NREGS	(4) NREGS	(5) NREGS	(6) NREGS
Enrollment	0.40 (0.34)					
Grade progression		-0.51** (0.22)				
Writing test scores				-0.08 (0.13)		
Numeracy test scores					-0.08 (0.32)	
Raven's test scores						0.02 (0.017)
Electricity (=1 if electricity available, 0 otherwise)	-0.11 (0.14)	-0.28* (0.14)	-0.07 (0.15)	-0.07 (0.15)	-0.15 (0.13)	-0.12 (0.12)
Secondary Education (% of the population with secondary education and or more)	-0.32 (0.45)	-0.32 (0.45)	-0.32 (0.45)	-0.32 (0.45)	-0.32 (0.45)	-0.40 (0.47)
Population (in 000s)	0.09** (0.04)	0.085** (0.04)	0.87** (0.04)	0.09** (0.04)	0.09** (0.04)	0.088** (0.04)
Water (=1 if drinking water is available, 0 otherwise)	0.12 (0.10)	0.10 (0.09)	0.11 (0.09)	0.10 (0.10)	0.11 (0.09)	0.09 (0.10)
Hospital (=1 if public or private hospital is available, 0 otherwise)	-0.29** (0.13)	-0.26** (0.13)	-0.26** (0.13)	-0.27** (0.13)	-0.27** (0.13)	-0.22 (0.14)
Health center (=1 if health center or health post	-0.31** (0.13)	-0.27** (0.14)	-0.30** (0.13)	-0.31*** (0.13)	-0.30** (0.13)	-0.31** (0.13)

[15] A similar picture emerges from examining the 2005 Indian Human Development Survey. We find that on average households residing in the treatment districts (early phase-in districts) are more likely to be below the poverty line in comparison to the control districts (late phase-in districts).

available, 0 otherwise)						
Constant	0.50	1.42***	1.04***	0.98***	0.99***	0.43
	(0.41)	(0.26)	(0.20)	(0.22)	(0.37)	(0.41)
Sample size	82	82	82	82	82	82
R-squared	0.34	0.38	0.35	0.34	0.34	0.35

Notes: *** p<0.01, ** p<0.05, * p<0.10. The sample covers rural areas only.

5.2 Intent-to-treat effects

To address selection on time-invariant unobservables, including those related to program placement as well as at the household and individual level, we estimate equations (2)-(4) using the first-difference OLS estimation strategy. The intent-to-treat effects of the NREGS on enrollment, grade progression, reading comprehension test scores, writing test scores, math test scores in percentile ranks and PPVT scores in percentile ranks are reported in Table 4. In Table 4, Panel A the impact estimates are reported without any pre-intervention controls. To improve the precision of our impact estimates in Table 4, Panel B we control for a set of pre-intervention socioeconomic and demographic characteristics. The set of pre-interventions controls included in the regressions are summarized in Table 1, Panel B.

Our preferred estimates for the short-run (2002-2007), medium-run/additional (2007-2009/10) and long-run (2002-2009/10) intent-to-treat effects of the NREGS on enrollment and grade progression are reported in Table 4, Panel B. We find that there is no impact of the NREGS on enrollment in both the short- and medium-run. Enrollment captures short run investments in schooling and in the absence of continuous waves of measurement does not fully reflect cumulative investments in schooling that may have occurred differentially in the NREGS districts during the post-intervention period. Grade progression is a better measure of cumulative investments in schooling. We find that the NREGS has a positive and statistically significant effect on grade progression in the short run, medium run and long run. We find that children residing in districts that receive the NREGS between rounds 1 and 2 on average get 8% closer to their potential grades compared to children residing in late phase-in districts during this period. We find that the effects in the short run continue to persist even after the program is phased-in to the late phase-in NREGS districts between rounds 2 and 3. We find that in the long run, children residing in districts that receive the NREGS between rounds 1 and 2 are on average 11% closer to their potential grades by round 3 compared to children who receive the NREGS only between rounds 2 and 3. We find the additional/medium-run ITT effects observed between rounds 2 and 3 reported in Table 4, Panel B are positive and statistically significant, augmenting the short-run impact estimates, suggesting that there is no decay of the short-run treatment effects. However, note that the gains from receiving the program early on remain significant pointing to the value of receiving interventions during primary school.

Improvements in grade progression reflect both improvements in enrollment and lower rates of grade repetition. Since we find no effects on enrollment, the improvements in grade progression are likely to accrue from improvements in performance that are manifested in part by reduced grade repetition. The persistence of these effects is not surprising because children who received access to the program between rounds 1 and 2 were between ages 11 and 12 years while children who received the program by round 3 were almost 13-14 years when a greater proportion of these children are not even likely to be in school.

Table 4: ITT Effects of the NREGS

	Enrollment (1)	**Grade progression (2)**	**Reading comprehension test scores (3)**	**Writing test scores (4)**	**Math test scores (in percentile ranks) (5)**	**PPVT scores (in percentile ranks) (6)**
Panel A: ITT effects without baseline controls						
NREGS x Time1 (2002-2007) *Short-run effect*	-0.022 (0.03)	0.063* (0.03)	0.22* (0.12)	0.07 (0.09)		
NREGS x Time2 (2002-2009/10) *long-run effect*	-0.07 (0.05)	0.08** (0.04)				
NREGS x Time (2007-2009/10) *Medium-run effect*	-0.05 (0.03)	0.02 (0.01)			5.65 (3.98)	11.65*** (3.89)
Panel B: ITT effects with baseline controls						
NREGS x Time1 (2002-2007) *Short-run effect*	-0.02 (0.03)	0.08** (0.03)	0.31*** (0.11)	0.10 (0.08)		
NREGS x Time2	-0.06 (0.05)	0.11*** (0.037)				

(2002-2009/10) *long-run effect*						
NREGS x Time	-0.04	0.032**			5.78*	11.88***
(2007-2009/10) *Medium-run effect*	(0.03)	(0.012)			(3.45)	(3.53)
Mean of late phase-in districts (control)	-0.09	-0.012	0.50	0.34	-3.71	-7.65
	(0.32)	(0.20)	(1.13)	(0.92)	(33.03)	(30.03)
Sample size	1406	1406	703	703	703	703

Notes: Robust standard errors (in parentheses) clustered at the community level. The pre-intervention controls included in Panel B are male dummy, age in months, male dummy interacted with age in months, wealth index, household size, number of school age children, SC/ST dummy, OBC dummy, religion dummy, Raven's test scores, mother's schooling and father's schooling. *** p<0.01, ** p<0.05, * p<0.10. The sample covers rural areas only.

We use pre (2002) and post (2007) intervention data on reading comprehension test scores and writing test scores to compute the short-run intent-to-treat effects of the NREGS. We find that the program in a short time improved average reading comprehension test scores by 0.31 and average writing test scores by 0.10 as reported in columns 3 and 4 Table 4, Panel B though the effects on writing scores are not statistically significant at even the 10% level. The improvement in reading comprehension is augmented in the medium run (additional effects between rounds 2 and 3 of exposure to the program between rounds 1 and 2) as depicted by the improvement in PPVT scores of receptive vocabulary reported in column 6 Table 4, Panel B.

We use two rounds of post-intervention initiation data from the 2007 and 2009/10 to compute the additional/medium-run effect of early exposure to the NREGS between rounds 1 and 2 on improvements in PPVT and math test scores between rounds 2 and 3. The additional effect of the NREGS on math test scores is reported in column 5 Table 4, Panel B and indicates that children residing in the early phase-in districts scored 6 percentage points higher on math tests compared to children residing in the late-phase-in districts. The additional effect of NREGS on PPVT scores reported in column 6 Table 4, Panel B indicates that children residing in early phase-in districts are likely to score almost 12 percentage points higher on the PPVT compared to children residing in the late-phase-in districts.[16]

[16] We impute PPVT and math test scores for 4% and 7% of the panel sample respectively. We re-estimate our preferred specification for PPVT and math test scores in percentile ranks for the smaller panel sample of 672 and 652 observations respectively, for whom there are no missing test scores. We find that the medium-run intent-to-treat effects of the NREGS on math and PPVT scores in percentile ranks are 4.18 (s.e=3.40) and 12.83 (s.e=3.48) respectively. These effects are not significantly different from the estimates reported in Table 4, Panel B at even the 10% significance level.

5.3 Index measures

The probability of a false positive, that is, Type I error, increases in the number of outcomes tested. Since we examine the impact of the NREGS for six outcome variables, we would like to lessen the possibility of a false positive. To do so we use the method outlined in Kling, Liebman and Katz (2007). We construct index1 by combining enrollment, grade progression, reading comprehension test scores and writing test scores to measure the short-run intent-to-treat effect of the NREGS using pre (2002) and post-intervention data from 2007 rounds of the Young Lives Panel Study. Similarly, we construct index2 by combining enrollment, grade progression, math test scores in percentile ranks and PPVT scores in percentile ranks using two rounds of post-intervention data from 2007 and 2009-10 to measure the additional/medium-run effect of early exposure to NREGS compared to late phase-in of the program. This index method requires us to first convert the outcome variables into standardized outcomes, where the standardized outcomes are constructed using the mean and the standard deviation of the control group (late phase-in districts) as the reference category. Note that higher values in the outcome variable must consistently indicate better performance. We take an equally weighted average of all the standardized outcomes within a domain to construct these indices.

We estimate equations (3) and (4) for index1 and index2 respectively. Once again, equations (3) and (4) are estimated in first-differences and these specifications control for a full set of pre-intervention characteristics on the right side. The associated impact estimates are reported in Table 5. The short- and medium-run intent-to-treat effects of the NREGS are statistically significant. In the short run, assignment to the NREGS districts increases schooling outcomes by 0.10 standard deviation and in the medium run, early exposure to the NREGS increases intellectual human capital by an additional 0.16 standard deviations compared to children residing in the control/late phase-in districts. The null that NREGS has no effect on intellectual human capital can be rejected at the 10% and 5% significance levels in the short run and the medium run, alleviating concerns relating to incorrect inference that comes with the use of multiple outcome variables.

Table 5: ITT Effects and Average Effect Size of the NREGS

	Index1 **(1)**	**Index2** **(2)**
Panel A: ITT effects with baseline controls		
NREGS x Time1	0.10*	
(2002-2007)	(0.06)	

short-run effect		
NREGS x Time		0.16**
(2007-2009/10)		(0.06)
medium-run effect		
Panel B: Average effect size with baseline controls		
	(1)	**(2)**
NREGS x Time1	0.08*	
(2002-2007)	(0.05)	
short-run effect		
NREGS x Time		0.15**
(2007-2009/10)		(0.06)
medium-run effect		
Sample size	703	703

Notes: Robust standard errors (in parentheses) clustered at the community level. The pre-intervention controls included in panels A and B are male dummy, age in months, male dummy interacted with age in months, wealth index, household size, number of school age children, SC/ST dummy, OBC dummy, religion dummy, Raven's test scores, mother's schooling and father's schooling. *** p<0.01, ** p<0.05, * p<0.10. The sample covers rural areas only.

As a further check of our ITT effects we also compute average effect size using the method outlined in Kling, Liebman and Katz (2007) and Clingingsmith et. al (2009). The average effect size is constructed by taking a weighted average of the individual treatment effects within a domain of related outcomes computed using a system of Seemingly Unrelated Regression (SUR) equations to allow for correlation between the error terms across equations. This method improves the precision of the treatment effects thereby reducing type II error, that is, the risk of attaining low statistical power.[17] The average effect size for the short-run treatment effects are reported in column 1 Table 5, panel B takes an average over standardized (z-score) measures of – enrollment, grade progression, reading comprehension test scores and writing test scores. The average effect size can only be interpreted with ease if the units of measurement for the different outcome variables are same. In this case they are all measured in terms of the standard deviation units of the control group. Similarly, we compute the average effect size for the medium-run treatment effects reported in column 2 Table 5, panel B taking an average over standardized (z-score) measures of – enrollment, grade progression, math test scores in percentile ranks and PPVT scores in percentile ranks. We find that assignment to

[17] The ITT effects obtained from the SUR regressions are reported in Appendix Table A1. Notice the treatment effects reported here have lower standard errors compared to the estimates reported in Table 4 owing to higher precision.

NREGS districts between rounds 1 and 2 increases the short-run average effect size by 0.08 standard deviation units and the medium-run average effect size by 0.15 standard deviation units. Notice the additional/medium-run effects are positive and significantly greater than the short-run effects. This gain is primarily due to the large gains observed in the math and PPVT scores in percentile ranks observed between rounds 2 and 3.

To our knowledge, we are the first to evaluate the impact of public works programs on intellectual human capital using performance on reading, writing, math and vocabulary (PPVT). Hence, we cannot compare the magnitude of our effects to other public works programs. However, we can compare the impact estimates with the effects obtained from conditional cash transfer programs aimed at poverty alleviation. Fiszbein and Schady (2009) in their review of CCT programs show that pre-school interventions are more effective in improving test scores as found in Nicaragua and Ecuador [Paxson and Schady (2010), Macours et. al (2012)] in comparison to CCT interventions targeted among school-age children as found in Mexico and Cambodia [Behrman et. al (2005), Filmer and Schady (2011)]. The magnitude of the average effect size for Nicaragua and Ecuador (poorest 10%) varies between 0.13 and 0.18 standard deviation improvements in the distribution of test scores. In our sample a 0.31 improvement in the reading comprehension test scores translates into a 0.25 standard deviation improvement in reading comprehension/vocabulary, a 6 percentage point increase in math test scores in percentile rank translates into a 0.20 standard deviation improvement in math, and a 12 percentage point increase in PPVT scores in percentile rank translates into an approximately 0.40 standard deviation improvement in receptive vocabulary. The average effect size computed here in the short run results in a 0.08 standard deviation improvement in cognitive outcomes and in the medium run the effects are sustained and results in a 0.15 standard deviation improvement in test scores. The impact estimates reported here are close to the impact estimates reported for two CCT programs in Nicaragua (Macours et. al 2012, Barham et. al 2013) and also for a pre-school nutrition program implemented in Guatemala (Maluccio et. al 2009).

5.4 Gender Differential Treatment Effects

The NREGS specifically aims to improve women's labor force participation by assigning at least one-third of all beneficiaries of the scheme to be women. Azam (2011) show that the scheme was successful in achieving this target as the labor force participation rate among women was substantially higher in the post-intervention period compared to male labor force participation rates. These differences in gender-specific labor force participation rates could result in differential impact estimates for male and female children if fathers and mothers differentially direct resources to their sons versus their daughters. Thomas (1994) and Duflo (2000) show that changes in income and resources among fathers and grandfathers only benefit their sons and grandsons. Similarly changes in income and resources for mothers and grandmothers only trickle down to their daughters and granddaughters. Our aim here is to examine if the NREGS results in differential effects for male and female children. The preferred intent-to-treat effects reported in Table 4 are now separately estimated for males and females in Table 6.

The intent-to-treat effect of the NREGS for male children is reported in Table 6, Panel A. The impact estimates show that the scheme is positively related to schooling enrollment for sons though the effect is not significant. We find that the scheme is positively and significantly related to grade progression. Male children residing in the early phase-in program districts in the short run are 9% and in the long run 12% closer to their potential grades compared to male children residing in the late phase-in districts. We find that improvements in enrollment and grade progression do not translate to improvements in measures of intellectual human capital for sons as captured by the ITT effects on reading comprehension test scores, writing test scores, math test scores in percentile ranks and PPVT scores in percentile ranks.

The intent-to-treat effects of the NREGS for females is reported in Table 6, Panel B. Female enrollment drops by 12 percentage points in the long-run though this decline does not affect grade progression. We find that female children residing in early phase-in districts are 5% and 9% closer to their potential grades in the short- and medium-runs respectively. We find that in the short run reading comprehension test scores increase by almost 40 percentage points. We observe similar positive effects of early phase-in of the program in the medium run with a 15 percentage point increase in math test scores in percentile ranks and a 17 percentage point increase in PPVT scores in percentile ranks among females. It appears that male children benefit from both staying longer in school and reductions in grade repetition whereas, female children appear to benefit primarily from reductions in grade repetition and positive-selection into enrollment.

Table 6: ITT Effects of the NREGS by Gender

	Enrollment (1)	**Grade progression (2)**	**Reading comprehension test scores (3)**	**Writing test scores (4)**	**Math test scores (in percentile ranks) (5)**	**PPVT scores (in percentile ranks) (6)**
Panel A: Male						
NREGS x	0.025	0.09**	0.20	0.06		

Time1 (2002-2007) *short-run effect*	(0.04)	(0.04)	(0.17)	(0.12)		
NREGS x Time2 (2002-2009/10) *long-run effect*	-0.004	0.12***				
	(0.05)	(0.04)				
NREGS x Time (2007-2009/10) *medium-run effect*	-0.03	0.03			-3.59	6.66*
	(0.04)	(0.01)			(3.82)	(3.84)
Mean of late phase-in districts (control)	-0.09	-0.02	0.50	0.39	6.42	-0.36
	(0.32)	(0.22)	(1.12)	(0.92)	(30.73)	(26.60)
Sample size	684	684	342	342	342	342
Panel B: Female						
NREGS x Time1 (2002-2007) *short-run effect*	-0.06	0.05	0.38***	0.12		
	(0.04)	(0.04)	(0.11)	(0.11)		
NREGS x Time2 (2002-2009/10) *long-run effect*	-0.12*	0.09**				
	(0.06)	(0.04)				
NREGS x Time (2007-2009/10) *medium-run effect*	-0.06	0.03**			15.7***	16.82***
	(0.05)	(0.02)			(4.61)	(4.34)
Mean of late phase-in districts (control)	-0.08	-0.007	0.50	0.29	-13.60	-14.77
	(0.33)	(0.18)	(1.05)	(0.91)	(32.32)	(31.55)

Sample size	722	722	361	361	361	361

Notes: Robust standard errors (in parentheses) clustered at the community level. The pre-intervention controls included in panels A and B are male dummy, age in months, male dummy interacted with age in months, wealth index, household size, number of school age children, SC/ST dummy, OBC dummy, religion dummy, Raven's test scores, mother's schooling and father's schooling. *** p<0.01, ** p<0.05, * p<0.10. The sample covers rural areas only.

Following the methods outlined in section 5.3, we also compute short- and medium-run intent-to-treat effects using the index measures and the average effect size over multiple domains of related outcomes for the male and female sub-samples in Table 7. The short- and medium-run ITT effects on the index measures are reported in columns 1 and 2 Table 7, Panel A. We find that the program has no impact on male schooling and cognitive outcomes in either the short run or the medium run. However, the program has benefitted females a little in the short run but substantially more in the medium-run. These are consistent with the positive findings on grade progression but no effects on test scores for males and the overall positive findings on grade progression and test scores for females.

Table 7: ITT Effects and Average Effect Size of the NREGS

	Index1	**Index2**	**Index1**	**Index2**
	(1)	**(2)**	**(3)**	**(4)**
	Male	**Male**	**Female**	**Female**
Panel A: ITT effects with baseline controls				
NREGS x Time1	0.14		0.05	
(2002-2007)	(0.09)		(0.08)	
short-run effect				
NREGS x Time		0.04		0.29***
(2007-2009/10)		(0.07)		(0.08)
medium-run effect				
Panel B: Average effect size with baseline controls				
	(1)	**(2)**	**(3)**	**(4)**
	Male	**Male**	**Female**	**Female**
NREGS x Time1	0.11*		0.04	
(2002-2007)	(0.07)		(0.08)	
short-run effect				
NREGS x Time		0.05		0.26***
(2007-2009/10)		(0.07)		(0.08)
medium-run effect				
Sample size	342	342	361	361

Notes: Robust standard errors (in parentheses) clustered at the community level. The pre-intervention controls included in panels A and B are male dummy, age in months, male dummy interacted with age in months, wealth index, household size, number of school age children, SC/ST dummy, OBC dummy, religion dummy, Raven's test scores, mother's schooling and father's schooling. *** p<0.01, ** p<0.05, * p<0.10. The sample covers rural areas only.

The index measures reported in Table 7, Panel A do not account for correlation in the errors across equations. However, the average effect size reported in Table 7, Panel B accounts for this correlation improving the precision of the estimates (reducing standard errors). Male children residing in districts that receive the program between rounds 1 and 2 experience approximately a 0.11 standard deviation units improvement in schooling and cognitive outcomes in the short run. Whereas in the medium run the effects of receiving the program early on (between rounds 1 and 2) relative to receiving the program later on (between rounds 2 and 3) is only marginally greater but insignificant for males but notably larger and significant for females. The magnitude of the medium run effects are almost five times higher for females compared to males suggesting that timing of interventions matter more for females than males.

5.5 Spillover Effects

The NREGS is also found to improve welfare outcomes in urban areas. Ravi, Kapoor and Ahluwalia (2013) show that access to the NREGS in rural areas has reduced rural-urban migration rates and reduced urban unemployment rates by 7 percentage points. This spillover effect on improved employment and earnings is also likely to result in improvements in human capital outcomes in urban areas. To examine the extent spillover effects generated through access to the NREGS, we re-estimate our preferred specifications now restricting the analysis sample to urban areas. The spillover effect of the NREGS is provided in Table 8 and is replicated for our preferred specification reported in Table 4, panel B. We find that in general the program is positively associated with improvements in schooling outcomes with most significant effects on grade progression.

Table 8: ITT Effects of the NREGS for Urban Sample

	Enrollment (1)	Grade progression (2)	Reading comprehension test scores (3)	Writing test scores (4)	Math test scores (in percentile ranks) (5)	PPVT scores (in percentile ranks) (6)
Panel A: ITT effects with baseline controls						

NREGS x Time1 (2002-2007) *short-run effect*	0.04 (0.05)	0.12*** (0.04)	0.21 (0.13)	-0.09 (0.15)		
NREGS x Time2 (2002-2009/10) *long-run effect*	0.0006 (0.05)	0.12** (0.05)				
NREGS x Time (2007-2009/10) *medium-run effect*	-0.04 (0.04)	-0.0003 (0.02)			-0.54 (3.41)	11.16 (8.80)
Mean of late phase-in districts (control)	-0.07 (0.27)	0.007 (0.27)	0.29 (0.89)	0.30 (0.82)	-0.13 (27.15)	-5.87 (31.17)
Sample size	448	448	224	224	224	224

Notes: Robust standard errors (in parentheses) clustered at the community level. The pre-intervention controls included in panel A are male dummy, age in months, male dummy interacted with age in months, wealth index, household size, number of school age children, SC/ST dummy, OBC dummy, religion dummy, Raven's test scores, mother's schooling and father's schooling. *** p<0.01, ** p<0.05, * p<0.10. The sample covers urban areas only.

5.6 Other programs?

Two other popular government programs in India – the mid day meal scheme and the integrated child development services program (ICDS) are often associated with improvements in children's health and schooling outcomes [Dercon et. al 2013; Afridi 2010, 2011; Khera, 2006]. The ICDS program is only targeted to children between 0-6 years and therefore none of the school age children in our sample are eligible for the ICDS scheme. Consequently, the NREGS effects are not likely to be confounded by any pre-school participation in the ICDS scheme. The mid day meal scheme is the governments flagship program aimed at keeping children enrolled in school. In October 2007 the government of India expanded access of the mid day meal scheme to target not just primary school age children enrolled in classes 1-5 but expanded the beneficiaries of the mid day meal scheme to include all children enrolled in classes 1-8. This expansion took effect in all government schools in India and would be captured by the time dummies specified in our preferred equations. Finally, we find that more than 90% of children in our sample are enrolled in government schools in both treatment and control districts and experience similar levels of exposure to the mid day meal scheme, and therefore the treatment effects can be accrued to only the NREGS effects.

5.7 Robustness

Since enrollment is defined as a dichotomous variable, OLS estimates reported in Table 4 are not likely to provide us with consistent and unbiased treatment effects. To address this concern, we estimate the treatment effects using conditional logit fixed-effects estimator. The conditional logit fixed-effects estimates for enrollment are reported in column 1, Table 9. The short run and medium run impact estimates suggest no effect of the NREGS on enrollment. These results are consistent with the preferred first-difference OLS estimates reported in column 1, Panel B, Table 4.

As described in the data section, our reading comprehension test scores and writing test scores capture ordered choices and once again OLS estimation would not yield consistent and unbiased estimates. To obtain consistent estimates, we modify the ordered choices into a binary choice model where the modified reading comprehension test score takes a value 1 if the child can read words and or read sentences, 0 otherwise. The modified writing test score takes a value 1 if the child can write without difficulties, 0 otherwise. Once again we employ the conditional logit fixed-effects estimation strategy to obtain the short run impact estimates on the modified reading and writing test scores reported in columns 2 and 3, Table 9 below. We find that these estimates are similar to the OLS estimates reported in Table 4, Panel B.

Table 9: Conditional Logit-Fixed Effects Estimates for Enrollment and Reading and Writing Test Scores

	Enrollment (1) MLE	**Modified reading comprehension test scores (2) MLE**	**Modified writing test scores (3) MLE**
NREGS x Time1	-0.045	0.69*	0.20
(2002-2007)	(0.66)	(0.40)	(0.27)
Short-run effect			
NREGS x Time2	-0.70		
(2002-2009/10)	(0.78)		
long-run effect			
Sample size	546	520	

Notes: *** p<0.01, ** p<0.05, * p<0.10. The sample covers rural areas only.

6. Discussion

Public works programs are often seen as critical policy instruments for decreasing unemployment rates, facilitating consumption smoothing, creating assets and alleviating multidimensional poverty. Yet, existing cost-effectiveness analyses of public works program do not take into account the spillover effects of the program that accrue to the next generation (Ravallion 1991). This is partly due to the unavailability of evaluation studies in this area. To our knowledge, this paper is the first to examine the impact of a public works program on grade progression, reading comprehension test scores, writing test scores, math test scores and PPVT scores.

We use longitudinal data from the 2002 (round 1), 2007 (round 2) and 2009-10 (round 3) waves of the Young Lives Panel Study administered in Andhra Pradesh, India to assess the impact of the world's largest public works program, the National Rural Employment Guarantee Scheme on measures of intellectual human capital. The NREGS was phased-in two of the four young lives districts between rounds 1 and 2 and phased-in to the remaining four districts by round 3. We combine pre- and two rounds of post-intervention initiation data from the Young Lives Panel Study in a quasi-experimental framework to capture the short- and medium-run intent-to-treat effects of having access to the NREGS on schooling enrollment, grade progression, reading comprehension test scores, writing test scores, and performance on math test and PPVT.

A number of important findings emerge from our analysis. First, we find that NREGS was primarily placed initially in poor communities. We find that communities that have higher levels of pre-intervention population, and fewer hospitals and health centers were more likely to receive the program first. Therefore the control for program placement in our estimates through first-differencing is important to avoid biases because there probably are important unobserved as well as observed pro-poor determinants of program placement. Second, the NREGS has no effect on schooling enrollment, a short-run measure of investment in intellectual human capital. Third, access to the program has large and positive effects on children's performance on reading comprehension test, math test and PPVT. The impact estimates vary between 0.25 and 0.40 standard deviations. The average effect size computed over several outcomes is 0.08 in the short run and 0.15 in the medium run and is similar to the estimates found for CCT programs in Latin America. Fourth, short-run effects of the program are all sustained and indeed generally augmented in the medium-run, that is, there is no decaying of observed treatment effects. Fifth, the average effects size for males do not vary between the short and medium run. This suggests more catch-up among male children in the control districts. However, the impact of receiving the intervention earlier is substantially large for females suggesting little catch-up among females. Finally, our impact estimates are robust to a number of other concerns – attrition bias, type I errors, type II errors and endogenous placement.

Observed improvements in children's human capital accumulation can operate either via improvement in household monthly per capita consumption expenditure and or improvement in community infrastructure and or other smoothening mechanisms. The Young Lives Panel Study did not collect any pre-intervention data on per capita household consumption expenditure, however, a simple comparison of the post-intervention household monthly per capita consumption expenditure from round 2 between the early phase-in and late phase-in districts in rural Andhra Pradesh suggest that on average children residing in the early phase-in districts experienced an almost 40% increase in household monthly per capita consumption expenditure relative to children residing in the late phase-in districts. We find that these differences continue to persist through round 3. We also find that children residing in households registered for the NREGS in rounds 2 and 3 report 10% and 18% higher per capita consumption expenditures compared to children residing in households that did not register for the NREGS in rounds 2 and 3 respectively. We find that the increase is in per capita consumption expenditures for registered households is smaller and is reflective of the potential spill over effects operating through larger public goods available in the community that are beneficial to both households registered and non-registered in the NREGS districts therefore, reiterating the importance of using an intent-to-treat effect approach to impact evaluation.

Our findings have several important policy implications. One, public works program can be extremely beneficial in improving children's human capital. Two, cost-effectiveness analysis of public works program based solely on outcomes such as labor force participation and income are likely to underestimate the total gains from such programs that are likely to accrue at both the household level and the individual level including children. Moreover, effects on intellectual human capital are likely to have substantial spillover effects and intergenerational effects, which are not easily measurable. Third, the gains from receiving the program early on remain significant pointing to the value of receiving interventions during primary school. Fourth, the timing of these interventions is critical particularly for females.

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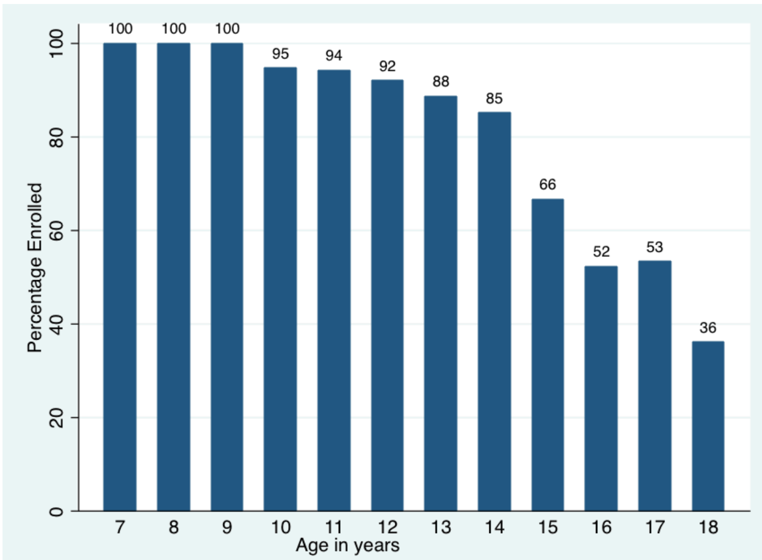
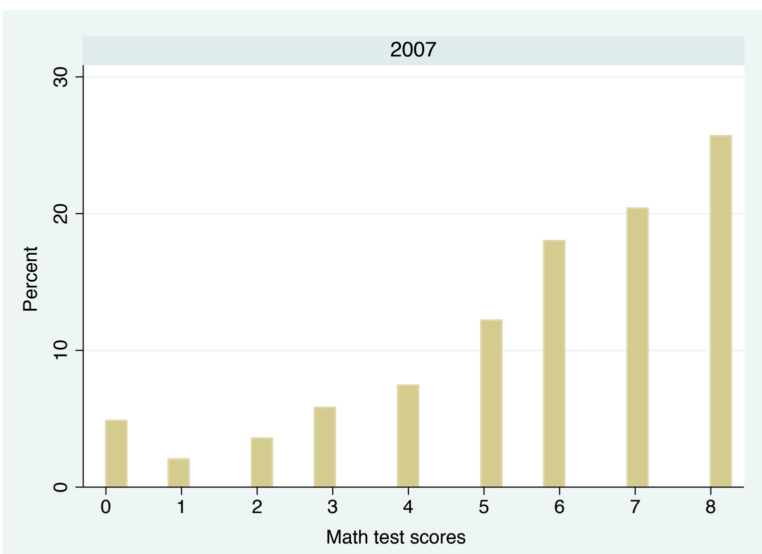
Appendix

Figure A1: Enrollment by Age in years for rural AP using IHDS



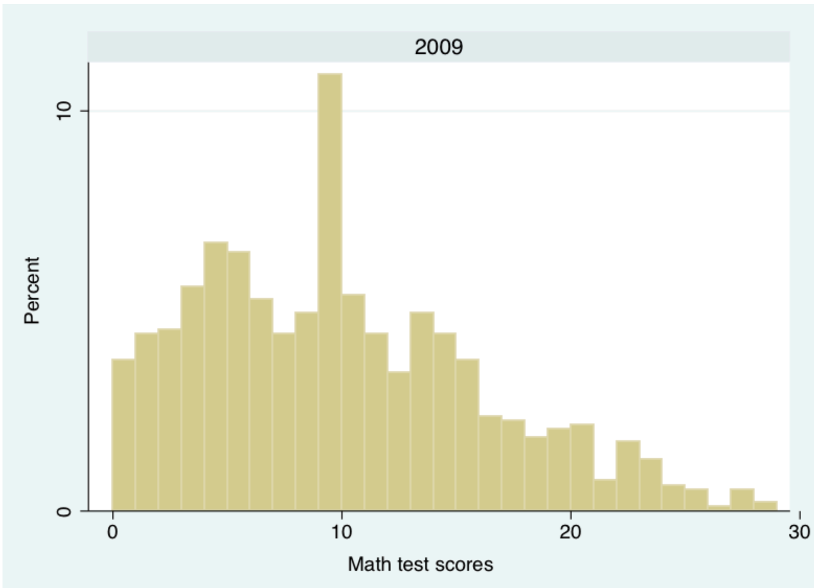
Source: 2005 Indian Human Development Survey (IHDS)

Figure A2: Distribution of raw Math test scores in 2007 for rural AP



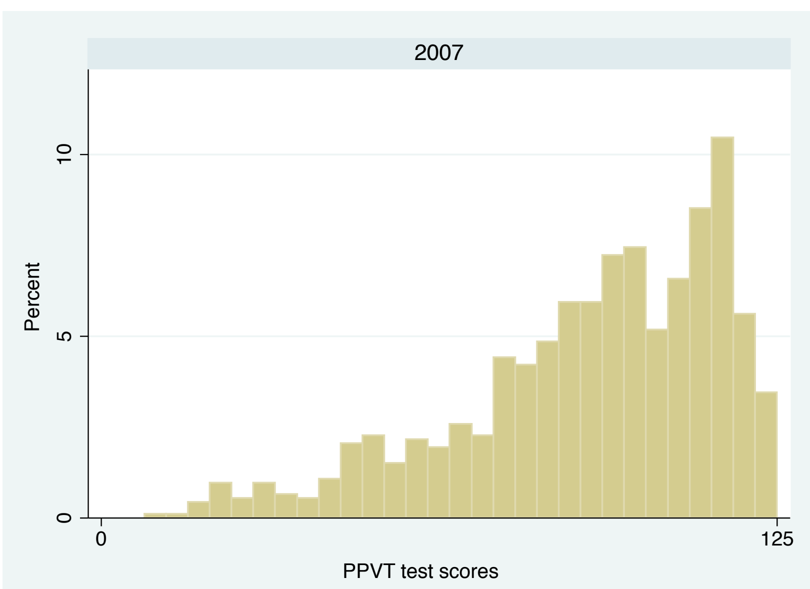
Source: Young Lives Panel Study

Figure A3: Distribution of raw Math test scores in 2009 for rural AP



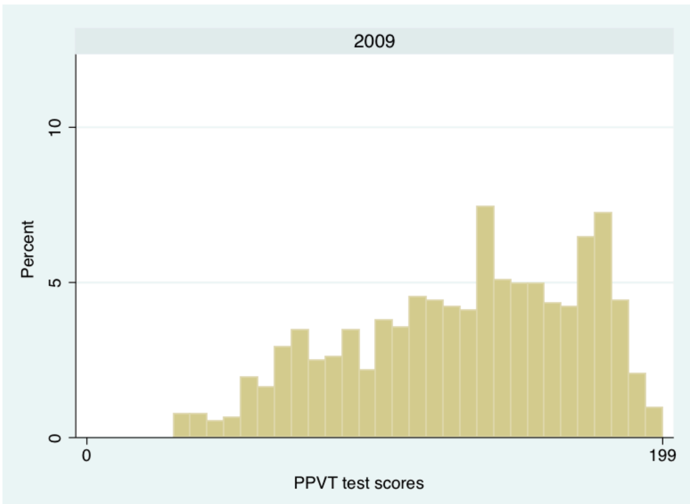
Source: Young Lives Panel Study

Figure A4: Distribution of raw PPVT scores in 2007 for rural AP



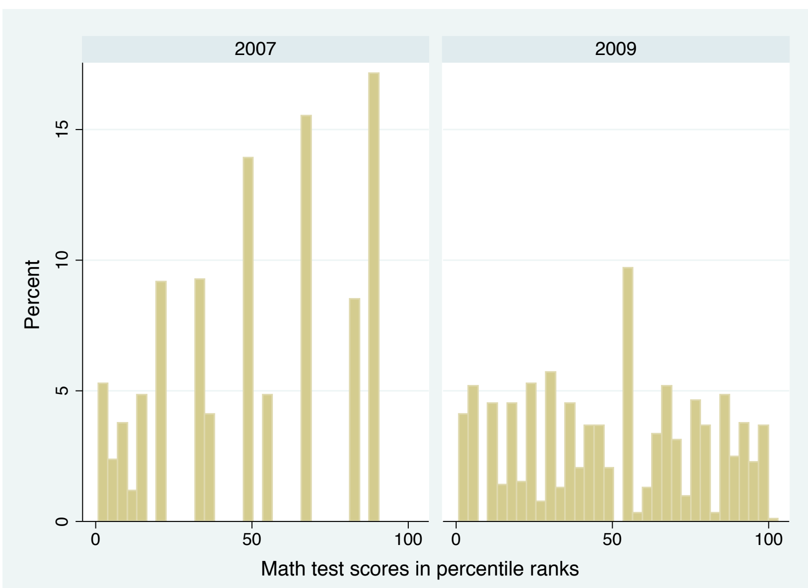
Source: Young Lives Panel Study

Figure A5: Distribution of raw PPVT scores in 2009 for rural AP



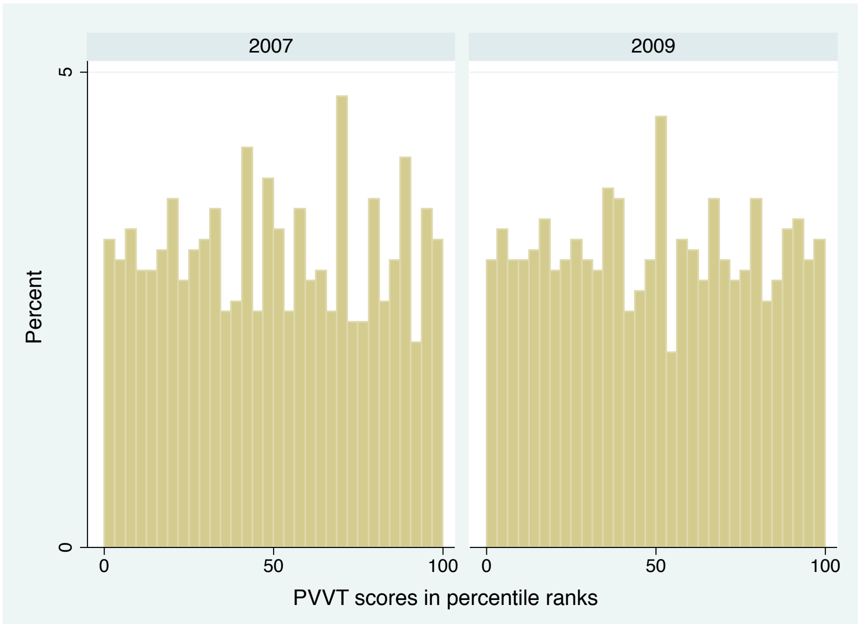
Source: Young Lives Panel Study

Figure A6: Distribution of Math test scores in percentile ranks by year for rural AP



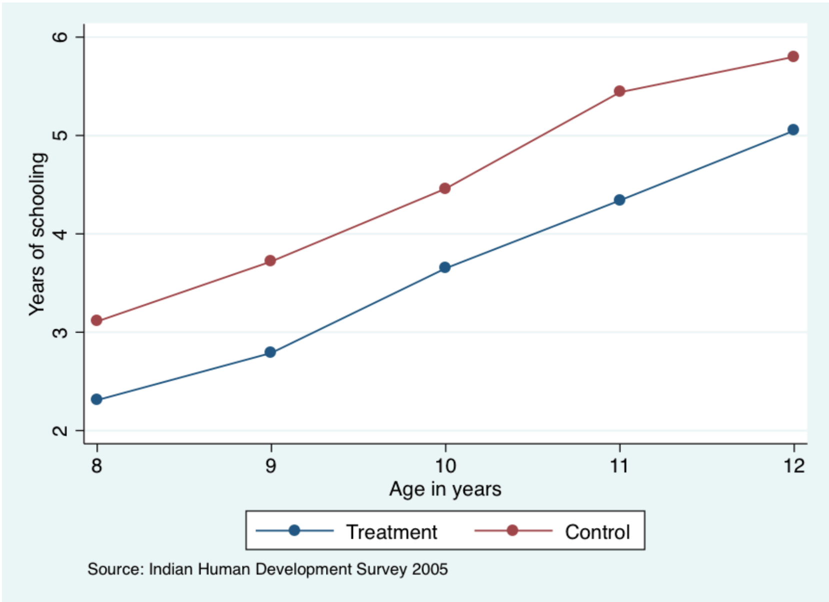
Source: Young Lives Panel Study

Figure A7: Distribution of PPVT scores in percentile ranks by year for rural AP



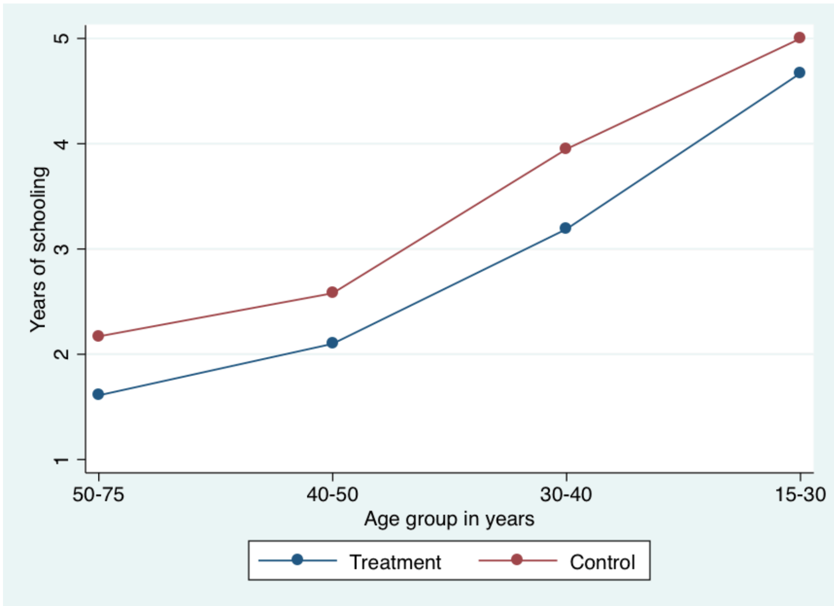
Source: Young Lives Panel Study

Figure A8: Pre-intervention Years of Schooling by Age in Years



Source: 2005 Indian Human Development Survey. Years of schooling is same as grades of schooling.

Figure A9: Pre-intervention Years of Schooling by Age Groups



Source: 2005 Indian Human Development Survey. Years of schooling is same as grades of schooling.

Table A1: ITT Effects of the NREGS from a SUR Framework

	Enrollment (1)	Grade progression (2)	Reading comprehension test scores (3)	Writing test scores (4)	Math test scores (in percentile ranks) (5)	PPVT scores (in percentile ranks) (6)
Panel A: ITT effects with baseline controls						
NREGS x Time1 (2002-2007) *Short-run effect*	-0.04 (0.13)	0.05 (0.11)	0.31** (0.12)	0.08 (0.11)		
NREGS x Time (2007-2009/10) *Medium-run effect*	-0.08 (0.09)	0.09 (0.07)			0.22* (0.12)	0.42*** (0.12)
Sample size	703	703	703	703	703	703

Notes: Robust standard errors (in parentheses) clustered at the community level. The pre-intervention controls included in panel A are male dummy, age in months, male dummy interacted with age in months, wealth index, household size, number of school age children, SC/ST dummy, OBC dummy, religion dummy, Raven's test scores, mother's schooling and father's schooling. *** p<0.01, ** p<0.05, * p<0.10. The sample covers only rural areas. The short-run effects correspond to the average effect size reported in column 1 Table 5, panel B and the medium-run effects correspond to the average effect size reported in column 2 Table 5, panel B.

Table A2: ITT Effects of the NREGS on Grade Progression

	Grade progression
	(1)
NREGS x Time1	0.07**
(2002-2007)	(0.03)
Short-run effect	
NREGS x Time	0.011***
(2007-2009/10)	(0.03)
Medium-run effect	
Sample size	703

Notes: Robust standard errors (in parentheses) clustered at the community level. The pre-intervention controls included are male dummy, age in months, male dummy interacted with age in months, wealth index, household size, number of school age children, SC/ST dummy, OBC dummy, religion dummy, Raven's test scores, mother's schooling and father's schooling. *** p<0.01, ** p<0.05, * p<0.10. The sample covers only rural areas.