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Impact of the NREGS on Children's Intellectual Human Capital

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Abstract

This paper uses panel data from the Young Lives Survey to examine the effect of the world's largest public works program and India's flagship social protection program, the National Rural Employment Guarantee Scheme (NREGS), on children's learning outcomes such as grade progression, reading comprehension test scores, writing test scores, math test scores, and Peabody Picture Vocabulary Test (PPVT) scores. We find that the program has strong positive effects on these outcomes in both the short-and-medium run. Finally, the impact estimates reported here are robust to a number of econometric concerns such as –program placement, selective attrition, and type I error.

Keywords

NREGS; Schooling; Test Scores; Panel Data; India; Public Works Program

JEL Classification

I 25; I38; J 13

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).¹ The NREGS provides 100 days of unskilled

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¹<http://www.hindustantimes.com/India-Budget-2013/Chunk-HT-UI-IndiaBudget2013-Economy/NREGA-losing-sheen-Allocation-unchanged/SP-Article1-1019269.aspx>

wage employment to any household residing in rural areas whose adult members choose to work in the program. Employment opportunities made available through 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 poverty eradication, critics of these programs remain skeptical.

The majority of studies evaluating the effectiveness public works programs around the world focus on outcomes such as employment, wage rates, and consumption expenditure. Existing findings suggest a positive impact of social protection programs, including the NREGS, on both labor market outcomes and consumption expenditures (Ravi and Engler 2015, Liu and Deininger 2010, Azam 2012, Subbarao 2003 and Betcherman et. al 2000).

Anti-poverty programs like the NREGS, however, may serve as a powerful policy instrument for improving multidimensional welfare outcomes. In particular, the NREGS has the potential to impact the lives of children in many ways. First, the NREGS is likely to directly result in changes in parents' labor-supply decisions, which are positively (negatively) related to improvements in children's human capital if the income (substitution) effect outweighs the substitution (income) effect.² Second, NREGS-led improvements in women's bargaining power/empowerment can further augment both the quantity and quality of children's human capital. Third, NREGS-led improvements in community-level public-good provision such as roads and water supplies are also likely to positively impact children's human capital. Fourth, the NREGS can result in improvements in household per capita consumption and associated improvements in educational expenditure and schooling outcomes. Conversely, participation in NREGS led increase in manual work by adult members may increase the demand for child labor in household production, decreasing their human capital. As a result, the net effect of access to NREGS for children remains ambiguous.

There have been a few studies that examine the intent-to-treat/net effects of NREGS on different outcomes. Das and Singh (2015) find no impact on years of schooling. Li and Shekhri (2013) report an increase in private school enrollments. Shah and Steinberg (2015) report positive effects for younger children's enrollment and math and negative effects for older children. Our study is distinct from the existing literature in that none of the existing papers examine the intent-to-treat/net effects of the NREGS on measures of cognitive skills such as – performance on grade progression, reading, and writing tests scores, which in recent studies have been found to be more strongly related to wage earnings than schooling attainment (Hanushek and Woessman, 2008). Our contribution and goal is to examine the Intent-to-treat/"net" effect of having access to the NREGS on children's learning outcomes.

The NREGS was introduced 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 to the 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 roll-out), the NREGS was placed in all remaining rural districts in

²Afridi et. al (2016) exploit the natural variation in NREGS implementation to identify the causal impact of maternal labor force participation on children's educational outcomes.

India. The absence of random assignment of districts to the NREGS makes simple OLS estimates of the program effects biased. That said, many countries including India, Ethiopia, South Africa, Argentina, Bolivia, Senegal, Madagascar, and others have not experimented with the allocation of major public works programs; exceptions include Malawi, which experimented with only certain features of their public works program (Beegle et. al 2015). Given the enormous amount of money spent on the NREGS, it is worth investigating the impacts of the largest public works program in the world on children's human capital.

We combine the availability of pre- and two rounds of post-intervention-initiation data in a quasi-experimental framework to estimate both the short- and medium-run ITT effects of early access to the NREGS on learning outcomes. To address program placement-related concerns, our preferred first-difference estimates control for the presence of both time-invariant unobservables and pre-intervention observables. Further, we will also allow for differential time-trends between early and late phase-in districts.

A number of important findings emerge from our analysis. First, early 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 short run effects suggests a 0.15 standard deviation improvement in cognitive outcomes for children in the early phase-in NREGS districts in comparison to children in districts that received the late. Second, short-run effects of the program are all sustained in the medium run. Finally, our impact estimates are robust to a number of econometric concerns – endogenous program placement, attrition bias, and type I errors.

2. The program: National Rural Employment Guarantee Scheme

NREGS came into effect in September 2005 in all states of India, 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).³ The 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 also 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 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

³We 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.

130 districts and finally, by April 2008 the program reached 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 states of Andhra Pradesh and Telangana alone, were employed under the NREGS during 2006–2007. Given the large-scale nature of this public works program, our focus is to provide evidence on the impact of the NREGS on cognitive development, which in the long run is 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 administered in the states of Telangana and Andhra Pradesh in India. The Young Lives sample 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 2007 (round 2) and 2009–10 (round 3). Our analysis sample is restricted to only include the older cohort since the pre-intervention data on grade progression, reading comprehension, and writing ability is only available for this cohort. The sentinel sites were chosen to represent all three agro-climatic (Coastal Andhra Pradesh, Rayalseema, and Telangana) regions of pre-split Andhra Pradesh. See Kumra (2008) for further details on the sampling approach adopted by the Young Lives Panel Study.

The Young Lives Panel Study in India covers six districts – Cuddapah, Anantapur, Mahbubnagar, Karimnagar, West Godavari, Srikakulam, and the city of Hyderabad. Since the primary objective of this paper is to examine the impact of the NREGS, which is only implemented in rural areas, we restrict our analysis sample to include only rural areas. The National Rural Employment Guarantee Act was enacted in 2005 and by 2007 (round 2) was implemented in four (Cuddapah, Anantapur, Mahbubnagar, Karimnagar) of the six Young Lives districts. 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) covered under the Young Lives Panel Study. 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 effects of access to this scheme on measures of schooling and cognitive development.

3.2 Key Variables

We focus on five outcome variables – grade progression, reading comprehension test scores, writing test scores, math test scores, and Peabody Picture Vocabulary Test (PPVT) scores. These 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].

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. We also use scores from reading comprehension 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].

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 PPVT scores in each round.

Summary statistics on all outcome variables are reported below in Table 1, Panel A. Grade progression increases between rounds 1 and 2, 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 rounds 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.

Approximately 48% of the children in our sample are male, 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 including on average at least one school-age sibling. The children are primarily Hindu (91%) and 85% of the sample belongs to backward castes (Scheduled Castes and Tribes = 38% and Other Backward Castes = 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. 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 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 95% (N=719) of the rural children surveyed in rounds 1 and 2. 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 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 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. We show in Appendix Table A1, the null that there is no selective attrition based on differences in pre-intervention outcomes between early-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.

4. Conceptual framework

Our first outcome variable of interest is 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 ITT effects of the program as specified by equation (1).

$$Y_{it} = \beta_0 + \beta_1 \text{NREGS}_i + \beta_2 \text{Time1}_t + \beta_3 \text{NREGS} \times \text{Time1}_{it} + \beta_4 \text{Time2}_t + \beta_5 \text{NREGS} \times \text{Time2}_{it} + \varepsilon_i + \varepsilon_c + \varepsilon_h + \varepsilon_{it} \quad (1)$$

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, 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 ITT effect of the NREGS, the relative increase in 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 the 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 grade progression that are greater than the short-run effects.

The disturbance term in equation (1) includes four components – ε_i , ε_c , and ε_h are time-invariant unobserved characteristics of the individual child, community and household, respectively and ε_{it} is a random shock for the i^{th} child in the t^{th} period. The program placement related concerns are discussed in detail in Section 5.3.

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 ITT effects of the program using a simple difference-in-difference specification (2).

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

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 β_1 again captures all pre-existing differences between treatment and control districts. Time1 is a dummy variable that again takes a value 1 if year is 2007, 0 otherwise. The coefficient estimate on the interaction term (NREGS x Time1), β_3 again captures the short-run ITT 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 (3) 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.

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

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 would suggest that changes in test scores between rounds 2 and 3 for the treatment group are greater than observed changes in test scores between rounds 2 and 3 for the control districts that received the program later. This would suggest that the effect of earlier exposure to the program is augmented in the medium run even after the control group starts receiving access to the program. A negative coefficient on the interaction term would suggest that the control group tends to at least partially catch-up to the treatment group.

5. Results

5.1 Intent-to-treat effects

To address selection on time-invariant unobservables, we estimate equations (1)–(3) using the first-difference OLS specification. The ITT effect of the NREGS is reported in Table 2. Our preferred estimates in Table 2 also control for pre-intervention socioeconomic and demographic characteristics.

Our preferred estimates for the short-run (2002–2007), medium-run/additional (2007–2009/10), and long-run (2002–2009/10) ITT effects of the NREGS on grade progression are reported in Table 2. We find that the NREGS has positive and statistically significant effects on grade progression in the short, medium, and long runs. We find that children residing in districts that receive the NREGS between rounds 1 and 2 on average get approximately 8% closer to their potential grade levels 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 approximately 11% closer to their potential grade levels 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 2 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.

Next, we use pre (2002) and post (2007) intervention data on reading comprehension test scores and writing test scores to compute the short-run ITT 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 2 and 3 Table 2, 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 5 Table 2.

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 4 Table 2, 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 5 Table 3 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.

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 five 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 grade progression, reading comprehension test scores, and writing test scores to measure the short-run ITT 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 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 3. The short- and medium-run ITT effects of the NREGS are statistically significant. In the short run, assignment to the NREGS districts increases schooling outcomes by 0.15 standard deviations and in the medium run, early exposure to the NREGS increases intellectual human capital by an additional 0.24 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 5% and 1% 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.

To our knowledge, we are the first to evaluate the overall impact of a public works program on children's 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 these impact estimates with the effects obtained from conditional cash transfer (CCT) programs aimed at poverty alleviation. Fiszbein and Schady (2009) in their review show that CCT programs targeted at pre-school children in Nicaragua and Ecuador are more effective in improving test scores [Paxson and Schady

(2010), Macours et. al (2012)] than CCT interventions targeted to school-age children 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. The short run results indicate a 0.15 standard deviation improvement in cognitive outcomes and in the medium run the effects are sustained and result in a 0.24 standard deviation improvement in test scores. The impact estimates reported here are close to the impact estimates reported for the CCT program in Nicaragua (Macours et. al 2012, Barham et. al 2013) and also for the pre-school nutrition program implemented in Guatemala (Maluccio et. al 2009).

5.2 Gender Differential Treatment Effects

There is long emphasis in the literature on India on females being treated more as luxury goods, suffering more from intra-household responses to negative shocks and benefiting more from positive shocks (e.g., Behrman 1988, Behrman and Deolalikar 1990, Sen 1990, Jacoby and Skoufias 1997, Rose 1998). Our aim here is to examine if the NREGS results in differential effects for male and female children.

The ITT effect of the NREGS on grade progression, reading and writing scores reported in Table 4 below remains similar for males and females, and largely consistent with those in Table 2. However, we find that NREGS has strong positive effects on females' math and PPVT scores, with no impact on males generating large gender differences in these impacts.

5.3 Endogenous placement

To account for non-random program placement⁴, our preferred first-difference estimates control for both time-invariant unobservables and pre-intervention observables. Further, to also allow for differential trends between early and late phase-in districts, we estimate our preferred specification with individual fixed-effects along with a full-set of pre-intervention control variables interacted with time dummies. These estimates are reported in Table 5 below. We find that these estimates are similar to the preferred estimates reported earlier in Table 2.

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 possible 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 overall/net effects of having access to public works programs on child education – namely, in this case, grade progression, reading comprehension test scores, writing test scores, math test scores, and PPVT scores.

⁴We now show in Appendix Table A2 mean pre-intervention differences between early and late implementers of the NREGS. Out of the 14 variables tested in Table A2, 7 are statistically significant and except for Raven's test score, all suggest pro-poor implementation of the NREGS.

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 and Telangana states of India to assess the impact of the world's largest public works program, the National Rural Employment Guarantee Scheme, on measures of children's intellectual human capital. The NREGS was phased-in two of the six 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 ITT effects of having access to the NREGS on 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, access to the program has large and positive effects on children's performance on reading comprehension tests, math tests, and the PPVT. Second, 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. Finally, our impact estimates are robust to a number of other concerns – attrition bias, type I errors, and endogenous program placement.

Our findings have several important policy implications. First, public works programs can be beneficial in improving children's human capital. Second, cost-effectiveness analysis of public works programs based solely on outcomes such as labor force participation and income probably 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 measured. Third, the gains from receiving the program early-on remain significant pointing to the value of receiving interventions during primary school.

Some caveats remain. First, given the non-random nature of the program roll-out and implementation, our results are susceptible to biases associated with endogenous program placement that relate to the presence of time-varying unobservables. Second, the findings reported in this paper cannot be used to generalize the impact of NREGS in other states of India as Andhra Pradesh and Telangana are noted to be “star states” responsible for high levels of NREGS employment provision (see Imbert and Papp, 2015).

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Appendix A – Additional tables

Table A1:

Linear Probability Model of Attrition

	(1)	(2)	(3)	(4)
	Attrit	Attrit	Attrit	Attrit
NREGS	0.039 (0.04)	0.008 (0.04)	0.042 (0.03)	0.03 (0.03)
Grade progression	-0.010 (0.02)			
NREGS*grade progression	0.008 (0.04)			
Reading comprehension test scores		-0.022 [*] (0.013)		
NREGS*reading comprehension test scores		0.017 (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 scores				0.02 (0.034)
Household size	0.013 ^{**} (0.006)	0.013 ^{**} (0.006)	0.013 ^{**} (0.006)	0.014 ^{**} (0.006)
Number of school age children	-0.032 ^{***} (0.011)	-0.032 ^{***} (0.011)	-0.032 ^{***} (0.011)	-0.032 ^{***} (0.011)
Raven's test scores	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)
Wealth index	-0.023 (0.07)	-0.012 (0.07)	-0.022 (0.07)	-0.034 (0.07)
Male dummy	0.44 (0.44)	0.45 (0.44)	0.44 (0.44)	0.42 (0.44)
Age in months	0.003 (0.004)	0.004 (0.004)	0.003 (0.004)	0.003 (0.004)
Male dummy*age in months	-0.005 (0.005)	-0.005 (0.005)	-0.004 (0.005)	-0.005 (0.005)
SC/ST dummy	-0.086 [*] (0.05)	-0.088 [*] (0.05)	-0.086 [*] (0.05)	-0.085 [*] (0.05)
OBC dummy	-0.098 ^{**} (0.047)	-0.10 ^{**} (0.048)	-0.098 ^{**} (0.048)	-0.095 ^{**} (0.047)
Religion dummy	0.019 (0.031)	0.022 (0.031)	0.020 (0.031)	0.015 (0.032)
Mother's schooling	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.004)
Father's schooling	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.004)
Constant	-0.23 (0.35)	-0.23 (0.35)	-0.24 (0.34)	-0.26 (0.33)
Sample size	757	757	757	757
R-squared	0.039	0.041	0.039	0.044

Notes: Standard errors in parentheses.

p<0.01,

p<0.05,
*
p<0.10.

The sample covers rural areas only.

Table A2:

Pre-intervention differences between early and late phase-in districts

	Mean Early-NREGS (1)	Mean Late-NREGS (2)	Difference (se) (3)
Grade progression	0.802	0.942	-0.14 *** (0.027)
Reading comprehension test scores	0.565	0.697	-0.132 *** (0.038)
Writing test scores	0.407	0.548	-0.141 *** (0.039)
Raven's test scores	23.29	21.35	1.94 *** (0.407)
SC-ST	0.350	0.427	-0.077 ** (0.038)
OBC	0.480	0.468	0.012 (0.039)
No. of children	1.50	1.20	0.30 *** (0.08)
Hindu	0.924	0.896	0.03 (0.022)
Household size	5.744	5.290	0.454 *** (0.162)
Wealth index	0.333	0.325	0.008 (0.013)
Male	0.482	0.494	-0.011 (0.039)
Age in months	96.283	96.376	-0.093 (0.314)
Mother's schooling	1.673	2.020	-0.347 (0.243)
Father's schooling	3.586	3.493	0.093 (0.352)
Observations	462	241	

Notes: Standard errors in parentheses.

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

The sample covers rural areas only and is restricted to the pre-intervention period, that is, 2002.

Appendix B: Model

We assume that households maximize utility, U (1), subject to an income constraint (2), and a schooling production function (3).

$$U = u(C_t, S_t, L_t) \quad (1)$$

$$p_t^c C_t + p_t^m M_t = w_t(NREGS_t)H_t + \pi_t \quad (2)$$

$$S_t = f(M_t, I_t(NREGS); \theta_c, \mu_h) \quad (3)$$

The utility function depends upon food and non-food consumption goods, C_t , leisure, L_t , child's schooling outcome, S_t . It is assumed that the household does not derive any direct utility from the consumption of school inputs, M_t except via its impact in determining S_t . S_t is modeled here as a pure consumption good from which the household derives utility.

P_t^c includes prices of food and non-food consumption goods, P_t^m captures prices of schooling inputs, w_t is wage rate reflecting the price of parents' time, and H_t is hours worked. Labor income is the product of wage rate and hours worked, where labor income in a village also depends on NREGS opportunities. Early phase in districts (and villages there in) will have a positive source of labor income owing to access to NREGS, however, the late phase in districts would have no access to NREGS related wage income. Finally, profit income from farm and non-farm activities and all other sources of non-labor income will be captured by π_t , which in turn depends on the opportunity cost of labor, and consequently NREGS.

The schooling production function specified in (3), S_t is written as a function of schooling inputs (M_t), community resources (I_t) which also include NREGS related projects and activities. Finally, child characteristics and household characteristics are captured by θ_c and μ_h .

Schooling outcomes, S_t include measures of grade progression and performance on reading, writing and math tests. Schooling inputs M_t include books, school uniform, and other home inputs. Environmental characteristics, I_t capture overall resource availability in the community and include factors such as number of schools, access to electricity and other community infrastructure that affects schooling outcomes. θ_c include child specific characteristics such as child's sex and age capturing age and gender specific differences in the accumulation of schooling outcomes. It also includes time-invariant measures of child's own innate ability to perform well in school capturing overall cognitive development and learning potential. μ_h captures household demographic characteristics and other time-invariant rearing and caring practices, all of which affect schooling/learning outcomes.

Using simple first-order conditions, we can solve for the optimal amount of schooling input, M_t^* as follows:

$$M_t^* = f(p_t^c, p_t^m, w_t, \pi_t, I_t, \mu_h, \theta_c) \quad (4)$$

Where as noted before, the opportunity cost of parental time as measured by village wage rates and community infrastructure are both impacted by the public works program. NREGS lead changes in parents' opportunity cost of time (that is, wage rate) will also result in changes in farm and non-farm labor supply and consequently, profit income. Over time,

access to NREGS related opportunities and associated incomes, will be accompanied by increase in the price of food and non-food consumption goods as well. Hence, the NREGS impacts child's schooling through changes in prices, incomes as well as access to infrastructure as shown in equation (5).

$$S_t^* = f(g(p_t^c p_t^m w_t I_t \pi_t (\text{NREGS}_t)), \mu_h, \theta_c) \quad (5)$$

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Table 1:

Summary Statistics

Panel A: Outcome variables	2002 Mean (s.d)	2007 Mean (s.d)	2009–10 Mean (s.d)
Grade progression	0.85 (0.35)	0.89 (0.18)	0.88 (0.18)
Reading comprehension test scores	1.99 (1.05)	2.64 (0.81)	NA
Writing test scores	1.21 (0.80)	1.60 (0.60)	NA
Math test scores (in percentile ranks)	NA	50 (28.5)	50 (28.8)
PPVT scores (in percentile ranks)	NA	50 (28.9)	50 (28.9)
Panel B: Control Variables	2002		
Male dummy (=1 if male, 0 = female)		0.48 (0.50)	
Age in months		96.3 (3.95)	
Household size		5.58 (2.06)	
Number of school-age children in the household		1.40 (1.03)	
Wealth index		0.33 (0.16)	
Raven's test scores		22.6 (5.20)	
SC/ST dummy (=1 if Scheduled caste/Scheduled tribe, 0 otherwise)		0.38 (0.48)	
OBC dummy (=1 if other backward class, 0 otherwise)		0.47 (0.49)	
Religion dummy (=1 if Hindu, 0 otherwise)		0.91 (0.28)	
Mother's schooling (completed grades)		1.8 (3.1)	
Father's schooling (completed grades)		3.6 (4.4)	
Sample size		703	

Notes: The sample covers rural areas only.

Table 2:

ITT Effects of the NREGS

	Grade progression (1)	Reading comprehension test scores (2)	Writing test scores (3)	Math test scores (in percentile ranks) (4)	PPVT scores (in percentile ranks) (5)
NREGS × Time1 (2002-2007) <i>Short-run effect</i>	0.076 ** (0.03)	0.31 *** (0.11)	0.10 (0.08)		
NREGS × Time2 (2002-2009/10) <i>Long-run effect</i>	0.108 *** (0.037)				
NREGS × Time (2007-2009/10) <i>Medium-run effect</i>	0.032 ** (0.012)			5.78 * (3.45)	11.88 *** (3.53)
Sample size	1406	703	703	703	703

Notes: Robust standard errors (in parentheses) clustered at the community level. The pre-intervention controls include 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 medium-run effect reported in column (1) is computed as the difference between the long-run effect and the short-run effect.

Table 3:

ITT Effects of the NREGS

	Index1 (1)	Index2 (2)
NREGS × Time1 (2002-2007) <i>short-run effect</i>	0.15 ** (0.06)	
NREGS × Time (2007-2009/10) <i>medium-run effect</i>		0.24 *** (0.07)
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.

Table 4:

ITT Effects of the NREGS by Gender

	Grade progression (1)	Reading comprehension test scores (2)	Writing test scores (3)	Math test scores (in percentile ranks) (4)	PPVT scores (in percentile ranks) (5)
NREGS × Time1 (2002-2007) <i>Short-run effect</i>	0.055 (0.037)	0.38 ^{***} (0.11)	0.12 (0.11)		
NREGS × Time2 (2002-2009/10) <i>Long-run effect</i>	0.091 ^{**} (0.042)				
NREGS × Time (2007-2009/10) <i>Medium-run effect</i>	0.036 ^{**} (0.017)			15.70 ^{***} (4.62)	16.83 ^{***} (4.35)
NREGS × Time1 × Male (2002-2007)	0.041 (0.045)	-0.18 (0.18)	-0.06 (0.16)		
NREGS × Time2 × Male (2002-2009/10)	0.032 (0.047)				
NREGS × Time × Male (2007-2009/10) <i>Medium-run effect</i>	0.009 (0.021)			-19.30 ^{***} (4.96)	-10.16 ^{**} (4.20)
Sample size	1406	703	703	703	703

Notes: Robust standard errors (in parentheses) clustered at the community level. The pre-intervention controls include 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.

Table 5:

ITT Effects of the NREGS with individual fixed-effects and differential time trends

	Grade progression (1)	Reading comprehension test scores (2)	Writing test scores (3)	Math test scores (in percentile ranks) (4)	PPVT scores (in percentile ranks) (5)
NREGS × Time1 (2002-2007) <i>Short-run effect</i>	0.087*** (0.02)	0.31*** (0.09)	0.10 (0.07)		
NREGS × Time2 (2002-2009/10) <i>Long-run effect</i>	0.11*** (0.02)				
NREGS × Time (2007-2009/10) <i>Medium-run effect</i>	0.02 (0.02)			5.78** (2.34)	11.88*** (2.09)
Sample size	2109	1406	1406	1406	1406

Notes: Robust standard errors (in parentheses) clustered at the community level. To allow for differential time trends in the specification, we allow for interactions between the time dummies and each of these covariates (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 medium-run effect reported in column (1) is computed as the difference between the long-run effect and the short-run effect.