Workfare and Human Capital Investment: Evidence from India*

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Abstract

Higher incomes are generally thought to increase human capital investment. However, recent evidence has shown that increases in low-skill wages can decrease school enrollment. We examine the impact of an increase in the demand for low-skill labor caused by a large public works program (NREGS) on test scores and schooling outcomes from a household survey of 2.5 million children in rural India. Exploiting the staged rollout of the program for causal identification, we show that each year of exposure to workfare decreases math scores by 2% of a standard deviation and enrollment by 2 percentage points amongst adolescents. While the impacts of NREGS on human capital are similar for boys and girls, we use additional household survey data on employment to show that adolescent boys are primarily substituting into market work when they leave school while adolescent girls are substituting into unpaid domestic work. We find mixed results for younger children, with the youngest children benefiting from the increased income. We conclude that the opportunity cost of schooling is an important determinant of educational investment. Anti-poverty programs which raise wages could have the unintended effect of lowering human capital investment for adolescents.

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1 Introduction

Education is an important input into economic growth and development (Mankiw et al., 1992). There is some debate over the response of human capital investment to wage growth, particularly in the short run in developing countries. While it has been shown that income effects result in higher investment in education when wages are higher (Jacoby and Skoufias, 1997; Jensen, 2000; Bharadwaj et al., 2013; Maccini and Yang, 2009), there is increasing evidence that increases in low-skill wages can decrease investment in human capital, as adolescents substitute out of school and into the labor market to take advantage of higher wages (Atkin, forthcoming; Shah and Steinberg, forthcoming; Cascio and Narayan, 2015; Charles et al., 2015). The direction and magnitude of this effect matters for development policy, as transfer schemes that operate through wage labor are becoming increasingly popular as a means of redistribution, particularly in poor countries.

In this paper, we examine the effect of a policy-induced increase in low-skill wages on human capital investment in rural India. We we exploit the three-phase rollout of India's National Rural Employment Guarantee Scheme (NREGS), one of the largest workfare programs in the world. This program raised local wages through the guarantee of 100 days work at a fixed, above-market wage (Muralidharan et al., 2015; Berg et al., 2012; Azam, 2012; Zimmermann, 2014; Imbert and Papp, 2015). The program was introduced gradually throughout rural India starting with the poorest districts in early 2006 and extending to the entire country by 2008. We measure human capital using a unique data set on test scores for children in rural India: the Annual Status of Education Report (ASER). ASER tests nearly 500,000 rural children (aged 5-16) per year on basic literacy and numeracy skills. These are children who are currently enrolled, never enrolled and have dropped out.

We find that adolescents score significantly lower on math and reading test scores once NREGS enters their district, and they are also significantly less likely to be currently enrolled and on track in school. These effects are strongest for the 13 to 16 year olds. Each additional year of exposure to NREGS between the ages of 13 and 16 causes math scores to decrease by 2% of a standard deviation and enrollment rates to fall by two percentage points.

The results for younger children are mixed. We find little effect of overall exposure to NREGS for primary school children (aged 5-12). However, NREGS exposure from ages 2-4 significantly improves test scores and the likelihood that these children will enroll and be on track in school when measured at age 5, suggesting the increased income due to NREGS likely plays a positive role for younger children in the household. This is consistent with Shah and Steinberg (forthcoming), who show that higher wages have positive impacts on the human capital of children under 5, but negative impacts on the human capital of older children.

We argue that the decrease in enrollment and test scores is due to adolescents substituting out of school and into the labor market in response to the increase in low-skill wages. Using several rounds of the National Sample Survey, we show that adolescents are 2 percentage points less likely to report school as their primary activity, and 2 percentage points more likely to report productive work as their primary activity. We find that adolescent girls are more likely to substitute for their mothers in domestic work, while boys are more likely to work outside the home for pay. Consistent with the ASER findings, we see no impact of the program on children ages 5-12.

We then examine various threats to identification. We investigate pre-trends of outcome variables, endogenous program rollout, and NREGS-induced selective migration, and we show these threats are unlikely to be driving the empirical results. Conditional on district fixed effects, there is little difference in early vs. late NREGS districts in the years leading up to the rollout of the program, and access to the program next year does not predict work or school enrollment. Our results are similar using actual or predicted rollout (from a "backwardness ranking" created by the federal government), and too large to be explained by any changes in short-term migration patterns induced by the program.

As far as we know, this is the first paper to document the possibility that a workfare program can lead to lower levels of human capital attainment using nationally representative test score data and school enrollment rates. This paper contributes to the literature in several ways. First, it adds to the growing evidence that the income gradient of education might not always be positive. This has been seen in the U.S. context (Cascio and Narayan, 2015; Charles et al., 2015), as well as in developing countries (Atkin, forthcoming; Shah and Steinberg, forthcoming). This paper differs from those mentioned above in that it identifies the effects of a policy-induced change in wages, which might be more relevant in informing the debate about the impacts of wage-based transfers such as public works schemes and wage impacts. Second, it adds to a growing literature which attempts to document the general equilibrium price effects of poverty alleviation programs, both in the United States (Hastings and Washington, 2010; Rothstein, 2010) and in the developing world (Jayachandran et al., 2013; Angelucci and de Giorgi, 2009; Attanasio et al., 2011; Kablonski and Townsend, 2011) which have thus far not focused on public works schemes. Third, we provide some evidence on the elasticity of labor supply for children, a relatively understudied labor supply parameter whose importance has potentially outsized impact due to the substitution between labor and education (see Edmonds (2008) for a review).

This paper is one of several recent papers that examine the impact of NREGA on human capital investment in various ways. Afridi et al. (2016) and Mani et al. (2014) both find that NREGS increases human capital investment in Andhra Pradesh, using Young Lives data. Islam and Sivasankaran (2014) finds that NREGA increases child labor. The most similar paper to this one is Li and Sehkri (2013), which estimates the impact of NREGS on school enrollment numbers using school level data (District Information System for Education), and find that enrollment numbers decrease in treated districts. Our paper differs from Li and Sehkri (2013) in three important ways. First, our outcome data is from a representative household survey, which measures enrollment rates (not levels) and test scores for 2.5 million children, both in- and out-of-school. Test scores matter when measuring the impact of increased income on human capital, since families might substitute into higher-quality private schools, thus enrollment numbers in public schools could decrease (but test scores would likely increase). Second, we provide evidence on the impact of NREGA for young children, which is positive for both test scores and enrollment. Third, we are able to shed light on the mechanisms for this effect using separate data on adult and child labor from the NSS.

Lastly, given the importance of NREGS in particular, and workfare programs more broadly, these effects might be of direct interest to policy makers. Given the importance of education for economic growth, if these types of programs raise prevailing wages and cause older students to substitute toward work and away from school, lump sum grants or conditional cash transfers might be other options to consider for providing social insurance.

2 Background and Data

2.1 Background on NREGS

Workfare programs have become a popular anti-poverty tool. First, they are self-targeting: only those who are willing to work for a low wage will receive the subsidy (Besley and Coate, 1992). Second, they prevent dependency as participants will turn away from public works as better labor market opportunities arise. Third, these programs may increase private sector wages as well, thereby further increasing incomes of the poor.

India's National Rural Employment Guarantee Act (NREGA), passed in 2005, provides a legal guarantee of up to 100 days of annual employment at the statutory minimum wage rate[1] to rural households willing to supply manual labor on local public works in a financial year (Ministry of Rural Development, 2005). The Act mandates equality of wages for men and women and one-third of program beneficiaries to be women. It is operationalized through the National Rural Employment Guarantee Scheme which began in 2006 and has an annual budget of around Rs. 48,000 crores (approx. 9 billion dollars), amounting to more than 11% of the 2011 Union budget expenditure. In 2009-10, approximately 53 million households across India were beneficiaries of NREGS (Dutta et al., 2012).

To obtain work on a project, interested adult members of a rural household must apply

[1] The statutory minimum wage rate varies across states but it is approximately 2USD per day.

for a Job Card at the local Gram Panchayat.[2] After due verification, the Gram Panchayat issues a Job Card, and the card should be issued within 15 days of application. The Job Card bears the photograph of all adult members of the household willing to work under NREGS and is free of cost. Workers can apply for work at any time once they have a job card. The applicants must be assigned to a project within 15 days of submitting the application. If they are not given a job, they are eligible for unemployment compensation. Applicants have no choice over the project. The particular types of projects allowed under NREGS are typical rural employment projects such as road construction, earthworks related to irrigation and agriculture, and water conservation. The federal government bears the entire cost of wages of the workers and 75 percent of the cost of materials. The state governments bear the remaining 25 percent. In addition, state governments bear the cost of unemployment allowance payable when the state government cannot provide wage employment on time (Azam, 2012; Ministry of Rural Development, 2008).[3]

2.2 Rollout of NREGS

The National Rural Employment Guarantee Act was passed in 2005, and the scheme began to rollout in February 2006. Two hundred districts were given access to the program in February 2006. In April 2007, a further 130 districts were added, and in April 2008, the program became available in the remaining 270 districts. In this paper, we will refer to these groups of districts as "wave 1," "wave 2," and "wave 3," respectively. While the actual assignment mechanism to each wave is unknown, the government stated that an explicit goal of the roll out was to target the poorest districts first. However, it also guaranteed each state would receive at least one district in the first wave of the program. Zimmermann (2014) argues that based on the allocation of similar programs, it is likely that states were given

[2]The Gram Panchayat is the lowest level of administration in the Indian government comprising a group of villages.

[3]There is a growing literature on corruption and leakage related to NREGS as well as potential interventions that might help improve these institutional problems (Niehaus and Sukhtankar, 2013; Niehaus et al., forthcoming; Afridi, 2008).

slots to allocate to each wave based on poverty levels. Actual allocation was likely based on the government's own "backwardness rankings" which is a ranking of districts based on agricultural wages, percent of scheduled caste/scheduled tribe, and agricultural productivity (Planning Commission, 2003), though this ranking does not perfectly explain allocation to each wave. For the main analysis of the paper, we will use actual exposure to the program as our independent variable, but in a robustness test we use this backwardness ranking to predict access to NREGS, and we see similar effects.

2.3 Cognitive Testing and Schooling Data

Every year since 2005, the NGO Pratham has implemented the Annual Status of Education Report (ASER), a survey on educational achievement of children aged 3-16 in India which reaches every rural district in the country.[4] The math and literacy tests are administered to children ages 5-16 and so this is the age group we use in the analysis.[5] We have yearly data on children for 2005–2009, giving us a sample size of approximately 2.5 million rural children. We have one round of pre-NREGS test score data (2005), three rounds of test score data during the rollout (2006-2008), and one round after the program has been rolled out throughout the country (2009).

The sample is a representative repeated cross section at the district level. The ASER data is unique in that its sample is extremely large and includes both in- and out-of-school children. Since cognitive tests are usually administered in schools, data on test scores is necessarily limited to the sample of children who are enrolled in school (and present when the test is given). However, ASER enumerators survey at the household on Sundays, when people generally do not work and children are not in school, and must return to households where children are not present at the time of the survey. Therefore, our sample includes

[4] This includes over 570 districts, 15,000 villages, and 300,000 households in a given year. ASER is the largest annual data collection effort with children in India. For more information on ASER, see `http://www.asercentre.org/`

[5] In 2005, only children aged 6-14 were tested. Our results are robust to restricting to these ages for all five years.

children currently enrolled, children who have dropped out, and children who never enrolled. In Table 1 we describe the characteristics of the children in our sample as well as their test scores.

ASER surveyors ask each child four questions each in math and reading (in their native language). The four math questions are whether the child can recognize numbers 1-9, recognize numbers 10-99, subtract, and divide. The scores are coded as 1 if the child correctly answers the question, and 0 otherwise. We calculate a "math score" variable which ranges from 0-4, and is the sum of the scores of the four numeracy questions. For example, if a child correctly recognizes numbers between 1-9 and 10-99, and correctly answers the subtraction question, but cannot correctly answer the division question, then that child's math score would be coded as 3. In 2006 and 2007, children were also asked two subtraction word problems. Thus, we also define "total math score" which includes these word problems in the years in which they were asked. Total math score ranges from 0-6.

Each child was also asked to complete four reading tasks. The child is tested as to whether he/she can recognize letters, recognize words, read a paragraph, and read a story. We generate a total reading score that ranges from 0 to 4 depending on how many tasks the child can complete. The ASER dataset also includes information on whether the child is currently enrolled in school and the highest grade completed which we use as additional measures of schooling.[6]

2.4 Labor and Employment Data

To examine the impact of NREGS on schooling outcomes and work, we use the National Sample Survey (NSS) Rounds 60, 61, 62, 64 and 66 which was collected between 2003 and 2009 by the Government of India's Ministry of Statistics. This is a national labor and employment survey collected at the household level all over India, and we use data from all rural households in these surveys. Rounds 60 and 61 (2004 and 2005) are pre-rollout, round

[6]More information on the ASER survey questions, sampling, and procedures can be found in the ASER data appendix.

62 was collected from July 2005-June 2006, which includes some early districts who received the program in Spring 2006, round 64 is collected during the rollout (2007-2008), and round 66 is collected after the rollout (2009-2010). Because of the timing of the surveys, most of the variation in program access in the NSS analysis will be between those in waves 1 and 2, and those in wave 3.

This dataset gives us measures of employment and schooling status at the individual level. The survey asks about the "primary activity" of each member of the household. We define "domestic work" as individuals who report to attending domestic duties and/or engaging in free collection of goods (vegetables, roots, firewood, cattle feed, etc.), sewing, tailoring, weaving, etc. for household use; we define "works at home" as individuals who report to being self-employed and working in their household enterprise (either as an own account worker, an employer or as an unpaid family worker). We define "works outside of home" as someone who reports their status as a regular salaried/wage employee or as a paid casual laborer. An individual whose primary activity is going to school is defined as "attends school."[7] These four categories of primary activities do not sum to 100 percent. The omitted options include being retired, disabled and unable to work, seeking work, and other (begging/prostitution). We note that all of these categories together only sum to 4 percent for the children, 2 percent for mothers, and 5 percent for fathers.

3 Empirical Strategy and Results

Because NREGS was rolled out in three waves, we can compare districts before and after the program was in place while controlling for overall year and district effects. This strategy allows us to identify effects off the within-district timing of the rollout, rather than simply comparing districts in earlier and later waves.

We estimate the following regression:

[7] Note that this question is asked slightly differently than the school enrollment question in the ASER data. ASER asks if each child is currently enrolled, and if so, in which type of school. NSS asks about the child's "primary activity." Thus, 94% of children in the ASER report being currently enrolled, while only 82% of children the same age report "attending school" as their primary activity.

$$S_{ijty} = \alpha + \beta_1 \mathbf{I}_{jt} + \gamma_j + \phi_t + \psi_y + \epsilon_{ijty} \tag{1}$$

where S_{ijty} is a schooling outcome variable (such as test score or enrollment status) for child i in district j in year t who is age y, $\mathbf{I}_{jt}$ is an indicator of whether district j had NREGS in year t, γ_j is a vector of district fixed effects, ϕ_t is a vector of year fixed effects, and ψ_y is a vector of child age fixed effects. β_1 is our coefficient of interest, and it measures the effect of NREGS on the outcome variable S. Standard errors are clustered at the district level.

Identification relies on the assumption that in the absence of NREGS, the districts that received the program earlier and those that received the program later did not have systematically different time patterns in our outcome variables. We will rely on the assumption that the program was rolled out based on static characteristics of the districts, rather than underlying trends in school enrollment or child labor. This is similar to the method employed in Imbert and Papp (2015), which finds that NREGS increased private sector labor wages and rural incomes. We will investigate threats to identification explicitly in Section 5.

One might expect that children who were exposed to the program for a longer period of time experience larger effects. For example, in 2008, a 13 year old in wave 1 was exposed to NREGS longer than a 13 year old in wave 2. To test for this, we calculate "years of exposure" to NREGS for various cohorts in each district. We calculate years of exposure for children at ages 13-16, 9-12, 5-8, and 2-4, since Shah and Steinberg (forthcoming) show that increased wages have differential effects at different ages. For these regressions, we estimate the following equation:

$$S_{ijty} = \alpha + \beta_2 \mathbf{C}_{jt} + \gamma_j + \phi_t + \psi_y + \delta_{t,y} + \epsilon_{ijty} \tag{2}$$

where $\mathbf{C}_{jt}$ is a vector which measures years of exposure to NREGS for each age group. For the eldest three age categories, years of exposure ranges from 0-4 and for the 2-4 year olds, it ranges from 0-3. γ_j is a vector of district fixed effects, ϕ_t is a vector of year fixed effects, and ψ_y is a vector of child age fixed effects, and $\delta_{t,y}$ is a vector of cohort fixed effects.

3.1 ASER Main Results

Table 2 shows our primary estimates of β_1 from equation 1 in the ASER data. Panel A shows results for the full sample of children, aged 5-16. In Panels B and C of Table 2, we separate children into two broad age groups: primary school age (5-12), and secondary school age (13-16). Columns 1-3 show the effect of NREGS on test scores while columns 4-5 show the estimates of the effect of NREGS on the probability that a child reports being enrolled in school and the effect of NREGS on a child's highest grade completed.

In Panel A, the results suggest that NREGS has a negative impact on total math score, currently enrolled, and highest grade completed. The coefficient on total math score is -.085, and statistically significant at the 1% level. This represents a 3% decrease from a mean of 3.02. Column 4 shows our estimate of the effect of NREGS on the probability that a child reports being enrolled in school. This coefficient is negative and statistically significant, and indicates that NREGS decreases the probability that a child is currently enrolled in school by .66 percentage points. Finally, column 5 shows estimates of the effect of NREGS on a child's highest grade completed, which decreases by .05 years. Broadly, these results are consistent with NREGS reducing overall human capital investment, both on the intensive and extensive margins.

The results in Panel B for the older children are larger and statistically significant relative to the full sample. Enrollment rates for older children are dropping by a full percentage point when NREGS rolls out, which represents an 8% increase in the probability that these children are out of school. Highest grade completed also decreases by .1 year when NREGS rolls out for this older group. In addition, total math score and reading test scores have negative and statistically significant coefficients, which indicates that learning, not just reported enrollment rates, are decreasing for these children.

The results in Panel C for the younger children are smaller and generally not statistically significant. Younger children are 0.46 percentage points less likely to be currently enrolled in school (from a mean of .96). Some coefficients are positive while others are negative. The magnitudes are small and none of the other coefficients on math/reading scores or grade are statistically significant. Therefore, it seems the declines in observed human capital are primarily being driven primarily by the older children.

In Table A1, we investigate the results by gender. Though all of the coefficients on the interaction of female and NREGS are negative, the only coefficient which is statistically significant is total math score. Overall, it seems that NREGS does not differentially affect human capital investment for girls and boys. In Figure A1 we show the coefficients of the test score regressions for each age. The figure highlights that the negative effects are coming from the older children. In Figure A2, we graph coefficients for each test score estimated separately for the two age groups. The Figure highlights that the results are not being driven by one test score and that the results are broadly consistent for all test score measures. In Table A2 we re-estimate Table 2 but we generate additional measures of test scores. In columns 1-2, the index ranges from 0-1 and is equal to the child's total score divided by the total possible score in that year (in column 1, the total possible score is 8 in every year, and in column 2 it is 10 in 2006 and 2007, and 8 otherwise). In columns 3-4, the dependent variables are z-scores equal to the child's total score minus the mean in that year, divided by the standard deviation in each year. As the results in Table A2 indicate, they are robust to various definitions of the dependent variable. Lastly, in Table A3, we re-estimate the regressions in Table 2 with the addition of age-by-year and age-by-district fixed effects. This does not substantively change the results.

3.2 ASER Exposure Results

Table 3 reports the results from regressions of exposure to NREGS at each age group on all outcome variables (equation 2). Again, exposure to NREGS has the largest negative impacts for the 13-16 year olds. Each year of exposure to NREGS during adolescence decreases school enrollment by 1.8 percentage points and math scores by about 2% of a standard deviation. For total math score, the decrease is 13% of a standard deviation per year of exposure. This implies that by age 16, children who live in districts with NREGS for their entire adolescence score 8% of a standard deviation lower on math scores, and are 8 percentage points less likely to be in school. Figure 1 shows the effect of each year of exposure from age 13-16 on school attendance and math scores.

The results are less stark for exposure from age 9 to 12. Negative magnitudes decrease and the majority of the coefficients are not statistically significant. For the younger children, the results are quite different. For each year of exposure from 2 to 4, NREGS increases math test scores by 5% of a standard deviation, and increases the likelihood that children are enrolled in school by almost 1 percentage point. This is consistent with Shah and Steinberg (forthcoming) who find that increased wages in rural India are positively associated with human capital before age 5 (when the income effect is likely to dominate), but negatively associated with human capital at older ages.

Figures 2 and 3 show the effect of exposure for each age group (2-4, 5-8, 9-12, and 13-16), estimated for all children aged 5-16, for math scores and school enrollment respectively. In these figures, though adolescent exposure to NREGS is clearly negative and decreasing, the effects at younger ages are quite different. For math scores in particular, the youngest age group (2-4) have markedly higher and increasing test scores with increased exposure to NREGS.

3.3 NSS Results

We use the NSS data to corroborate the ASER human capital results. In addition, while the ASER results are informative about the reduced form effect of the program on human capital investment, we cannot learn whether children are substituting into productive work (as hypothesized) with the ASER data, since we have no information on their other activities. In Table 4, we show estimates of the effect of NREGS on children's reported "primary activity" using the NSS data for children aged 13-17 and 5-12. We include children up the age of 17 since they become eligible for NREGS at age 18. However, we note that all results are qualitatively similar if we restrict kids to a maximum age of 16 (the max age for ASER children). Columns 1 and 2 show the estimates of NREGS on whether the child reports that his or her primary activity is productive work versus attending school. We find that children ages 13-17 are 2 percentage points less likely to report attending school, and 2 percentage points more likely to report working. This result is consistent with Islam and Sivasankaran (2014) who find an increase in time spent working outside the household for older children due to NREGS. The results for the younger children shown in Table 4 are neither economically meaningful nor statistically significant. Therefore, the NSS results corroborate the ASER results.

4 What is driving the adolescent decline in human capital?

We have shown that the introduction of NREGS caused a decrease in human capital investment amongst adolescents. In this section, we will outline the evidence for possible mechanisms. First, we will examine whether NREGS created a shift in labor demand that increased the opportunity cost of schooling. In addition, we will assess whether changes in the returns to schooling and/or decreased parental supervision could be driving the results.

Labor Demand. From previous work, we know that NREGS caused an increase in labor demand and wages in the districts in which it was operational (Imbert and Papp, 2015; Zimmermann, 2014; Azam, 2012). Though work on NREGS projects was legally limited to those over the age of 18, this could have caused an increase in labor demand for adolescents in a few ways. First, there could be leakage in who was allowed to work for the program, with either adolescents lying about their age, or program administrators looking the other way. Second, the introduction of NREGS jobs could create additional jobs, such as selling tea or food to workers. Lastly, adolescent labor could be a substitute for adult labor, so that when adults begin working for NREGS, adolescents take their place doing household, farm, and/or domestic work.[8]

While we know from Table 4 that adolescents report increases in productive work at the onset of NREGS, in order to understand what is causing this, we look more closely at the shifts in labor amongst both parents and children. In Table 5, we show estimates β_1 from Equation 1, where S is one of primary activity categories, and δ is an indicator for the rollout of NREGS, estimated separately for fathers and mothers.[9] Column 1 shows the effect of NREGS on working in a home enterprise (primarily farms), either as its head, an employer, or an unpaid family worker. We find that for fathers, this work is decreasing by 3 percentage points. Working outside the home increases for both mothers and fathers, though the increase is larger for fathers (4 percentage points versus 1.6 percentage points). Lastly, domestic work decreases for mothers by 3.3 percentage points. Overall this is consistent with mothers switching out of domestic work and into market work, either at home on the farm or outside in the market; while fathers are switching from working at home to working for wages in the market.[10]

In Table 6, we show our estimates of the impact of NREGS on primary activity, broken down by the same categories for children aged 13-17 with gender interactions.[11] While we do not observe differences in the effects of NREGS on human capital investments by gender, we do observe differences in work type by gender. The pattern for boys is similar to that of adult men: they reduce working at home (and attending school) and increase the probability of working outside the home. Thus, for boys it looks as though they are either working directly for NREGS or in market jobs as substitutes for adult labor.

[8] A related story is that when parents increase labor supply, they take their children with them to work. The children may have some marginal economic contribution, but really work is daycare (Edmonds and Pavcnik, 2005). This possibility is unlikely since the main effects are coming from adolescents who would not require daycare from their parents, and there is little effect on the youngest children, for whom this channel would likely be strongest.

[9] Male heads of household age 18-65 and female spouses of household heads aged 18-65, respectively.

[10] NSS does not directly ask households if they are working for NREGS until 2009, so we do not know whether the switch to outside work is into NREGS jobs or other paid work.

[11] In Appendix Table A7, we report these results for children age 5-12. None of the results are statistically significant and most of the coefficients are close to zero.

For girls, the results are quite different. Girls appear to be substituting almost entirely into domestic work when leaving school. Since mothers are substantially decreasing domestic work when NREGS becomes available, it is likely that these girls are substituting for their mothers' labor inside the household. We show these results graphically in Figures 4 and 5, respectively. If girls are indeed substituting for their mothers inside the home when NREGS rules out, one would expect that mothers with good substitutes at home would be more likely to take up NREGS work. In Table 7, we test this prediction directly using the NSS data. We interact the introduction of NREGS with a dummy for whether there is a teenage daughter (13-17) in the household (conditional on family size). We find that women with teenage girls in the home are almost twice as likely to begin working outside the home when NREGS rolls out than women without teenaged daughters.

Returns to Schooling. Since NREGS is a transfer program to the poor and largely uneducated, it can lower the average returns to schooling. If families are forward looking, they could reduce their schooling investment to adjust to the new, lower returns, which could explain our findings (Oster and Steinberg, 2013; Jensen, 2012). For this to explain our results, it would need to be the case that expectations were responding not to the announcement of the program, but to the actual rollout of the program (to early districts first). In addition, the increase in the agricultural wages due to NREGS is about 5 percent (Imbert and Papp, 2015; Berg et al., 2012; Azam, 2012). Returns to schooling in the developing world are estimated at 7-14% per year (Duflo, 2001; Card, 1999). If families expect the program to last for many years, it is possible that they chose to reduce their investment in human capital in expectation of lower returns. While we believe it is unlikely that parents believed the program would last for so long, we cannot rule out that the expectation of increased future wages, not just current increased wages, is driving decreases in school enrollment.[12]

[12]Since our estimates are identified off of the staged rollout, it would also have to be the case that expectations moved with the implementation of the program, rather than its announcement, since that did not vary across districts.

Parental Supervision. Another possible channel through which NREGS could impact human capital investment is if parents are integral in ensuring that their kids show up to school. If many children go from having mothers who work primarily in the home to mothers working outside the home, the lack of supervision could allow them to stay home from school without detection from their parents (Bursztyn and Coffman, 2012). In addition, perhaps now there is no one to get the child to and from school since both parents work outside the home. While it is certainly possible that some of this is going on, we think it is unlikely that this effect is driving our results. First, we primarily see the reduction in schooling for older children aged 13-17. These adolescents are almost surely not being walked to school by their mothers and could probably skip school even with mothers at home. Second, we see commensurate increases in domestic work and work outside the home on surveys administered to the head of household. That is, parents are reporting that their children are not in school, but rather engaging in productive work. This seems incongruous with the idea that children are simply sneaking around when their parents are out of the house working.

5 Threats to Identification

5.1 Pre–Trends

Our identification relies on the assumption that in the absence of NREGS, the districts that received the program earlier and those that received the program later did not have systematically different time patterns in our outcome variables. While we cannot test this assumption directly, we can test whether it appears as though our results are being driven by the program or by underlying trends in eligible districts.

To address this with the NSS data, we take two approaches. First, in Figure 6, we graph coefficients of a regression of adolescents' primary activity (attending school or working) on year dummies interacted with early access to the program (waves 1 and 2). The coefficients can be interpreted as the difference in the outcomes between early and late districts, relative to 2006 (which is normalized to zero). We see little difference in the outcomes in 2004 and

2005, and large differences in 2007 and 2008, once the program had been rolled out to all of the early districts.[13] In Table 8, we regress each of our outcomes on *future* access to the program. If the differences we see are a result of the actual rollout of the program, and not trends leading up to its rollout, there should be no effect of getting NREGS next year on working and school enrollment. Reassuringly, we see no effect of future access to the program on either outcome, and controlling for future access does not effect our main results.

For our analysis of the ASER data we cannot examine pre–trends because our data begins in 2005, one year before the program is rolled out in wave 1 districts. Thus, we have no way of assessing the differential trends in enrollment between early and late wave districts. However, we do have information on maternal education. Since mothers are an older cohort we can test whether maternal education is increasing over time differentially in early and late phase districts. If mothers' education, which should be fixed by 2005 when our analysis begins, is correlated with NREGS rollout, this would indicate that there are likely differential pre–trends in school enrollment across waves. The results in Table 9 show that the effect of NREGS on maternal education is small, positive and insignificant, suggesting that our results are not likely biased by differential pre–trends.

In addition, since we have five years of ASER data and three separate roll–out groups, we can re–estimate the main analysis from Table 2 including wave specific linear time trends. If our results are picking up gradual differences in outcomes over the five year period, then the waves trends should absorb this effect, and the coefficients should go to zero. However, if the impact is occurring due to the change from NREGS, the coefficients should remain the same. In Table A4, we show estimates of our coefficients from Table 2 with the addition of wave-specific linear time trends. Reassuringly, the coefficients are all still negative and similar in magnitude, though some are less precisely estimated. Lastly in Table A5 we re-estimate equation 1 (as in Table 2), including controls for rainfall this year and last year,

[13]Because the NSS round 62 surveyed until June 2006, some of the early districts will have had access to the program for a month or two by the time the survey was conducted. However, the 2007 data begins in July, so all of the early districts will have had NREGS for at least two months by the time the survey rolls out.

since this can affect both wages and school enrollment. The coefficients in this estimation are very similar to those in Table 2.

5.2 Endogenous Roll Out

Early access to the NREGS program was not allocated randomly; the government explicitly set out to roll out the program to the neediest districts first. Each state was allocated a certain number of slots in each wave of the program, and was allowed to allocate those slots as it saw fit, though the process was supposed to be guided by the official backwardness rankings, based on agricultural productivity, percent scheduled caste and scheduled tribe, and agricultural wages (Zimmermann, 2014). The algorithm used to decide which districts actually received early access was not disclosed, and there was likely some discretion on the part of government officials.

If the endogenous choice of early districts was correlated with the levels of human capital investment, then district fixed effects would be enough to ensure that our results were not biased. If, however unlikely, state officials chose earlier districts based on the trend in educational investments and child labor, this could bias our results upward.

To test for this, we follow a simplified version of the methodology in Zimmermann (2014), in which we assign treatment status based solely on backwardness rankings from Planning Commission (2003) and the number of NREGS slots allocated to each state for each wave. That is, if Andhra Pradesh was allocated thirteen wave 1 slots, six wave 2 slots, and two wave 3 slots, we assign NREGS to the 13 most backward ranked districts in 2006, the next 6 lowest ranked districts in 2007, and the rest in 2008, regardless of when the district actually got access to NREGS. This can be thought of as an “intent to treat” estimate since some districts who would have received NREGS under this simpler assignment mechanism did not, presumably for political reasons.

Table A6 shows the results for our main specification using this alternative measure of treatment for the full sample (Panel A), the adolescents (Panel B), and the younger children

(Panel C). The sample sizes in Table A6 are smaller as the Planning Commission only ranked 447 districts. Therefore, districts which are not ranked are not included in the analysis. The results in Table A6 are similar to the main results in Table 2. The coefficients are similar in magnitude to our main results, and generally not statistically distinguishable from our main estimates in Table 2.

5.3 Selective Migration

If NREGS affected the migration patterns of adolescents, this could affect the sample of children who are available to be surveyed by both the ASER and the NSS.

In general, migration rates in rural India are extremely low (Munshi and Rosenzweig, 2009; Topalova, 2005). In particular, migration between early and late NREGS districts is not a major concern since Indian Census data from 2001 estimates that rural interdistrict migration for employment was limited to 0.4% of all adults 18-60. However, there is evidence that NREGS might decrease short-term migration from rural to urban areas for work (Imbert and Papp, 2014). This could bias the sample of children observed when enumerators survey the village and affect our analysis of test scores and school enrollment. It is important to note that to the extent that migrants are positively selected, we would expect this to bias our test score results downward, since these children would be more likely to show up in the ASER sample once NREGS has rolled out.

In addition, temporary migration for work is limited almost entirely to males in India. Women tend to stay in their parents' home until marriage, when they move to the home of their husband's family. Therefore, if migration is driving our results (presumably through negative selection of migrants), we should expect to observe this effect only in boys. However, our results show that adolescent girls experience similar reduction in test scores, enrollment rates, and grade, as well as commensurate increases in productive work.

Perhaps most importantly, our results are simply too large to be explained by migration. Imbert and Papp (2014) finds that NREGS decreased migration from rural to urban areas by .5 percentage points among adults in the NSS. Using the same data, we show a decrease in enrollment of 2.4 percentage points due to NREGS. Assuming that the migration rate is the same among adolescents, and that all of the temporary migrants would otherwise be in school, this could explain at most 20% of the decrease in enrollment we find in the NSS sample. In short, even under the most generous assumptions, changes in migration are simply not large enough to explain the decreases in school enrollment found in this paper.

6 Discussion and Conclusion

This paper examines the effect of NREGS, a large workfare program in India, on school enrollment, test scores, and child labor. We show that NREGS decreased human capital investment, primarily for children over the age of twelve, and that this was likely caused by boys responding to the increase in labor demand by working outside the home, and girls substituting for their mothers in domestic work. Each year of exposure to NREGS decreases school enrollment by 2 percentage points and math scores by 2% of a standard deviation amongst children aged 13-16. Our estimates suggest that NREGS may have caused anywhere from 700,000 to 2 million adolescents to leave school prematurely.[14] These results are consistent with earlier findings on the effect of wages on human capital investment in India (Shah and Steinberg, forthcoming), though the current results may be of more interest to policy makers, since the wage increase is being caused by a government anti-poverty effort.

It is worth noting that NREGS was designed with the intent to both lower poverty and increase female empowerment by increasing women's labor force participation and earnings potential. These results suggest, however, that it could be unintentionally decreasing the future earnings potential of some of its beneficiaries by inducing them to drop out of school earlier than they otherwise would have. This is especially true for girls, who, rather than gaining market experience and their own earnings like their male counterparts are substitut-

[14] Numbers are based on estimates of decreased enrollment from 1-2.2 percentage points, and the total population of rural Indians between the age of 13-16 (for ASER estimates) and 13-17 (for NSS estimates) from the 2011 Census of India.

ing for their mothers at home. Based on our estimates, for every 20 women induced into the labor force by NREGS, between 1.2 and 4 adolescent girls may have dropped out of school, nearly all of them to go into full-time domestic work in their parents' homes.[15]

It is important to note that this analysis represents the effect of NREGS on one particular outcome that may be of interest to policy makers (human capital investment). While we would argue that this is quite an important outcome for economic growth, we are not measuring any of the potential benefits that the program provides in terms of consumption, protection against income shocks, or any number of other outcomes. Thus, we are not in a position to measure the overall welfare impact of this particular anti-poverty program.

Rather, the takeaway from these results is that social programs have price effects, and that these price effects can have very real consequences. If workfare programs raise prevailing wages and cause older students to substitute toward work and away from school, lump sum grants or conditional cash transfers might be alternative options to consider.[16] Ultimately, it is important to understand the price effects so that social programs can be designed in order to maximize their potential to increase economic growth and alleviate poverty.

[15]We note there may be significant heterogeneity across India and that we are reporting all India averages. For example, in the state of Andhra Pradesh, Afridi et al. (2016) find that mother's participation in the labor force due to NREGS results in almost two additional months of attendance in a school year by her children and reduces the gap between a child's actual and ideal grade by more than a quarter.

[16]In a recent working paper, Alik-Lagrange and Ravallion (2015) shows that a basic income guarantee dominates net workfare earnings in terms of the impact on poverty for a given budgetary outlay, once expected welfare losses from work requirements are incorporated into the model.

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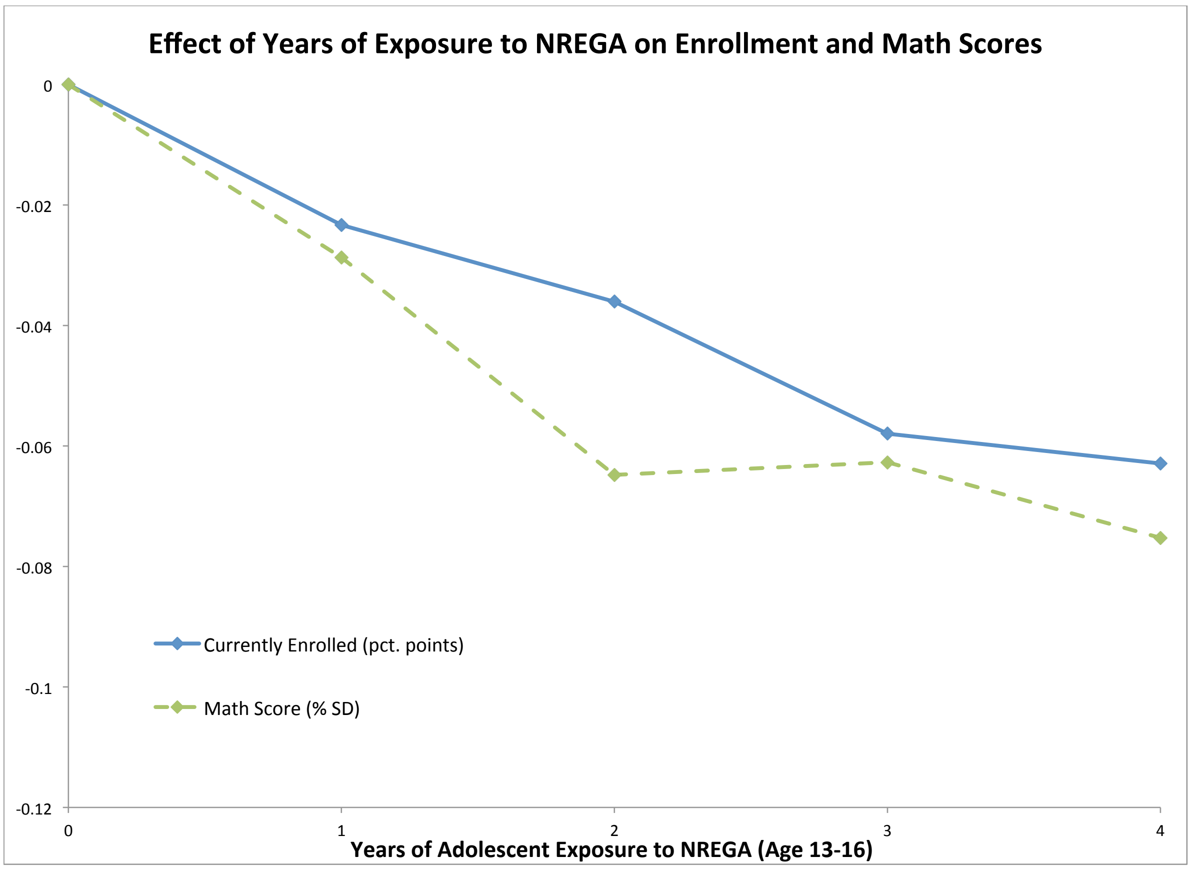
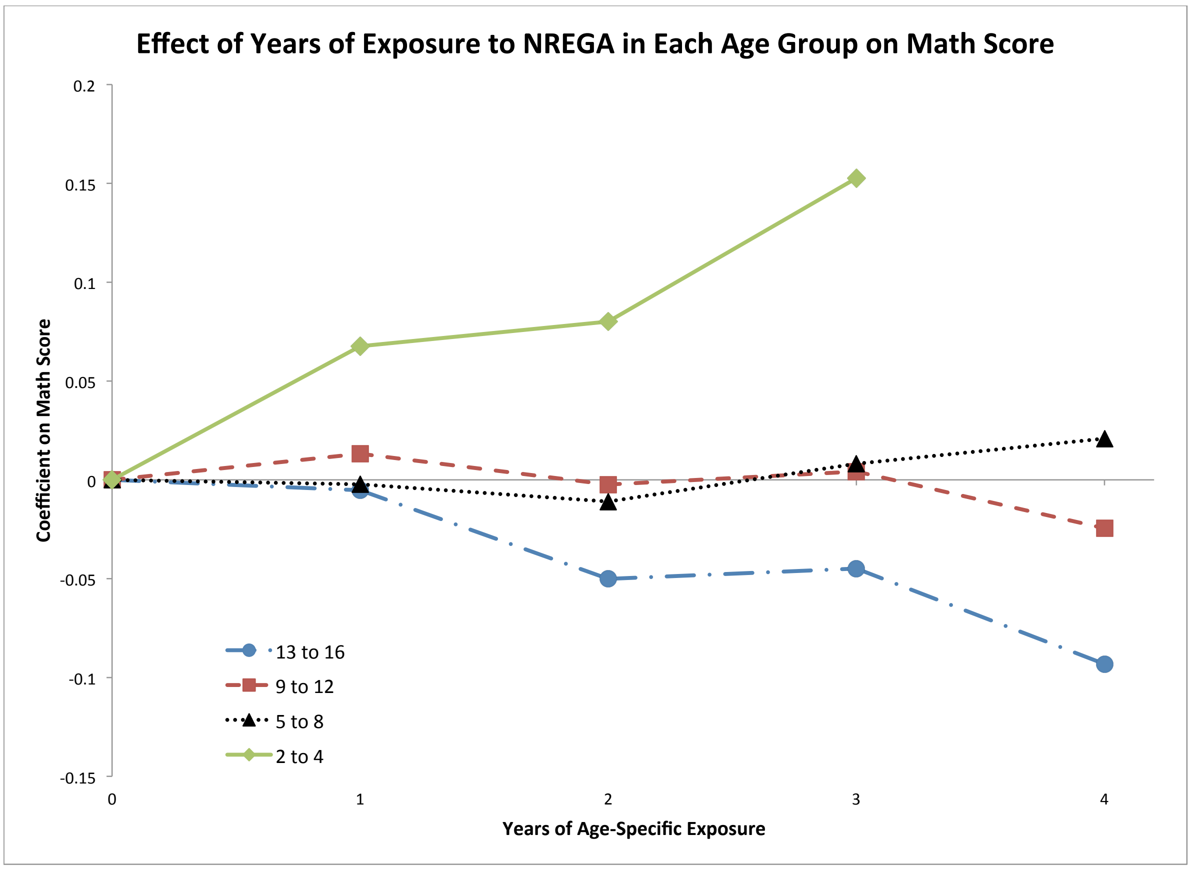


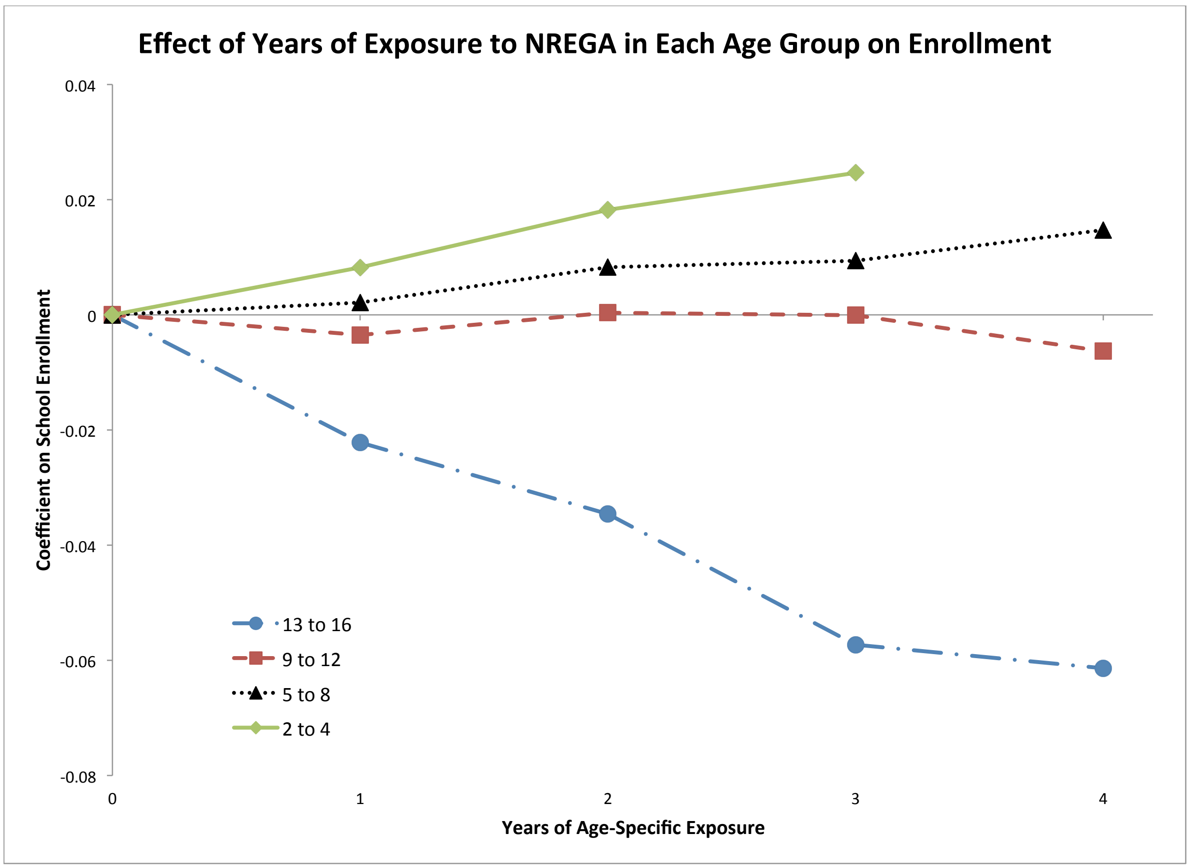
Source: ASER (2005-2009)
Notes: This figure shows estimates of β_1 for the effect of each year of exposure to NREGS on math score in the ASER data, for children aged 13-16. The estimating equation is similar to Equation (1) where δ is years of exposure to NREGS between the ages of 13 and 16, where "exposure" is defined as a year in which NREGS has rolled out to the child's district, and the child is between 13 and 16. All regressions contain fixed effects for child age, year of birth, year of survey, and district.

Figure 1: Effect of Years of Adolescent Exposure on Math Test Scores and School Enrollment



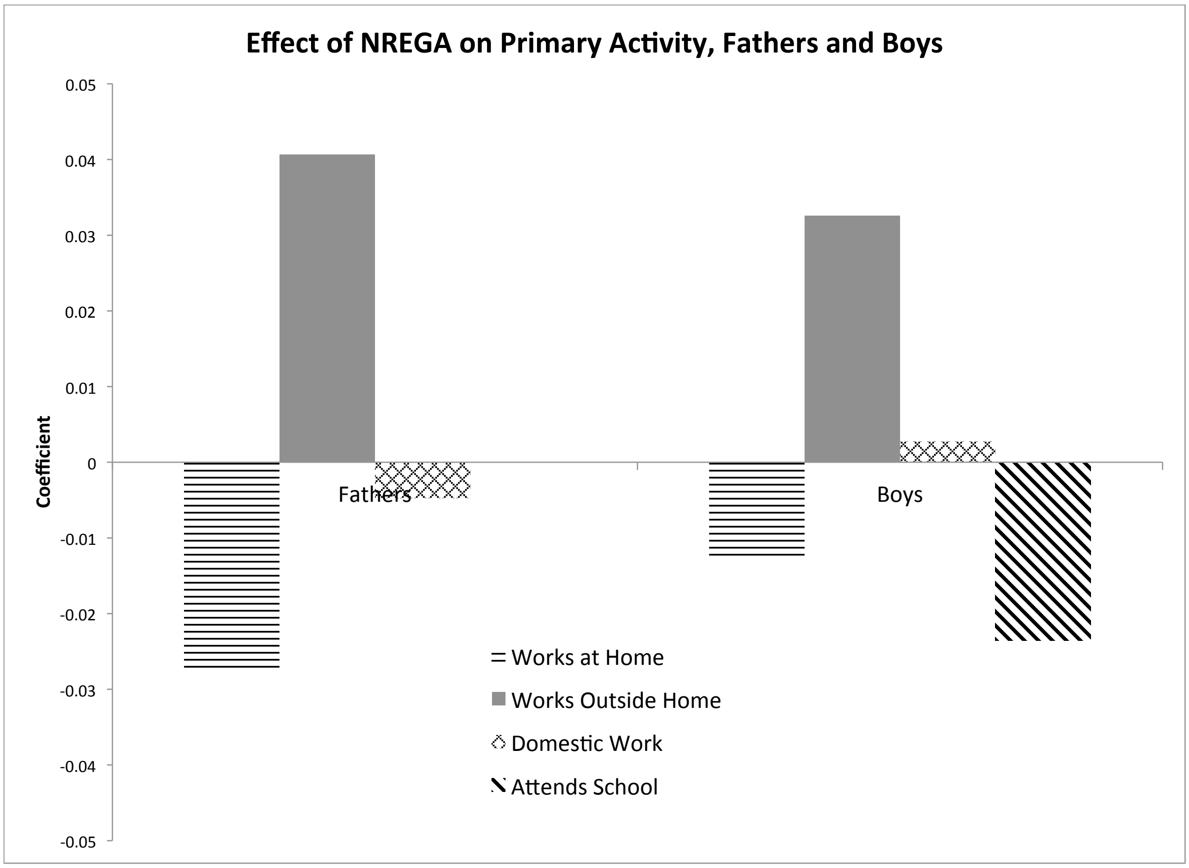
Source: ASER (2005-2009)
Notes: This figure shows estimates of β_1 for the effect of each year of exposure to NREGS on math score in the ASER data, for children aged 5-16. The estimating equation is similar to Equation (1) where δ is a vector for each age category of years of exposure to NREGS, where "exposure" is defined as a year in which NREGS has rolled out to the child's district, and the child is in the given age group. All regressions contain fixed effects for child age, year of birth, year of survey, and district.

Figure 2: Effect of Years of Exposure of NREGS by Age on Math Test Scores



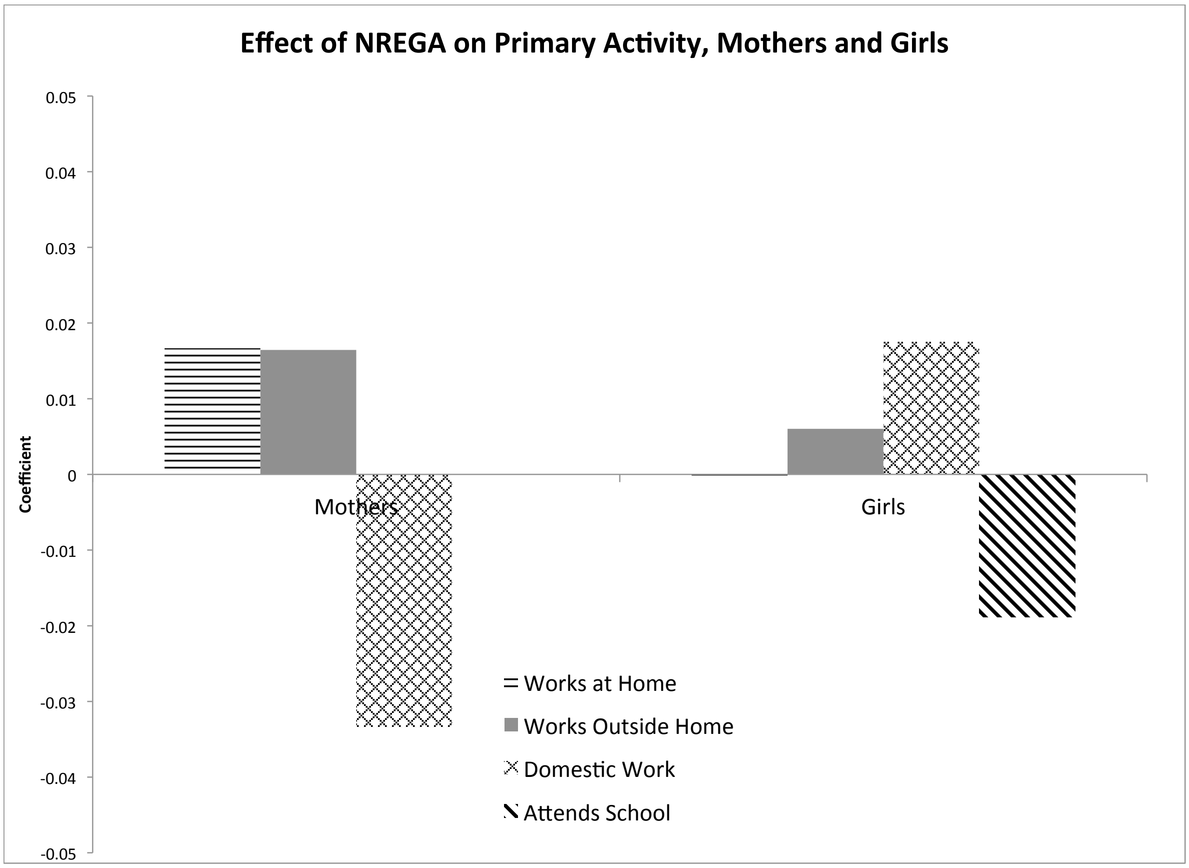
Source: ASER (2005-2009)
Notes: This figure shows estimates of β_1 for the effect of each year of exposure to NREGS on school enrollment in the ASER data, for children aged 5-16. The estimating equation is similar to Equation (1) where δ is a vector for each age category of years of exposure to NREGS, where "exposure" is defined as a year in which NREGS has rolled out to the child's district, and the child is in the given age group. All regressions contain fixed effects for child age, year of birth, year of survey, and district.

Figure 3: Effect of Years of Exposure of NREGS by Age on School Enrollment



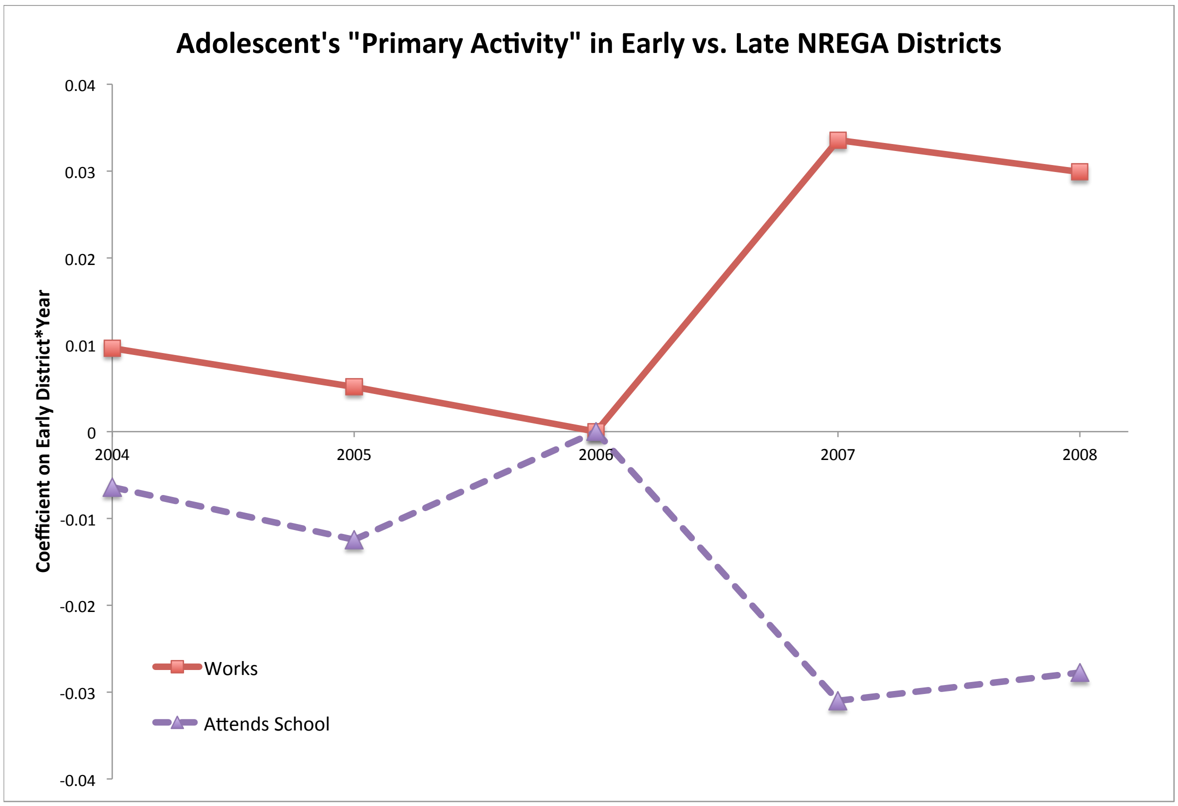
Source: NSS 2003-2009 (rounds 60, 61, 62, 64, 66)
Notes: This figure shows estimates of the coefficients from Table 5 (Panel B) and Table 6 represented graphically. The coefficients are estimates of β_1 from the OLS estimation of Equation (1) where S is a dummy for each specified primary activity (market work at home, market work outside the home, domestic work, or attends school), and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. All regressions contain fixed effects for district and year. "Fathers" are defined as males between the age of 18 and 65 who report their household status as "head of household." "Boys" are defined as males between the ages of 13 and 17.

Figure 4: Effect of NREGS on Primary Activity, Dads and Boys



Source: NSS 2003-2009 (rounds 60, 61, 62, 64, 66)
Notes: This figure shows the coefficients from Table 5 (Panel A) and Table 6 represented graphically. The coefficients are estimates of β_1 from the OLS estimation of Equation (1) where S is a dummy for each specified primary activity (market work at home, market work outside the home, domestic work, or attends school), and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. All regressions contain fixed effects for district and year. "Mothers" are defined as females between the age of 18 and 65 who report their household status as "spouse of head of household". "Girls" are defined as females between the ages of 13 and 17.

Figure 5: Effect of NREGS on Primary Activity, Moms and Girls



Source: NSS 2003-2009 (rounds 60, 61, 62, 64)
Notes: This figure shows coefficients from two separate regressions of outcomes on year*early NREGS dummies, where outcomes are an adolescent listing "attends school" (squares) or work (either outside or inside the home) (triangles) as his or her primary activity. The coefficients can be interpreted as the difference in each outcome between early and late NREGS districts, conditional on a district fixed effect, in each year. 2006 is normalized to zero. "Early" districts are defined as those who got the program in wave 1 (Spring 2006) or wave 2 (Spring 2007). The data from 2006 comes from the 2005-2006 round of the NSS (62), and is collected before the NREGS rollout to most early districts. The data from 2007 is from the 2007-2008 NSS wave (64), and is entirely collected after the rollout of NREGS to all early districts. Both regressions contain fixed effects for district, year, age, sex, and survey month.

Figure 6: Primary Activity of Adolescents in Early vs. Late Districts, by Year

Table 1: Summary Statistics

ASER Summary Statistics (ages 5-16)			
	Mean	Std. Dev.	Observations
Age	10.20	3.17	2,790,804
Female	.45	.50	2,790,804
Math Score	2.61	1.30	2,592,076
Total Math Score (with Word Problems)	3.01	1.77	2,593,105
Reading Score	2.71	1.41	2,606,483
Currently Enrolled	.94	.25	2,691,590
Highest Grade Completed	4.59	2.84	2,507,599
ASER Summary Statistics (ages 13-16)			
	Mean	Std. Dev.	Observations
Age	14.3	1.08	743,341
Female	.45	.50	743,341
Math Score	3.43	.98	698,584
Total Math Score (with Word Problems)	4.12	1.54	699,100
Reading Score	3.56	.980	700,835
Currently Enrolled	.87	.34	742,802
Highest Grade Completed	7.65	2.54	655,802
NSS Sample (ages 13-17)			
	Mean	Std. Dev.	Observations
Age	14.97	1.36	147,110
Female	.47	.49	147,110
Primary Activity:			
Attends School	.70	.46	147,110
Works at Home	.08	.27	147,110
Works outside Home	.07	.26	147,110
Domestic Work	.11	.31	147,110

Source: ASER (2005-2009) and NSS (rounds 60, 61, 62, 64, and 66).
Notes: This table contains summary statistics for the outcome and control variables in this paper.

Table 2: Effect of NREGS on Test Scores and Schooling

Dependent Variable:	Math Score	Total Math Score	Reading Score	Currently Enrolled	Highest Grade
			Panel A: Full Sample		
NREGS	-.00093	-.085	.0026	-.0066	-.049
	(.019)	(.028)***	(.019)	(.0028)**	(.021)**
Observations	2,592,076	2,592,076	2,606,483	2,691,590	2,507,599
Mean DV	2.61	3.02	2.71	.94	4.60
			Panel B: Ages 13-16		
NREGS	-.026	-.131	-.036	-.010	-.111
	(.019)	(.032)***	(.019)*	(.0049)**	(.041)***
Observations	698,584	698,584	700,835	742,097	655,802
Mean DV	3.43	4.12	3.57	.87	7.66
			Panel C: Ages 5-12		
NREGS	.0059	-.049	.015	-.0046	-.021
	(.020)	(.029)*	(.021)	(.0025)*	(.017)
Observations	1,893,492	1,893,492	1,905,648	1,949,493	1,851,797
Mean DV	2.32	2.61	2.40	.96	3.51
District FEs	YES	YES	YES	YES	YES
Year FEs	YES	YES	YES	YES	YES
Age FEs	YES	YES	YES	YES	YES

Source: ASER (2005-2009)
Notes: This table reports estimates of β_1, the effect of NREGS on math and reading test scores and schooling outcomes from the ASER data. The coefficients are estimates of β_1 from the OLS estimation of Equation (1) where S is the listed dependent variable, and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. Math score ranges from 0-4, total math score ranges from 0-6, and read score ranges from 0-4. All panels include year, child age, and district fixed effects. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 3: Effect of NREGS on Test Scores and Schooling, by Years of Exposure

Dependent Variable:	Math Score	Total Math Score	Reading Score	Currently Enrolled	Highest Grade
Exposure (ages 13-16)	-.022	-.19	.0087	-.018	-.311
	(.010)**	(.015)***	(.012)	(.0032)***	(.030)***
Exposure (ages 9-12)	-.0038	-.021	.0035	.00049	-.047
	(.0082)	(.012)*	(.0083)	(.0011)	(.0094)***
Exposure (ages 5-8)	-.00098	.011	-.0094	.0042	.032
	(.0079)	(.011)	(.0088)	(.0012)***	(.014)**
Exposure (ages 2-4)	.047	-.00040	.041	.0089	.068
	(.014)***	(.016)	(.014)***	(.0020)***	(.021)***
Observations	2,592,076	2,592,076	2,606,483	2,691,590	2,507,599
Mean DV	2.61	3.02	2.71	.94	4.60
District FEs	YES	YES	YES	YES	YES
Year FEs	YES	YES	YES	YES	YES
Age FEs	YES	YES	YES	YES	YES
Year of Birth FEs	YES	YES	YES	YES	YES

Source: ASER (2005-2009)
Notes: This table shows estimates of the years of exposure of NREGS on math and reading test scores and schooling outcomes from the ASER data. The coefficients are estimates of β_2 from the OLS estimation of Equation (2) where S is the listed dependent variable, and δ is a vector for each age group of years of exposure to NREGS and ranges from 0-4 (or 0-3 for exposure ages 2-4). Math score ranges from 0-4, total math score ranges from 0-6, and read score ranges from 0-4. All panels include child age, year of birth, year child age, and district fixed effects. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 4: Effect of NREGS on Working and School Attendance

	Ages 13-17		Ages 5-12	
Dependent Variable:	Works	Attends School	Works	Attends School
NREGS	.024	-.022	.0020	-.00014
	(.0072)***	(.0074)***	(.0028)	(.0059)
Observations	147,110	147,110	255,273	255,273
Mean of DV	.25	.70	.03	.85
District FEs	YES	YES	YES	YES
Year FEs	YES	YES	YES	YES
Age FEs	YES	YES	YES	YES

Source: NSS 2003-2009 (rounds 60, 61, 62, 64, 66)
Notes: This table reports estimates of β_1, the effect of NREGS on children who report their “primary activity” as work or school attendance. The coefficients are from the OLS estimation of Equation (1) where S is a dummy for each specified primary activity and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. Standard errors clustered at the district are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 5: Effect of NREGS on Parents' Primary Activities

Panel A: Mothers			
Dependent Variable:	Works at Home	Works Outside Home	Domestic Work
NREGS	.017	.016	-.033
	(.0084)**	(.0062)***	(.010)***
Observations	229,094	229,094	229,094
Mean of DV	.24	.16	.58
Panel B: Fathers			
Dependent Variable:	Works at Home	Works Outside Home	Domestic Work
NREGS	-.027	.041	-.0047
	(.0093)***	(.0096)***	(.0037)
Observations	235,417	235,417	235,417
Mean of DV	.53	.40	.02
District FEs	YES	YES	YES
Year FEs	YES	YES	YES
Age FEs	YES	YES	YES

Source: NSS 2003-2009 (rounds 60, 61, 62, 64, 66)
Notes: This table reports estimates of β_1, the effect of NREGS on primary activities of parents using the NSS 2003-2009 (rounds 60, 61, 62, 64, 66). The coefficients are from the OLS estimation of Equation (1) where S is a dummy for each specified primary activity (market work at home, market work outside the home, or domestic work), and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. Panel A restricts to "mothers," defined as female spouses of heads of household, ages 18-64. Panel B restricts to "fathers", defined as male heads of household ages 18-64. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 6: Children's Primary Activity (Ages 13-17)

Dependent Variable:	Works at Home	Works Outside Home	Domestic Work	Attends School
NREGS	-.013	.033	.0028	-.024
	(.0061)**	(.0065)***	(.0026)	(.0089)***
NREGS X Female	.013	-.027	.015	.0047
	(.0070)*	(.0072)***	(.0082)*	(.011)
Observations	147,110	147,110	147,110	147,110
Mean of DV (Boys)	.08	.08	.01	.77
Mean of DV (Girls)	.05	.04	.19	.69
District, Year, and Age	YES	YES	YES	YES
District X Female FEs	YES	YES	YES	YES
Year X Female FEs	YES	YES	YES	YES
Age X Female FEs	YES	YES	YES	YES

Source: NSS 2003-2009 (rounds 60, 61, 62, 64, 66)
Notes: This table reports estimates of β_1, the effect of NREGS on primary activities of adolescents using the NSS. The coefficients are from the OLS estimation of Equation (1) where S is a dummy for each specified primary activity (market work at home, market work outside the home, domestic work, or school attendance), and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 7: Mother's and their Teenage Daughters

Dependent Variable:	Works Outside Home	Works Outside Home
NREGS	.01144	.01146
	(.00625)*	(.00625)*
Has Teenage Daughter	.01547	-.01739
	(.00278)***	(.02819)
NREGS X Has Teenage Daughter	.00742	.0075
	(.00408)*	(.00404)*
Household Size	-.00722	-.00725
	(.00044)***	(.00045)***
Observations	229,094	229,094
Mean of DV	.14	.14
District, Year, and Age FEs	YES	YES
District X Teenage Daughter FE	No	YES
Age X Teenage Daughter FE	No	YES

Source: NSS 2003-2009 (rounds 60, 61, 62, 64, 66)
Notes: This table estimates whether mothers with teenage daughters are more likely to work outside the home once NREGS rolls out using OLS estimation. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 8: Placebo Test: Effect of Future NREGS on Work and School Enrollment

Dependent Variable:	Works (Ages 13-17)			Attends School (Ages 13-17)		
	(1)	(2)	(3)	(4)	(5)	(6)
NREGS	.02581 (.00792)***			-.02333 (.00819)***		
NREGS next year	-.00603 (.00667)	-.00587 (.00654)		.0037 (.00703)	.00349 (.00694)	
NREGS in two years	.00571 (.00652)		-.00415 (.00594)	-.00558 (.00674)		.00312 (.00618)
p(NREGS = NREGS next year)	.0020			.0145		
p(NREGS = NREGS in two years)	.0114			.0316		
Observations	147,110	147,110	147,110	147,110	147,110	147,110
Mean DV	.25	.25	.25	.70	.70	.70
District FEs	YES	YES	YES	YES	YES	YES
Year FEs	YES	YES	YES	YES	YES	YES
Age FEs	YES	YES	YES	YES	YES	YES

Source: NSS 2003-2009 (rounds 60, 61, 62, 64, 66)
Notes: This table shows estimates of the effect of future access to the NREGS program on adolescents who report their "primary activity" as work or school attendance. The coefficients are from the OLS estimation of Equation (1) where S is a dummy for each specified primary activity and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year, whether it will roll out next year, and whether it will roll out in two years. In columns (1) and (4), all three independent variables are included simultaneously, while in columns (2) and (5), only the depended variable "NREGS next year" is included, and in columns (3) and (6), only "NREGS in two years" is included. Standard errors clustered at the district are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 9: Placebo Test: Effect of NREGS on Mother's Schooling

Dependent Variable:	Mother Attended School
NREGS	.0098 (.0072)
Observations	2,332,955
Mean of DV	.45
District FEs	YES
Year FEs	YES
Age FEs	YES

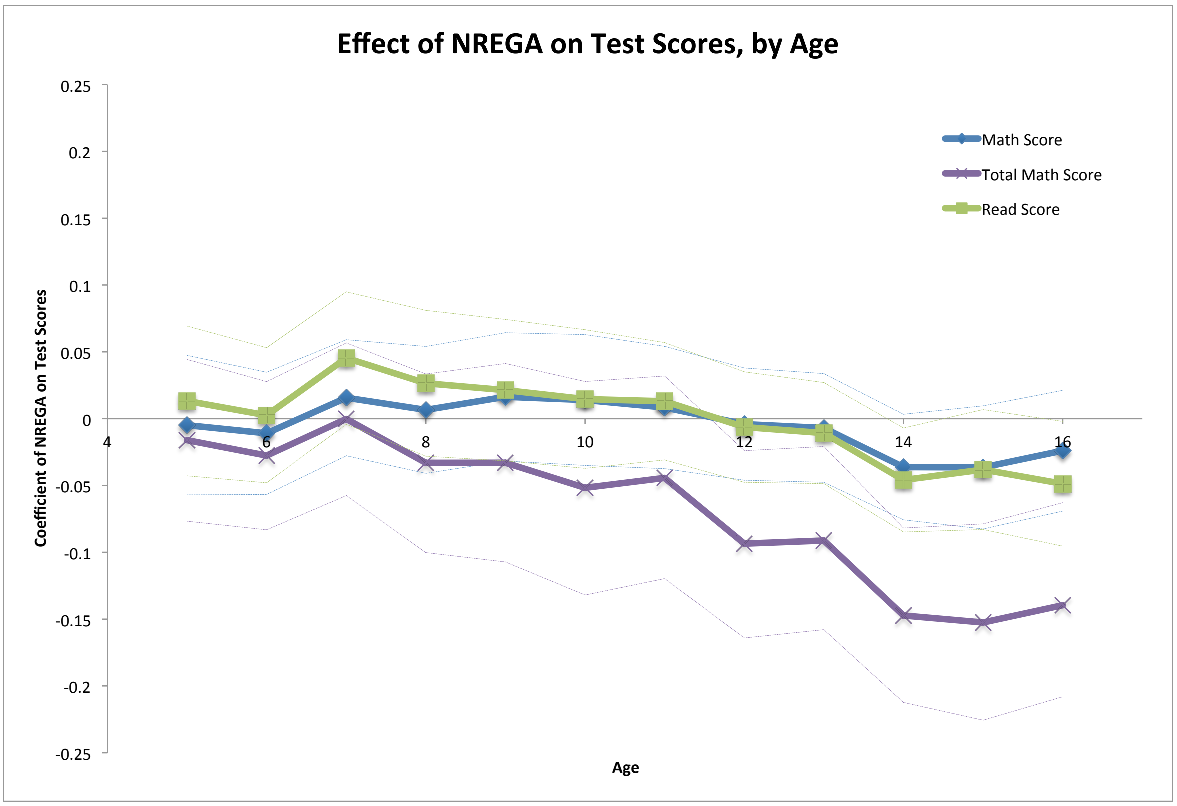
Source: ASER 2005-2009
Notes: This table shows estimates of the effect of NREGS on maternal education using ASER data from 2005-2009. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

A Appendix Tables and Figures

Table A1: Effect of NREGS on Test Scores and Schooling By Gender

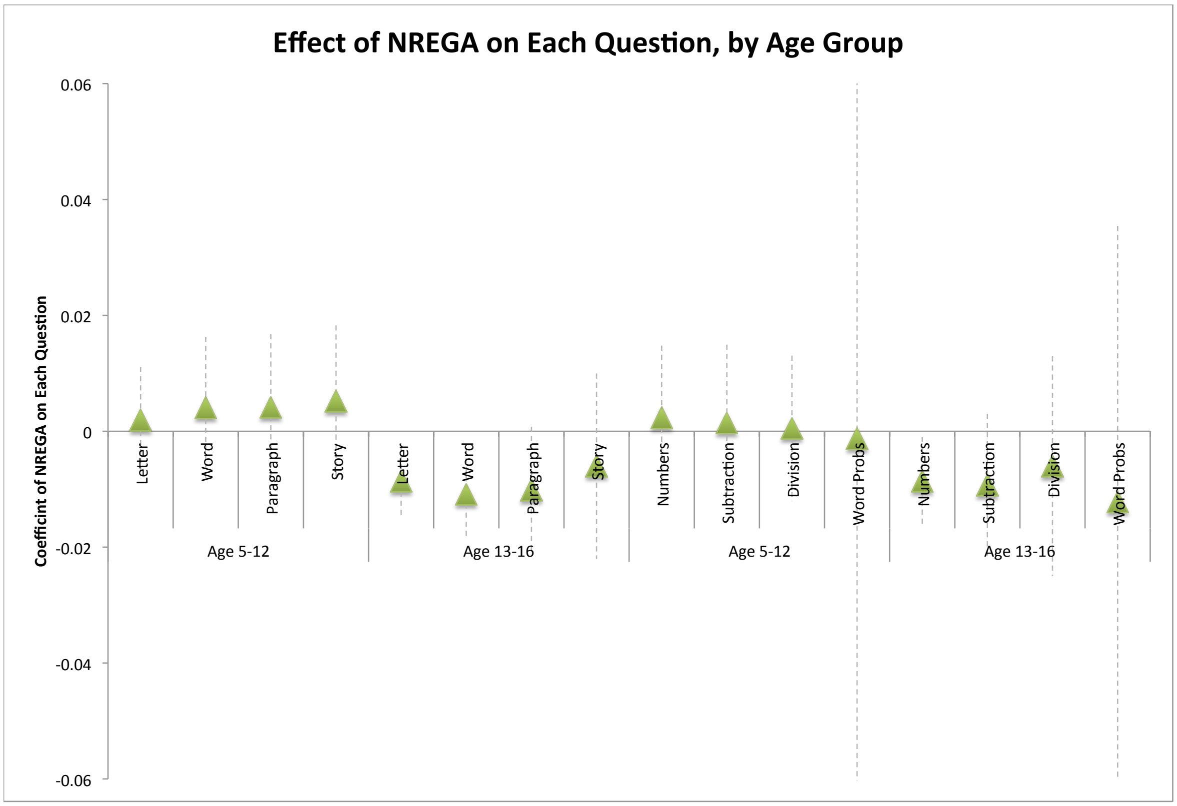
Dependent Variable:	Math Score	Total Math Score	Reading Score	Currently Enrolled	Highest Grade
	Panel A: Ages 13-16				
NREGS	-.021	-.110	-.030	-.0094	-.107
	(.018)	(.032)***	(.018)	(.0050)*	(.042)**
NREGS X Female	-.0073	-.042	-.0088	.00017	-.0017
	(.013)	(.019)**	(.012)	(.0038)	(.029)
Observations	698,584	698,584	700,835	742,097	655,802
DV (Boys)	3.48	4.20	3.60	0.88	7.71
Mean DV (Girls)	3.37	4.03	3.53	0.86	7.70
	Panel B: Ages 5-12				
NREGS	.0087	-.037	.015	-.0040	-.015
	(.020)	(.029)	(.021)	(.0024)*	(.017)
NREGS X Female	-.0048	-.026	.0012	-.0012	-.012
	(.0081)	(.012)**	(.0089)	(.0015)	(.0095)
Observations	1,893,492	1,893,492	1,905,648	1,949,493	1,851,797
Mean DV (Boys)	2.35	2.65	2.42	0.97	3.52
Mean DV (Girls)	2.28	2.57	2.37	0.96	3.50
District, Age, and Year FEs	YES	YES	YES	YES	YES
Year X Female FEs	YES	YES	YES	YES	YES
District X Female FEs	YES	YES	YES	YES	YES
Age X Female FEs	YES	YES	YES	YES	YES

Source: ASER (2005-2009)
Notes: This table reports estimates of β_1, the effect of NREGS on math and reading test scores and schooling outcomes from the ASER data. The coefficients are estimates of β_1 from the OLS estimation of Equation (1) where S is the listed dependent variable, and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. Math score ranges from 0-4, total math score ranges from 0-6, and read score ranges from 0-4. All panels include year child age, and district fixed effects. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.



Source: ASER (2005-2009)
Notes: This figure shows estimates of β_1 for the effect of NREGS on math score, read score, and total math score for children of each age. The coefficients are estimates of β_1 from the OLS estimation of Equation (1) where S is the listed dependent variable, and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. Each dot represents a separate regression run only on children of a given age. All regressions contain fixed effects for year of survey and district.

Figure A1: Effect of NREGS on Test Scores, by Age



Source: ASER (2005-2009)
Notes: This figure shows estimates of β_1 for the effect of NREGS on each math and reading question separately for children of each age group. The coefficients are estimates of β_1 from the OLS estimation of Equation (1) where S is the listed dependent variable, and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. 95% confidence intervals are represented by the vertical dashed lines. All regressions contain fixed effects for child age, year of survey, and district.

Figure A2: Effect of NREGS on Each Test Question

Table A2: Effect of NREGS on Test Scores: Alternative Measures

Dependent Variable:	Index (0-1)		Z-Score	
	Math and Read Only	All Questions	Math and Read Only	All Questions
	Panel A: Full Sample			
NREGS	.00019	-.00065	-.0009	.00013
	(.0046)	(.0048)	(.014)	(.014)
Observations	2,581,646	2,581,646	2,581,646	2,582,675
Mean DV	.874	.860	.674	
	Panel B: Ages 13-16			
NREGS	-.0078	-.012	-.025	-.034
	(.0046)*	(.0050)**	(.014)*	(.015)**
Observations	696291	696,291	696,291	696,807
Mean DV	.874	.860	.674	
	Panel C: Ages 5-12			
NREGS	.0026	.0036	.0069	.013
	(.0051)	(.0051)	(.016)	(.015)
Observations	1,885,355	1,885,355	1,885,355	1,885,868
Mean DV	.874	.860	.674	
District FEs	YES	YES	YES	YES
Year FEs	YES	YES	YES	YES
Age FEs	YES	YES	YES	YES

Source: ASER (2005-2009)
Notes: This table reports estimates of β_1, the effect of NREGS on math and reading test scores and schooling outcomes from the ASER data. The coefficients are estimates of β_1 from the OLS estimation of Equation (1) where S is the listed dependent variable, and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. Index ranges from 0-1 and is equal to the child's total score divided by the total possible score in that year (in column 1, the total possible score is 8 in every year, and in column 2 it is 10 in 2006 & 2007, and 8 otherwise). Z-scores are equal to the child's total score minus the mean in that year, divided by the standard deviation in each year. All panels include year, child age, and district fixed effects. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A3: Effect of NREGS on Test Scores and Schooling, with Age-by-Year and Age-by District FEs

Dependent Variable:	Math Score	Total Math Score	Reading Score	Currently Enrolled	Highest Grade
			Panel A: Full Sample		
NREGS	-.0026	-.065	.0025	-.0057	-.042
	(.019)	(.028)**	(.019)	(.0028)**	(.020)**
Observations	2,592,076	2,592,076	2,606,483	2,691,590	2,507,599
Mean DV	2.61	3.02	2.71	.94	4.60
			Panel B: Ages 13-16		
NREGS	-.025	-.13	-.034	-.0086	-.093
	(.019)	(.032)***	(.019)*	(.0049)*	(.041)**
Observations	698,584	698,584	700,835	742,097	655,802
Mean DV	3.43	4.12	3.57	.87	7.66
			Panel C: Ages 5-12		
NREGS	.0057	-.041	.016	-.0046	-.024
	(.02)	(.029)	(.021)	(.0025)*	(.016)
Observations	1,893,492	1,893,492	1,905,648	1,949,493	1,851,797
Mean DV	2.32	2.61	2.40	.96	3.51
District, Age, and Year FEs	YES	YES	YES	YES	YES
Age X District FEs	YES	YES	YES	YES	YES
Age X Year FEs	YES	YES	YES	YES	YES

Source: ASER (2005-2009)

Notes: This table reports estimates of β_1, the effect of NREGS on math and reading test scores and schooling outcomes from the ASER data. The coefficients are estimates of β_1 from the OLS estimation of Equation (1) where S is the listed dependent variable, and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. Math score ranges from 0-4, total math score ranges from 0-6, and read score ranges from 0-4. All panels include age-by-year and age-by-district fixed effects. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A4: Robustness: Effect of NREGS on Test Scores and Schooling, with Trends

Dependent Variable:	Math Score	Total Math Score	Reading Score	Currently Enrolled	Highest Grade
			Panel A: Full Sample		
NREGS	-.0095	-.083	.0060	-.0039	-.050
	(.020)	(.029)***	(.021)	(.0027)	(.022)**
Observations	2,592,076	2,592,076	2,606,483	2,691,590	2,507,599
Mean DV	2.61	3.02	2.71	.94	4.60
			Panel B: Ages 13-16		
NREGS	-.018	-.101	-.019	-.0054	-.099
	(.020)	(.031)***	(.019)	(.0050)	(.052)*
Observations	698,584	698,584	700,835	742,097	655,802
Mean DV	3.43	4.12	3.57	.87	7.66
			Panel C: Ages 5-12		
NREGS	-.0001	-.054	.015	-.0015	-.011
	(.021)	(.031)*	(.023)	(.0023)	(.018)
Observations	1,893,492	1,893,492	1,905,648	1,949,493	1,851,797
Mean DV	2.32	2.62	2.40	.96	3.51
District FEs	YES	YES	YES	YES	YES
Age FEs	YES	YES	YES	YES	YES
Year FEs	YES	YES	YES	YES	YES
Wave*Year Trends	YES	YES	YES	YES	YES

Source: Source: ASER 2005-2009
Notes: This table reports estimates of β_1, the effect of NREGS on math and reading test scores and schooling outcomes from the ASER data. Math score ranges from 0-4, total math score ranges from 0-6, and read score ranges from 0-4. All panels include year, child age, and district fixed effects and wave*year fixed effects. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A5: Robustness: Effect of NREGS on Test Scores and Schooling, with Rainfall Controls

Dependent Variable:	Math Score	Total Math Score	Reading Score	Currently Enrolled	Highest Grade
			Panel A: Full Sample		
NREGS	-.0012	-.0761	.0020	-.0052	-.063
	(.019)	(.029)***	(.020)	(.0028)*	(.022)***
Observations	2,032,679	2,032,679	2,043,441	2,118,505	1,968,387
Mean DV	2.61	3.02	2.71	.94	4.60
			Panel B: Ages 13-16		
NREGS	-.016	-.11	-.026	-.0066	-.120
	(.020)	(.033)***	(.019)	(.0050)	(.045)***
Observations	534,762	534,762	536,313	567,956	499,553
Mean DV	3.43	4.12	3.57	.87	7.66
			Panel C: Ages 5-12		
NREGS	.0032	-.045	.010	-.0033	-.024
	(.021)	(.030)	(.022)	(.0025)	(.018)
Observations	1,497,917	1,497,917	1,507,128	1,550,549	1,468,834
Mean DV	2.32	2.61	2.40	.96	3.51
District FEs	YES	YES	YES	YES	YES
Year FEs	YES	YES	YES	YES	YES
Age FEs	YES	YES	YES	YES	YES
Rainfall Controls	YES	YES	YES	YES	YES

Source: ASER 2005-2009

Notes: This table reports estimates of β_1, the effect of NREGS on math and reading test scores and schooling outcomes from the ASER data. Math score ranges from 0-4, total math score ranges from 0-6, and read score ranges from 0-4. All panels include year, child age, and district fixed effects and controls for rainfall this year and rainfall last year. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A6: Robustness: Effect of NREGS on Test Scores and Schooling (ITT)

Dependent Variable:	Math Score	Total Math Score	Reading Score	Currently Enrolled	Highest Grade
Panel A: Full Sample					
NREGS	-.017	-.129	-.013	-.0060	-.050
	(.021)	(.030)***	(.021)	(.0032)*	(.022)**
Observations	2,055,034	2,055,034	2,065,228	2,126,570	1,994,536
Mean DV	2.58	2.97	2.68	0.93	4.58
Panel B: Ages 13-16					
NREGS	-.030	-.150	-.037	-.0076	-.086
	(.023)	(.035)***	(.020)*	(.0055)	(.045)*
Observations	545,055	545,055	546,495	576,250	509,354
Mean DV	3.40	4.08	3.55	0.86	7.66
Panel C: Ages 5-12					
NREGS	-.015	-.105	-.0075	-.0048	-.032
	(.022)	(.031)***	(.023)	(.0029)*	(.017)*
Observations	1,509,979	1,509,979	1,518,733	1,550,320	1,485,182
Mean DV	2.28	2.57	2.37	0.96	3.52
District FEs	YES	YES	YES	YES	YES
Age FEs	YES	YES	YES	YES	YES
Year FEs	YES	YES	YES	YES	YES

Source: ASER 2005-2009
Notes: This table reports estimates of β_1, the effect of NREGS on math and reading test scores and schooling outcomes from the ASER data. The The coefficients are from the OLS estimation of Equation (1) where S is a dummy variable which is equal to one if the district would have gotten the NREGS program had their state allotted slots in the program solely based on backwardness rank, and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. Math score ranges from 0-4, total math score ranges from 0-6, and read score ranges from 0-4. All panels include year, child age, and district fixed effects. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A7: Children's Primary Activity (Ages 5-12)

Dependent Variable:	Works at Home	Works outside Home	Domestic Work	Attends School
NREGS	-.00081	.00082	.0020	-.00014
	(.0015)	(.00075)	(.0020)	(.0059)
Observations	255,273	255,273	255,273	255,273
Mean of DV	.007	.004	.016	.85
District FEs	YES	YES	YES	YES
Age FEs	YES	YES	YES	YES
Year FEs	YES	YES	YES	YES

Source: NSS 2003-2009 (rounds 60, 61, 62, 64, 66)
Notes: This table reports estimates of β_1, the effect of NREGS on primary activities of children ages 5-12 using the NSS. The coefficients are from the OLS estimation of Equation (1) where S is a dummy for each specified primary activity (market work at home, market work outside the home, domestic work, or school attendance), and δ is a dummy variable for whether NREGS has rolled out in the respondent's district and year. Standard errors clustered at the district level are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.