Workfare and Human Capital Investment

Evidence from India

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ABSTRACT

We examine the impact of an increase in the demand for low-skill labor caused by a large public works program (NREGS) on schooling outcomes. Exploiting the staged rollout of the program for causal identification, we show exposure to workfare decreases enrollment by 1–3.5 percentage points and increases labor by four percentage points among adolescents, with girls primarily substituting into unpaid domestic work. We also find evidence that children exposed to the program in utero to age four benefit later in life. We conclude that the opportunity cost of schooling is an important determinant of educational investment.

I. Introduction

Education is an important input into economic growth and development (Mankiw, Romer, and Weil 1992). There is debate over the responsiveness of human capital investment to wage growth, particularly in the short run in developing

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countries. It has been shown that income effects result in higher investment in education when wages are higher (Jacoby and Skoufias 1997; Jensen 2000; Bharadwaj, Lakdawala, and Li 2020; Maccini and Yang 2009). However, 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 2016; Shah and Steinberg 2017; Cascio and Narayan 2015; Charles, Hurst, and Notowidigdo 2018). The direction and magnitude of this effect matter for policy, as transfer schemes that operate through wage labor are becoming increasingly popular as a means of redistribution in poor countries. Between 2007 and 2009, the World Bank alone supported 24 workfare programs in the developing world (Subbarao et al. 2013).

We examine the effect of a policy-induced increase in low-skill wages on human capital investment in rural India. We exploit the phased 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 of work at a fixed, above-market wage (Muralidharan, Niehaus, and Sukhtankar 2017; Berg et al. 2018; Azam 2012; Zimmermann 2014; Imbert and Papp 2015). This structure differs from government wage subsidy programs, like the Earned Income Tax Credit (EITC) in the United States, which, if anything, may have depressed low-skill wages (Rothstein 2010). The program was introduced gradually throughout rural India, starting with the poorest districts in early 2006 and extending to the entire country by 2008.

To estimate the effect of NREGS on educational investment, we use several rounds of the National Sample Survey (NSS), a large, nationally representative household survey conducted by the Government of India. We show that once NREGS is rolled out, adolescents are 2–3.5 percentage points less likely to report attending school as their primary activity and four percentage points more likely to report productive work as their primary activity. Although the dropout effects are similar for both sexes, adolescent girls are more likely to substitute into domestic work, and boys are more likely to take up work outside the home for pay.

We corroborate our main results with a unique data set on enrollment and test scores for children in rural India: the Annual Status of Education Report (ASER). The ASER tests nearly 500,000 rural children per year (aged 5–16) on basic literacy and numeracy skills. The survey covers children who are currently enrolled in school, have never been enrolled in school, or have dropped out of school. Consistent with the NSS results, we find that children are less likely to be in school after NREGS rolls out and that these effects are strongest for children 13–16 years olds, whose enrollment decreases by one percentage point. Using an exposure model, we show evidence that children who were exposed to the program at young ages (in utero–age 4) are more likely to be in school later in life, indicating that the program also generates positive income effects for the young.

We examine various threats to identification. We investigate pre-trends of outcome variables, NREGS-induced selective migration, and changes in school quality, and we show that these factors are unlikely to be driving the empirical results. Conditional on district fixed effects, there is little difference in the trends of outcome variables between early vs. late NREGS districts in the years leading up to the rollout of the program.

This work contributes to several literatures. First, it adds to the growing body of evidence that the income gradient of education might not always be positive. This has

been seen in the United States context (Cascio and Narayan 2015; Charles, Hurst, and Notowidigdo 2018), as well as in developing countries (Atkin 2016; Shah and Steinberg 2017). This study 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. Importantly, it illustrates that age of exposure may matter since we find differential impacts of NREGS on education for young vs. older children. Second, it adds to a growing literature that 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 (Cunha, De Giorgi, and Jayachandran 2018; Angelucci and De Giorgi 2009; Attanasio, Meghir, and Santiago 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 substitution between labor and education (see Edmonds 2008 for a review).

This paper is one of several recent papers that examine the impact of NREGS on educational outcomes. Li and Sehkri (2020), using school-level data (District Information System for Education [DISE]) on enrollment numbers, find that enrollment numbers decrease in treated districts. Islam and Sivasankaran (2014) find that NREGS increases child labor.

However, Afridi, Mukhopadhyay, and Sahoo (2016) and Mani et al. (2019), using Young Lives data from one state (Andhra Pradesh), find that NREGS increases human capital investment.[1] Our study is unique in that we use two separate, large, nationally representative household surveys to examine the impact of NREGS on school enrollment and child labor. We are also able to shed light on the market and intrahousehold mechanisms that may be driving our results.

Given the importance of NREGS in particular, and workfare programs more broadly, the findings from this study might be of direct interest to policymakers. Because education is important for economic growth and development, if these types of programs raise prevailing wages and cause older students to substitute into work and out of school, policymakers may want to consider lump-sum or conditional cash transfers as alternative forms of social insurance.

II. Background and Data

A. Background on NREGS

Workfare programs have become a popular antipoverty tool, particularly in developing countries, for several reasons. First, they are self-targeting: only those who are willing to work for a low wage will receive the subsidy (Besley and Coate 1992), and thus they eliminate the often considerable cost of determining who is eligible for the program. Second, they may prevent dependency, as participants will turn away from public works as better labor market opportunities arise. Third, if they create a large enough labor demand shift, these programs may increase private-sector wages as well, thereby further increasing incomes of the poor.

1. See Sukhtankar (2017) for a nice review of these papers.

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[2] 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 at least one-third of program beneficiaries to be women. It is operationalized through the National Rural Employment Guarantee Scheme (NREGS), which began in 2006 and has an annual budget of around 48,000 crores Rupees (approximately 6.7 billion dollars), amounting to more than 4 percent of the 2010–2011 Union Budget Expenditure (Ministry of Finance 2011–2012). In 2009–2010, 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 for a Job Card at the local Gram Panchayat.[3] After due verification, the Gram Panchayat issues a Job Card, and the card should be issued within 15 days of application. The Job Card is free of cost and bears the photograph of all adult members of the household willing to work under NREGS. Once a worker has a Job Card, they can apply for work at any time.

The applicants must be assigned to a project within 15 days of submitting the application. If there are no jobs available, they are eligible for unemployment compensation. Applicants have no choice over the project. The types of projects funded by NREGS are labor-intensive rural employment projects, such as road construction, earthworks related to irrigation and agriculture, and water conservation. The federal government bears the entire cost of worker wages and 75 percent of the cost of materials, with state governments covering 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).[4]

B. Rollout of NREGS

The National Rural Employment Guarantee Act was passed in 2005, and the scheme began to roll out in February 2006. Two hundred districts gained 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. Here we 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 was to target the poorest districts first. However, it was also guaranteed that each state would receive at least one district in the first wave of the program. The federal government assigned each district a "backwardness ranking," based on agricultural wages, percent of scheduled caste/scheduled tribe, and agricultural productivity

2. The statutory minimum wage rate varies across states, but it is approximately US$2 per day.

3. The Gram Panchayat is the lowest level of administration in the Indian government and comprises a group of villages.

4. 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; Muralidharan, Niehaus, and Sukhtankar 2016).

(Planning Commission 2003), though this ranking does not perfectly explain allocation to each wave. Zimmermann (2014) argues that based on the allocation of similar programs, it is likely that states were each given an allocation of Wave 1 and Wave 2 "slots" based on their overall poverty/backwardness, and they then allocated these slots as they saw fit.

Regardless of the exact mechanism of assignment, it is clear that early districts were not randomly selected. It is likely they were poorer, had more low-caste and tribal residents, and were potentially more politically connected. In Table A1 in the Online Appendix, we show summary statistics by rollout wave for both the NSS and the ASER data. While age and percent female are similar across waves, school enrollment is lower in districts that got NREGS earlier, and child labor is higher. We discuss how these differences could affect our estimates in Section IV.B.

C. National Sample Survey

To examine the impact of NREGS on schooling outcomes and work, we use the National Sample Survey (NSS) Rounds 61, 64, and 66, which were collected between 2004 and 2010 by the Government of India's Ministry of Statistics. This is a national labor and employment survey collected at the household level in every state in India, and we use data from all rural households in these surveys. We use three "thick" (large sample) rounds of NSS Schedule 10, which focus on employment and unemployment. Round 61 (July 2004–June 2005) was collected before NREGS went into effect, Round 64 was collected during the rollout (July 2007–June 2008), when Wave 1 and 2 districts (but not Wave 3 districts) have the program, and Round 66 was collected after the rollout (July 2009–June 2010), when all districts have access to NREGS. We use thick rounds for our main estimation in order to ensure that our sample is large and representative and to follow the previous literature as closely as possible (Imbert and Papp 2012).

In Figure 1, we use five additional NSS survey rounds to extend the time series further back: Schedule 1 Rounds 57 (January–December 2001), 58 (January–December 2002), and 59 (January–December 2003),[5] as well as "thin" (small sample) Rounds 60 (January– June 2004) and 62 (July 2005–June 2006). While this does not provide more variation in the implementation of the program to estimate the effects, it does provide many more time periods in which to examine the potential for differential trends between early and late districts.[6]

This data set 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 an individual whose primary activity is going to school as "attends school." "Domestic work" is for individuals who report attending to domestic duties and/or engaging in free collection of goods (vegetables, roots, firewood, cattle feed, etc.),

5. Though the purpose of Schedule 1 is not to measure employment, the survey asks about the "primary activity" of every household member, which is the main outcome variable of interest.

6. The 2005–2006 thin round could, in principle, allow us to estimate effects for Wave 1 districts separately since Wave 1 districts started being eligible for the program in February 2006, and at least some of the households were surveyed after this date. In practice, this is a small number of households, and the sample is not necessarily representative. In addition, in many areas of the country, NREGS rollout in the first year was quite slow, and thus many households would not have been "treated" until later in the year (Mehra 2006). However, in a previous version of this paper we did use the 2005–2006 thin round, and the results are similar.

sewing, tailoring, weaving, etc. for household use. "Works at home" describes individuals who report 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. These four categories of primary activities represent almost 100 percent of individuals aged 5–65. The omitted options include being retired, disabled and unable to work, seeking work, and other (begging, prostitution, etc.). We note that these other categories together sum to only 4 percent for the children, 5 percent for household heads, and 2 percent for spouses of the household head. Lastly, "child labor" equals one if the child's primary activity is domestic work, work at home, or work outside the home. We provide summary statistics in Table 1. In this data set, 71 percent of adolescents aged 13–17 describe their primary activity as attending school, while 25 percent report working, either in a home enterprise such as a farm, in market work, or in domestic work (cooking, childcare, fetching water, etc.). Younger children aged 5–12 are much more likely to report attending school as their primary activity (85 percent) and much less likely to report working (about 3 percent).

D. ASER 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.[7] As children under five are typically too young to enter school and are not tested in math or literacy by ASER, we restrict the analysis to children aged 5–16.[8] 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 enrollment and test score data (2005), three rounds of enrollment and test score data during the rollout (2006, 2007, 2008), and one round after the program has been rolled out throughout the country (2009). The ASER data provide us with one year of data per each year of the NREGS rollout, unlike the NSS data. However, because ASER starts in 2005, we cannot investigate pre-trends with this data.

The sample is a representative repeated cross-section at the district level.[9] The ASER survey asks whether each child in the household age 5–16 is enrolled in school,[10] as well as other school-related questions that tend to rotate from year to year. In addition,

7. This includes more than 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/.

8. In 2005, only children aged 6–14 were tested. Our results are robust to restricting to these ages for all five years.

9. The ASER data are 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 are 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 children currently enrolled, children who have dropped out, and children who never enrolled.

10. Note that this question is asked slightly differently than the school enrollment question in the NSS 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 percent of children in the ASER report being currently enrolled, while only 82 percent of children the same age report "attending school" as their primary activity in NSS data.

ASER surveyors ask each child four questions each in math and reading (in their native language). The four math questions measure whether the child can recognize numbers 1–9, recognize numbers 10–99, subtract, and divide. The scores are coded as one if the child correctly answers the question, and zero otherwise. We calculate a "math score" variable that ranges from zero to four and is the sum of the scores of the four numeracy questions. Similarly, each child is also asked to complete four reading tasks: recognize letters, recognize words, read a paragraph, and read a story. We generate a total reading score that ranges from zero to four depending on how many tasks the child can complete. In Table 1 we describe the characteristics of the children in our sample, as well as their test scores. Our ASER sample contains 1,949,493 children aged 5–12 and 742,097 adolescents aged 13–16. By this measure, 96 percent of children aged 5–12 are currently enrolled in school, while 87 percent of adolescents aged 13–16 are currently enrolled.

The ASER also collects school-level data from various schools in a district. In 2005, 2007, and every year since 2009, ASER has included a visit to one government primary school in each sampled village. Basic information is collected on school infrastructure, enrollment, teachers, and fund flows. To explore whether NREGS affects school quality, we use ASER school data from 2005, 2007, and 2009.

III. Empirical Strategy

Because NREGS was rolled out in stages, 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 using the within-district timing of the rollout, rather than simply comparing districts in earlier and later waves.

We estimate the following equation:

$$(1) \quad S_{ijty} = \alpha + \beta_1 I_{jt} + \gamma_j + \phi_t + \psi_y + \epsilon_{ijty}$$

where S_{ijty} is an outcome of interest (such as enrollment status or child labor) for individual i in district j in year t who is age y, 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 year–child–age fixed effects. β_1 is the coefficient of interest, and it measures the effect of NREGS on the outcome variable S. Standard errors are clustered at the district level.

The main specification using the NSS data can be thought of as two differences-in-differences (DDs): one compares early and late districts before and after the program introduction in early districts, and one compares early and late districts before and after the program extends to the whole country. The identification comes from comparing the difference in school enrollment and child labor between early and late districts during the rollout (when Waves 1 and 2 had NREGS, but Wave 3 did not) to before the rollout (when no one had the program) and after the rollout (when everyone had the program). The identifying assumption is that in the absence of the program, the differences in school enrollment and child labor between early and late districts in this time period would have been the same as the differences before and after the rollout. When we use the ASER data, we have data every year from 2005–2009, which means we effectively have many more DDs, but the identifying assumption is the same.

Table 1
Summary Statistics

	Mean	SD	Observations
NSS sample (ages 13–17)			
Age	14.9	1.35	92,892
Female	0.47	0.50	92,892
Primary activity:			
Attends school	0.71	0.45	92,892
Works at home	0.08	0.26	92,892
Works outside home	0.07	0.25	92,892
Domestic work	0.10	0.30	92,892
NSS sample (ages 5–12)			
Age	8.5	2.32	166,290
Female	0.47	0.50	166,290
Primary activity:			
Attends school	0.85	0.35	166,290
Works at home	0.007	0.09	166,290
Works outside home	0.004	0.06	166,290
Domestic work	0.015	0.12	166,290
NSS sample (female spouse of household head, ages 18–65)			
Age	39.1	11.5	146,136
Primary activity:			
Attends school	0.0006	0.0247	146,136
Works at home	0.216	0.411	146,136
Works outside home	0.137	0.344	146,136
Domestic work	0.631	0.483	146,136
NSS sample (male household head, ages 18–65)			
Age	41.4	11.5	153,070
Primary activity:			
Attends school	0.0013	0.036	153,070
Works at home	0.541	0.498	153,070
Works outside home	0.387	0.487	153,070
Domestic work	0.020	0.141	153,070
ASER sample (ages 13–16)			
Age	14.3	1.08	742,097
Female	0.45	0.50	742,097
Currently enrolled	0.869	0.336	742,097
Math score	3.43	0.98	696,291
Reading score	3.57	0.98	696,291
ASER sample (ages 5–12)			
Age	8.87	2.13	1,949,493
Female	0.454	0.497	1,949,493
Currently enrolled	0.961	0.195	1,949,493
Math score	2.31	1.3	1,885,355
Reading score	2.39	1.4	1,885,355

Source: ASER (2005–2009) and NSS 2004–2010 (Rounds 61, 64, and 66).
Notes: This table contains summary statistics for the outcome and control variables in this paper.

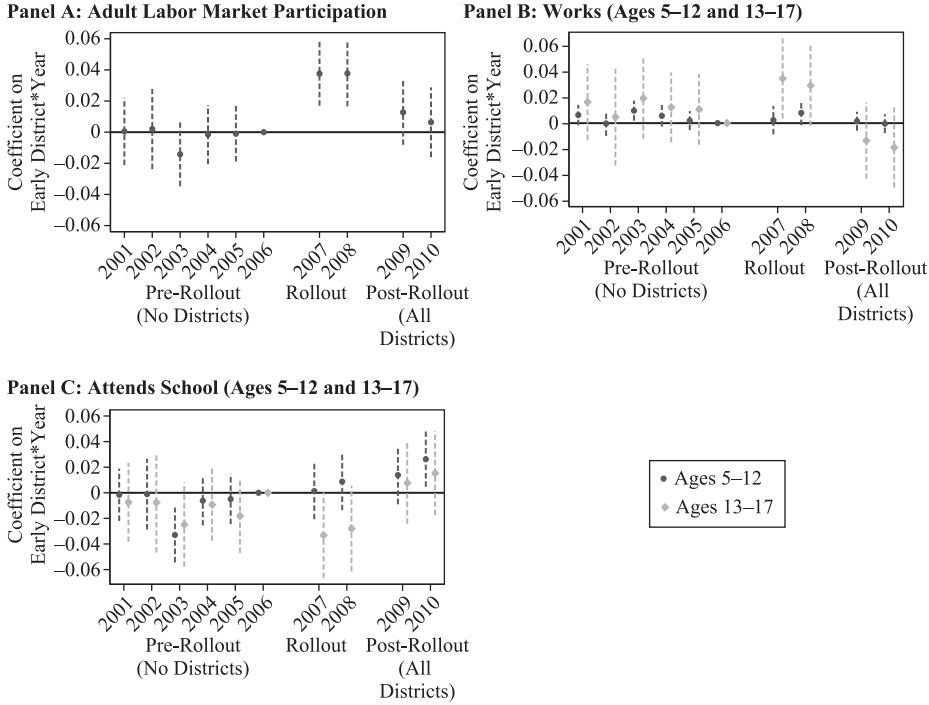

Figure 1

Primary Activity of Parents and Adolescents in Early vs. Late Districts, by Year

Source: NSS 2001–2010 (Rounds 57, 58, 59, 60, 61, 62, 64, and 66).

Notes: This figure shows coefficients from three separate regressions of outcomes regressed on year*early NREGS dummies. These regressions also include district, year, age, and sex fixed effects. The outcomes are adult labor market participation and adolescent attends school or works (either outside or inside the home) as their 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 that got the program in Wave 1 (Spring 2006) or Wave 2 (Spring 2007). The data from 2006 come from the 2005–2006 round of the NSS (62) and were collected before the NREGS rollout to most early districts. The data from 2007 are from the 2007–2008 NSS wave (64) and were collected after the rollout of NREGS to all early districts.

In terms of pre-trends, as evidenced in Table A1 in the Online Appendix, districts that received the program earlier were initially worse off on a number of indicators: school enrollment, child labor, test scores, etc. This would pose an issue for our estimation strategy if these districts were also trending differentially in the outcomes of interest.

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 the outcome variables. We explore this assumption in Figure 1 using the NSS data. We graph coefficients from three distinct regressions where the dependent variables are parent labor market participation and adolescents' primary activity (attends school or works) regressed 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 use NSS Rounds 57, 58, 59, 60, 61, 62, 64, and 66 to extend the time series back to 2001.

If early districts were on a different trend leading up to the rollout of NREGS, we would expect to see nonzero coefficients for early districts in the years 2001–2006 and potentially an upward trend in these coefficients. Instead, the coefficients hover near zero, and they look roughly flat over time. In 2007–2008, when the program rolls out to the early districts, we see an increase for working in these coefficients for adolescents, and a decrease in the coefficients on schooling. In these figures, it does not appear that the difference in enrollment and working in 2007–2008 in early districts is part of an underlying trend, but rather an outlier relative to the trend leading up to the rollout of NREGS. In Figure A1 in the Online Appendix, we make the same figures using only the thick NSS Rounds 61, 64, and 66 since these are the rounds we use in the main analysis. The figures look remarkably similar.

IV. Results

A. NSS Results

Table 2 shows the primary estimates of β_1 on children's reported "primary activity" from Equation 1 using the NSS data. Panel A shows results for the full sample of children, aged 5–17. 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 (13–17). In Column 1, the dependent variable is a dummy for whether the household reports "attends school" as the child's primary activity. In Column 2, the dependent variable is a dummy for whether the household lists other productive work, including wage work, domestic work, and working at home, as the child's primary activity.

We find that children ages 5–17 are 1.3 percentage points less likely to report attending school as their primary activity and 1.7 percentage points more likely to report productive work (either inside or outside the home) as their primary activity when their district becomes eligible for NREGS. This effect is almost entirely driven by adolescents. Panel B shows that children ages 13–17 are 3.5 percentage points less likely to report attending school and four percentage points more likely to report working. This implies a 5 percent decrease in reporting school as a primary activity and a 16 percent increase in reporting work as the primary activity. The results for the younger children, shown in Table 2 (Panel C), are neither economically meaningful nor statistically significant.

While we know from Table 2 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 among both children and their parents. In Table 3, we show estimates of β_1 from Equation 1, where S is one of the primary activity categories (works at home, works outside home, domestic work, and attends school) for children aged 13–17 and 5–12 with gender interactions. While we do not observe differences in the effect of NREGS on human capital investment by gender (Column 4), we do observe differences in type of work by gender.

Table 2
Effect of NREGS on School Attendance and Work

	Dependent Variable	
	Attends School	Works
Panel A: Full Sample		
NREGS	–0.01276	0.01705
	(0.00693)*	(0.00493)***
Observations	259,182	259,182
Mean DV	0.80	0.10
Panel B: Ages 13–17		
NREGS	–0.03506	0.04053
	(0.01011)***	(0.00954)***
Observations	92,892	92,892
Mean of DV	0.71	0.24
Panel C: Ages 5–12		
NREGS	–0.00021	0.00503
	(0.00769)	(0.00388)
Observations	166,290	166,290
Mean of DV	0.85	0.027
District FEs	Yes	Yes
Year FEs	Yes	Yes
Age FEs	Yes	Yes

Source: NSS 2004–2010 (Rounds 61, 64, and 66).
Notes: This table reports estimates of β_1, the effect of NREGS on children who report their "primary activity" as school attendance or work. 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. All regressions include district, year, age, and sex fixed effects. Significance: $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

Female adolescents aged 13–17 appear to be substituting almost entirely into domestic work when leaving school. Male adolescents, on the other hand, substitute into work outside the home, reducing both their school attendance and at-home work. Adolescent males appear to be working directly for NREGS or in market jobs as substitutes for adult labor.[11] For boys aged 5–12, most of the coefficients are close to zero. For girls aged 5–12, there is a small increase in domestic work, but otherwise none of the results are statistically significant, and most of the coefficients are close to zero.

11. 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.

Table 3
Effect of NREGS on Children's Primary Activity

	Dependent Variable			
	Works at Home	Works Outside Home	Domestic Work	Attends School
Panel A: Ages 13–17				
NREGS	–0.00771	0.03518	0.01175	–0.03831
	(0.00728)	(0.00775)***	(0.00333)***	(0.01181)***
NREGS × Girl	0.00541	–0.02321	0.01965	0.00875
	(0.00821)	(0.00856)***	(0.01037)*	(0.01251)
Observations	92,892	92,892	92,892	92,892
Mean of DV (Boys)	0.097	0.091	0.015	0.74
Mean of DV (Girls)	0.052	0.042	0.20	0.68
p-value total effect = 0	0.68	0.035	0.003	0.014
Panel B: Ages 5–12				
NREGS	–0.00112	0.00231	0.00217	–0.00306
	(0.00256)	(0.0014)*	(0.00272)	(0.00852)
NREGS × Girl	–0.00201	–0.00059	0.0063	0.00606
	(0.0021)	(0.00158)	(0.00327)*	(0.00768)
Observations	166,290	166,290	166,290	166,290
Mean of DV (Boys)	0.008	0.005	0.008	0.86
Mean of DV (Girls)	0.006	0.003	0.023	0.84
p-value total effect = 0	0.19	0.082	0.021	0.73
District, year, and age FEs	Yes	Yes	Yes	Yes
District × Female FEs	Yes	Yes	Yes	Yes
Year × Female FEs	Yes	Yes	Yes	Yes
Age × Female FEs	Yes	Yes	Yes	Yes

Source: NSS 2004–2010 (Rounds 61, 64, and 66).
Notes: This table reports estimates of β_1 from Equation 1, the effect of NREGS on primary activities of children using the NSS as well as the coefficient from the interaction of female with NREGS. All regressions contain fixed effects for age, district, and year, and each of these is also interacted with female. Standard errors clustered at the district level are reported in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

In Table 4, we show estimates of β_1 from Equation 1, where S is one of the primary activity categories, estimated separately for adult men (male heads of household aged 18–65) and women (female spouses of household heads aged 18–65). Column 1 shows the effect of NREGS on working in a home enterprise (primarily farms), either as its head, an employee, or an unpaid family worker. We find that for men, this work decreases by five percentage points. Working outside the home increases for both men

Table 4
Effect of NREGS on Adult Primary Activities

	Dependent Variable			
	Works at Home	Works Outside Home	Domestic Work	Attends School
NREGS	–0.05385	0.0399	0.00903	–0.00029
	(0.00524)***	(0.00489)***	(0.00201)***	(0.00024)
NREGS × Female	0.0708	–0.0145	–0.04999	–0.00023
	(0.01187)***	(0.00883)	(0.01406)***	(0.0004)
Observations	299,206	299,206	299,206	299,206
Mean of DV (Male)	0.54	0.39	0.02	0.001
Mean of DV (Female)	0.22	0.14	0.63	0.0006
p-value total effect = 0	0.11	0.002	0.003	0.13
District, year, and age FEs	Yes	Yes	Yes	Yes
District × Female FEs	Yes	Yes	Yes	Yes
Year × Female FEs	Yes	Yes	Yes	Yes
Age × Female FEs	Yes	Yes	Yes	Yes

Source: NSS 2004–2010 (Rounds 61, 64, and 66).
Notes: This table reports estimates of β_1 from Equation 1, the effect of NREGS on primary activities of parents, defined as adults 18-65 who are either male heads of household or female spouses of head of household, as well as the coefficient from the interaction with female spouse and NREGS. All regressions contain fixed effects for age, district, and year, and each of these is also interacted with female spouse. Standard errors clustered at the district level are reported in parentheses. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

and women, (four percentage points versus 2.5 percentage points for women). Domestic work decreases for women by four percentage points. Overall this is consistent with women switching out of domestic work and into market work, either at home on the farm or outside in wage work, while male household heads are switching from working at home to working for wages in the market.

Since women are decreasing domestic work when NREGS becomes available, it is likely the adolescent females are substituting for their mothers' labor inside the household. We show these results graphically in Figure 2, where we graph coefficients from Tables 3 and 4. Figure 2 suggests that while boys' substitution patterns mirror those of adult men, girls' substitution patterns are the opposite of adult women, suggesting that they may be substituting for their labor, particularly inside the home.

B. Robustness

In Table A2 in the Online Appendix, we reestimate Table 2 using all thin and thick rounds (both Schedule 1 and Schedule 10) between 2001 and 2010. This is the sample we use for Figure 1 because it contains additional years of outcome data before the

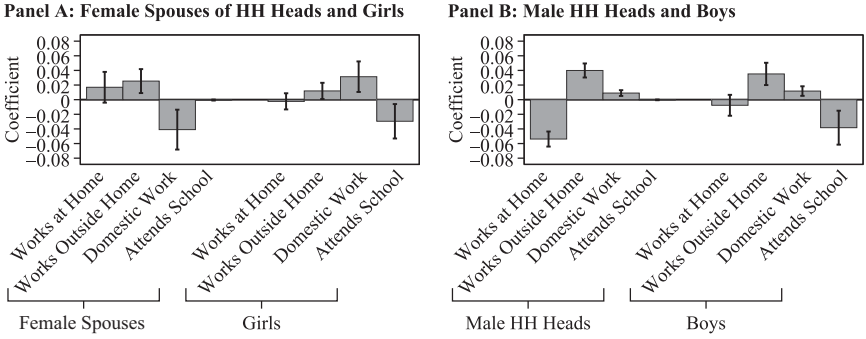

Figure 2

Effect of NREGS on Primary Activity

Source: NSS 2004–2010 (Rounds 61, 64, and 66).

Notes: Panel A shows the coefficients from Table 3, Panel A and Table 4 represented graphically for females. Panel B shows the coefficients from Table 3, Panel A and Table 4 for males. "Female Spouses of HH Heads" are defined as females aged 18–65 who report their household status as "spouse of head of household." "Male Household Heads" are defined as males aged 18–65 who report their household status as "head of household." "Girls/boys" are defined as females/males aged 13–17.

rollout of NREGS. We find that the coefficients in this specification are somewhat smaller than those in Table 2, but still qualitatively similar and statistically significant for adolescents (Panel B).

In Table A3 in the Online Appendix, we reestimate the main specification from Table 2 (Panel B) with different sets of controls to test whether the estimates are sensitive to the specific choice of fixed-effects model. First, it is possible that we are just picking up a trend in adolescent educational outcomes, or that adolescents are less likely to be in school in some districts. To address this, in Panel A, we add age-by-year and age-by-district fixed effects to the model. The results do not change significantly.

It is also possible that certain districts have differential trends in outcomes by cohort, which could be driving the results. To test for this, in Panel B, we add district–cohort trends. Likewise, more backward districts could be trending differently than other districts. To address this concern, in Panel C, we add district–year trends. Lastly, one might be worried about convergence, divergence, or mean reversion in school enrollment and/or child labor across districts. In Panel D, we interact baseline school enrollment in Column 1 and baseline child labor in Column 2 with year dummies and add this to our standard model.

In all four panels, the results are remarkably similar to Panel B in Table 2, both in magnitude and precision. This gives us confidence that we are not just picking up an underlying trend across districts, cohorts, or years. Rather, we seem to be estimating a trend break in enrollment in early NREGS districts relative to late NREGS districts in the 2007–2008 round of the NSS. This is consistent with the graphical evidence presented in Figure 1.

We now turn to another household survey, the ASER data, to corroborate the main results in Table 2. The ASER data have a few distinct advantages. First, the sample size

Table 5
Effect of NREGS on Schooling and Test Scores

	Dependent Variable		
	Currently Enrolled	Math Score	Reading Score
Panel A: Full Sample			
NREGS	–0.0065	–0.001	0.0032
	(0.0028)**	(0.0186)	(0.0195)
Observations	2,691,590	2,581,646	2,581,646
Mean DV	0.935	2.61	2.71
Panel B: Ages 13–16			
NREGS	–0.0099	–0.0254	–0.0354
	(0.0049)**	(0.0193)	(0.0187)*
Observations	742,097	696,291	696,291
Mean DV	0.868	3.41	3.54
Panel C: Ages 5–12			
NREGS	–0.0046	0.0055	0.016
	(0.0025)*	(0.02)	(0.0214)
Observations	1,949,493	1,885,355	1,885,355
Mean DV	0.96	2.32	2.39
District FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Age FEs	Yes	Yes	Yes

Source: ASER (2005–2009).
Notes: This table reports estimates of β_1, the effect of NREGS on currently enrolled, math, and reading test scores 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 and read score ranges from 0-4. All regressions include year, child age, sex, and district fixed effects. Standard errors clustered at the district level are reported in parentheses. Significance: $*p<0.10$, $**p<0.05$, $***p<0.01$.

is much bigger—it surveys roughly 500,000 children per year. Second, the survey occurs every year in November, which means that we can use variation from all three waves of the rollout (rather than just early vs. late). Third, in addition to questions about school enrollment, ASER gives simple math and reading tests to children both in and out of school. We note that the primary disadvantage of the ASER data set is that it begins in 2005, one year before the rollout of NREGS. Thus, we have no way to assess the validity of the parallel trends assumption using pre-trends of outcome variables by wave.

Table 5 shows estimates of β_1 from Equation 1 for three outcome variables: whether the child is currently enrolled in school, math score, and reading score. Panel A shows

results for all children, Panel B for adolescents aged 13–16, and Panel C for children aged 5–12. As in the NSS, when all ages are pooled, children are less likely to be in enrolled in school (0.65 percentage points) when NREGS rolls out to their district. Again, this effect is driven primarily by adolescents: the coefficient in Panel B is twice that in Panel C and is statistically significant at the 5 percent level.

The results suggest that children are one percentage point less likely to be currently enrolled in school when NREGS rolls out. It is worth noting that the mean of enrollment (0.94) is much larger than the mean of "attends school" from Table 2 (0.80), even though these are ostensibly samples of the same population of children over roughly the same time period.[12] This is because the questions are asked in a slightly different way: the ASER question asks whether a child is currently enrolled, dropped out, or never enrolled. The NSS question asks, for each member of the household, what is their primary activity: attending school, domestic work, wage work, etc. There are around 15 percent of kids who report that they are currently enrolled in school (in ASER) but that their primary activity is something other than attending school (in NSS). Thus, the ASER might be capturing a more permanent notion of "dropout," while the NSS measures those not currently attending school.

The last two columns in Table 5 show estimates of the effect of NREGS on math and reading test scores. There is no overall impact of NREGS on test scores. For adolescents, we might see some small reduction in math and reading scores, but the estimates are imprecise and small (Panel B). For the younger children, aged 5–12, the test score coefficients are positive though not statistically significant (Panel C).

C. Exposure to NREGS

So far we have estimated the impact of NREGS based on the rollout timing for each district as an indicator that switches on. However, it is possible that those with more years of exposure to the program experience a stronger impact on school outcomes. 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 possibility, we calculate "years of exposure" to NREGS for various cohorts in each district, so that those who were exposed to the program earlier will have larger "doses" of the treatment. We calculate years of exposure for children using the previous age categories (ages 5–12 and 13–16). However, we can now investigate the impact of children who were exposed to NREGS in utero to age four using data from kids who are currently ages 5–16. These categories roughly correspond to different phases of child development: early childhood, primary school, and secondary school.

We estimate the following equation:

$$(2) \quad S_{ijty} = \alpha + \beta_2 C_{jt} + \gamma_j + \phi_t + \psi_y + \delta_{t,y} + \epsilon_{ijty}$$

12. The results are more similar as a percentage of out-of-school children. The ASER point estimates imply an increase in out-of-school 13–16-year-olds of 7.6 percent, while the NSS results imply an increase in out-of-school 13–16-year-olds of 12 percent.

Table 6
Effect of NREGS on Schooling, by Years of Exposure

	Dependent Variable: Currently Enrolled
Exposure (in utero–age 4)	0.0067
	(0.0017)***
Exposure (ages 5–12)	0.0015
	(0.0019)
Exposure (ages 13–16)	–0.0164
	(0.0036)***
Observations	5,532,693
Mean DV	0.875
District FEs	Yes
Year FEs	Yes
Age FEs	Yes
Year of Birth FEs	Yes

Source: ASER (2005–2014).
Notes: This table shows estimates of the years of exposure of NREGS on 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 C is a vector for each age group of years of exposure to NREGS. All panels include child age, year of birth, year, and district fixed effects. Standard errors clustered at the district level are reported in parentheses. Significance: $^*p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

where C_{ij} is a vector that measures years of exposure to NREGS for each age group (in utero–age 4, 5–12, 13–16). γ_j is a vector of district fixed effects, ϕ_t is a vector of year fixed effects, ψ_y is a vector of individual year child age fixed effects, and $\delta_{t,y}$ is a vector of cohort fixed effects.

The identifying assumption for Equation 2 is that any differences between cohorts, districts, or ages is fixed over time and that any remaining differences between cohort-wave is due to exposure to NREGS. In addition, we assume that this exposure affects school enrollment linearly—that is, each year of exposure (in a given age bin) affects school enrollment by the same amount. To estimate Equation 2, we include an additional five years of ASER data, so we have data for 2005–2014. We include more rounds of data so that we can have more common support by wave and district for each cohort. However, the main results are robust to using data for 2005–2009 as well.

Table 6 reports the results from regressions of exposure to NREGS at each age group for the outcome of interest currently enrolled. Again, exposure to NREGS has the largest negative impacts for those 13–16 years old. Each year of exposure to NREGS during adolescence decreases school enrollment by 1.6 percentage points. This implies that by age 16, children who live in districts with NREGS for two years during ages 13–16 are three percentage points less likely to be currently enrolled in school.

For the younger children, the results are quite different. Each year of exposure between the in-utero period and age four increases the probability that children are enrolled by 0.67 percentage points when we observe them later at school age. This is consistent with Shah and Steinberg (2017), who find that increased wages in rural India are positively associated with human capital before age five (when the income effect is likely to dominate) but negatively associated with human capital at older ages. As the results in Table 2 and 5 indicate, exposure during the primary school years (ages 5–12) does not significantly impact enrollment.

These results should be interpreted carefully, as they require stronger assumptions than the main specification. However, with this caveat, these results imply that the income effects of NREGS could cause an increase in human capital investment for children exposed at young ages. If this continues to hold, then the long-run impacts of NREGS on education might be quite different than those we estimate in the short run.

V. What Is Driving the Adolescent Decline in Human Capital?

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

A. Labor Demand

From previous work, we know that NREGS caused an increase in labor demand and wages (Imbert and Papp 2015; Muralidharan, Niehaus, and Sukhtankar 2017; 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 in the labor market or doing household, farm, and/or domestic work.[13]

If girls are indeed substituting for their mothers inside the home when NREGS rolls out, as suggested by Figure 2, one would expect that mothers with good substitutes at home would be more likely to take up NREGS work and/or work outside the home. In Table 7, we test this prediction directly using the NSS data. We interact the introduction of NREGS with the number of teenage girls aged 13–17 in the household, controlling

13. 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.

Table 7
Women and Adolescent Daughters

	Dependent Variable	
	Works Outside Home	Works Outside Home
NREGS	0.02249 (0.00834)***	0.02244 (0.00825)***
Adolescent girls (aged 13–17)	0.00399 (0.00273)	0.00839 (0.00281)***
NREGS × Adolescent girls (aged 13–17)	0.00921 (0.00376)**	0.00951 (0.00373)**
Adolescent boys (aged 13–17)		0.01031 (0.00277)***
NREGS × Adolescent boys (aged 13–17)		–0.00165 (0.00323)
Children (aged 5–12)		0.02013 (0.00184)***
NREGS × Children (aged 5–12)		0.00069 (0.00193)
Young children (aged 0–4)		0.00442 (0.0026)*
NREGS × Young children (aged 0–4)		–0.00048 (0.00294)
Household size	–0.00305 (0.00056)***	–0.01014 (0.00063)***
Observations	146,136	146,136
Mean of DV	0.134	0.134
District FEs	Yes	Yes
Year FEs	Yes	Yes
Age FEs	Yes	Yes

Source: NSS 2004–2010 (Rounds 61, 64, and 66).
Notes: This table estimates whether mothers with adolescent daughters are more likely to work outside the home once NREGS rolls out, using OLS estimation. All regressions include year, child age, and district fixed effects. Standard errors clustered at the district level are reported in parentheses. Significance: *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

for household size. We find that women with more teenage girls in the home are more likely to begin working outside the home when NREGS rolls out (Column 1). The increase in labor force participation of women with teenage daughters is about half the magnitude of the increase due to the program overall.

In Column 2 we test whether the interaction of any other types of children with the program impacts adult female labor force participation. Since we only see adolescent girls substituting for their mothers in domestic work, we would not expect boys or younger children to affect NREGS uptake in the same way. We include interactions for NREGS and number of teenage boys aged 13–17, NREGS and number of young children aged 0–4, and NREGS and the number of children aged 5–12. In contrast to the results for teenage girls, there is no significant effect on the probability that the mother works once NREGS rolls out based on the number of teenage boys, children, and/or young children in the household.

B. 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 potentially reduce their schooling investment to adjust to the new, lower returns. While it is certainly possible that this change could affect investment decisions, we think it is unlikely to be driving our empirical results for two reasons.

First, the effects are identified off of the rollout of NREGS, rather than its announcement, which happened when the law was passed in 2005. The announcement would have affected all districts at the same time. Of course, it is possible that some people did not know about the program (though it was widely publicized by the governing party) or did not believe that the government would really implement it until it was actually up and running in their district. However, if beliefs did not adjust immediately (enough to affect dropout choices that same year), they would not be identified by our empirical strategy. In other words, for a change in expectations to be driving the results, it would need to be the case that people did not expect the program to be rolled out when it was announced, but once it had been in their district for a few months, they believed it would be around long enough to impact the majority of their children's working lives.

Second, the program should only reduce the returns to the lowest levels of schooling, since it effectively raises the minimum wage for workers and incorporates poor, rural workers into the labor market. In Table A4 in the Online Appendix, we measure the effect of NREGS on Mincerian returns to schooling, and while the program does lower overall returns, it has no effect on returns for people with at least five years of education. Thus, if families are responding to the change in the returns, then their children should be more likely to bunch at zero years of education (since the return to zero education has increased), and conditional on having at least five years of education, their investment decisions should remain unchanged. However, this is the opposite of what we find. In Tables 2, 5, and 6 we show that it is adolescents, those whose highest grade is almost Grade 8, who are reducing investments. If anything, those with the lowest initial levels of schooling (young children) are increasing their investments, as shown in Table 6.

C. Changes in School Quality

Because NREGS is such a large program, and its secondary role (after work provision) is building infrastructure, one might worry that the funding of government schools could have been affected by the program. In that case, children would be dropping out of school not because of an increase in the opportunity cost of schooling, but rather because of a decrease in school quality. We test for this directly using the ASER school survey, which surveyed schools in 2005, 2007, and 2009 and provides data on infrastructure and other school characteristics.

In Table A5 in the Online Appendix, we show estimates of β_1 from Equation 1 at the school level on three outcomes that were asked consistently over the course of the three surveys: number of classrooms, mid-day meal provision, and whether the school has toilets available. We find that there is little change in any of these when NREGS rolls out. If anything, it appears that school quality may have increased slightly, as all three coefficients are positive, and number of classrooms is marginally significant.

VI. Threats to Identification

A. Parallel Trends

The 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 the results are being driven by the program or by underlying trends in eligible districts.

The most straightforward way to address this is visually. In Figure 1, we graph coefficients from a regression of parent labor market participation and 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). There are few differences and no discernible trend between early and late districts in either working or school attendance between 2001 and 2005. In 2007 and 2008, adolescents in early districts are more likely to be working and less likely to be in school, and after 2008, when all districts have access to the program, these differences decrease.

In addition, we plot the raw 1999–2012 data for the NSS for each outcome by age group and NREGS wave in Figures A2–A5 in the Online Appendix. While there are upward trends in education over this time period (and some convergence across districts), it is clear that 2007 and 2008 are off-trend for adolescents in early wave districts (Figure A2 and A4). We plot similar figures for ASER data in Figures A6–A7 in the Online Appendix, but since ASER data was first collected in 2005, there is no pre-existing trend to assess in these figures. Instead we can see the variation that primarily drives our results: 2006 is an outlier year for adolescents in Wave 1.[14] 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.

14. ASER is collected each year in November, and thus the 2006 ASER data are post-treatment for Wave 1 districts and pre-treatment for Waves 2 and 3. NSS data from 2006 are almost entirely collected pre-Wave 1 rollout, which is why we use 2007 data as the beginning of the program when using the NSS data.

B. Selective Migration

In general, migration rates in rural India are extremely low (Munshi and Rosenzweig 2016; Topalova 2007). In particular, migration between early and late NREGS districts is not a major concern since Indian Census data from 2001 estimates that rural inter-district migration for employment was limited to 0.4 percent of all adults aged 18–60. However, there is evidence that NREGS might decrease short-term migration from rural to urban areas for work (Imbert and Papp 2016). This could bias the sample of children observed when enumerators survey the village and affect our analysis of test scores and school enrollment.

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 a similar reduction in enrollment rates and commensurate increases in productive work.

Perhaps most importantly, our results are simply too large to be explained by migration. While Muralidharan, Niehaus, and Sukhtankar (2017) find no impact of the program on migration, Imbert and Papp (2016) find that NREGS decreased migration from rural to urban areas by 0.5 percentage points among adults in the NSS. Using the same data, we show a decrease in enrollment of 3.5 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, migration could explain at most 14 percent 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 study.

VII. Discussion

Our analysis should not be interpreted as a statement on the overall welfare impacts of NREGS. First, education is just one outcome of the program, and reductions should be weighed against the large, documented benefits of the program in providing income to the poor (Imbert and Papp 2015; Muralidharan, Niehaus, and Sukhtankar 2017; Cook and Shah 2020). Second, our analysis hints at the idea that the long-term effects might be different than the short-run effects: for instance, we see that, if anything, young children exposed to the program are doing better than those who were not exposed. Third, and perhaps most importantly, schooling for adolescents decreased because of a change in demand, not a change in supply—if households are optimizing, perhaps the value of the current income is worth the reduction in future income, at least for some households. This would make the welfare cost of this aspect of the program much lower than the lost wages. If households are not perfectly optimizing because of present bias, incomplete information about returns, imperfect altruism within the household, liquidity constraints, or other factors, then it is possible that NREGS may have reduced welfare on this margin. Whether or not dropping out of school in response to NREGS is "rational" from the perspective of the household is beyond the scope of this paper.

That being said, we do find a decrease of one to 3.5 percentage points in educational enrollment caused by the rollout of the program for those 13–17 years old. To give a

sense of magnitudes, if we assume that the effects are uniform across rural India, then the introduction caused between 850,000 and 3 million children to drop out of school prematurely. Even if we assume that every child who dropped out would have completed only one additional year of schooling in the absence of the program, the net present value of lost earnings is between 18 and 60 billion Rupees (US$250–$850 million),[15] a figure that dwarfs the total expenditure of the NREGS program (370 million Rupees in 2013–2014).[16]

We can also think about the magnitudes of these effects in terms of costs to mitigate them in the Indian context. Muralidharan and Prakash (2017) find that a program that provides bicycles to secondary school girls in Bihar increases secondary school enrollment in that group by 5.2 percentage points, at a cost of 2,500 Rupees (US$36) per girl (plus administrative costs). Adukia (2017) finds in a study based in several Indian states that building a latrine at a secondary school decreases dropout by 5.3 percentage points in upper primary school, presumably at an even lower cost per pupil. Thus, it is possible that targeted, proven educational interventions could mitigate the negative effects of workfare on the opportunity cost of education for adolescents.

It is also worth noting that in Figure 1, if anything, it appears that adolescents in early wave districts are actually more likely to be enrolled in 2009 and 2010 than at baseline, though not significantly so. This could be consistent with some fadeout of the initial effects of the program (since in 2009, for instance, Wave 3 districts would only have had the program in place for one year, and Wave 1 districts would have had it for three years). Unfortunately, our main identification strategy is not set up to identify differential effects of the program over time. Our results on exposure from the ASER data (in Table 6) indicate that, if anything, longer exposure to the program in adolescence seems to be worse for educational outcomes, though exposure earlier in life is beneficial. It is also possible, however, that the changes in education in 2009–2010 are not due to NREGS at all, but to the Right to Education Act that coincided with an increase in children of all ages reporting schooling as their primary activity, as shown in Shah and Steinberg (2019). This is consistent with Figures A2 and A3 in the Online Appendix. Ultimately, the causes of increases in schooling that occurred after the rollout of NREGS are beyond the scope of this paper.

VIII. Conclusion

This work 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 for children aged 13–17, and that this was likely caused by boys responding to the increase in labor demand by working outside the

15. This assumes an 8 percent return to schooling on a base of 116 Rupees per day, or 29,225 Rupees per year with 250 working days. Both these figures are calculated using the NSS data from 2001–2010 for rural adults aged 18–65. Average wages are calculated for those with eight years of education. We assume 40 years of work and a 10 percent discount rate.

16. Another way to think about this is that fully altruistic households that internalized the return to education would need a 90 percent discount rate to rationalize giving up one year of education for the wage increases induced by NREGS.

home, and girls substituting for their mothers into domestic work. We also show that when young children were exposed to the program in utero to age four, there are positive impacts on human capital outcomes for children later in life, most likely because of an income effect.

It is worth noting that NREGS was designed not just to decrease poverty, but also to increase women's labor force participation, earnings potential, and empowerment. These results suggest, however, that the program 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 substituting for their mothers at home. Based on our estimates, for every 20 adult women induced into the labor force by NREGS, four adolescent girls may have dropped out of school. Nearly all of them go into full-time domestic work in their parents' homes. However, we note the caveat that there is significant heterogeneity across India and that we are reporting all India averages in this paper.

We also note that this analysis represents the effect of NREGS on one particular outcome that may be of interest to policymakers (human capital investment). While 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 antipoverty program.

Rather, the takeaway from these results is that social programs have price effects and that these price effects can have important 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. Alik-Lagrange and Ravallion (2015) show 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. 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.

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