Spillover Impacts on Education from Employment Guarantees *

Anjali Adukia
University of Chicago

March 2019

Abstract

Programs that guarantee some basic level of low-skill employment are a popular anti-poverty strategy in developing countries, with India's employment-guarantee program (MGNREGA) employing adults in 23% of Indian households. An important concern is these employment programs may discourage children's education and, thus, more-sustained long-run income growth. Using large-scale administrative data and household survey data, I estimate precise spillover impacts on education that reject substantive declines in children's education from the rollout of MGNREGA. These negative spillovers are inexpensive to counteract, and small compared to positive effects of MGNREGA on rural employment and poverty alleviation.

KEYWORDS: *Education, human capital, India*

---

*Contact: adukia@uchicago.edu, http://home.uchicago.edu/adukia, University of Chicago, Harris School of Public Policy. For helpful feedback, I thank Liz Cascio, Eric Edmonds, Alejandro Ganimian, Richard Hornbeck, Caroline Hoxby, Ofer Malamud, and others including seminar participants at AEFP, APPAM, CIES, and the University of Chicago. For financial support, I thank the Population Research Center (2015-16), the National Institute of Child Health and Human Development (grant R24 HD051152-05), Grace Tsiang (2014-15), and the UChicago Becker-Friedman Institute Development Economics Initiative. For excellent research assistance, I thank Anjali Desai, Chuni Fann, Yuan Fei, Luis Herskovic, Isabella Lee, Sophia Mo, Olga Namen, and Steven Walker. For their help with data acquisition, I thank Arun Mehta, Shalender Sharma, Aparna Mookerjee, Wilima Wadhwa, and Manjistha Banerji.

The rural poor in developing countries can sometimes be in critical need of basic income support, yet supporting low-skill worker incomes may discourage educational attainment and sustained long-term income growth. Many developing-country governments support labor-intensive production to provide low-skill income opportunities, often through direct public works programs. These programs follow a long tradition of linking government income support with work requirements, such as the English Poor Laws and American New Deal work programs.

India's MGNREGA program is the world's largest employment-guarantee program, providing annual employment to adults in 53 million households or 23% of Indian households (MSPI, 2009; NCAER, 2009).[1] The program guarantees 100 days of minimum wage labor on public works projects, at a cost of USD$8.6 billion or 0.5% of GDP. The program provides basic income support to poor households, increasing rural wages and household consumption (Azam, 2012; Berg et al., 2012; Zimmerman, 2012; Deininger and Liu, 2013; Bhupal and Sam, 2014; Imbert and Papp, 2015; Ravi and Engler, 2015; Muralidharan, Niehaus and Sukhtankar, 2017). An important consideration is whether MGNREGA discourages children's educational attainment and long-term income growth, and particularly whether the magnitude of this spillover impact on children's education is substantial relative to immediate adult employment opportunities provided by MGNREGA. Estimating the effects of MGNREGA on educational outcomes has been a recent active research area (Das and Singh, 2013; Mani et al., 2014; Islam and Sivasankaran, 2015; Li and Sekhri, 2015; Shah and Steinberg, 2015; Afridi, Mukhopadhyay and Sahoo, 2016; Das, 2018), as MGNREGA employs millions within India and represents an important test case.

I analyze the spillover impacts on children's educational attainment from the introduction of adult employment guarantees through MGNREGA, which was rolled out across districts between 2006 and 2008. The program was initially introduced in historically poorer districts, though several political considerations influenced which districts first received MGNREGA

[1]MGNREGA stands for the Mahatma Gandhi National Rural Employment Guarantee Act.

even after controlling for the targeted measures of historical poverty. Comparing districts that first received MGNREGA to similarly poor districts in the same states, education outcomes had been trending similarly prior to the introduction of MGNREGA. Using a school-level administrative census (DISE) and a large household-level survey (ASER), the empirical analysis has statistical power to estimate precise impacts of MGNREGA rollout on children's school enrollment and academic achievement.

I estimate that MGNREGA does not substantially reduce children's education. The estimated impacts are generally negative, and some are statistically significant, but the estimated magnitudes are small and sufficiently precise to reject substantive declines in education from the introduction of MGNREGA. In particular, I calculate that a one-child decline in school enrollment is associated with providing adult employment to 43 households (using DISE data) or 50 households (using ASER data). The estimated confidence interval rejects, at the 5% level, a one-child decline in enrollment being associated with providing employment to fewer than 19 households. I estimate that these incidental declines in student enrollment could be counterbalanced by directing less than 0.35% of MGNREGA expenditures toward education interventions. Under current MGNREGA policy, however, I estimate little impact of MGNREGA spending on school infrastructure and teachers.

The impacts of MGNREGA on children's education helps to provide insight into the factors that may push children to leave school (Atkin, 2016; Cascio and Narayan, 2017; Shah and Steinberg, 2017). The impacts of MGNREGA are sometimes larger for older children, which could reflect several mechanisms. While adolescent children would not generally have been employed directly by MGNREGA, as employment was restricted to those over 18, the program may influence low-skill market wages of adolescents and adolescent males in particular. Further, MGNREGA specifically targeted adult female employment and, by drawing household adult females into working outside the home, adolescent females may have increasingly worked in non-market domestic labor and childcare. I estimate little difference in effects by child gender, suggesting both mechanisms may be operating. As a potential countervailing force, MGNREGA also may have encouraged families to invest in their children's education by increasing household income and financial security (Das, 2018).

In estimating the impacts of MGNREGA, or other policy initiatives, I emphasize the importance of adjusting the empirical analysis to the context in which the policy was introduced. In this setting, it is important to control for the targeted rollout of MGNREGA to historically poorer districts and particular regions, which were otherwise experiencing differential changes in education outcomes.[2] Other research has omitted these controls, which increases the estimated impacts of MGNREGA on education (see Section V for details).

My interpretation focuses on the magnitude of the estimated spillover effects on education, in contrast to the statistical significance of the estimates emphasized by other research on this topic. Some of the estimated impacts on children's education are statistically significant, but the substantive magnitude of these spillover effects is of greater importance in the context of growing concerns about whether negative spillover impacts of employment-guarantee programs on education might restrict future income prospects for children. It is important to clarify for policy decisions that these spillover effects on education are small in magnitude.

There is an important distinction between estimated effects that are statistically significant and estimated effects that are substantial in a policy-relevant sense. The estimated declines in education are not "substantial" in the particular sense that I estimate many households are employed through MGNREGA for every one-child decline in school enrollment. Further, I estimate that the declines in school enrollment could be entirely counterbalanced by directing a small portion of MGNREGA public works investment toward more-targeted education interventions. Given the impacts of MGNREGA on other household outcomes, and associated rationales for the program, the spillover impact on education is not a large cost or an unavoidable cost associated with the introduction of low-skilled employment guarantees to counteract rural poverty. I find no substantive tradeoff between providing income

[2] See also, for example, research by Stephens and Yang (2014) who emphasize the importance of controlling for regional changes in education and labor markets.

support to alleviate current poverty and providing an incentive structure that encourages long-run reductions in poverty through increases in education.

I Impacts of an Employment-Guarantee Program (MGNREGA)

I.A Direct Impacts of MGNREGA on Labor Markets

The introduction of MGNREGA in India follows a long tradition of employment-guarantee programs, in both developed and developing countries, as a mechanism for targeting and distributing money to poor populations. In September 2005, the Government of India announced the National Rural Employment Guarantee Act (NREGA or NREGS) and in 2009 named the program after Mahatma Gandhi (MGNREGA). By 2010, MGNREGA had become the world's largest employment-guarantee program, providing 2.3 billion days of work for adult men and women in 53 million households or 23% of all Indian households. The program ensures minimum income levels in rural districts by guaranteeing up to 100 days of temporary low-skilled work annually to each household, and pays a state-wide minimum daily wage of approximately INR 100 (USD $2).[3]

MGNREGA is estimated to have had substantial labor market impacts in rural districts. MGNREGA increases low-skilled worker wages (Azam, 2012; Berg et al., 2012; Zimmerman, 2012; Imbert and Papp, 2015; Muralidharan, Niehaus and Sukhtankar, 2017) and reduces rural-urban migration of low-skilled workers (Ravi, Kapoor and Ahluwalia, 2012; Imbert and Papp, 2017). These wage impacts are reflected in increased household consumption and expenditure (Deininger and Liu, 2013; Bhupal and Sam, 2014; Ravi and Engler, 2015), reduced exposure to seasonal drops in consumption (Klonner and Oldiges, 2014), reduced adult depression (Ravi and Engler, 2015), and increased child height-for-age and weight-for-age (Uppal, 2009).

MGNREGA targets female labor force participation, reserving one-third of jobs for women and stipulating that women and men be paid equal wages. MGNREGA has been

[3]In practice, the implementation of MGNREGA is susceptible to corruption (Niehaus and Sukhtankar, 2013a,b), which decreases with the use of biometric "Smartcards" (Muralidharan, Niehaus and Sukhtankar, 2016).

found to increase female labor force participation (Azam, 2012; Kar, 2013; Afridi, Mukhopadhyay and Sahoo, 2016) and to empower women to exert greater influence over household expenditures (Khera and Nayak, 2009; Pankaj and Tankha, 2010). MGNREGA worksites are supposed to provide childcare facilities, though these are often unavailable (Bhatty, 2006; Khera and Nayak, 2009; Kar, 2013), and women are encouraged to bring small children to the worksite or leave them with older siblings or other caregivers (Bhatty, 2006).

I.B Indirect Impacts of MGNREGA on Education

MGNREGA helps to alleviate poverty in the short-term, but an important consideration is whether the program discourages educational investment and thereby long-term poverty alleviation (Das and Singh, 2013; Mani et al., 2014; Islam and Sivasankaran, 2015; Li and Sekhri, 2015; Shah and Steinberg, 2015; Afridi, Mukhopadhyay and Sahoo, 2016; Das, 2018). This recent and ongoing literature draws on a variety of datasets and empirical specifications, and Section V discusses in detail how the estimates in this paper relate to this literature.

There are a variety of mechanisms through which MGNREGA may impact children's education, as there are several ways in which the program changes both the returns from schooling and the costs of schooling. Further, children of different ages and genders may be affected differentially by these potential mechanisms. Understanding these differential impacts can help in the design of complementary policies to mitigate any negative spillover impacts of MGNREGA on children's education.

MGNREGA may raise the opportunity cost of schooling for older children, in particular, and for older boys and older girls through separate channels. While older children are not employed by MGNREGA directly, as the MGNREGA work is restricted to adults over the age of 18, older boys may earn more from low-skilled work as MGNREGA increasingly employs other low-skilled workers. One particular consequence of MGNREGA employing adult women is that the resulting loss in household labor may increase the demand for older girls to take on more childcare and domestic responsibilities at home (Bhatty, 2006; GOI, 2009; Palriwala and Neetha, 2009).

MGNREGA may also increase the cost of schooling for younger children, as adult women in the household are encouraged to enter the paid labor force. Women are more often responsible for the schooling of younger children, including their transportation and other logistics, such that less time may be available to support younger children's education.

MGNREGA also potentially lowers the long-run returns to schooling by shifting the local wage distribution to more favor lower-skilled jobs (Berg et al., 2012). Indeed, in the reverse, the increased local availability of high-skilled call-center jobs has been seen to encourage educational attainment in India (Jensen, 2012; Oster and Steinberg, 2013). Further, connecting rural Indian villages to urban centers with higher returns to schooling has led to increased educational attainment (Adukia, Asher and Novosad, forthcoming).

These negative indirect impacts could be counterbalanced, however, by increases in household income that support greater investment in children's education. If households are credit-constrained, then increased parental income reduces the costs of investing in children's education. If households become less subject to seasonal income shocks, then there may be less need to pull children from school at particular times of the year. Increases in household wealth that increase child health, perhaps by decreasing food-insecurity, could also be associated with increased school participation and increased student performance (Uppal, 2009; Deininger and Liu, 2013; Klonner and Oldiges, 2014; Ravi and Engler, 2015).

While the above factors are associated with changes in the demand for education, MGNREGA may also impact the supply of educational services. MGNREGA workers may be employed in construction to improve school infrastructure, and the labor market for teachers may be affected by MGNREGA.

The empirical analysis explores the net impact of these potential mechanisms, and explores heterogeneity by student gender and age that may reflect one mechanism more than another. I also estimate impacts of MGNREGA on school infrastructure and teachers.

II Databases on Indian Education: DISE and ASER

The empirical analysis uses the two main databases on education in India: the District Information System for Education (DISE) and the Annual Status of Education Report (ASER). Details of these datasets are provided below, along with variable definitions.[4]

II.A DISE Database

The District Information System for Education (DISE) is an annual school-level panel administrative dataset in India, overseen by the National University of Educational Planning and Administration and established by India's Ministry of Human Resource Development and UNICEF. These data cover every registered government upper-primary school (6th grade to 8th grade) and primary school (1st grade to 5th grade), in addition to private-aided schools and some unaided private schools.[5] The data are collected through a survey, completed by the school headmaster and checked by government officials, and reflect the school's information as of September 30th in each year (DISE, 2009).[6]

The main outcome variables of interest are: school enrollment, examination outcomes,[7]

[4]For simplicity, I refer to academic years as the first year in the school year (i.e., academic year 2005-06 is referred to as 2005).

[5]DISE data collection began in the mid-1990s, but they do not make the data available until after 2001. There are fluctuations in the data until 2005, after which the database is considered to be the census of government-funded schools.

[6]Headmasters fill out survey forms, which are then checked by cluster and district education officials. District officials compile the DISE data for all schools in a given district and send it to the state office. Each state then collects the information and passes it to the national office. From there, independent post-enumeration teams are sent back to roughly 5% of schools to verify the information.

[7]For a subset of states (16 states), which cover roughly one-third of schools in the main sample, there is information on the number of students who appeared for board exams at the end of primary school and upper-primary school, the number of students who passed these board exams, and the number of students who scored "high marks" on these board exams. These data are disaggregated by student gender. There are missing data on the number of students by gender who appeared for the exam for 0.027% of the observations, so there are small differences in the number of observations in the regression specifications.

teacher characteristics,[8] and school infrastructure.[9] The analysis estimates impacts on all enrolled children (1st to 8th grades), separately by primary school (1st to 5th grades) and upper-primary school (6th to 8th grades), and separately by student gender.

In Table 1, I show that average enrollment in primary and upper-primary schools is 118 children and 39 children, respectively, in 2005 prior to the introduction of MGNREGA. Average enrollment in 8th-grade classes that offer upper-primary-school completion exams is 21 children. On average, in my main sample, 96% of these enrolled children show up to take the exam at the end of the year, 84% pass the exam, and 32% score high marks.

In Appendix Table 1, I show that schools have 4 teachers, on average, of which 37% are female and 80% are "qualified" as defined above. Regarding school infrastructure, in 2005 on average, 29% of schools have electricity, 61% have a school latrine, 85% have access to water, and 20% have tap water.

Because DISE data are based on interviews with school headmasters, there are potential concerns of misreporting and inflated student numbers. Misreporting in DISE would only bias the empirical estimates, however, if it occurs disproportionately in areas that began receiving MGNREGA employment at different points in time (and if this misreporting changes over time). For an independent data source, the empirical analysis also draws on privately-collected ASER data.

II.B ASER Database

The Annual Status of Education Report (ASER) is an annual survey of children's literacy and numeracy, reflecting efforts to provide a standardized framework through which to evaluate

[8]Schools report data on each teacher's gender, qualifications, years of experience, and years at that school. I then construct measures of: the total number of teachers (overall and by gender); the number of "qualified" teachers or those with a Diploma in Elementary Education, a Bachelor's degree of Elementary Education or B.El.Ed., a Bachelor's degree in Education or B.Ed., or a Master's degree in Education or M.Ed. (following DISE (2009)); the number of "experienced" teachers with at least 4 years experience teaching; and the number of "new teachers" (that are in their first year at that school); and teacher turnover (defined as the fraction of teachers that are new to that school in that year).

[9]These data include the presence or absence of electricity, boundary walls, library, regular medical check-ups, ramps, sanitation facilities by type (female-only versus unisex), blackboard, computers, playground, and water source by type (tap, pumped, well).

children. A central distinguishing feature of the survey is that it reaches children in their homes, and so includes children both enrolled in school and not enrolled in school. Since 2005, volunteers have annually surveyed approximately 300,000 households throughout India, covering approximately 600,000 school-age children, making it the largest non-government survey of children in India (ASER, 2015).[10]

The ASER data include measures of children's ability to read and perform basic arithmetic, for those children aged 5 to 16. I use the measured reading score, which ranges between zero (no recognition of letters) and four (able to read a story).[11] I also use the measured math score, which ranges between zero (no recognition of numbers) and three (able to divide numbers).[12] I use a second math word score, available only in years 2006 and 2007, which ranges between zero and two according to how many of two math word problems were answered correctly. Mapping child literacy and numeracy into these cardinal scores is an imperfect but convenient simplifying assumption.[13] The ASER data also include whether the child is enrolled in school.

The main outcome variables are: fraction of children enrolled in school, literacy score, and numeracy scores. The analysis estimates impacts on all school-age children (ages 5 to 16), separately for younger children (ages 5 to 11) and older children (ages 12 to 16), and separately by child gender.

Table 2 shows baseline summary statistics for the main sample drawn from the ASER dataset. In 2005, 94% of school-age children were enrolled in school. Among older children, 89% were enrolled in school. The average math score for younger children was 1.4 (reading score: 2.3) and for older children the average math score was 2.2 (reading score: 3.4).

[10] The survey is overseen by the NGO Pratham. The sampling scheme includes 20 to 30 villages in every rural district of India, sampling villages with a probability in proportion to its population, and then selects from each village 20 households with children. The survey is administered in a village over two days in a given year, typically on a Saturday and Sunday.

[11] The reading score is equal to one if the child can read letters, equal to two if able to read words, and equal to three if the child can read a paragraph.

[12] The math score is equal to one if the child can recognize numbers (one-digit or two-digit) and equal to two if able to subtract numbers.

[13] The simplification is that the change in "ability" is not necessarily the same in moving from recognizing numbers (score of one) and doing subtraction (score of two) as then doing division (score of three).

Because ASER data is collected from families in their homes, there are concerns that households may over-report their children's school participation. Concerns with ASER surveying would only bias the empirical estimates, however, if it differentially affects the measurement of education outcomes in areas that received MGNREGA employment earlier or later and if this misreporting changes over time. The ASER data are also not available to estimate pre-trends in schooling outcomes prior to the introduction of MGNREGA, as ASER data collection began in 2005. The analysis of ASER data is thereby complemented by the analysis of school-level administrative data from DISE.

III Empirical Methodology

III.A Rollout of MGNREGA

In September 2005, the central government of India announced the program that would later be named MGNREGA. The program was then introduced to all rural districts over the following three years: 200 districts in February 2006 ("Phase I"), 130 districts in April 2007 ("Phase II"), and the remaining rural districts in April 2008 ("Phase III"). The central government targeted earlier program rollout to poorer districts, based on their degree of economic "backwardness" as determined by three characteristics: district population share of Scheduled Caste groups and Scheduled Tribe groups in 1991, district-level agricultural wages in 1996-97, and output per agricultural worker in 1990-93 (GOI, 2003).

Early rollout of the program was modified, however, by two main political considerations that will have a role in the empirical analysis. First, the central government wanted the early program rollout to be more spread across states, rather than concentrated in states with the most "backward" districts (UNDP, 2010). While poorer districts received the program earlier, every state had treated districts in the early phase. Therefore, MGNREGA was received earlier by relatively poorer districts within a state. The empirical analysis can then estimate impacts on educational outcomes in districts that receive MGNREGA in a particular year, relative to other districts in the same state, controlling for the absolute level of district "backwardness" that might otherwise be associated with changes in district educational outcomes. For example, poorer districts may have otherwise improved more rapidly or may have been targeted for other policies.

A second political consideration was the national government's goal of directing early rollout of MGNREGA to districts exhibiting "left-wing extremism" (LWE) (Fetzer, 2014). There is a long history of fringe support for communist and Maoist groups in India, who sometimes use violent methods and are associated with the plight of extreme rural poverty. The Congress Party gained control of the national government in 2004, taking over from the more right-wing Bharatiya Janata Party, and the political calculus was that directing MGNREGA toward districts with a history of "left-wing extremism" might help placate that extremism and gain political support for the Congress party. The empirical analysis can use this variation in program rollout due to political considerations, which might not otherwise be associated with changes in district educational outcomes, though robustness checks can also control for a district's association with "left-wing extremism."

III.B Estimating Equation

I estimate the impact of MGNREGA on education outcome Y for school i (or child i) in district d, in state s, and in year t, using the following estimating equation:

$$Y_{idst} = \beta MGNREGA_{dt} + \alpha_d + \lambda_{st} + \gamma_t^1 B_d^{SCST} + \gamma_t^2 B_d^{AW} + \gamma_t^3 B_d^{AE} + \epsilon_{idst}. \tag{1}$$

The variable $MGNREGA_{dt}$ indicates that district d offered MGNREGA in year t. The estimated parameter β is the coefficient of interest. This parameter captures the average effect on school (or child) outcomes from a district offering rural employment guarantees through MGNREGA. If the estimate of β is negative, then the introduction of MGNREGA is indicated to decrease educational outcome Y. I sometimes estimate this equation separately by child gender and/or by child grade (or age), whereby the estimated parameter β then reflects the average effect on educational outcomes for children of that gender and grade (or age).

The estimating equation controls for district fixed effects (α_d), which capture any district-level time-invariant characteristics. The estimated impact of MGNREGA then reflects changes in districts that start receiving MGNREGA in a particular year relative to changes in districts that do not start receiving MGNREGA in that year.

The estimating equation also controls for state-by-year fixed effects (λ_{st}) and the three targeted measures of district "backwardness" (B_d^{SCST}, B_d^{AW}, B_d^{AE}) interacted with a dichotomous variable indicating the academic year.[14] The specification then compares changes in educational outcomes in districts that receive MGNREGA in a particular year to changes in educational outcomes in other districts from the same state in that year, and adjusting for any changes in educational outcomes in similarly "backward" districts in other states relative to changes in other districts from those other states.

The identification assumption is that district educational outcomes would have changed similarly if there had been no rollout of MGNREGA, comparing districts that receive MGNREGA in a particular year to other districts in the same state and after adjusting for changes associated with district "backwardness."

Using the DISE dataset, which is reported at the school level, I analyze a balanced sample of 743,163 schools that appear in the data in each year (2005 to 2009, which includes one year before and after the rollout of MGNREGA). Combining this sample of schools with the available data on district "backwardness," the regression sample includes 437 rural districts: 173 districts from Phase I (2006), 97 districts from Phase II (2007), and 167 districts from Phase III (2008).[15] I cluster the standard errors by district to allow for correlated outcomes across schools and over time within each district.

Figure 1 maps these 437 sample districts. The empirical analysis uses the mapped within-

[14] I collected these measures of district "backwardness" from the 2003 report of the Planning Commission, which was used in targeting districts for MGNREGA rollout. The three measures are: district population share of Scheduled Caste groups and Scheduled Tribe groups from the 1991 census (SCST), district-level agricultural wages in 1996-97 (AW), and output per agricultural worker in 1990-93 (AE).

[15] In some specifications using DISE data, omitting controls for historical district "backwardness," I also analyze a larger sample of 570 rural districts (as in Li and Sekhri, 2015).

state variation in when districts received MGNREGA, conditional on changes associated with district "economic backwardness." The non-sample districts are concentrated in 11 non-sample states in the North and Northeast, which were not included in the planning report on "economic backwardness." Within the sample states, the included districts cover 92% of enrolled students in 2005 and 92% of schools in 2005.

Using the ASER dataset, which is reported at the child level, I include all children aged 5 to 16 in each year (2005 to 2009). In specifications using the ASER data, I control for the age of the child. The ASER sample of children varies in each year, but I restrict the regression sample to the 405 rural districts covered by ASER in each year (2005 to 2009) and with available data on district "backwardness." These 405 districts include 160 districts from Phase I (2006), 90 districts from Phase II (2007), and 155 districts from Phase III (2008).[16] Figure 2 maps these 405 sample districts, which cover 84% of schools in 2005 in the sample states (using DISE data).

In Tables 1 and 2, columns 2 – 4, I report average characteristics for districts that receive MGNREGA in Phase I, Phase II, and Phase III, respectively. Districts in Phase I have slightly lower school enrollment and academic achievement in some characteristics, as Phase I districts are historically poorer, but the average characteristics are similar across districts in these three phases. The empirical analysis estimates changes in these characteristics, controlling for district fixed effects, state-by-year fixed effects, districts' historical "backwardness," and reports relative changes in years prior to the introduction of MGNREGA.

IV Estimated Impacts of MGNREGA

IV.A Estimated Impacts on Enrollment (DISE data)

In Table 3, I report the estimated impacts of MGNREGA on school enrollment. In column 1, panel A, I report that school enrollment decreased by 1.03 students, on average, with the district-wide introduction of MGNREGA. The estimate is not statistically significant

[16] In some specifications using ASER data, omitting controls for historical district "backwardness," I also analyze a larger sample of 468 rural districts (as in Shah and Steinberg, 2015).

at conventional levels, and can reject with 95% confidence a decline in school enrollment of more than 2.30 students.

The estimated coefficient (-1.03) implies that a one-student decline in enrollment was associated with providing MGNREGA employment to 43 households.[17] The lower bound of the estimated 95% confidence interval (-2.30) rejects a one-student decline in enrollment being associated with providing MGNREGA employment to fewer than 19 households.

The estimated coefficient (-1.03) also implies that MGNREGA expenditures of $6,915 were associated with a one-student decline in enrollment.[18] The estimated 95% confidence interval rejects MGNREGA expenditures of less than $3,087 being associated with a one-student decline in enrollment.

By contrast, targeted education interventions in developing countries generally have much greater impact on student enrollment per dollar spent. For example, a one-student increase in enrollment costs approximately $11 through construction of school latrines in rural India (Adukia, 2017). MGNREGA employment is often directed toward infrastructure construction, and so allocating an additional 0.16% of the MGNREGA budget to school latrine construction (or other education initiatives with similar cost effectiveness) would offset the estimated decline in school enrollment associated with providing employment guarantees.[19] At the lower bound of the 95% confidence interval, the negative spillover impact on education could be offset through an additional 0.35% of expenditure. The estimated costs of increasing school enrollment vary across interventions and developing country contexts, with

[17]The regression sample includes 743,163 schools, and total DISE enrollment across all MGNREGA districts is 1.18 times the DISE enrollment in sample schools, so the estimated coefficient of -1.03 implies an approximate nationwide decline in school enrollment of 905,636 students. In fiscal year 2009-10, MGNREGA paid for the employment of 52.6 million households, with 2.8 billion person-days of work. Dividing by an average 87,643 households employed in a district gives approximately 43 households per one-student decline in enrollment.

[18]MGNREGA program expenditures were approximately $159.86 per household (MSPI, 2009), which is then multiplied by 43.26 households employed for a one-student decline in enrollment. I convert Indian Rupees to U.S. Dollars using the average currency conversation rate in December 2009 of $46.52 USD per Indian Rupee.

[19]This number corresponds to the increased spending on school latrines ($11), divided by the resulting total expenditure ($6,915 plus $11). Note that, as of 2005, there was still no latrine in 40% of government schools and, therefore, substantial remaining scope for intervention along this margin (DISE, 2009).

estimates between \$2.81 and \$130.82 per additional student (surveyed in Kazianga et al., 2013), but typical estimates are much smaller than MGNREGA expenditures of \$3,087 per one-student decline in enrollment. Different policy interventions may impact different types of students, but these numbers give a rough sense of overall magnitudes.

IV.B Estimated Impacts on Enrollment (ASER data)

I also report, in Table 3, the estimated impact of MGNREGA on whether a child is enrolled in school based on household ASER data (column 4 and panel A). I estimate a 0.53 percentage point decline in the probability of a child being enrolled in school from the district-wide introduction of MGNREGA. This estimate is not statistically significant, and can reject with 95% confidence a decline in enrollment probability of more than 1.2 percentage points.

The estimated coefficient (-0.0053) implies a district-wide decline in school enrollment of 1,747 children, which is similar to an implied district-wide decline of 2,026 children estimated above in the DISE data.[20] The estimated coefficient (-0.0053) then implies that a one-student decline in enrollment was associated with providing MGNREGA employment to 50 households or MGNREGA expenditures of \$8,019, and could potentially be offset by an additional 0.14% of expenditure.

The estimated magnitude and statistical precision reject small impacts on overall student enrollment, similar to the above estimates using DISE data. The estimated 95% confidence interval also rejects a one-student decline in enrollment being associated with providing MGNREGA employment to fewer than 21 households (or expenditures of less than \$3,495, which could potentially be offset by an additional 0.31% of expenditure).

[20]The ASER data include in each district, on average, 741 school-age children (aged 5 to 16) and 693 children in school (or 93.5%). It is not known exactly how many school-age children there are in 2005 in these districts, but the DISE data report district-wide enrollment of 308,439 children and so the ASER data on school enrollment implies there may be approximately 329,672 school-age children (or 308,439 divided by 0.935). Multiplying this number of school-age children by 0.53 percentage points gives an estimated decline in district-wide enrollment of 1,747 children.

IV.C Estimated Impacts on Enrollment, by Age and Gender

I estimate similar enrollment effects on older children and younger children (Table 3, panels B and C) in the DISE data (column 1) and in the ASER data (column 4). For the DISE data, the subgroup effects should approximately add up to the overall effect on the level of enrollment. For the ASER data, the subgroup effects should approximately average to the overall effect on the probability of enrollment.

I also estimate similar enrollment effects on females and males (Table 3, panel A) in the DISE data (columns 2 and 3) and the ASER data (columns 5 and 6).[21] There are some moderate differences in the estimates, broken out separately by child age and gender, though the estimates are not statistically or substantively different from each other.

IV.D Robustness of Enrollment Effects

Left-Wing Extremism. The main empirical estimates use variation in MGNREGA roll-out timing due to political considerations, partly related to the intensity of "left-wing extremism" in the district as determined by the government. One concern is that educational outcomes might otherwise have changed differently in these areas exhibiting "left-wing extremism." In Table 4, I report similar estimates to Table 3 when controlling for a measure of local "left-wing extremism" interacted with year.[22] This measure of "left-wing extremism" is associated with districts receiving MGNREGA earlier, and so the similarity of these estimates implies that local "left-wing extremism" is not associated with changes in local educational outcomes.

[21] The total sample size is slightly decreased for the ASER analysis, as data on gender are missing for 1% of children.

[22] I proxy for local "left-wing extremism" using a 2013 report from India's Ministry of Home Affairs, which includes a list of districts associated with particular concerns of "left-wing extremism" (GOI, 2013). While it would be preferable to have data on "left-wing extremism" from before 2005, this long-standing political movement has persisted in particular areas and so these data may reasonably proxy for a district's general intensity of "left-wing extremism."

Functional Form. In Table 5, I report estimated impacts on the natural logarithm of school enrollment, rather than the level of school enrollment as in columns 1 to 3 of Table 3.[23] The estimated magnitudes then reflect approximate percentage changes in school enrollment, and are of a similar magnitude to the estimated percentage changes in whether a child is enrolled in school when using ASER data (Table 3, columns 4 to 6).

Migration. MGNREGA has been estimated to reduce seasonal rural-to-urban migration, such that decreases in school enrollment could be counterbalanced by increased numbers of children in these rural districts. These demographic changes would not directly affect the estimated impacts on enrollment rates using the ASER data, however, which found similar implied effects on district-wide enrollment numbers in the DISE data. The estimated migration impacts of MGNREGA focus on within-district short-term migration from rural areas to urban areas. By contrast, the estimated impacts on school enrollment would only be affected by cross-district migration, and more-permanent migration associated with migration of children, which is less frequent (Topalova, 2010; Ravi, Kapoor and Ahluwalia, 2012; Munshi and Rosenzweig, 2016; Imbert and Papp, 2017).[24]

IV.E Relative Changes in Enrollment, Prior to MGNREGA (Pre-Trends)

A natural check on the empirical research design is whether enrollment had been changing similarly in districts from different phases of MGNREGA rollout, in years prior to the rollout of MGNREGA. For a subset of schools in the DISE sample, which are observed continuously back to 2002, it is possible to measure these pre-trends in school enrollment.[25]

[23] Because some schools have zero enrollment or very low enrollment in some years, especially for particular genders and ages, I report estimated impacts on the natural logarithm of enrollment plus one.

[24] Using the Indian Population Census, rural-to-urban migration rates among young males (aged 15 to 24) were 5.4 percent from 1991 to 2001 (Munshi and Rosenzweig, 2016). Topalova (2010) notes that geographic mobility in India is lowest among the poorest households, and shows that cross-district migration plays little detectable role in Indian labor markets. Using NSS data, from 1987 to 1999, Topalova reports that 3 to 4 percent of rural residents moved across districts or to a different sector (rural-to-urban or urban-to-rural) within the prior 10 years.

[25] The ASER data collection began in 2005, and so ASER data are not available in multiple years before the rollout of MGNREGA. For DISE data, prior to 2005, the coverage of schools fluctuates, but I restrict the sample to a balanced panel of schools from my main sample that are also in the data continuously from 2002 through 2005.

In Table 6, I report estimated relative changes in school enrollment prior to the introduction of MGNREGA in: Phase I districts relative to Phase II districts (column 1), Phase I districts relative to Phase III districts (column 2), and Phase II districts relative to Phase III districts (column 3). As in my main empirical specification, these specifications control for district fixed effects, state-by-year fixed effects, and the three targeted measures of district "backwardness" interacted with year. I report estimated changes in levels (panel A) and in logs (panel B).

There is no strong pattern of increasing or decreasing school enrollment in districts that would later go on to receive MGNREGA earlier (Phase I relative to Phase II, Phase I relative to Phase III, Phase II relative to Phase III). There is some indication of enrollment increases in districts that would go on to receive MGNREGA earlier, though the estimates are not statistically significant. These estimates are less precise than my main estimates, in part because the sample size is smaller. The schools in this sample are also slightly larger than all schools in the main sample.

IV.F Estimated Impacts on Academic Achievement

Table 7 reports estimated impacts of MGNREGA on child math and reading ability, using the ASER data. The introduction of MGNREGA, and small declines in school enrollment, is associated with small and statistically insignificant declines in measured math and reading ability. For all children, the point estimates represent a decline of 0.003 standard deviations in math score (in column 1), 0.014 standard deviations in math word score (in column 4), and 0.004 standard deviations in reading score (in column 7). These effects represent small impacts on child learning.

The estimates reject, at the 95% confidence level, a decline of more than 0.036 standard deviations in math score, 0.094 standard deviations in math word score, and 0.037 standard deviations in reading score. MGNREGA spending in these districts was the equivalent of $61 per student. For similarly impactful spending to change math scores, math word scores, or reading scores by 0.2 standard deviations, the estimated 95% confidence intervals imply that spending would need to increase by more than \$339, \$130, or \$330 per student, respectively. By comparison, total public educational spending in India was only \$296 per student (MHRD, 2013).

The estimated effects of MGNREGA are more negative for older children, and marginally statistically significant for reading ability, but the effect magnitudes remain small at between 0.017 and 0.036 standard deviations. For older children, the estimates reject a decline of more than 0.059 standard deviations in math score, 0.099 standard deviations in math word score, and 0.076 standard deviations in reading score.

For younger children, the estimated effects are smaller and reject declines of more than 0.026 to 0.110 standard deviations in child learning. The estimates are similar by child gender.

Table 8 reports estimated impacts on student exam performance, using the DISE data. There are small and statistically insignificant declines in the number of students appearing for primary-school completion exams and upper-primary-school completion exams, which effectively provide a measure of school enrollment at the end of the academic year. The estimated declines are largest for older girls, and marginally statistically significant, though the estimated magnitude remains small (-0.278) and is similar to the estimated decline in school enrollment for older girls (-0.286) reported in Table 3. The estimated declines in the number of children passing each exam, and the number of children scoring high marks on each exam, are similarly small and statistically insignificant. These magnitudes are interpreted similarly to the enrollment effects discussed from Table 3.

IV.G Estimated Impacts on Teachers and School Infrastructure

Table 9 reports little estimated impact of MGNREGA on school teachers. There is perhaps a small decline in the number of teachers, corresponding to the small declines in school enrollment, though the estimated magnitudes are not statistically significant. The estimates are fairly precise, however, as the estimates in column 1 reject with 95% confidence a decrease of 0.07 teachers per school, or 1.7% of the average number of teachers per school. While previous research has found estimated impacts of MGNREGA on low-skilled labor market outcomes, these impacts do not naturally translate into impacts on the labor market for school teachers.

Table 10 reports little estimated impact of MGNREGA on school infrastructure. The estimated magnitudes are small, relative to the standard deviation in infrastructure across schools, and mostly statistically insignificant. The estimates are sufficiently precise to reject meaningful impacts, relative to the baseline mean and standard deviation of the infrastructure outcomes. MGNREGA does not appear to crowd-out investment in school infrastructure or encourage further investments in school infrastructure.

Overall, impacts on teachers and school infrastructure do not appear to be substantial mechanisms through which MGNREGA affects student outcomes.

V Interpretation and Connection to Other Research

My empirical estimates suggest that the potential education-employment tradeoff is small in magnitude, and relatively inexpensive to counteract through compensatory efforts. In considering this potential tradeoff between educational investment and lower-skill employment guarantees through MGNREGA, I estimate some statistically significant spillover effects of MGNREGA on education. However, my empirical estimates reject substantive declines in educational outcomes from the introduction of MGNREGA. Given impacts of MGNREGA on other household outcomes, and associated rationales for the program, the spillover impact on education is not a substantively large cost associated with the introduction of low-skilled employment guarantees to counteract rural poverty.

Estimating the spillover effects of MGNREGA on educational outcomes has been a recent active research area, as MGNREGA represents the world's largest employment guarantee program. The program is directed toward alleviating rural poverty in millions of households, yet an important consideration is whether these short-term efforts at poverty alleviation might worsen long-run income growth for these millions of households. Given the data availability in India, the example of MGNREGA also represents an important test case more broadly.

Other research emphasizes the existence of a negative impact from MGNREGA on educational outcomes, using DISE data (Li and Sekhri, 2015) and ASER data (Shah and Steinberg, 2015). These papers emphasize statistically significant effects, though I calculate below that these other estimates do not imply substantive declines in educational outcomes. These papers use similar empirical specifications, though they omit some important control variables that reflect the context in which MGNREGA was introduced. Below, I show that omitting these control variables increases the estimated effects.

Table 11 reports the sensitivity of my estimated impacts on enrollment (in columns 1 and 4) to omitting state-by-year fixed effects (in columns 2 and 5) and then also omitting controls for district "backwardness" (in columns 3 and 6). The specification in column 2 is more similar to Li and Sekhri (2015) and the specification in column 6 is more similar to Shah and Steinberg (2015). Table 12 reports the sensitivity of my estimated impacts on academic achievement. Appendix Tables 2 and 3 report these estimates, separately by child gender and age.

These tables report larger estimates, which are more often statistically significant, and these estimated magnitudes are similar to those in Li and Sekhri (2015) and Shah and Steinberg (2015). The estimates are generally within one standard error of my previous estimates, with the greatest increase in estimated magnitudes for older children. Appendix Tables 4 and 5 report similar estimates to those in Tables 11 and 12, when expanding the regression sample to 570 districts (DISE data) and 468 districts (ASER data). By including additional districts that do not have data on historical "backwardness," these sample districts from DISE and ASER then correspond to the sample districts used by Li and Sekhri (2015) and Shah and Steinberg (2015), respectively.

I calculate that these larger estimates continue to suggest only small declines in school enrollment, however, from providing employment to rural households. The larger estimated impacts on enrollment, in columns 3 and 6 of Table 11, imply that a one-student decline in enrollment is associated with providing MGNREGA employment to 26 households (using DISE data, in row 1, column 3) or 34 households (using ASER data, in column 6). The estimated magnitudes, and standard errors, reject at the 5% level a one-student decline in enrollment from providing employment to fewer than 14 households (using DISE data, in column 3) or fewer than 19 households (using ASER data, in column 6). The larger estimated impact rejects a one-student decline in enrollment from MGNREGA spending of $2,243 (using DISE data, in column 3), which could perhaps be offset by an additional 0.49% in spending on education infrastructure.[26]

I also calculate that these estimates suggest only small declines in academic achievement from providing employment to rural households. The introduction of MGNREGA is estimated to decrease average test scores by 0.01 standard deviations (math score, in column 3 of Table 12), 0.02 standard deviations (math word score, in column 6 of Table 12), and 0.007 standard deviations (reading score, in column 9 of Table 12).[27] These estimates reject, at the 5% level, declines in average test scores of 0.04 standard deviations in math score, 0.125 standard deviations in math word score, and 0.03 standard deviations in reading score. The estimated impacts are larger for older children, but reject declines in average test scores of 0.07 standard deviations (math score), 0.115 standard deviations (math word score), and 0.07 standard deviations (reading score). For younger children, the estimated effects are smaller and reject declines of 0.02 to 0.16 standard deviations. The estimated effects are similar by child gender.

Other research uses different datasets, estimating mixed impacts of MGNREGA on educational outcomes. Using data from the District Level Household and Facility Survey (DLHS), Das and Singh (2013) estimate no impact of MGNREGA on children's completed years of education, using a similar estimating equation as in columns 3 and 6 of Table 11.[28]

[26] These calculations replicate those in Section IV, under different estimated magnitudes and standard errors.

[27] These estimated impacts on math score and reading score are similar magnitudes to those in Shah and Steinberg (2015). The estimated impact on math word score is larger in Shah and Steinberg (2015), though this math word score is constructed using a different scale.

[28] Das (2018) estimates no impact of MGNREGA on schooling in West Bengal, but estimates increases in

Using data from the National Sample Survey (NSS), Islam and Sivasankaran (2015) estimate increased time spent on education for younger children and increased time spent working outside the household for older children, using a similar estimating equation as in columns 3 and 6 of Table 11. Using the NSS data, Shah and Steinberg (2015) estimate increases in adolescent male paid child labor and increases in adolescent female unpaid domestic labor, which is consistent with increases in NSS child labor estimated by Li and Sekhri (2015). Using data from the Young Lives Survey, from the Indian state of Andhra Pradesh, Mani et al. (2014) estimate no impact of MGNREGA on school enrollment and positive impacts on test scores. Using these data from Andhra Pradesh, Afridi, Mukhopadhyay and Sahoo (2016) estimate that greater participation in MGNREGA by children's mothers (relative to their fathers) increases their school attendance and grade progression. In the DISE and ASER data, for Andhra Pradesh only, I estimate positive but imprecise effects on school enrollment and learning outcomes.

Across these papers, datasets, and empirical specifications, the estimates generally range from there being no impact of MGNREGA on educational outcomes to there being small negative impacts of MGNREGA on educational outcomes. Using DISE and ASER, the two largest datasets on educational outcomes across India, there are some indications of negative and statistically significant impacts on some educational outcomes for some subsets of children. Even for these groups, however, the estimated magnitudes are small and reject substantive declines in educational outcomes. These small declines in educational outcomes come from providing employment guarantees to large numbers of rural households, which has been estimated to have substantial impacts on poverty alleviation for those households.

VI Conclusion

A tradeoff can sometimes exist between providing income support to alleviate current poverty and providing an incentive structure that encourages long-run reductions in poverty. MGNREGA is the world's largest employment guarantee program, providing annual employment

household expenditures on tutors.

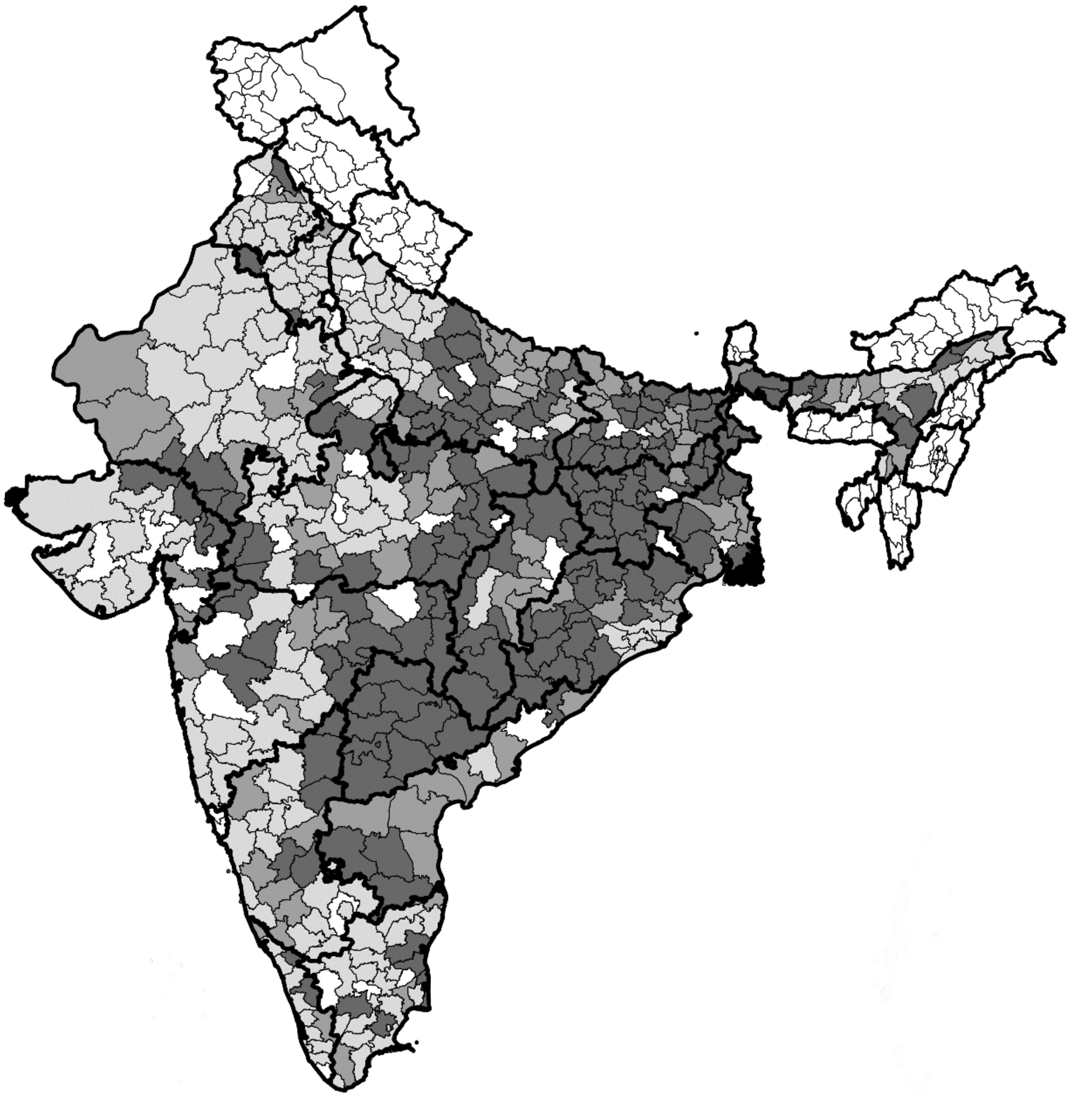
to 53 million households or 23% of Indian households. The program raises rural wages and household consumption, particularly during critical moments for those households. A potentially important concern, however, is whether providing low-skill employment might discourage educational attainment and, thereby, long-run income growth. MGNREGA has expanded to take on a central role in rural Indian labor markets, also serving as a large test-case for other developing countries.

My estimates imply that MGNREGA can provide income support to rural poor households without an associated substantive decline in children's educational attainment. Some estimates indicate small negative spillover effects on education outcomes, which are sometimes statistically significant, but the estimates can reject substantive declines in educational attainment. I reject, at the 5% level, that a one-child decline in educational enrollment is associated with providing MGNREGA employment to 19 households or fewer. These incidental declines in student enrollment could also be counterbalanced by directing less than 0.35% of MGNREGA expenditures toward more-targeted education interventions.

The estimated spillover impacts of MGNREGA on education may reflect a variety of mechanisms, which can inform why children leave school in lower-income settings. Increased low-skill wages may encourage adolescent males to seek paid employment, while employment of adult females through MGNREGA may increase domestic responsibilities of adolescent females, in particular. I estimate little difference in effects by child gender, suggesting both mechanisms may be operating. Any negative spillover impacts on education may also be mitigated as households gain financial security through MGNREGA and become more able to invest in their children's education. MGNREGA does not seem to have directly changed schools, based on measures of school infrastructure and teacher characteristics.

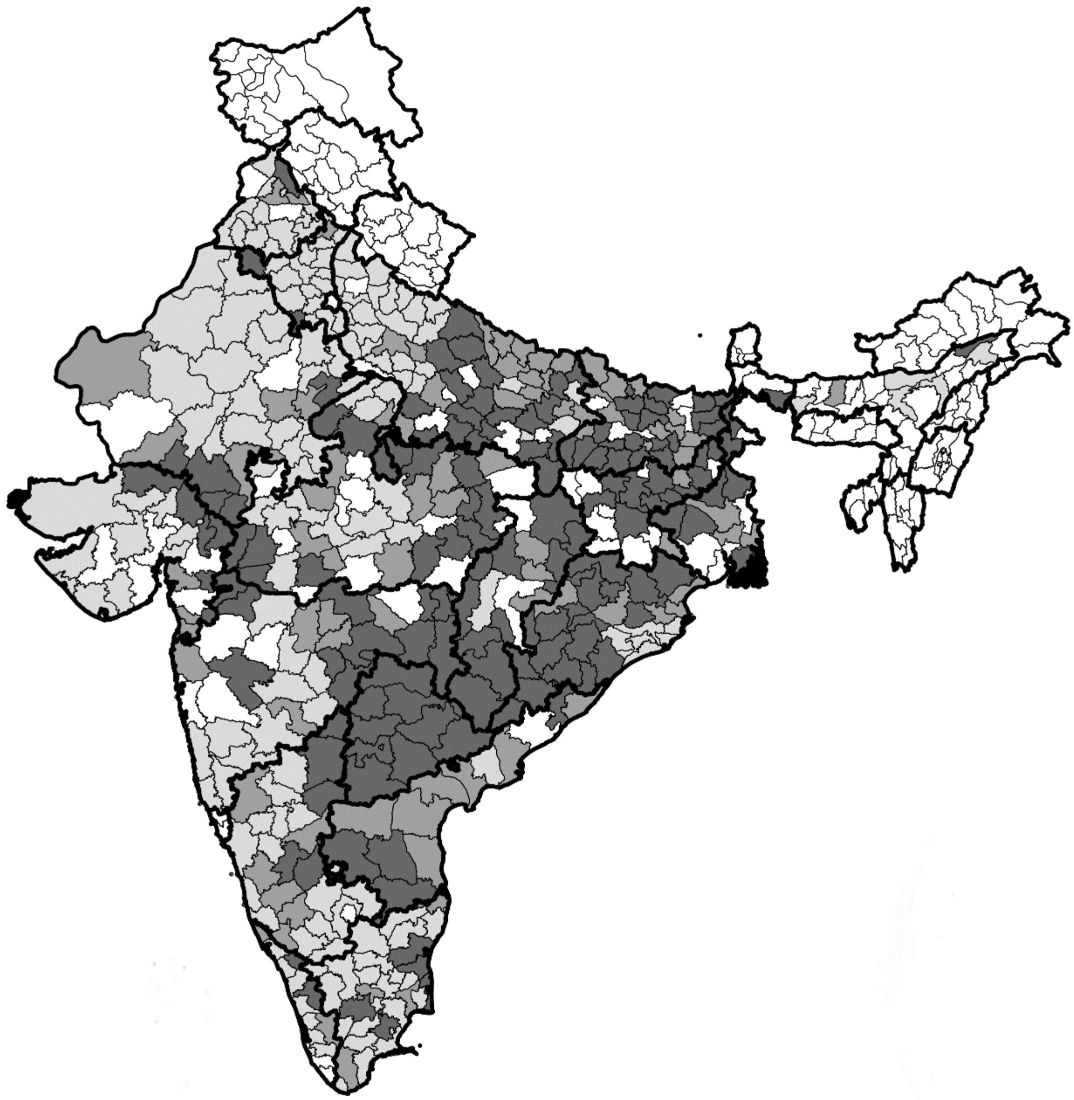
In recent years, political pressure has been building against MGNREGA. Among the concerns about its growing cost and implementation challenges, there are concerns about whether negative spillover impacts on education might restrict future income prospects for children. These spillover effects on education are small in magnitude, however, which is important to clarify for informing policy decisions. The estimated spillover effects on education may affect many children, in absolute numbers, but affect few children relative to the larger number of households (and children) receiving income support through MGNREGA employment. Further, these spillover effects can be counteracted by directing a small portion of MGNREGA's public works budget toward the construction of school infrastructure. Employment guarantees and low-skill income support for households can address short-run poverty alleviation without substantively reducing long-run sustained income growth through educational attainment.

Figure 1. Map of Sample Districts using DISE Data



Notes: Figure 1 shows the 437 sample districts that are represented in the school-level DISE dataset. The districts that are shaded with the darkest grey received MGNREGA in Phase I. The districts that are shaded with the medium grey received MGNREGA in Phase II. The districts that are shaded with the lightest grey received MGNREGA in Phase III. The districts that are not shaded (white) are excluded from the main analysis sample and are included in the analysis in Appendix Tables 4 and 5.

Figure 2. Map of Sample Districts using ASER Data



Notes: Figure 2 shows the 405 sample districts that are represented in the household-level ASER dataset. The districts that are shaded with the darkest grey received MGNREGA in Phase I. The districts that are shaded with the medium grey received MGNREGA in Phase II. The districts that are shaded with the lightest grey received MGNREGA in Phase III. The districts that are not shaded (white) are excluded from the main analysis sample and are included in the analysis in Appendix Tables 4 and 5.

Table 1. Baseline School Characteristics: DISE Dataset

	All Districts (1)	Phase I Districts (2)	Phase II Districts (3)	Phase III Districts (4)
Enrollment				
Enrollment - All Students	156.2	153.2	163.3	155.3
	(153.2)	(151.4)	(159.1)	(151.3)
All Female Students	73.6	72.2	76.9	73.3
	(78.5)	(75.7)	(81.7)	(79.7)
All Male Students	82.5	80.9	86.4	81.9
	(90.2)	(88.9)	(94.6)	(88.9)
Primary-School Enrollment	117.6	116.6	125.1	114.0
	(116.5)	(114.3)	(119.8)	(116.7)
Primary-School Females	56.1	55.6	59.5	54.6
	(58.1)	(56.1)	(58.9)	(59.8)
Primary-School Males	61.5	61.0	65.6	59.4
	(65.6)	(64.1)	(67.9)	(65.7)
Upper-Primary-School Enrollment	38.6	36.6	38.2	41.3
	(93.0)	(92.3)	(96.9)	(91.2)
Upper-Primary-School Females	17.5	16.6	17.4	18.8
	(48.3)	(46.8)	(51.1)	(48.3)
Upper-Primary-School Males	21.1	20.0	20.9	22.5
	(55.4)	(54.8)	(58.1)	(54.4)
Exam Outcomes (Number of Students)				
End of Primary School				
Enrolled in 5th Grade	28.6	28.2	30.6	27.6
	(46.0)	(39.1)	(61.1)	(40.6)
Appeared for Exam	26.7	26.0	28.3	26.3
	(41.8)	(35.3)	(53.4)	(39.0)
Passed Exam	25.4	24.9	26.7	25.1
	(43.3)	(45.2)	(48.1)	(37.4)
Scored High Marks on Exam	12.4	11.7	13.2	12.8
	(24.6)	(22.4)	(24.6)	(26.6)
End of Upper-Primary School				
Enrolled in 8th Grade	20.9	19.9	19.2	23.0
	(61.5)	(59.7)	(77.7)	(50.0)
Appeared for Exam	20.0	18.9	18.4	22.3
	(56.6)	(50.1)	(74.3)	(48.5)
Passed Exam	17.5	16.4	15.9	19.9
	(49.6)	(43.6)	(63.8)	(44.3)
Scored High Marks on Exam	6.6	5.9	5.5	8.1
	(21.1)	(18.3)	(23.8)	(21.9)

Notes: This table reports baseline average characterics for schools in the sample that draws from the DISE data. In column 1, I report the average values of children in all districts at baseline (2005). In columns 2, 3, and 4, I report the average values of children in districts treated in Phase I, Phase II, and Phase III, respectively, at baseline (2005). Primary school refers to grades 1 through 5. Upper-primary school refers to grades 6 through 8. Standard deviations are reported in parentheses.

Table 2. Baseline Child Characteristics: ASER Dataset

	All Districts (1)	Phase I Districts (2)	Phase II Districts (3)	Phase III Districts (4)
Enrollment				
School-Age Children	0.94	0.92	0.94	0.95
	(0.25)	(0.27)	(0.24)	(0.22)
All Females	0.93	0.91	0.93	0.94
	(0.26)	(0.28)	(0.26)	(0.24)
All Males	0.94	0.93	0.94	0.95
	(0.23)	(0.25)	(0.23)	(0.21)
Younger Children	0.95	0.94	0.96	0.97
	(0.21)	(0.23)	(0.21)	(0.18)
Younger Females	0.95	0.94	0.95	0.96
	(0.22)	(0.25)	(0.22)	(0.19)
Younger Males	0.96	0.95	0.96	0.97
	(0.20)	(0.22)	(0.19)	(0.17)
Older Children	0.89	0.87	0.89	0.91
	(0.31)	(0.33)	(0.31)	(0.29)
Older Females	0.87	0.86	0.88	0.89
	(0.33)	(0.35)	(0.33)	(0.31)
Older Males	0.90	0.89	0.90	0.92
	(0.29)	(0.32)	(0.30)	(0.27)
Math Score (0-3)				
School-Age Children	1.62	1.54	1.61	1.72
	(1.13)	(1.14)	(1.13)	(1.12)
Younger Children	1.37	1.30	1.35	1.47
	(1.08)	(1.08)	(1.08)	(1.08)
Older Children	2.23	2.14	2.24	2.31
	(1.02)	(1.06)	(1.00)	(0.97)
Math Word Score (0-2)				
School-Age Children	1.69	1.67	1.72	1.71
	(0.64)	(0.67)	(0.62)	(0.63)
Younger Children	1.55	1.54	1.59	1.56
	(0.75)	(0.76)	(0.73)	(0.75)
Older Children	1.76	1.73	1.79	1.77
	(0.58)	(0.61)	(0.55)	(0.56)
Reading Score (0-4)				
School-Age Children	2.59	2.50	2.56	2.71
	(1.49)	(1.51)	(1.49)	(1.45)
Younger Children	2.27	2.19	2.23	2.39
	(1.48)	(1.50)	(1.48)	(1.46)
Older Children	3.38	3.30	3.39	3.45
	(1.17)	(1.24)	(1.16)	(1.11)

Notes: This table reports baseline average charactecrics for children in the sample that draws from the DISE data. In column 1, I report the average values of children in all districts at baseline. In columns 2, 3, and 4, I report the average values of children in districts treated in Phase I, Phase II, and Phase III, respectively, at baseline. All values are reported for 2005 except for math word score, which is first collected in 2006. Younger children refer to those between the ages of 5 and 11 years old. Older children refer to those between the ages of 12 and 16 years old. Standard deviations are reported in parentheses.

Table 3. Effect of MGNREGA on School Enrollment, by Child Gender and Age/Grade

	Enrollment Impacts by School (DISE Data)			Enrollment Impacts by Child (ASER Data)		
	All	Females	Males	All	Females	Males
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Children						
MGNREGA	-1.026	-0.535	-0.491	-0.0053	-0.0053	-0.0051
	(0.649)	(0.333)	(0.345)	(0.0035)	(0.0038)	(0.0036)
Observations	3,715,815	3,715,815	3,715,815	2,164,445	966,000	1,177,308
R-squared	0.156	0.133	0.134	0.074	0.087	0.069
Panel B: Older Children						
MGNREGA	-0.420	-0.286	-0.135	-0.0079	-0.0043	-0.0098
	(0.234)	(0.122)	(0.123)	(0.0053)	(0.0061)	(0.0055)
Observations	3,715,815	3,715,815	3,715,815	805,998	355,509	442,341
R-squared	0.035	0.031	0.028	0.075	0.090	0.071
Panel C: Younger Children						
MGNREGA	-0.606	-0.250	-0.356	-0.0038	-0.0055	-0.0025
	(0.567)	(0.293)	(0.296)	(0.0031)	(0.0034)	(0.0032)
Observations	3,715,815	3,715,815	3,715,815	1,358,447	610,491	734,967
R-squared	0.189	0.167	0.175	0.047	0.056	0.044

Notes: The dependent variable is enrollment. The specifications use balanced DISE data and ASER data from years 2005 through 2009. Older children refer to children in upper-primary school (DISE Data) or aged 12 to 16 (ASER Data). Younger children refer to children in primary school (DISE Data) or aged 5 to 11 (ASER Data). All regressions control for district fixed effects, state-by-year fixed effects, and the three district "backwardness" characteristics interacted with year. Columns 4 through 6 also include child age fixed effects (ASER Data). Robust standard errors clustered at district level in parentheses.

Table 4. Effect of MGNREGA on School Enrollment, Controlling for "Left-Wing Extremism"

	Enrollment Impacts by School (DISE Data)			Enrollment Impacts by Child (ASER Data)		
	All	Females	Males	All	Females	Males
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Children						
MGNREGA	-1.012	-0.531	-0.481	-0.0055	-0.0054	-0.0053
	(0.679)	(0.360)	(0.352)	(0.0035)	(0.0038)	(0.0036)
Observations	3,715,815	3,715,815	3,715,815	2,164,445	966,000	1,177,308
R-squared	0.156	0.133	0.134	0.074	0.087	0.069
Panel B: Older Children						
MGNREGA	-0.479	-0.316	-0.163	-0.0081	-0.0046	-0.0100
	(0.240)	(0.124)	(0.126)	(0.0053)	(0.0061)	(0.0055)
Observations	3,715,815	3,715,815	3,715,815	805,998	355,509	442,341
R-squared	0.035	0.031	0.028	0.075	0.090	0.071
Panel C: Younger Children						
MGNREGA	-0.533	-0.215	-0.318	-0.0039	-0.0054	-0.0027
	(0.598)	(0.319)	(0.303)	(0.0031)	(0.0035)	(0.0032)
Observations	3,715,815	3,715,815	3,715,815	1,358,447	610,491	734,967
R-squared	0.189	0.167	0.175	0.048	0.056	0.044

Notes: The dependent variable is enrollment. The specifications use balanced DISE data and ASER data from years 2005 through 2009. Older children refer to children in upper-primary school (DISE Data) or aged 12 to 16 (ASER Data). Younger children refer to children in primary school (DISE Data) or aged 5 to 11 (ASER Data). All regressions control for district fixed effects, state-by-year fixed effects, the three district "backwardness" characteristics interacted with year, and an indicator variable denoting whether the district is classified by the government as being having concerns related to "left-wing extremism" interacted with year. Columns 4 through 6 also include child age fixed effects (ASER Data). Robust standard errors clustered at district level in parentheses.

Table 5. Effect of MGNREGA on Log School Enrollment, by Child Gender and Age/Grade

	All	Females	Males
	(1)	(2)	(3)
Panel A: All Children			
MGNREGA	-0.0064	-0.0074	-0.0068
	(0.0039)	(0.0041)	(0.0040)
Observations	3,715,815	3,715,815	3,715,815
R-squared	0.215	0.157	0.146
Panel B: Older Children			
MGNREGA	-0.0157	-0.0139	-0.0118
	(0.0073)	(0.0060)	(0.0059)
Observations	3,715,815	3,715,815	3,715,815
R-squared	0.061	0.054	0.055
Panel C: Younger Children			
MGNREGA	-0.0103	-0.0083	-0.0108
	(0.0046)	(0.0042)	(0.0044)
Observations	3,715,815	3,715,815	3,715,815
R-squared	0.113	0.111	0.117

Notes: The dependent variable is the natural logarithm of enrollment plus one. The specifications use balanced DISE data from years 2005 through 2009. Older children refer to children in upper-primary school. Younger children refer to children in primary school. All regressions control for district fixed effects, state-by-year fixed effects, and the three district "backwardness" characteristics interacted with year. Robust standard errors clustered at district level in parentheses.

Table 6. Relative Changes in School Enrollment, Prior to MGNREGA (Pre-Trends)

Change from:	Phase I relative to Phase II (1)	Phase I relative to Phase III (2)	Phase II relative to Phase III (3)
Panel A: Level Enrollment			
2002 to 2003	1.287	3.053	1.766
	(1.945)	(1.977)	(1.603)
2003 to 2004	0.400	0.925	0.525
	(1.462)	(1.516)	(1.276)
2004 to 2005	1.197	0.849	-0.348
	(1.342)	(1.201)	(1.329)
2005 to 2006			-0.752
			(1.440)
Observations	1,796,168	1,796,168	2,245,210
R-squared	0.155	0.155	0.161
Panel B: Log Enrollment			
2002 to 2003	0.002	0.015	0.013
	(0.008)	(0.009)	(0.009)
2003 to 2004	0.007	0.013	0.006
	(0.006)	(0.008)	(0.008)
2004 to 2005	0.005	0.0075	0.002
	(0.007)	(0.0077)	(0.009)
2005 to 2006			0.0014
			(0.0081)
Observations	1,796,168	1,796,168	2,245,210
R-squared	0.237	0.237	0.245

Notes: The dependent variable in panel A is level enrollment. The dependent variable in panel B is the natural logarithm of enrollment plus one. The specifications use DISE data. All regressions control for district fixed effects, state-by-year fixed effects, and the three district "backwardness" characteristics interacted with year. Robust standard errors clustered at district level in parentheses.

Table 7. Effect of MGNREGA on Math and Reading Ability, by Child Gender and Age

	Math Score (0-3)			Math Word Score (0-2)			Reading Score (0-4)		
	All	Females	Males	All	Females	Males	All	Females	Males
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: School-Age Children									
MGNREGA	-0.0033	-0.0042	-0.0004	-0.0088	-0.0133	-0.0042	-0.0065	-0.0066	-0.0043
	(0.0190)	(0.0201)	(0.0188)	(0.0265)	(0.0284)	(0.0263)	(0.0244)	(0.0259)	(0.0240)
Observations	2,028,423	905,277	1,103,523	630,341	280,141	350,200	2,038,513	909,813	1,108,839
R-squared	0.409	0.399	0.420	0.376	0.372	0.382	0.436	0.430	0.443
Panel B: Older Children									
MGNREGA	-0.0183	-0.0209	-0.0127	-0.0099	-0.0168	-0.0054	-0.0426	-0.0433	-0.0394
	(0.0213)	(0.0239)	(0.0206)	(0.0240)	(0.0270)	(0.0234)	(0.0238)	(0.0263)	(0.0233)
Observations	764,599	337,471	419,518	293,666	128,605	165,061	766,790	338,506	420,612
R-squared	0.116	0.135	0.109	0.084	0.094	0.080	0.082	0.102	0.074
Panel C: Younger Children									
MGNREGA	0.0071	0.0079	0.0079	-0.0078	-0.0081	-0.0057	0.0162	0.0163	0.0180
	(0.0198)	(0.0205)	(0.0199)	(0.0380)	(0.0400)	(0.0382)	(0.0279)	(0.0290)	(0.0281)
Observations	1,263,824	567,806	684,005	336,675	151,536	185,139	1,271,723	571,307	688,227
R-squared	0.335	0.334	0.338	0.325	0.325	0.328	0.379	0.381	0.379

Notes: The dependent variable is score on math or reading test. The specifications use ASER data from years 2005 through 2009. Older children refer to children aged 12 to 16. Younger children refer to children aged 5 to 11. All regressions control for district fixed effects, state-by-year fixed effects, the three district "backwardness" characteristics interacted with year, and child age fixed effects. Robust standard errors clustered at district level in parentheses.

Table 8. Effect of MGNREGA on Completion Exams, by Child Gender and Grade

	Primary-School Completion (Grade 5)			Upper-Primary-School Completion (Grade 8)		
	All	Females	Males	All	Females	Males
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Appeared for Exam						
MGNREGA	-0.143	-0.062	-0.092	-0.253	-0.278	0.019
	(0.501)	(0.220)	(0.287)	(0.333)	(0.154)	(0.188)
Observations	1,246,145	1,245,815	1,245,815	1,246,145	1,245,814	1,245,814
R-squared	0.125	0.110	0.108	0.058	0.047	0.046
Panel B: Passed Exam						
MGNREGA	-0.243	-0.115	-0.128	-0.239	-0.202	-0.036
	(0.467)	(0.204)	(0.269)	(0.278)	(0.124)	(0.160)
Observations	1,246,145	1,246,145	1,246,145	1,246,145	1,246,145	1,246,145
R-squared	0.122	0.092	0.114	0.066	0.056	0.052
Panel C: Scored High Marks						
MGNREGA	-0.352	-0.174	-0.179	-0.092	-0.076	-0.016
	(0.251)	(0.112)	(0.142)	(0.156)	(0.075)	(0.085)
Observations	1,246,145	1,246,145	1,246,145	1,246,145	1,246,145	1,246,145
R-squared	0.121	0.105	0.102	0.090	0.073	0.072

Notes: The dependent variable is the exam outcome as described in each panel header: the number of students who appeared for the completion exam (panel A), the number of students who passed the completion exam (panel B), and the number of students who scored high marks on the completion exam (panel C). The specifications use balanced DISE data from years 2005 through 2009. All regressions control for district fixed effects, state-by-year fixed effects, and the three district "backwardness" characteristics interacted with year. Robust standard errors clustered at district level in parentheses.

Table 9. Effect of MGNREGA on Teachers

	Total Number of Teachers (1)	Female Teachers (2)	Male Teachers (3)	Qualified Teachers (4)	New Teachers (5)	Experienced Teachers (6)	Teacher Turnover (7)
MGNREGA	-0.0189	-0.0054	-0.0135	0.0133	-0.0017	0.0063	-0.0053
	(0.0261)	(0.0135)	(0.0157)	(0.0273)	(0.0297)	(0.0217)	(0.0090)
Observations	3,700,422	3,700,422	3,700,422	3,700,422	3,700,422	3,700,422	3,630,095
R-squared	0.135	0.181	0.093	0.209	0.194	0.137	0.187

Notes: The dependent variables are teacher outcomes as noted by column headers. The specifications use balanced DISE data from years 2005 through 2009. All regressions control for district fixed effects, state-by-year fixed effects, and the three district "backwardness" characteristics interacted with year. Columns 1 through 6 refer to the number of teachers. Robust standard errors clustered at district level in parentheses.

Table 10. Effect of MGNREGA on School Infrastructure

	Electricity (1)	Boundary Wall (2)	Library (3)	Latrine (4)	Playground (5)	Water Source (6)	Computers (7)
MGNREGA	-0.0005	-0.0031	0.0111	-0.0001	-0.0029	-0.0003	-0.0037
	(0.0036)	(0.0036)	(0.0062)	(0.0087)	(0.0023)	(0.0034)	(0.0035)
Observations	3,710,241	3,686,005	3,704,564	3,711,581	2,972,052	3,710,484	3,714,926
R-squared	0.356	0.118	0.190	0.200	0.120	0.102	0.151

Notes: The dependent variables are infrastructure outcomes as noted by column headers. The specifications use balanced DISE data from years 2005 through 2009. All regressions control for district fixed effects, state-by-year fixed effects, and the three district "backwardness" characteristics interacted with year. Robust standard errors clustered at district level in parentheses.

Table 11. Effect of MGNREGA on School Enrollment, Alternative Specifications

	Enrollment Impacts by School (DISE Data)			Enrollment Impacts by Child (ASER Data)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Children						
MGNREGA	-1.026	-1.053	-1.706	-0.0053	-0.0058	-0.0079
	(0.649)	(0.902)	(0.743)	(0.0035)	(0.0032)	(0.0031)
Observations	3,715,815	3,715,815	3,715,815	2,164,445	2,164,445	2,164,445
R-squared	0.156	0.154	0.154	0.074	0.074	0.072
Panel B: Older Children						
MGNREGA	-0.420	-0.870	-0.934	-0.0079	-0.0080	-0.0105
	(0.234)	(0.394)	(0.313)	(0.0053)	(0.0049)	(0.0046)
Observations	3,715,815	3,715,815	3,715,815	805,998	805,998	805,998
R-squared	0.035	0.034	0.034	0.075	0.075	0.072
Panel C: Younger Children						
MGNREGA	-0.606	-0.183	-0.772	-0.0038	-0.0043	-0.0059
	(0.567)	(0.723)	(0.601)	(0.0031)	(0.0027)	(0.0029)
Observations	3,715,815	3,715,815	3,715,815	1,358,447	1,358,447	1,358,447
R-squared	0.189	0.187	0.187	0.047	0.047	0.044
District FE	x	x	x	x	x	x
Year FE	x	x	x	x	x	x
State * Year	x	x		x	x	
Backwardness * Year	x			x		

Notes: The dependent variable is enrollment. The specifications use balanced DISE data and ASER data from years 2005 through 2009. Older children refer to children in upper-primary school (DISE Data) or aged 13 to 16 (ASER Data). Younger children refer to children in primary school (DISE Data) or aged 5 to 12 (ASER Data). The regressions in columns 1 and 4 control for district fixed effects, state-by-year fixed effects, and the three district "backwardness" characteristics interacted with year. Columns 2 and 5 omit the controls for the the "backwardness" characteristics interacted with year. Columns 3 and 6 further omit state-by-year fixed effects and include year fixed effects instead. Columns 4 through 6 also include child age fixed effects (ASER Data). Robust standard errors clustered at district level in parentheses.

Table 12. Effect of MGNREGA on Math and Reading Ability, Alternative Specifications

	Math Score (0-3)			Math Word Score (0-2)			Reading Score (0-4)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: School-Age Children									
MGNREGA	-0.0033	-0.0000	-0.0143	-0.0088	-0.0086	-0.0141	-0.0065	-0.0014	-0.0103
	(0.0190)	(0.0169)	(0.0171)	(0.0265)	(0.0268)	(0.0340)	(0.0244)	(0.0211)	(0.0207)
Observations	2,028,423	2,028,423	2,028,423	630,341	630,341	630,341	2,038,513	2,038,513	2,038,513
R-squared	0.409	0.409	0.404	0.376	0.376	0.369	0.436	0.435	0.432
Panel B: Older Children									
MGNREGA	-0.0183	-0.0073	-0.0330	-0.0099	-0.0092	-0.0118	-0.0426	-0.0315	-0.0430
	(0.0213)	(0.0192)	(0.0195)	(0.0240)	(0.0243)	(0.0278)	(0.0238)	(0.0206)	(0.0202)
Observations	764,599	764,599	764,599	293,666	293,666	293,666	766,790	766,790	766,790
R-squared	0.116	0.116	0.108	0.084	0.083	0.075	0.082	0.082	0.077
Panel C: Younger Children									
MGNREGA	0.0071	0.0065	-0.0012	-0.0078	-0.0070	-0.0188	0.0162	0.0181	0.0098
	(0.0198)	(0.0175)	(0.0176)	(0.0380)	(0.0382)	(0.0526)	(0.0279)	(0.0242)	(0.0235)
Observations	1,263,824	1,263,824	1,263,824	336,675	336,675	336,675	1,271,723	1,271,723	1,271,723
R-squared	0.335	0.335	0.327	0.325	0.325	0.314	0.379	0.379	0.374
District FE	x	x	x	x	x	x	x	x	x
Year FE	x	x	x	x	x	x	x	x	x
State * Year	x	x		x	x		x	x	
Backwardness * Year	x			x			x		

Notes: The dependent variable is score on math or reading test. The specifications use ASER data from years 2005 through 2009. Older children refer to children aged 13 to 16. Younger children refer to children aged 5 to 12. The regressions in columns 1, 4, and 7 control for district fixed effects, state-by-year fixed effects, and the three district "backwardness" characteristics interacted with year. Columns 2, 5, and 8 omit the controls for the the "backwardness" characteristics interacted with year. Columns 3, 6, and 9 further omit state-by-year fixed effects and include year fixed effects instead. All specifications include child age fixed effects. Robust standard errors clustered at district level in parentheses.

Appendix Table 1. Baseline School Characteristics: DISE, Teachers and School Infrastructure

	All Districts (1)	Phase I Districts (2)	Phase II Districts (3)	Phase III Districts (4)
Teachers				
Number of Teachers	4.02	3.79	3.90	4.37
	(3.63)	(3.26)	(3.40)	(4.14)
Females	1.49	1.27	1.37	1.84
	(2.57)	(2.17)	(2.33)	(3.08)
Males	2.52	2.52	2.53	2.53
	(2.32)	(2.21)	(2.33)	(2.45)
Qualified	3.20	2.88	3.01	3.72
	(3.45)	(3.09)	(3.15)	(3.95)
Experienced	2.76	2.52	2.69	3.10
	(3.04)	(2.67)	(2.80)	(3.54)
New	0.07	0.11	0.08	0.02
	(0.50)	(0.60)	(0.53)	(0.31)
Teacher Turnover	0.02	0.02	0.02	0.00
	(0.11)	(0.14)	(0.11)	(0.05)
Infrastructure				
Blackboard	0.95	0.94	0.96	0.96
	(0.22)	(0.23)	(0.20)	(0.21)
Boundary Wall	0.38	0.34	0.33	0.46
	(0.48)	(0.47)	(0.47)	(0.50)
Computer	0.09	0.08	0.09	0.11
	(0.29)	(0.27)	(0.29)	(0.31)
Electricity	0.29	0.22	0.24	0.40
	(0.45)	(0.41)	(0.43)	(0.49)
Latrine: Any	0.61	0.53	0.61	0.70
	(0.49)	(0.50)	(0.49)	(0.46)
Latrine: Female-Only	0.38	0.32	0.36	0.48
	(0.49)	(0.47)	(0.48)	(0.50)
Latrine: Unisex	0.54	0.48	0.55	0.62
	(0.50)	(0.50)	(0.50)	(0.49)
Library	0.51	0.52	0.47	0.52
	(0.50)	(0.50)	(0.50)	(0.50)
Medical Checkups	0.57	0.55	0.50	0.64
	(0.50)	(0.50)	(0.50)	(0.48)
Ramps	0.19	0.19	0.16	0.22
	(0.39)	(0.39)	(0.37)	(0.42)
Water Source: Any	0.85	0.84	0.84	0.87
	(0.36)	(0.36)	(0.36)	(0.34)
Water Source: Pump	0.55	0.60	0.59	0.46
	(0.50)	(0.49)	(0.49)	(0.50)
Water Source: Tap	0.20	0.15	0.17	0.28
	(0.40)	(0.36)	(0.37)	(0.45)
Water Source: Well	0.05	0.05	0.05	0.05
	(0.22)	(0.22)	(0.21)	(0.23)

Notes: In column 1, I report the average values of schools in all DISE districts at baseline (2005). In columns 2, 3, and 4, I report the average values of children in districts treated in Phase I, Phase II, and Phase III, respectively, at baseline (2005). There is no information on playground in 2005.

Appendix Table 2. Effect of MGNREGA on School Enrollment, by Child Gender and Age/Grade, Alternative Specifications

	Enrollment Impacts by School (DISE Data)			Enrollment Impacts by Child (ASER Data)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Females						
MGNREGA	-0.535	-0.488	-0.929	-0.0053	-0.0055	-0.0082
	(0.333)	(0.285)	(0.388)	(0.0038)	(0.0035)	(0.0035)
Observations	3,715,815	3,715,815	3,715,815	966,000	966,000	966,000
R-squared	0.133	0.133	0.130	0.087	0.086	0.084
Panel B: Older Female Children						
MGNREGA	-0.286	-0.121	-0.505	-0.0043	-0.0045	-0.0085
	(0.122)	(0.100)	(0.162)	(0.0061)	(0.0056)	(0.0052)
Observations	3,715,815	3,715,815	3,715,815	355,509	355,509	355,509
R-squared	0.031	0.031	0.030	0.090	0.090	0.087
Panel C: Younger Female Children						
MGNREGA	-0.249	-0.368	-0.424	-0.0055	-0.0055	-0.0072
	(0.293)	(0.250)	(0.314)	(0.0034)	(0.0031)	(0.0032)
Observations	3,715,815	3,715,815	3,715,815	610,491	610,491	610,491
R-squared	0.167	0.167	0.165	0.056	0.055	0.052
Panel D: All Male Children						
MGNREGA	-0.491	-0.479	-0.777	-0.0051	-0.0057	-0.0073
	(0.345)	(0.307)	(0.377)	(0.0036)	(0.0031)	(0.0031)
Observations	3,715,815	3,715,815	3,715,815	1,177,308	1,177,308	1,177,308
R-squared	0.134	0.134	0.132	0.069	0.069	0.066
Panel E: Older Male Children						
MGNREGA	-0.135	-0.066	-0.428	-0.0098	-0.0097	-0.0111
	(0.123)	(0.105)	(0.158)	(0.0055)	(0.0050)	(0.0046)
Observations	3,715,815	3,715,815	3,715,815	442,341	442,341	442,341
R-squared	0.028	0.028	0.027	0.071	0.071	0.069
Panel F: Younger Male Children						
MGNREGA	-0.356	-0.412	-0.348	-0.0025	-0.0032	-0.0047
	(0.296)	(0.255)	(0.304)	(0.0032)	(0.0027)	(0.0028)
Observations	3,715,815	3,715,815	3,715,815	734,967	734,967	734,967
R-squared	0.175	0.175	0.174	0.044	0.044	0.041
District FE	x	x	x	x	x	x
Year FE	x	x	x	x	x	x
State * Year	x	x		x	x	
Backwardness * Year	x			x		

Notes: The specifications use balanced DISE data and ASER data from years 2005 through 2009. Older children refer to children in upper-primary school (DISE) or aged 13 to 16 (ASER). Younger children refer to children in primary school (DISE) or aged 5 to 12 (ASER). Columns 4 through 6 also include child age fixed effects (ASER). Robust standard errors clustered at district level in parentheses.

Appendix Table 3. Effect of MGNREGA on Math and Learning Ability, by Child Gender and Age, Alternative Specifications

	Math Score (0-3)			Math Word Score (0-2)			Reading Score (0-4)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: School-Age Females (Ages 5 to 16)									
MGNREGA	-0.0042	0.0004	-0.0108	-0.0133	-0.0112	-0.0154	-0.0066	-0.0028	-0.0044
	(0.0201)	(0.0181)	(0.0181)	(0.0284)	(0.0286)	(0.0362)	(0.0259)	(0.0224)	(0.0218)
Observations	905,277	905,277	905,277	280,141	280,141	280,141	909,813	909,813	909,813
R-squared	0.399	0.399	0.393	0.372	0.372	0.364	0.430	0.430	0.426
Panel B: Older Females (Ages 12 to 16)									
MGNREGA	-0.0209	-0.0111	-0.0330	-0.0168	-0.0148	-0.0159	-0.0433	-0.0371	-0.0420
	(0.0239)	(0.0219)	(0.0220)	(0.0270)	(0.0272)	(0.0310)	(0.0263)	(0.0232)	(0.0226)
Observations	337,471	337,471	337,471	128,605	128,605	128,605	338,506	338,506	338,506
R-squared	0.135	0.135	0.125	0.094	0.093	0.084	0.102	0.102	0.096
Panel C: Younger Females (Ages 5 to 11)									
MGNREGA	0.0079	0.0102	0.0045	-0.0081	-0.0048	-0.0107	0.0163	0.0195	0.0179
	(0.0205)	(0.0181)	(0.0179)	(0.0400)	(0.0402)	(0.0545)	(0.0290)	(0.0250)	(0.0242)
Observations	567,806	567,806	567,806	151,536	151,536	151,536	571,307	571,307	571,307
R-squared	0.334	0.334	0.326	0.325	0.325	0.314	0.381	0.381	0.376
Panel D: School-Age Males (Ages 5 to 16)									
MGNREGA	-0.0004	0.0015	-0.0157	-0.0042	-0.0055	-0.0107	-0.0043	0.0020	-0.0127
	(0.0188)	(0.0165)	(0.0169)	(0.0263)	(0.0266)	(0.0331)	(0.0240)	(0.0207)	(0.0204)
Observations	1,103,523	1,103,523	1,103,523	350,200	350,200	350,200	1,108,839	1,108,839	1,108,839
R-squared	0.420	0.420	0.415	0.382	0.381	0.375	0.443	0.443	0.439
Panel E: Older Males (Ages 12 to 16)									
MGNREGA	-0.0127	-0.0005	-0.0301	-0.0054	-0.0054	-0.0083	-0.0394	-0.0239	-0.0405
	(0.0206)	(0.0182)	(0.0186)	(0.0234)	(0.0237)	(0.0267)	(0.0233)	(0.0197)	(0.0195)
Observations	419,518	419,518	419,518	165,061	165,061	165,061	420,612	420,612	420,612
R-squared	0.109	0.108	0.101	0.080	0.080	0.072	0.074	0.073	0.069
Panel F: Younger Males (Ages 5 to 11)									
MGNREGA	0.0079	0.0044	-0.0054	-0.0057	-0.0072	-0.0216	0.0180	0.0186	0.0046
	(0.0199)	(0.0178)	(0.0180)	(0.0382)	(0.0385)	(0.0520)	(0.0281)	(0.0245)	(0.0239)
Observations	684,005	684,005	684,005	185,139	185,139	185,139	688,227	688,227	688,227
R-squared	0.338	0.338	0.331	0.328	0.327	0.317	0.379	0.379	0.374
District FE	x	x	x	x	x	x	x	x	x
Year FE	x	x	x	x	x	x	x	x	x
State * Year	x	x		x	x		x	x	
Backwardness * Year	x			x			x		

Notes: The dependent variable is score on math or reading test using ASER data from years 2005-2009. Older children refer to children aged 13 to 16. Younger children refer to children aged 5 to 12. The specifications include child age fixed effects. Robust standard errors clustered at district level in parentheses.

Appendix Table 4. Effect of MGNREGA on School Enrollment, All Districts

	Enrollment Impacts by School (DISE Data)		Enrollment Impacts by Child (ASER Data)	
	(1)	(2)	(3)	(4)
Panel A: All Children				
MGNREGA	-0.797	-1.319	-0.0058	-0.0080
	(0.514)	(0.6733)	(0.0030)	(0.0029)
Observations	4,382,550	4,382,550	2,485,644	2,485,644
R-squared	0.159	0.1574	0.0729	0.0705
Panel B: Older Children				
MGNREGA	-0.104	-0.748	-0.0087	-0.0114
	(0.256)	(0.3323)	(0.0045)	(0.0042)
Observations	4,382,550	4,382,550	928,786	928,786
R-squared	0.041	0.0397	0.0750	0.0724
Panel C: Younger Children				
MGNREGA	-0.693	-0.570	-0.0038	-0.0055
	(0.455)	(0.5585)	(0.0026)	(0.0027)
Observations	4,382,550	4,382,550	1,556,858	1,556,858
R-squared	0.186	0.1848	0.0473	0.0442
District FE	x	x	x	x
Year FE	x	x	x	x
State * Year	x		x	

Notes: The dependent variable is enrollment. The specifications use balanced DISE data and ASER data from years 2005 through 2009 and include all districts available in the data (570 in DISE, 468 in ASER), including those with and without baseline "backwardness" data. Older children refer to children in upper-primary school (DISE Data) or aged 13 to 16 (ASER Data). Younger children refer to children in primary school (DISE Data) or aged 5 to 12 (ASER Data). The regressions in columns 1 and 3 control for district fixed effects and state-by-year fixed effects. Columns 2 and 4 omit state-by-year fixed effects and include year fixed effects instead. Columns 3 and 4 also include child age fixed effects (ASER Data). Robust standard errors clustered at district level in parentheses.

Appendix Table 5. Effect of MGNREGA on Math and Reading Ability, All Districts

	Math Score (0-3)		Math Word Score (0-2)		Reading Score (0-4)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: School-Age Children						
MGNREGA	-0.0083	-0.0168	-0.0201	-0.0226	-0.0150	-0.0170
	(0.0159)	(0.0159)	(0.0259)	(0.0321)	(0.0200)	(0.0193)
Observations	2,325,751	2,325,751	726,247	726,247	2,338,552	2,338,552
R-squared	0.4113	0.4054	0.3751	0.3680	0.4362	0.4325
Panel B: Older Children						
MGNREGA	-0.0146	-0.0335	-0.0177	-0.0161	-0.0416	-0.0471
	(0.0183)	(0.0180)	(0.0235)	(0.0262)	(0.0201)	(0.0192)
Observations	879,867	879,867	338,759	338,759	882,777	882,777
R-squared	0.1166	0.1082	0.0842	0.0760	0.0820	0.0770
Panel C: Younger Children						
MGNREGA	-0.0022	-0.0048	-0.0213	-0.0310	0.0031	0.0018
	(0.0163)	(0.0163)	(0.0368)	(0.0497)	(0.0225)	(0.0217)
Observations	1,445,884	1,445,884	387,488	387,488	1,455,775	1,455,775
R-squared	0.3360	0.3280	0.3247	0.3142	0.3787	0.3730
District FE	x	x	x	x	x	x
Year FE	x	x	x	x	x	x
State * Year	x		x		x	

Notes: The dependent variable is score on math or reading test. The specifications use ASER data from years 2005 through 2009 and include all districts in the data (468), including those with and without baseline "backwardness" data.. Older children refer to children aged 13 to 16. Younger children refer to children aged 5 to 12. The regressions in columns 1, 3, and 5 control for district fixed effects and state-by-year fixed effects. Columns 2, 4, and 6 omit state-by-year fixed effects and include year fixed effects instead. All specifications include child age fixed effects. Robust standard errors clustered at district level in parentheses.

References

Adukia, Anjali. 2017. "Sanitation and education." *American Economic Journal: Applied Economics*, 9(2): 23–59.

Adukia, Anjali, Sam Asher, and Paul Novosad. forthcoming. "Educational investment responses to economic opportunity: evidence from Indian road construction." *American Economic Journal: Applied Economics.*

Afridi, Farzana, Abhiroop Mukhopadhyay, and Soham Sahoo. 2016. "Female labor force participation and child education in India: evidence from the National Rural Employment Guarantee Scheme." *IZA Journal of Labor and Development*, 5(1): 1–30.

ASER. 2015. "Data from Annual Status of Education Report, India, 2005-09." ASER Centre.

Atkin, David. 2016. "Endogenous skill acquisition and export manufacturing in Mexico." *American Economic Review*, 106(8): 2046–85.

Azam, Mehtabul. 2012. "The impact of Indian job guarantee scheme on labor market outcomes: evidence from a natural experiment." *Institute of Labor Economics*, Discussion Paper No. 6548.

Berg, Erlend, Sambit Bhattacharyya, Rajasekhar Durgam, and Manjula Ramachandra. 2012. "Can rural public works affect agricultural wages? Evidence from India."

Bhatty, Kiran. 2006. "Employment guarantee and child rights." *Economic and Political Weekly*, 41(20): 1965–1967.

Bhupal, Ganita, and Abdoul G. Sam. 2014. "Female income and expenditure on children: impact of the National Rural Employment Guarantee Scheme in India." *Applied Econometrics and International Development*, 14(2): 175–192.

Cascio, Elizabeth, and Ayushi Narayan. 2017. "Who needs a fracking education? The educational response to low-skill biased technological change."

Das, Shreyasee, and Abhilasha Singh. 2013. "The impact of temporary work guarantee programs on children's education: Evidence from the Mahatma Gandhi National Rural Guarantee Act from India."

Das, Upasak. 2018. "Rural employment guarantee programme in India and its impact on household educational decisions: a focus on private coaching."

Deininger, Klaus, and Yanyan Liu. 2013. "Welfare and poverty impacts of India's National Rural Employment Scheme: Evidence from Andhra Pradesh." International Food Policy Research Institute.

DISE. 2009. "Data from District Information System for Education, India, 2002-09."

Fetzer, Thiemo. 2014. "Social insurance and conflict: evidence from India." *EOPP Working Paper No. 53.*

GOI. 2003. "Government of India Planning Commission, Report of the Task Force: Identification of districts for wage and self employment programmes."

GOI. 2009. "Haryana Development Report." Planning Commission, Government of India.

GOI. 2013. "Government of India Ministry of Home Affairs, Lok Sabha Unstarred Question No. 1374: Left Wing Extremism."

Imbert, Clement, and John Papp. 2015. "Labor market effects of social programs: evidence from India's employment guarantee." *American Economic Journal: Applied Economics*, 7(2): 233–63.

Imbert, Clement, and John Papp. 2017. "Short-term migration, rural public works and urban labor markets: evidence from India."

Islam, Mahnaz, and Anitha Sivasankaran. 2015. "How does child labor respond to changes in adult work opportunities? Evidence from NREGA."

Jensen, Robert. 2012. "Do labor market opportunities affect young women's work and family decisions? Experimental evidence from India." *Quarterly Journal of Economics*, 127: 753–792.

Kar, Spandita. 2013. "Empowerment of women through MGNREGS: issues and challenges." *Odisha Review*, 69(7-8): 76–80.

Kazianga, Harounan, Dan Levy, Leigh L Linden, and Matt Sloan. 2013. "The effects of "girl-friendly" schools: evidence from the BRIGHT school construction program in Burkina Faso." *American Economic Journal: Applied Economics*, 5(3): 41–62.

Khera, Reetika, and Nandini Nayak. 2009. "Women workers and perceptions of the National Rural Employment Guarantee Act." *Economic and Political Weekly*, 44(43): 49–57.

Klonner, Stefan, and Christian Oldiges. 2014. "Safety net for India's poor or waste of public funds? Poverty and welfare in the wake of the world's largest job guarantee program." University of Heidelberg, Department of Economics Working Papers 0564.

Li, Tianshu, and Sheetal Sekhri. 2015. "The unintended consequences of employment-based safety net programs."

Mani, Subha, Jere Behrman, Shaikh Galab, and Prudhvikar Reddy. 2014. "Impact of the NREGS on schooling and intellectual human capital." *Grand Challenges Canada Economic Returns to Mitigating Early Life Risks Project.*

MHRD. 2013. "Government of India Ministry of Human Resource Development, Analysis of budgeted expenditure on education, 2009-10 to 2011-12."

MSPI. 2009. “Ministry of Statistics and Programme Implementation: Rural and Urban Development - Statistical Year Book India 2015. Implementation report under MGNREGA.”

Munshi, Kaivan, and Mark Rosenzweig. 2016. “Networks and misallocation: insurance, migration, and the rural-urban wage gap.” *American Economic Review*, 106(1): 46–98.

Muralidharan, Karthik, Paul Niehaus, and Sandip Sukhtankar. 2016. “Building state capacity: evidence from biometric smartcards in India.” *American Economic Review*, 106(10): 2895–2929.

Muralidharan, Karthik, Paul Niehaus, and Sandip Sukhtankar. 2017. “General equilibrium effects of (improving) public employment programs: Experimental evidence from India.” NBER Working Paper No. 23838.

NCAER. 2009. “National Council of Applied Economic Research estimates on Earnings and Spendings 2009-10.”

Niehaus, Paul, and Sandip Sukhtankar. 2013a. “The marginal rate of corruption in public programs: evidence from India.” *Journal of Public Economics*, 104: 52 – 64.

Niehaus, Paul, and Sandip Sukhtankar. 2013b. “Corruption dynamics: the golden goose effect.” *American Economic Journal: Economic Policy*, 5(4): 230–69.

Oster, Emily, and Bryce Steinberg. 2013. “Do IT service centers promote school enrollment? Evidence from India.” *Journal of Development Economics*, 104: 123–135.

Palriwala, Rajni, and N. Neetha. 2009. “Paid care workers: domestic workers and anganwadi workers.” *India Research Report 4 for the Political and Social Economy of Care Project, UNRISD, Geneva.*

Pankaj, Ashok, and Rukmini Tankha. 2010. “Empowerment effects of the NREGS on women workers: A study in four states.” *Economic and Political Weekly*, 45(30): 45–55.

Ravi, Shamika, and Monika Engler. 2015. “Workfare as an effective way to fight poverty: the case of India’s NREGS.” *World Development*, 67: 57 – 71.

Ravi, Shamika, Mudit Kapoor, and Rahul Ahluwalia. 2012. “The impact of NREGS on urbanization in India.”

Shah, Manisha, and Bryce Millett Steinberg. 2015. “Workfare and human capital investment: evidence from India.” NBER Working Paper 21543.

Shah, Manisha, and Bryce Millett Steinberg. 2017. “Drought of opportunities: contemporaneous and long term impacts of rainfall shocks on human capital.” *Journal of Political Economy*, 125(2): 527–561.

Stephens, Melvin, and Dou-Yan Yang. 2014. “Compulsory education and the benefits of schooling.” *American Economic Review*, 104(6): 1777–92.

Topalova, Petia. 2010. "Factor immobility and regional impacts of trade liberalization: evidence on poverty from India." *American Economic Journal: Applied Economics*, 2(4): 1–41.

UNDP. 2010. "Rights-based legal guarantee as development policy: MGNREGA." 1–105.

Uppal, Vinayak. 2009. "Is the NREGS a safety net for children? Studying the access to the National Rural Employment Guarantee Scheme for the Young Lives families and its impact on child outcomes in Andhra Pradesh." Master's diss. University of Oxford.

Zimmerman, Laura. 2012. "Labor market impacts of a large-scale public works program: evidence from the Indian Employment Guarantee Scheme." *Institute of Labor Economics*, Discussion Paper No. 6858.