WORKING PAPER · NO. 2019-44

Spillover Impacts on Education from Employment Guarantees

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FEBRUARY 2020

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February 2020

Abstract

Programs that provide lower-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. MGNREGA has reduced rural poverty, but some have raised concerns that guaranteeing lower-skill employment opportunities may discourage investment in human capital and 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 government's rollout of MGNREGA. Further, I estimate that these small negative impacts are inexpensive to counteract, particularly compared to MGNREGA expenditures on rural employment and poverty alleviation.

KEYWORDS: *Education, human capital, India*

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The rural poor in developing countries can sometimes be in critical need of basic income support. Many developing-country governments support labor-intensive employment to provide lower-skill income opportunities, often through direct public works 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 many 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).

Employment guarantee programs, such as MGNREGA, provide immediate relief and alleviate extreme poverty, though some have raised concerns that providing lower-skill employment may discourage investment in human capital and long-term income growth. By contrast, employment programs may increase investments in education by reducing poverty and relaxing liquidity constraints. Recent research by Li and Sekhri (2019) and Shah and Steinberg (2019) has estimated negative spillover impacts on education from the rollout of MGNREGA, though these analyses do not adjust for the annual within-state rollout of MGNREGA that was targeted toward historically poorer districts that were experiencing differential changes in educational outcomes. Further, this research has focused on statistical significance rather than the magnitude of these impacts on education, which is of primary importance given MGNREGA's direct role in alleviating rural poverty (Khera and Nayak, 2009; Uppal, 2009; Pankaj and Tankha, 2010; Azam, 2012; Berg et al., 2012; Zimmerman, 2012; Deininger and Liu, 2013; Kar, 2013; Bhupal and Sam, 2014; Klonner and Oldiges, 2014; Imbert and Papp, 2015; Ravi and Engler, 2015; Afridi, Mukhopadhyay and Sahoo, 2016; Muralidharan, Niehaus and Sukhtankar, 2017).

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

I analyze the spillover impacts on children's educational attainment from the introduction of 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 in addition to the targeted measures of historical poverty. When comparing districts that first received MGNREGA to similarly poor districts in the same states, I estimate that education outcomes had been trending similarly prior to the introduction of MGNREGA. Using a large-scale school-level administrative census (DISE) and a large nationally-representative 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 annual employment support to adults in 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 do not estimate that MGNREGA directly changed schools over this period, based on measures of school infrastructure and teacher characteristics.

The estimated impacts of MGNREGA on education provide insight into the factors that may push children to leave school (as in Atkin, 2016; Cascio and Narayan, 2017; Shah and Steinberg, 2017). The small negative 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 lower-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 countervailing force, MGNREGA also may encourage families to invest in their children's education by increasing household income and financial security (Das, 2018).

Estimating the effects of MGNREGA on educational outcomes has been a recent active research area (Das and Singh, 2013; Islam and Sivasankaran, 2015; Afridi, Mukhopadhyay and Sahoo, 2016; Das, 2018; Li and Sekhri, 2019; Mani et al., 2019; Shah and Steinberg, 2019), as MGNREGA employs millions within India and represents an important test case globally. When estimating the impacts of MGNREGA on education, I emphasize the importance of controlling for the targeted within-state rollout of MGNREGA to historically poorer districts that were experiencing differential changes in educational outcomes (see Sections IV and V for details).[2] My analysis is also the first to draw on both nationally-representative large-scale education datasets in India (DISE and ASER), which allow for statistically precise estimates of the indirect effects of MGNREGA on education.

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. This estimated "null result" is important, particularly given its statistical precision, in clarifying for policy decisions that these spillover effects on education are small in magnitude.

[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 and taking into account the context in which policies are introduced.

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 might be counterbalanced by directing a small portion of MGNREGA public works investment toward more-targeted education interventions. Given the estimated impacts of MGNREGA on household consumption and financial security, 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 lower-skilled employment guarantees to counteract rural poverty. I find no substantive tradeoff between guaranteeing lower-skill employment to alleviate current poverty and maintaining 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 2.8 billion days of work for adult men and women in 53 million households or 23% of all Indian households (MSPI, 2009). The program ensures minimum income levels in rural districts by guaranteeing up to 100 days of temporary lower-skilled work annually to each household, and pays a state-wide minimum daily wage of approximately INR 100 (USD $2).[3]

[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).

MGNREGA is estimated to have had substantial labor market impacts in rural districts. MGNREGA increases lower-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 lower-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 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 Potential Indirect Impacts of MGNREGA on Education

MGNREGA helps to alleviate poverty in the short-term, but an important consideration is whether the program affects educational investment and thereby long-term poverty alleviation (Das and Singh, 2013; Islam and Sivasankaran, 2015; Afridi, Mukhopadhyay and Sahoo, 2016; Das, 2018; Li and Sekhri, 2019; Mani et al., 2019; Shah and Steinberg, 2019).[4] While MGNREGA might decrease investments in education by raising the opportunity cost of schooling and decreasing the returns to education, MGNREGA may also increase educational attainment by reducing rural poverty and relaxing liquidity constraints.

[4] Sections IV and V discuss this research, which uses a variety of datasets and methodologies that yield varying estimates.

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 lower-skilled work as MGNREGA increasingly employs other lower-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, 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, 2020).

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

My 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 large nationally-representative datasets are provided below, along with variable definitions.[5]

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.[6] The data are collected through a survey, completed by

[5] 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).

[6] 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

the school headmaster and checked by government officials, and reflect the school's information as of September 30th in each year (DISE, 2009).[7]

The main outcome variables of interest are: school enrollment, examination outcomes,[8] teacher characteristics,[9] and school infrastructure.[10] 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 132 children and 124 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 poten-

government-funded schools.

[7]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.

[8]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.

[9]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).

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

tial 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 household survey of children's literacy and numeracy, reflecting efforts to provide a standardized framework through which to evaluate 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).[11]

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).[12] I also use the measured math score, which ranges between zero (no recognition of numbers) and three (able to divide numbers).[13] Mapping child literacy and numeracy into these cardinal scores is an imperfect assumption, and so I also report estimated impacts on whether children have achieved each level of academic proficiency.[14] The empirical specifications control for child age, with separate indicator variables for each age, which effectively adjusts the raw scores

[11]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.

[12]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.

[13]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.

[14]In using the cardinal scores, the concern 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).

for literacy and numeracy by child age. 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).

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 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 historically 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). In estimating the impacts of MGNREGA from its rollout, an empirical concern is that economically "backward" districts may otherwise have experienced differential changes in educational outcomes over the study period.

Early rollout of MGNREGA was also influenced by two political considerations, however, that have an important 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. My analysis, in a departure from other research, controls for the within-state annual rollout of MGNREGA to economically "backward" districts. The empirical analysis then estimates 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 differential changes in district educational outcomes.

A second political consideration was the national government's goal of directing early rollout of MGNREGA to districts exhibiting "left-wing extremism" or LWE (Fetzer, 2014). There is a 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:

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

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, using as a "control group" those districts that did not then begin offering employment 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. While these controls adjust for fixed differences between districts, the concern remains that initially-poorer districts targeted by MGNREGA for earlier within-state rollout might have otherwise changed differently over time.

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 year fixed effects.[15] These controls adjust flexibly for annual changes that might vary across states and,

[15] 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).

in particular, changes over time for initially-poorer districts that might differ from changes over time for initially-richer districts. 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 differential changes in educational outcomes for similarly "backward" districts.[16]

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). 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-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). Figure 2 maps these 405 sample districts, which cover 84% of schools in 2005 in the

[16]The identification assumption is then: 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."

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. These average characteristics are substantively similar across districts in these three phases. The empirical analysis estimates impacts of MGNREGA on these characteristics, controlling for district fixed effects, state-by-year fixed effects, and districts' historical economic "backwardness" interacted with year fixed effects.

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 per school, on average, with the district-wide introduction of MGNREGA. The estimate is not statistically significant at conventional levels, and can reject with 95% confidence a decline in school enrollment of more than 2.30 students per school.

The estimated coefficient (-1.03) implies that a one-student decline in enrollment was associated with MGNREGA providing annual employment to 43 households. This calculation reflects the number of households receiving annual employment through MGNREGA at an average of 54 person-days of employment per household (MSPI, 2009), compared to the estimated impact of MGNREGA on school enrollment. In fiscal year 2009-10, MGNREGA paid for the employment of 52.6 million households or 87,643 households per district. By comparison, the estimated coefficient of -1.03 implies a decline in school enrollment of 2,032 students per district.[17] MGNREGA thereby provided employment to approximately 43 households for a one-student decline in enrollment (87,643 / 2,032). The lower bound of the estimated 95% confidence interval (-2.30, instead of -1.03) rejects a one-student decline in enrollment being associated with providing MGNREGA employment to fewer than 19

[17]The estimated coefficient of -1.03 implies a decline in enrollment of 2,032 students per district, given 1,701 sample schools per district and total DISE enrollment that is 16% greater than DISE enrollment in sample schools.

households.

MGNREGA provided \$6,880 to rural households for an associated decline in school enrollment of one student, given data on MGNREGA program expenditures and the estimated impact of MGNREGA on school enrollment (-1.03).[18] The estimated 95% confidence interval rejects MGNREGA expenditures of less than \$3,040 being associated with a one-student decline in enrollment.

Spending on education interventions in developing countries generally has much greater impact on student enrollment, per dollar spent, and so even these negative impacts of MGNREGA on school enrollment could be counterbalanced by allocating a small fraction of the MGNREGA budget to education interventions. 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 already 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 approximately 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 0.36% of expenditure. The estimated costs of increasing school enrollment vary across interventions and developing country contexts, with 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 \$6,880 per one-student decline in enrollment.

[18] MGNREGA program expenditures were approximately \$160 per household (MSPI, 2009), which is then multiplied by 43 households employed for a one-student decline in enrollment (as calculated above). 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 (\$6,880 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).

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 2,032 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 $8,000, 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 rejects a one-student decline in enrollment being associated with providing MGNREGA employment to fewer than 21 households (or expenditures of less than $3,360, which could potentially be offset by an additional 0.33% of expenditure).

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 approximately add up to the overall effect on the level of enrollment. For the ASER data, the subgroup effects approximately average to the overall effect on the probability of enrollment.

[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 (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.

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 Estimated Impacts on Enrollment, Alternative Specifications

Other research has emphasized a negative impact of MGNREGA on school enrollment, using DISE data (Li and Sekhri, 2019) or ASER data (Shah and Steinberg, 2019). Table 4 replicates these papers' empirical specifications, which have some important differences from my main specification. The estimated impacts of MGNREGA are more negative, and more often statistically significant, but there continues to be relatively small-in-magnitude declines in school enrollment when compared to the number of households provided with employment income.

In Table 4, column 2, I report estimated impacts on school enrollment using DISE data from an alternative specification that replaces state-by-year fixed effects with year fixed effects and omits controls for districts' historical "backwardness" interacted with year. The rollout of MGNREGA was targeted toward historically "backward" districts within states, and the estimated impacts become more negative when omitting these controls, which implies that these targeted districts were experiencing differential changes in school enrollment over these years relative to other districts in the same state.

In Table 4, columns 3 and 4, I report estimated impacts on school enrollment using DISE data from alternative specifications that can be compared to the main specification from Li and Sekhri (2019). In contrast to my main specification, these alternative specifications control for endogenous changes in school characteristics, such as the number of classrooms and number of teachers. The estimated impacts are more difficult to interpret as the causal impacts of MGNREGA on school enrollment once conditioning on other outcomes that

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

might plausibly change along with changes in school enrollment.[22] In column 4, I report an estimated impact on school enrollment of -1.324 (0.805) that would be compared to an estimate of -1.96 (0.95) from Li and Sekhri (2019, Table 2, column 3).[23] This alternative specification in column 4 controls for state-specific time trends, rather than state-by-year fixed effects, which are less reflective of the annual within-state rollout of MGNREGA to districts.[24] When including state-by-year fixed effects, in column 3, to capture the annual within-state rollout of MGNREGA, the estimated effects are less negative than the estimates in column 4, and especially so for older children.

In Table 4, column 6, I report an estimated impact on child enrollment using ASER data of -0.0079 (0.0031) from an alternative specification that would be compared to an estimate of -0.0065 (0.0028) from Shah and Steinberg (2019, Table 5, column 1, panel A). As in column 2, this alternative specification replaces state-by-year fixed effects with year fixed effects and omits controls for districts' historical "backwardness" interacted with year.[25] Appendix Table 2 reports these estimates separately by child gender and age.[26]

When interpreting the estimated magnitudes from these alternative specifications, there are many households provided with employment income for every one-student decline in school enrollment. The estimated impacts on enrollment in DISE data from this alternative specification (Table 4, column 4, row 1) imply that a one-student decline in enrollment

[22] These specifications also control for school fixed effects, such that the impacts of these school characteristics are estimated from changes over time in those school characteristics. In contrast to my main analysis of a balanced panel of schools from 2005 to 2009, these alternative specifications analyze an unbalanced panel of schools and omit data from 2009.

[23] In replicating this estimate from Li and Sekhri (2019), my estimate is less negative but within one standard error of their estimate. When analyzing effects on older children and younger children, separately, my replicated estimates indicate more negative effects on older children than on younger children, in contrast to their estimates that are more negative for younger children than for older children.

[24] These specifications control for districts' historical "backwardness" and other district characteristics, interacted with year, but the annual rollout of MGNREGA targeted historically "backward" districts within states.

[25] Shah and Steinberg (2019) also report an estimated decline in school attendance of 0.013, using National Sample Survey (NSS) data on children's reported "primary activity," which they discuss is potentially more sensitive than reported enrollment because children enrolled in school sometimes report a different primary activity.

[26] Appendix Table 3 reports my estimates when expanding the regression sample to 570 districts (DISE data) and 468 districts (ASER data), which then includes additional districts that do not have data on historical "backwardness."

is associated with providing MGNREGA employment to 34 households. This estimated magnitude, and standard error, rejects at the 5% level a one-student decline in enrollment from providing employment to fewer than 15 households. The estimated impact rejects a one-student decline in enrollment from MGNREGA spending of $2,445, which could perhaps be offset by an additional 0.45% in spending on education infrastructure. Similarly, the estimated impacts on enrollment in ASER data from this alternative specification (Table 4, column 6, row 1) imply that: a one-student decline in enrollment is associated with providing MGNREGA employment to 34 households; this estimate rejects at the 5% level a one-student decline in enrollment from providing employment to fewer than 19 households; and this estimate rejects a one-student decline in enrollment from MGNREGA spending of $3,052, which could perhaps be offset by an additional 0.36% in spending on education infrastructure.

IV.E Robustness of Enrollment Effects

Left-Wing Extremism. The main empirical specifications use variation in MGNREGA rollout 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 5, I report similar estimates to Table 3 when controlling for a measure of local "left-wing extremism" interacted with year.[27] 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 differential changes in local educational outcomes.

[27] 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 6, 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.[28] 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 show similar implied effects on district-wide enrollment numbers as in the DISE data. The estimated migration impacts of MGNREGA focus on within-district seasonal migration of adults from rural areas to urban areas. By contrast, the estimated impacts on school enrollment would only be affected by cross-district permanent migration of families, which is less frequent (Topalova, 2010; Ravi, Kapoor and Ahluwalia, 2012; Munshi and Rosenzweig, 2016; Imbert and Papp, 2017).

IV.F 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.[29]

In Table 7, 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

[28]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.

[29]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.

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).[30]

IV.G Estimated Impacts on Academic Achievement

Table 8 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 conditional on child age. For all children, the point estimates represent a decline of 0.003 standard deviations in math score (in column 1) and 0.004 standard deviations in reading score (in column 4).

These effects represent small impacts on child learning, and the estimates reject (at the 95% confidence level) a decline of more than 0.036 standard deviations in math score and 0.037 standard deviations in reading score. As a comparison, for similarly impactful declines in educational spending to change math scores or reading scores by 0.2 standard deviations (i.e., a moderate effect size), the estimated 95% confidence intervals imply that educational spending would need to decrease by more than $339 or $330 per student, respectively.[31] These represent large changes in educational expenditures, as 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 0.018 standard deviations for math score and 0.036 standard deviations for reading score. For older

[30] Note that schools in this pre-trend analysis sample are larger, on average, than schools in the main analysis sample.

[31] MGNREGA spending in these districts was the equivalent of $61 per student, and so the estimate of $339 reflects $61 multiplied by 0.2 / 0.036.

children, the estimates reject a decline of more than 0.059 standard deviations in math score and 0.076 standard deviations in reading score. For younger children, the estimated effects are smaller and reject declines of more than 0.029 standard deviations in child learning for math and 0.026 standard deviations in child learning for reading. The estimates are similar by child gender.

Similarly, Appendix Table 4 reports little impact of MGNREGA on child math and reading ability, when analyzing each achievement level separately rather than grouping achievement levels into a cardinal ranking. Appendix Tables 5 and 6 report little impact on achievement levels, by child gender for older children and younger children.

Table 9 reports the estimated impacts of MGNREGA on child math and reading ability, when excluding state-by-year fixed effects and controls for districts' historical "backwardness" interacted with year (as in Shah and Steinberg, 2019). For these alternative specifications, some of the estimated impacts of MGNREGA are more negative and marginally statistically significant. Focusing on the estimated magnitudes, however, these estimates continue to imply only small declines in academic achievement. The introduction of MGNREGA is estimated to decrease average test scores by 0.01 standard deviations (math score, in column 2 of Table 9) and 0.007 standard deviations (reading score, in column 4 of Table 9).[32,33] The estimated impacts are larger for older children, but reject at the 5% level declines in average test scores of 0.07 standard deviations (math score) and 0.07 standard deviations (reading score). Appendix Table 7 reports similar estimates by child gender.[34]

Table 10 reports estimated impacts on student exam performance, using the DISE data and my main empirical specifications. 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

[32]These estimated impacts on math score and reading score are similar magnitudes to those in Shah and Steinberg (2019).

[33]These estimates reject, at the 5% level, declines in average test scores of 0.04 standard deviations in math score and 0.03 standard deviations in reading score.

[34]Appendix Table 8 reports similar estimates when expanding the regression sample to 468 districts, including additional districts that do not have data on historical "backwardness."

at the end of the academic year. 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.H Estimated Impacts on Teachers and School Infrastructure

Impacts on teachers and school infrastructure do not appear to be substantial mechanisms through which MGNREGA affects children's educational outcomes.

Table 11 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 lower-skilled labor market outcomes, these impacts do not naturally translate into impacts on the labor market for school teachers.

Table 12 reports little estimated impact of MGNREGA on school infrastructure. The estimated magnitudes are small, relative to the baseline mean and standard deviation in infrastructure across schools. MGNREGA does not appear to systematically crowd-out investment in school infrastructure or encourage further investments in school infrastructure.

V Interpretation and Connection to Other Research

I estimate that guaranteeing low-skill employment opportunities for rural households through the MGNREGA program, which alleviates extreme poverty for many households, does not have substantial negative impacts on investments in education. Further, potential small negative impacts on educational attainment would be relatively inexpensive to counteract through compensatory efforts and direct public investment in education. While in principle providing low-skill employment opportunities may dampen incentives for investment in education, in practice alleviating extreme poverty may also enable households to pursue greater investment in education. These estimates suggest that a negative spillover effect on education is not a substantively large cost associated with the introduction of lower-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 some have raised concerns that 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 globally. As discussed in Section IV, the negative spillover impacts on education emphasized by Li and Sekhri (2019) and Shah and Steinberg (2019) reflect alternative empirical specifications and estimates that are sometimes statistically significant but small in magnitude relative to the number of households employed by MGNREGA and relative to the approximate expenditures on educational interventions that might mitigate the negative impacts on education.

Other research, using different datasets, has estimated mixed impacts of MGNREGA on educational outcomes. Using aggregate district-level 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 column 2 of Table 4 that omits state-by-year fixed effects and omits controls for districts' historical "backwardness."[35] 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, also using a similar estimating equation as in column 2 of Table 4 that also omits state-by-year fixed effects and omits controls for districts' historical "backwardness." Using data from the Young Lives Survey, from only the Indian state of Andhra Pradesh, Mani et al. (2019) estimate no detectable

[35] Das (2018) estimates no impact of MGNREGA on schooling in West Bengal, but estimates increases in household expenditures on tutors.

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.

Using data from DISE and ASER, the two largest datasets on educational outcomes across India, I find that there are some indications of negative and statistically significant impacts on some educational outcomes. The estimated magnitudes are small, however, 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 (Khera and Nayak, 2009; Uppal, 2009; Pankaj and Tankha, 2010; Azam, 2012; Berg et al., 2012; Zimmerman, 2012; Deininger and Liu, 2013; Kar, 2013; Bhupal and Sam, 2014; Klonner and Oldiges, 2014; Imbert and Papp, 2015; Ravi and Engler, 2015; Afridi, Mukhopadhyay and Sahoo, 2016; Muralidharan, Niehaus and Sukhtankar, 2017).

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 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 lower-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. These workfare programs also follow a long tradition of linking government income support with work requirements, such as the English Poor Laws and American New Deal work programs.

I estimate that MGNREGA provides 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 reject substantive declines in educational attainment. I estimate that providing MGNREGA employment to 43 households is associated with a one-child decline in educational enrollment. Further, this decline in school enrollment could be offset by directing 0.16% 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 lower-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. These negative spillover impacts on education are counterbalanced, however, as households gain financial security through MGNREGA employment and become more able to invest in their children's education.

In recent years, some political pressure has been building against MGNREGA. Among the concerns about its growing cost and implementation challenges, some have raised concerns about whether negative spillover impacts on education might reduce future income prospects for children. Even small estimated spillover effects on education may affect many children, in absolute numbers, but I estimate that few children are affected relative to the larger number of households (and children) receiving income support through MGNREGA employment. Further, negative spillover effects can be offset by directing a small portion of MGNREGA's public works budget toward the construction of school infrastructure. Employment guarantee programs and lower-skill income support for households can address short-run poverty alleviation without substantively reducing long-run sustained income growth through investments in education.

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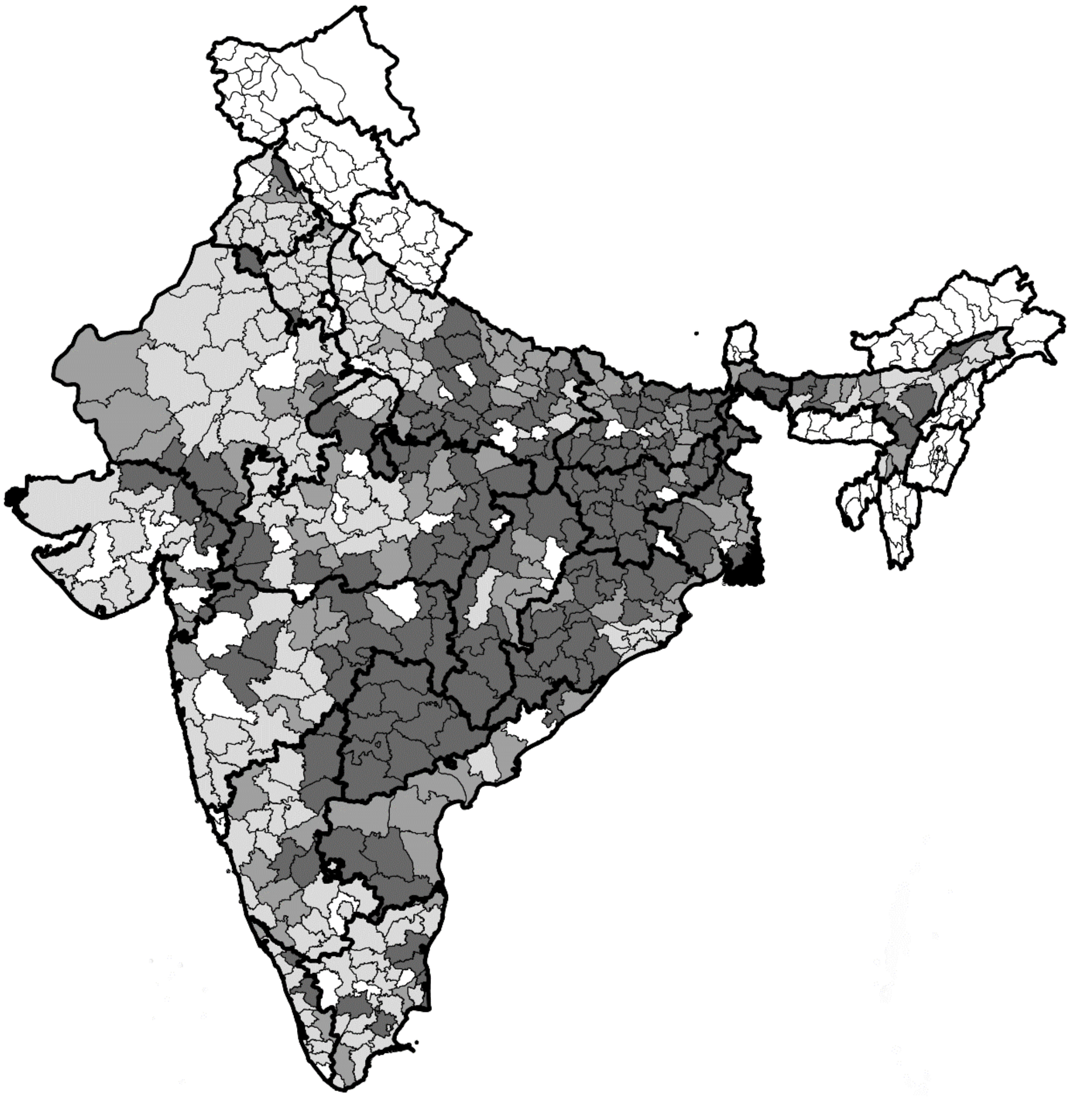
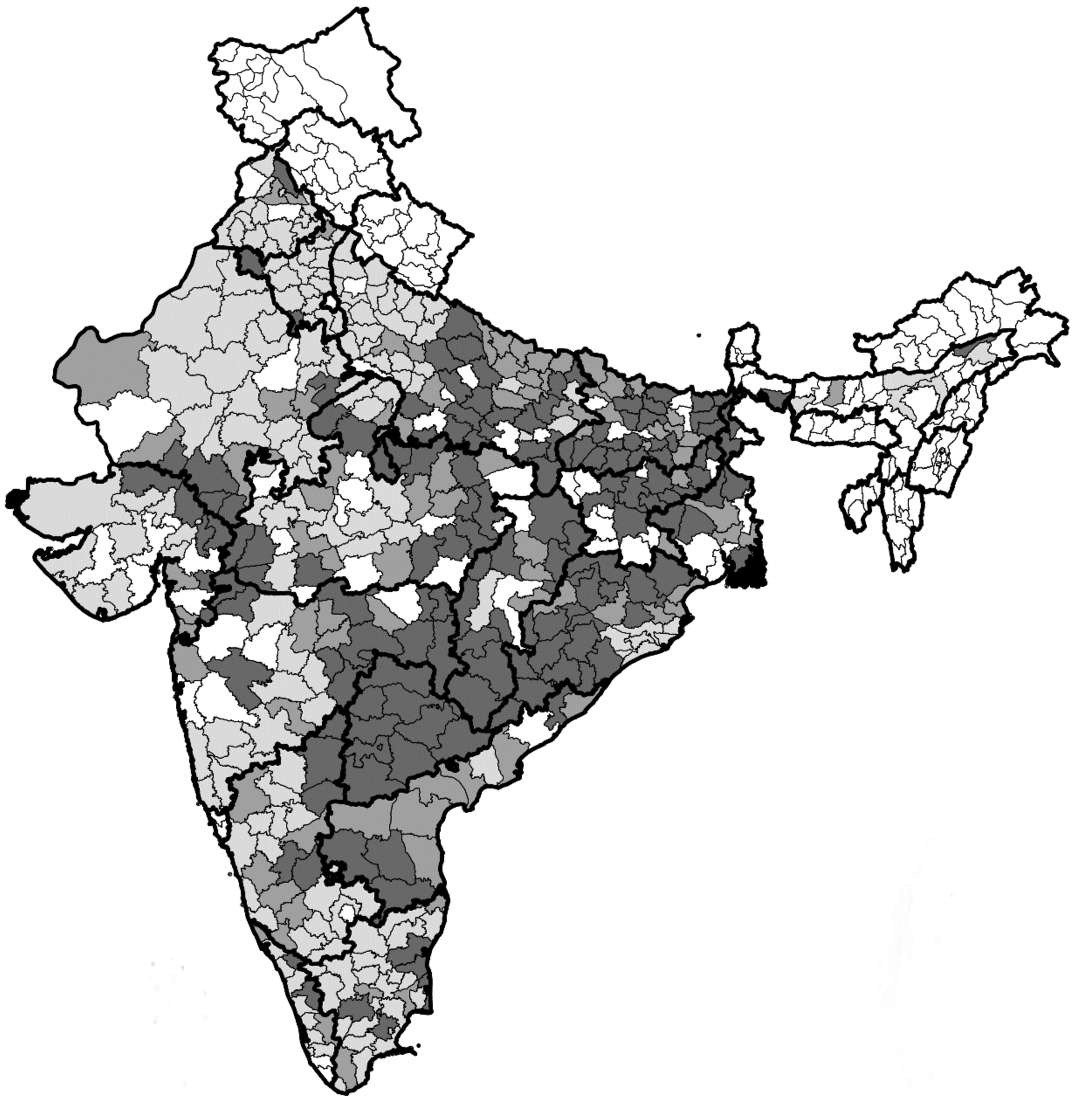
Figure 1. Map of Sample Districts using DISE Data



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 Table 3.

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 3 and 8.

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)
Upper-Primary-School Enrollment	124.4	132.2	133.4	112.7
(Grades 6 through 8)	(134.5)	(138.9)	(145.6)	(123.3)
Upper-Primary-School Females	56.5	60.1	60.5	51.2
	(74.8)	(75.2)	(82.6)	(69.9)
Upper-Primary-School Males	67.9	72.1	72.9	61.5
	(84.0)	(86.5)	(91.8)	(76.8)
Primary-School Enrollment	131.9	131.1	139.1	128.0
(Grades 1 through 5)	(115.3)	(113.1)	(118.2)	(115.8)
Primary-School Females	63.0	62.6	66.2	61.4
	(57.8)	(55.7)	(58.3)	(59.8)
Primary-School Males	68.9	68.6	72.9	66.7
	(65.5)	(64.0)	(67.6)	(65.8)
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 from the balanced 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). The values for upper-primary-school (primary-school) enrollment are for schools that were continuously operating for upper-primary (primary) grades. 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 *(< 5 - 11 years old)*	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 *(12 - 16+ years old)*	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)
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 characterics for children in the sample that draws from the ASER 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. 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	-0.452	-0.503	0.050	-0.0079	-0.0043	-0.0098
	(0.607)	(0.318)	(0.326)	(0.0053)	(0.0061)	(0.0055)
Observations	1,073,510	1,073,510	1,073,510	805,998	355,509	442,341
R-squared	0.258	0.209	0.178	0.075	0.090	0.071
Panel C: Younger Children						
MGNREGA	-0.447	-0.171	-0.276	-0.0038	-0.0055	-0.0025
	(0.624)	(0.325)	(0.324)	(0.0031)	(0.0034)	(0.0032)
Observations	3,251,750	3,251,750	3,251,750	1,358,447	610,491	734,967
R-squared	0.232	0.208	0.208	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 above 16 (ASER Data). Younger children refer to children in primary school (DISE Data) or aged below 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 the district level are reported in parentheses.

Table 4. Effect of MGNREGA on School Enrollment, Alternative Specifications

	Enrollment Impacts by School (DISE Data)				Enrollment Impacts by Child (ASER Data)	
	Main Specification	Alternative Specifications			Main Specification	Alternative Specification
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Children						
MGNREGA	-1.026	-1.706	-1.107	-1.324	-0.0053	-0.0079
	(0.649)	(0.743)	(0.730)	(0.805)	(0.0035)	(0.0031)
Observations	3,715,815	3,715,815	3,861,482	3,861,482	2,164,445	2,164,445
R-squared	0.156	0.154	0.886	0.885	0.074	0.072
Panel B: Older Children						
MGNREGA	-0.452	-1.499	-0.465	-1.252	-0.0079	-0.0105
	(0.607)	(0.771)	(0.674)	(0.711)	(0.0053)	(0.0046)
Observations	1,073,510	1,073,510	1,326,309	1,326,309	805,998	805,998
R-squared	0.258	0.253	0.912	0.912	0.075	0.072
Panel C: Younger Children						
MGNREGA	-0.447	-0.732	-0.567	-0.659	-0.0038	-0.0059
	(0.624)	(0.684)	(0.677)	(0.753)	(0.0031)	(0.0029)
Observations	3,251,750	3,251,750	3,382,027	3,382,027	1,358,447	1,358,447
R-squared	0.232	0.230	0.859	0.859	0.047	0.044

Notes: The dependent variable is enrollment. The main specifications use balanced school-level DISE data (column 1) and child-level ASER data (column 5) from years 2005 through 2009, as reported in Table 3, which control for district fixed effects, state-by-year fixed effects, and the three district "backwardness" characteristics interacted with year. For column 2, as compared to column 1, I replace state-by-year fixed effects with year fixed effects and omit the three district "backwardness" characteristics interacted with year. In columns 3 and 4, I estimate versions of the specification used in Li and Sekhri (2019, Table 2, column 3). For column 4, as compared to column 1, I replace state-by-year fixed effects with year fixed effects and state-specific time trends; add controls for school characteristics (the number of classrooms, classrooms in good condition, the presence of a unisex toilet, girls' toilet, electricity, water facilities, the number of male teachers and female teachers; school incentive programs for textbooks, stationery, uniforms, attendance scholarship, and other incentives; school fixed effects; and coding missing values as zero with separate indicator variables for missing values); add district characteristics interacted with year (from the Census of 2001: total population, urban population share, literacy rate, and women's literacy rate) along with the three district "backwardness" characteristics; and use an unbalanced sample of schools from 2005 through 2008. For column 3, I run a within-state version of column 4 (as in column 1) and replace the state time trends with state-by-year fixed effects. In column 6, I replicate the specification used in Shah and Steinberg (2019, Table 5, column 1): replacing state-by-year fixed effects with year fixed effects and omitting the three district "backwardness" characteristics interacted with year, while continuing to control for district fixed effects and child age fixed effects on child-level ASER data from 2005 through 2009 (as in column 5). Robust standard errors clustered at the district level are reported in parentheses.

Table 5. 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	-0.502	-0.539	0.037	-0.0081	-0.0046	-0.0100
	(0.624)	(0.328)	(0.333)	(0.0053)	(0.0061)	(0.0055)
Observations	1,073,510	1,073,510	1,073,510	805,998	355,509	442,341
R-squared	0.258	0.209	0.178	0.075	0.090	0.071
Panel C: Younger Children						
MGNREGA	-0.377	-0.137	-0.240	-0.0039	-0.0054	-0.0027
	(0.659)	(0.355)	(0.331)	(0.0031)	(0.0035)	(0.0032)
Observations	3,251,750	3,251,750	3,251,750	1,358,447	610,491	734,967
R-squared	0.232	0.208	0.208	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 above 16 (ASER Data). Younger children refer to children in primary school (DISE Data) or aged below 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 the district level are reported in parentheses.

Table 6. 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	-0.0007	-0.0054	0.0025
	(0.0043)	(0.0053)	(0.0043)
Observations	1,073,510	1,073,510	1,073,510
R-squared	0.234	0.144	0.092
Panel C: Younger Children			
MGNREGA	-0.0041	-0.0033	-0.0055
	(0.0039)	(0.0040)	(0.0040)
Observations	3,251,750	3,251,750	3,251,750
R-squared	0.306	0.226	0.220

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 the district level are reported in parentheses.

Table 7. 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 the district level are reported in parentheses.

Table 8. Effect of MGNREGA on Math and Reading Ability, by Child Gender and Age

	Math Score (0-3)			Reading Score (0-4)		
	All	Females	Males	All	Females	Males
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: School-Age Children						
MGNREGA	-0.0033	-0.0042	-0.0004	-0.0065	-0.0066	-0.0043
	(0.0190)	(0.0201)	(0.0188)	(0.0244)	(0.0259)	(0.0240)
Observations	2,028,423	905,277	1,103,523	2,038,513	909,813	1,108,839
R-squared	0.409	0.399	0.420	0.436	0.430	0.443
Panel B: Older Children						
MGNREGA	-0.0183	-0.0209	-0.0127	-0.0426	-0.0433	-0.0394
	(0.0213)	(0.0239)	(0.0206)	(0.0238)	(0.0263)	(0.0233)
Observations	764,599	337,471	419,518	766,790	338,506	420,612
R-squared	0.116	0.135	0.109	0.082	0.102	0.074
Panel C: Younger Children						
MGNREGA	0.0071	0.0079	0.0079	0.0162	0.0163	0.0180
	(0.0198)	(0.0205)	(0.0199)	(0.0279)	(0.0290)	(0.0281)
Observations	1,263,824	567,806	684,005	1,271,723	571,307	688,227
R-squared	0.335	0.334	0.338	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 above 16. Younger children refer to children aged below 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 the district level are reported in parentheses.

Table 9. Effect of MGNREGA on Math and Reading Ability, Alternative Specifications

	Math Score (0-3)		Reading Score (0-4)	
	Main Specification (1)	Alternative Specification (2)	Main Specification (3)	Alternative Specification (4)
Panel A: School-Age Children				
MGNREGA	-0.0033	-0.0143	-0.0065	-0.0103
	(0.0190)	(0.0171)	(0.0244)	(0.0207)
Observations	2,028,423	2,028,423	2,038,513	2,038,513
R-squared	0.409	0.404	0.436	0.432
Panel B: Older Children				
MGNREGA	-0.0183	-0.0330	-0.0426	-0.0430
	(0.0213)	(0.0195)	(0.0238)	(0.0202)
Observations	764,599	764,599	766,790	766,790
R-squared	0.116	0.108	0.082	0.077
Panel C: Younger Children				
MGNREGA	0.0071	-0.0012	0.0162	0.0098
	(0.0198)	(0.0176)	(0.0279)	(0.0235)
Observations	1,263,824	1,263,824	1,271,723	1,271,723
R-squared	0.335	0.327	0.379	0.374

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 above 16. Younger children refer to children aged below 5 to 11. The regressions in columns 1 and 3 use my main specifications, which control for district fixed effects, state-by-year fixed effects, and the three district "backwardness" characteristics interacted with year. Columns 2 and 4 replicate Shah and Steinberg (2019, Table 5, columns 2 and 3), which replace the state-by-year fixed effects with year fixed effects and omit controls for the three district "backwardness" characteristics interacted with year. All specifications include child age fixed effects. Robust standard errors clustered at the district level are reported in parentheses.

Table 10. 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 the district level are reported in parentheses.

Table 11. 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 school-level outcomes related to teachers 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 the district level are reported in parentheses.

Table 12. Effect of MGNREGA on School Infrastructure

	Electricity	Boundary Wall	Library	Latrine	Playground	Water Access	Computers
	(1)	(2)	(3)	(4)	(5)	(6)	(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 school-level outcomes related to infrastructure 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 the district level are reported 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)
Female Teachers	1.49	1.27	1.37	1.84
	(2.57)	(2.17)	(2.33)	(3.08)
Male Teachers	2.52	2.52	2.53	2.53
	(2.32)	(2.21)	(2.33)	(2.45)
Qualified Teachers	3.20	2.88	3.01	3.72
	(3.45)	(3.09)	(3.15)	(3.95)
Experienced Teachers	2.76	2.52	2.69	3.10
	(3.04)	(2.67)	(2.80)	(3.54)
New Teachers	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 playgrounds 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)	
	Main Specification	Alternative Specifications			Main Specification	Alternative Specification
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Females						
MGNREGA	-0.535	-0.929	-0.604	-0.716	-0.0053	-0.0082
	(0.333)	(0.388)	(0.386)	(0.429)	(0.0038)	(0.0035)
Observations	3,715,815	3,715,815	3,861,482	3,861,482	966,000	966,000
R-squared	0.133	0.130	0.888	0.888	0.087	0.084
Panel B: Older Female Children						
MGNREGA	-0.503	-1.084	-0.346	-0.756	-0.0043	-0.0085
	(0.318)	(0.414)	(0.339)	(0.352)	(0.0061)	(0.0052)
Observations	1,073,510	1,073,510	1,326,309	1,326,309	355,509	355,509
R-squared	0.209	0.204	0.918	0.918	0.090	0.087
Panel C: Younger Female Children						
MGNREGA	-0.171	-0.407	-0.295	-0.347	-0.0055	-0.0072
	(0.325)	(0.357)	(0.363)	(0.408)	(0.0034)	(0.0032)
Observations	3,251,750	3,251,750	3,382,027	3,382,027	610,491	610,491
R-squared	0.208	0.205	0.858	0.857	0.056	0.052
Panel D: All Male Children						
MGNREGA	-0.491	-0.777	-0.502	-0.608	-0.0051	-0.0073
	(0.345)	(0.377)	(0.371)	(0.403)	(0.0036)	(0.0031)
Observations	3,715,815	3,715,815	3,861,482	3,861,482	1,177,308	1,177,308
R-squared	0.134	0.132	0.889	0.889	0.069	0.066
Panel E: Older Male Children						
MGNREGA	0.050	-0.416	-0.119	-0.497	-0.0098	-0.0111
	(0.326)	(0.388)	(0.367)	(0.389)	(0.0055)	(0.0046)
Observations	1,073,510	1,073,510	1,326,309	1,326,309	442,341	442,341
R-squared	0.178	0.175	0.914	0.914	0.071	0.069
Panel F: Younger Male Children						
MGNREGA	-0.276	-0.324	-0.272	-0.312	-0.0025	-0.0047
	(0.324)	(0.346)	(0.338)	(0.367)	(0.0032)	(0.0028)
Observations	3,251,750	3,251,750	3,382,027	3,382,027	734,967	734,967
R-squared	0.208	0.206	0.860	0.860	0.044	0.041

Notes: The dependent variable is enrollment. The main specifications use balanced school-level DISE data (column 1) and child-level ASER data (column 5) from years 2005 through 2009, as reported in Table 3, which control for district fixed effects, state-by-year fixed effects, and the three district "backwardness" characteristics interacted with year. For column 2, as compared to column 1, I replace state-by-year fixed effects with year fixed effects and omit the three district "backwardness" characteristics interacted with year. In columns 3 and 4, I estimate versions of the specification used in Li and Sekhri (2019, Table 2, column 3). For column 4, as compared to column 1, I replace state-by-year fixed effects with year fixed effects and state-specific time trends; add controls for school characteristics (the number of classrooms, classrooms in good condition, the presence of a unisex toilet, girls' toilet, electricity, water facilities, the number of male teachers and female teachers; school incentive programs for textbooks, stationery, uniforms, attendance scholarship, and other incentives; school fixed effects; and coding missing values as zero with separate indicator variables for missing values); add district characteristics interacted with year (from the Census of 2001: total population, urban population share, literacy rate, and women's literacy rate) along with the three district "backwardness" characteristics; and use an unbalanced sample of schools from 2005 through 2008. For column 3, I run a within-state version of column 4 (as in column 1) and replace the state time trends with state-by-year fixed effects. In column 6, I replicate the specification used in Shah and Steinberg (2019, Table 5, column 1): replacing state-by-year fixed effects with year fixed effects and omitting the three district "backwardness" characteristics interacted with year, while continuing to control for district fixed effects and child age fixed effects on child-level ASER data from 2005 through 2009 (as in column 5). Robust standard errors clustered at the district level are reported in parentheses.

Appendix Table 3. Effect of MGNREGA on School Enrollment, All Districts

	Enrollment Impacts by School (DISE Data) (1)	Enrollment Impacts by Child (ASER Data) (2)
Panel A: All Children		
MGNREGA	-0.797	-0.0058
	(0.514)	(0.0030)
Observations	4,382,550	2,485,644
R-squared	0.159	0.0729
Panel B: Older Children		
MGNREGA	0.609	-0.0087
	(0.560)	(0.0045)
Observations	1,295,150	928,786
R-squared	0.246	0.0750
Panel C: Younger Children		
MGNREGA	-0.742	-0.0038
	(0.487)	(0.0026)
Observations	3,829,310	1,556,858
R-squared	0.230	0.0473

Notes: The dependent variable is enrollment. The specifications use DISE data (column 1) and ASER data (column 2) 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 12 to above 16 (ASER Data). Younger children refer to children in primary school (DISE Data) or aged below 5 to 11 (ASER Data). The regressions control for district fixed effects and state-by-year fixed effects. Column 2 also includes child age fixed effects (ASER Data). Robust standard errors clustered at the district level are reported in parentheses.

Appendix Table 4. Effect of MGNREGA on Math and Reading Ability, by Child Age

	Math Ability				Reading Ability				
	None	Recognition	Subtraction	Division	None	Letters	Words	Paragraph	Story
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: School-Age Children									
MGNREGA	0.0037	-0.0037	-0.0003	0.0008	-0.0001	0.0001	-0.0005	-0.0021	-0.0040
	(0.0060)	(0.0060)	(0.0075)	(0.0080)	(0.0051)	(0.0051)	(0.0065)	(0.0072)	(0.0084)
Observations	2,028,423	2,028,423	2,028,423	2,028,423	2,038,513	2,038,513	2,038,513	2,038,513	2,038,513
R-squared	0.1989	0.1989	0.3583	0.3179	0.1733	0.1733	0.3272	0.3840	0.3509
Panel B: Older Children									
MGNREGA	0.0107	-0.0107	-0.0049	-0.0027	0.0088	-0.0088	-0.0122	-0.0122	-0.0094
	(0.0043)	(0.0043)	(0.0079)	(0.0118)	(0.0039)	(0.0039)	(0.0049)	(0.0069)	(0.0109)
Observations	764,599	764,599	764,599	764,599	766,790	766,790	766,790	766,790	766,790
R-squared	0.0551	0.0551	0.0849	0.1246	0.0441	0.0441	0.0523	0.0646	0.0843
Panel C: Younger Children									
MGNREGA	0.0000	-0.0000	0.0025	0.0047	-0.0048	0.0048	0.0062	0.0044	0.0008
	(0.0078)	(0.0078)	(0.0084)	(0.0069)	(0.0067)	(0.0067)	(0.0084)	(0.0085)	(0.0083)
Observations	1,263,824	1,263,824	1,263,824	1,263,824	1,271,723	1,271,723	1,271,723	1,271,723	1,271,723
R-squared	0.2018	0.2018	0.2886	0.1928	0.1778	0.1778	0.2974	0.3107	0.2312

Notes: The dependent variable is a binary indicator indicating proficiency in a particular math or reading level, as indicated by the column header. For math ability, the estimates indicate whether the child was unable to do any math (column 1), recognize one- or two-digit numbers (column 2), perform subtraction (column 3), or perform division (column 4). For reading ability, the estimates indicate whether the child was unable to read anything (column 5), read letters (column 6), read words (column 7), read a paragraph (column 8), or read a story (column 9). The specifications use ASER data from years 2005 through 2009. Older children refer to children aged 12 to above 16. Younger children refer to children aged below 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. This table includes estimates for all children, regardless of gender. Robust standard errors clustered at the district level are reported in parentheses.

Appendix Table 5. Effect of MGNREGA on Math and Reading Ability, for Female Students by Child Age

	Math Ability				Reading Ability				
	None	Recognition	Subtraction	Division	None	Letters	Words	Paragraph	Story
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: School-Age Girls									
MGNREGA	0.0042	-0.0042	-0.0016	0.0016	0.0015	-0.0015	-0.0014	-0.0013	-0.0024
	(0.0065)	(0.0065)	(0.0081)	(0.0083)	(0.0056)	(0.0056)	(0.0070)	(0.0075)	(0.0088)
Observations	905,277	905,277	905,277	905,277	909,813	909,813	909,813	909,813	909,813
R-squared	0.2027	0.2027	0.3488	0.3049	0.1749	0.1749	0.3241	0.3806	0.3477
Panel B: Older Girls									
MGNREGA	0.0112	-0.0112	-0.0054	-0.0043	0.0096	-0.0096	-0.0132	-0.0128	-0.0077
	(0.0052)	(0.0052)	(0.0092)	(0.0127)	(0.0046)	(0.0046)	(0.0058)	(0.0075)	(0.0116)
Observations	337,471	337,471	337,471	337,471	338,506	338,506	338,506	338,506	338,506
R-squared	0.0751	0.0751	0.1045	0.1333	0.0609	0.0609	0.0706	0.0823	0.0955
Panel C: Younger Girls									
MGNREGA	0.0006	-0.0006	0.0007	0.0078	-0.0025	0.0025	0.0052	0.0061	0.0025
	(0.0082)	(0.0082)	(0.0088)	(0.0071)	(0.0071)	(0.0071)	(0.0090)	(0.0088)	(0.0085)
Observations	567,806	567,806	567,806	567,806	571,307	571,307	571,307	571,307	571,307
R-squared	0.2068	0.2068	0.2856	0.1884	0.1808	0.1808	0.2985	0.3125	0.2352

Notes: The dependent variable is a binary indicator indicating proficiency in a particular math or reading level, as indicated by the column header. For math ability, the estimates indicate whether the child was unable to do any math (column 1), recognize one- or two-digit numbers (column 2), perform subtraction (column 3), or perform division (column 4). For reading ability, the estimates indicate whether the child was unable to read anything (column 5), read letters (column 6), read words (column 7), read a paragraph (column 8), or read a story (column 9). The specifications use ASER data from years 2005 through 2009. Older children refer to children aged 12 to above 16. Younger children refer to children aged below 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. This table includes estimates for female children only. Robust standard errors clustered at the district level are reported in parentheses.

Appendix Table 6. Effect of MGNREGA on Math and Reading Ability, for Male Students by Child Age

	Math Ability				Reading Ability				
	None	Recognition	Subtraction	Division	None	Letters	Words	Paragraph	Story
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: School-Age Boys									
MGNREGA	0.0031	-0.0031	0.0015	0.0013	-0.0016	0.0016	0.0009	-0.0022	-0.0045
	(0.0057)	(0.0057)	(0.0075)	(0.0081)	(0.0049)	(0.0049)	(0.0064)	(0.0072)	(0.0085)
Observations	1,103,523	1,103,523	1,103,523	1,103,523	1,108,839	1,108,839	1,108,839	1,108,839	1,108,839
R-squared	0.1990	0.1990	0.3683	0.3298	0.1748	0.1748	0.3320	0.3888	0.3555
Panel B: Older Boys									
MGNREGA	0.0099	-0.0099	-0.0031	0.0003	0.0079	-0.0079	-0.0109	-0.0110	-0.0096
	(0.0041)	(0.0041)	(0.0076)	(0.0117)	(0.0037)	(0.0037)	(0.0046)	(0.0069)	(0.0110)
Observations	419,518	419,518	419,518	419,518	420,612	420,612	420,612	420,612	420,612
R-squared	0.0452	0.0452	0.0751	0.1225	0.0369	0.0369	0.0436	0.0568	0.0816
Panel C: Younger Boys									
MGNREGA	-0.0005	0.0005	0.0045	0.0029	-0.0068	0.0068	0.0076	0.0037	-0.0002
	(0.0076)	(0.0076)	(0.0085)	(0.0072)	(0.0067)	(0.0067)	(0.0084)	(0.0086)	(0.0085)
Observations	684,005	684,005	684,005	684,005	688,227	688,227	688,227	688,227	688,227
R-squared	0.1999	0.1999	0.2928	0.1979	0.1774	0.1774	0.2982	0.3109	0.2299

Notes: The dependent variable is a binary indicator indicating proficiency in a particular math or reading level, as indicated by the column header. For math ability, the estimates indicate whether the child was unable to do any math (column 1), recognize one- or two-digit numbers (column 2), perform subtraction (column 3), or perform division (column 4). For reading ability, the estimates indicate whether the child was unable to read anything (column 5), read letters (column 6), read words (column 7), read a paragraph (column 8), or read a story (column 9). The specifications use ASER data from years 2005 through 2009. Older children refer to children aged 12 to above 16. Younger children refer to children aged below 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. This table includes estimates for male children only. Robust standard errors clustered at the district level are reported in parentheses.

Appendix Table 7. Effect of MGNREGA on Math and Learning Ability, by Child Gender and Age, Alternative Specifications

	Math Score (0-3)		Reading Score (0-4)	
	(1)	(2)	(3)	(4)
Panel A: School-Age Females (Ages below 5 to above 16)				
MGNREGA	-0.0042	-0.0108	-0.0066	-0.0044
	(0.0201)	(0.0181)	(0.0259)	(0.0218)
Observations	905,277	905,277	909,813	909,813
R-squared	0.399	0.393	0.430	0.426
Panel B: Older Females (Ages 12 to above 16)				
MGNREGA	-0.0209	-0.0330	-0.0433	-0.0420
	(0.0239)	(0.0220)	(0.0263)	(0.0226)
Observations	337,471	337,471	338,506	338,506
R-squared	0.135	0.125	0.102	0.096
Panel C: Younger Females (Ages below 5 to 11)				
MGNREGA	0.0079	0.0045	0.0163	0.0179
	(0.0205)	(0.0179)	(0.0290)	(0.0242)
Observations	567,806	567,806	571,307	571,307
R-squared	0.334	0.326	0.381	0.376
Panel D: School-Age Males (Ages below 5 to above 16)				
MGNREGA	-0.0004	-0.0157	-0.0043	-0.0127
	(0.0188)	(0.0169)	(0.0240)	(0.0204)
Observations	1,103,523	1,103,523	1,108,839	1,108,839
R-squared	0.420	0.415	0.443	0.439
Panel E: Older Males (Ages 12 to above 16)				
MGNREGA	-0.0127	-0.0301	-0.0394	-0.0405
	(0.0206)	(0.0186)	(0.0233)	(0.0195)
Observations	419,518	419,518	420,612	420,612
R-squared	0.109	0.101	0.074	0.069
Panel F: Younger Males (Ages below 5 to 11)				
MGNREGA	0.0079	-0.0054	0.0180	0.0046
	(0.0199)	(0.0180)	(0.0281)	(0.0239)
Observations	684,005	684,005	688,227	688,227
R-squared	0.338	0.331	0.379	0.374

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 above 16. Younger children refer to children aged below 5 to 11. The regressions in columns 1 and 3 use my main specifications, which control for district fixed effects, state-by-year fixed effects, and the three district "backwardness" characteristics interacted with year. Columns 2 and 4 replicate the specifications from Shah and Steinberg (2019, Table 5, columns 2 and 3), which replace the state-by-year fixed effects with year fixed effects and omit controls for the three district "backwardness" characteristics interacted with year. All specifications include child age fixed effects. Robust standard errors clustered at the district level are reported in parentheses.

Appendix Table 8. Effect of MGNREGA on Math and Reading Ability, All Districts

	Math Score (0-3)	Reading Score (0-4)
	(1)	(2)
Panel A: School-Age Children		
MGNREGA	-0.0083	-0.0150
	(0.0159)	(0.0200)
Observations	2,325,751	2,338,552
R-squared	0.4113	0.4362
Panel B: Older Children		
MGNREGA	-0.0146	-0.0416
	(0.0183)	(0.0201)
Observations	879,867	882,777
R-squared	0.1166	0.0820
Panel C: Younger Children		
MGNREGA	-0.0022	0.0031
	(0.0163)	(0.0225)
Observations	1,445,884	1,455,775
R-squared	0.3360	0.3787

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 12 to above 16. Younger children refer to children aged below 5 to 11. The regressions control for district fixed effects, state-by-year fixed effects, and child age fixed effects.Robust standard errors clustered at the district level are reported in parentheses.