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The Spillovers of Employment Guarantee Programs on Child Labor and Education

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Abstract

Many developing countries use employment guarantee programs to combat poverty. This paper examines the consequences of such employment guarantee programs for the human capital accumulation of children. It exploits the phased roll-out of India's flagship Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGA) to study the effects on enrollment in schools and child labor. Introduction of MGNREGA results in lower relative school enrollment in treated districts. The authors find that the drop in enrollment is driven by primary school children. Children in higher grades are just as likely to attend school under MGNREGA, but their school performance deteriorates. Using nationally representative employment data, they find evidence indicating an increase in child labor highlighting the unintentional perverse effects of the employment guarantee schemes for Human capital.

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The Spillovers of Employment Guarantee Programs on Child Labor and Education

Tianshu Li and Sheetal Sekhri *

1 Introduction

Both developed and developing countries use employment guarantee programs as a safety net mechanism to reduce poverty and economic vulnerability.[1] This paper uses the temporal and spatial variation in the roll-out of the Indian government's 2005 National Rural Employment Guarantee Act (NREGA, now named MGNREGA) to evaluate the impact of the policy on children's educational and employment outcomes. Several studies have shown that MGNREGA increased the demand for labor and increased rural wages (Imbert and Papp, 2015; Zimmerman, 2012). In light of this, the scheme can have profound effects on childrens education and employment as it influences the opportunity cost of schooling. Potentially, the value of childrens time both in the labor force and at home can increase.

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[1]The earliest experiments with this policy lever date back to the 1817 Poor Employment Act and the 1834 Poor Law Amendment Act in Great Britain (Blaug, 1963, 1964), and the New Deal program of the 1930s in the United States (Kesselman, 1978; Bernstein, 1968). More recently Chile in 1987, India in 1978 and 2001, Pakistan in 1992, Bangladesh in 1983, Philippines in 1990, Botswana in 1960, and Kenya in 1992 have implemented variants of employment guarantee schemes. See Mukherjee and Sinha (2013) for details.

A number of factors make India's flagship MGNREGA program an ideal setting to study the impact of employment guarantee schemes on schooling and child labor. First, the scale of the program is massive. By its fifth year, the program provided employment opportunities to 53 million households for 2.3 billion man-days, making it the world's largest operating employment guarantee scheme. Second, the program was gradually rolled out in the districts of India based on pre-determined characteristics measured 10-15 years prior to the program. This variation provides an excellent opportunity to evaluate the impact of this program.

Using a longitudinal data set of 1.13 million primary and upper-primary schools in India, the study compares within school enrollment across the districts which received the program early versus late. We find that, conditional on school characteristics, school enrollment grew more slowly in districts where the program was phased in early. This result is driven by primary schools rather than upper primary schools. Using nationally representative survey data, we examine the effect on children's employment both outside and within the house. We find that children are more likely to be employed in early phase districts post treatment.[2]

Responses to the program are heterogeneous. Responses to the program vary by age, type of school and quality of private schools within private schools. Enrollment falls for younger primary school children whereas school outcomes deteriorate for upper primary children. Grade 7 school outcomes in early treated districts get worse for both government and private schools but effects are larger for private schools. Within private schools, the effects are driven by low quality private schools.

The timing of the roll-out was not random and was largely ascertained by three district characteristics: district Scheduled Castes and Scheduled Tribes population in the 1991 Census of India, 1996-97 agricultural wages, and the 1990-1993 output per

[2]In many settings, children who work a significant number of days in a year are less likely to be enrolled in school (Jensen, 2000). Our results show that safety net schemes can result in an increase in this phenomenon.

agricultural worker. We control these characteristics in our analysis. We also include both school and year fixed effects to control for school-specific time-invariant heterogeneity, and macro trends in enrollment. We also include state time trends to control for state-specific funding decisions that may affect school enrollment or performance. Additionally, we include a very comprehensive set of school- and district-level controls in our empirical analysis. Finally, we control for school-type by year fixed effects to allow for differential enrollment trends in government and private schools.

To further bolster the confidence in our identification, we conduct a series of robustness tests. First , using the data for three years before the policy was implemented (2003-2005) for a large subsample of the states,[3] we compare the pre-trends in the districts that received the program early to the ones that received it late. We do not see any evidence of differential pre-trends in enrollment. Second, using this sample, we demonstrate that controlling for changes in yearly enrollment from 2003 to 2005 and allowing the trend to vary over time in subsequent years does not change our results. Note that we also show that results are similar in the full sample and in the subsample for which we have pre-treatment data to rule out bias emerging from selection into the sample. Finally, we demonstrate that the timing of the change in enrollment coincides with the introduction of MGNREGA in early districts.

Our paper contributes to two strands of literature. The first strand examines the causal effects of employment guarantee schemes and other safety net programs on development outcomes. Several other studies have evaluated safety net programs, and in particular, this program.[4] Previous evaluations have shown that MGNREGA increased unskilled wages (Imbert and Papp, 2015; Azam, 2012; Berg et al, 2012; Zimmermann,

[3]Only 10 states and union territories covering a very small fraction of rural India are excluded in the pre-trend comparison.

[4]See Skoufias and Parker (2003) for an in-depth analysis of the effects of Mexico's Progresa on child outcomes and Skoufias et al. (2001) for effects of Progresa on child labor and schooling outcomes. Progresa is a conditional cash transfer program where transfers to the households were conditioned on children attending school. So the incentives households face are very different from MGNREGA. Berhane et al. (2014) study the effects of safety net schemes on food security and economic vulnerability.

2012), female labor force participation (Azam, 2012) and household consumption (Ravi and Engler, 2015). We complement this literature and examine the effects of the program on schooling outcomes. Our paper does not refute these clear benefits of the program but rather shows that there is an opportunity cost.

There are two studies that are closely related to our paper. Afridi et al (2012) find that MNREGA increases mothers income and through this channel improves childrens educational outcomes. Unlike Afridi et al (2012), we do not find an improvement in schooling outcomes. There are a number of differences between our paper and their study. First, Afridi et al (2012) focus on one state, whereas we use data from the entire country from 2005 to 2008. Hence, our design allows us to understand nation wide effects of the program. Second, they use data from 2007 to 2009. By 2007, MGNREGA was already implemented in the poorest parts of the country, and was being implemented in the rest of the districts. Hence, their study only makes post introduction comparisons and uses the intensity of exposure for identification. In contrast, the strength of this study design is that it uses the roll-out timing for identification and compare outcomes pre- and post-implementation. We also examine a very rich set of schooling outcomes, whereas they focus on time spent in school. Importantly, we show that the program has an unintended effect on child labor increasing likelihood of being employed. In a subsequent study, Shah and Steinberg (2015) use test score data and evaluate the impact of MGNREGA on human capital. Their paper reinforces our findings. However, there are important dissimilarities. Their study uses the Annual Status of Education Report (ASER) data on test scores[5] and finds that test scores worsen for children in early roll-out districts. They focus on gender based age specific differences in labor outcomes. Our study, on the other hand, uses school level data and highlights the heterogeneity in outcomes by school type, quality of private schools and age. There are two major differences that underscore the contributions of our paper. First, a novel result

[5]This data is collated by an NGO Pratham and is publicly available.

in our paper shows that the enrollment falls in private schools which are expensive rather than in the public schools and there is heterogeneity even within private schools. Low quality schools drive the fall in enrollment in private schools. Second, from identification perspective, a clear advantage of our paper is that we have three years of pre-treatment data for a large subset of our schools. Thus, we can strengthen the confidence in our findings by demonstrating that there are no differential pre-trends. However, their study design is limited by only 1 year of pre-treatment data in the ASER sample.[6]

Finally, our paper also complements the literature on child labor.[7] Basu and Van (1998) provide a theoretical model that examines conditions under which children work in the labor market. Edmonds (2005) uses data from Vietnam to examine whether improving standards of living reduces child labor. Edmonds and Pavnick (2005) examine the effect of international trade on children's outcomes. Jacoby and Skoufias (1997) study the effects of financial market incompleteness on human capital accumulation and document that seasonal fluctuation in school attendance is a form of self-insurance by households. More recently Cascio and Narayan (2015) demonstrate that economic booms resulting from increased fracking in the US increase high school dropout rates due to an increase in the demand for unskilled labor. We contribute to this literature by examining the effects of employment guarantee schemes on child labor. Program induced increased labor opportunities for adults induce an increase in child labor.

Our findings have important policy implications: without adequate changes in incentives to attend school, large scale safety net programs designed to smooth household consumption may result in decreased school enrollment and worsening of performance in schools.

The rest of the paper is organized as follows: In Section 2, we offer more detailed information on the MGNREGA in India. Section 3 presents the data used, and Section

[6]Both these studies use the National Sample Survey Office (NSSO) employment and unemployment survey data to substantiate that the program indeed results in higher labor supply of children.

[7]See Basu (1999) for a review of this literature.

4 presents the empirical strategy. Section 5 documents the results. Section 6 offers concluding remarks.

2 Contextual Information

2.1 Background-National Rural Employment Guarantee Act

The National Rural Employment Guarantee Act, passed in 2005 (now called Mahatma Gandhi National Rural Employment Guarantee Act), provides 100 days of guaranteed wage employment per financial year to every individual residing in rural India. The program provides unskilled manual work at the officially determined minimum wage of about 2 USD per day. In any district covered by the program, an adult can apply for work under MGNREGA and is entitled to public works employment works within 15 days; otherwise, the state government provides an unemployment allowance (Ministry of Rural Development, 2008b).[8] This program has been widely claimed to have increased rural wages despite significant leakages from the program. Imbert and Papp (2015) claim that despite its shortcomings, the program is effective at attracting casual labor relative to the private sector.

The budget for the program is almost 4 billion USD, 2.3 percent of total central government spending, which makes the program the best funded anti-poverty program in India (Ministry of Rural Development, 2008a; Azam, 2012). The program provided 2.27 billions person-days of employment to 53 millions households in 2010-11 with the whole budget in the country Rs. 345 billions (7.64 billions USD); representing 0.6% of the GDP (Imbert and Papp, 2015).

[8]We discuss some additional program details in the supplementary online appendix, available with this article at the World Bank Economic Review website - Section MGNREGA Implementation Details.

2.2 Roll-out of the MGNREGA Program

MGNREGA was implemented in three phases. Backwardness status of the districts was used to determine roll-out priority. However, each state was provided representation in Phase I. The Planning Commission of India calculated and ranked the backward status of Indian districts (Planning Commission, 2003). The official ranking of backwardness of the districts in each state was based on the Scheduled Castes and Scheduled Tribes population in 1991, agricultural wages in 1996-97, and output per agricultural worker in 1990-93. In Phase I, 200 backward districts implemented the program in February 2006. The program was then introduced in additional 130 districts in Phase II in April 2007,[9] and all the remaining 270 districts received the program in the last phase in April 2008.[10] This variation in the timing of the program roll-out enables us to identify the causal effect of this scheme on schooling outcomes.[11]

3 Data

The principal source of data is the annual panel of Indian schools called the District Information System for Education (DISE).[12] The data covers grades 1 through 8 in 1.13 million schools. School characteristics include: staff characteristics such as gender and qualification of teachers, infrastructure measures including availability of common toilets, gender specific toilets, drinking water facilities, and electrification, and enrollment by gender and grade. The data also include appearance and pass rates for school ex-

[9]The program commenced in May 2007 for 17 Phase II districts in Uttar Pradesh due to state legislative assembly elections

[10]Due to splitting of districts for which data for the parent and split district was not available in all years, the number of districts in our sample are 193, 123, and 254, respectively.

[11]Prior to February 2006, the government experimented with a pilot program (the Food for Work Program) in November 2004 in 150 of the 200 Phase I districts. Field observations (Bhatia and Dreze, 2006) and research studies (Imbert and Papp, 2015) have found little evidence of increase in public works due to this pilot.

[12]DISE is collected every year in a joint collaboration between the Government of India, UNICEF, and the National University of Educational Planning and Administration (NUEPA). The data is publicly available from NEUPA.

aminations for grades 5 and 7 and grade repetition for all grades. Primary schools in India may have only primary classes (grades 1 through 5), only upper-primary classes (grade 6 through 8), or both (grade 1 through 8). The data provide information about whether the school offers only primary classes, only upper-primary classes, or both. The school management categories in the data include (1) Department of Education, (2) Tribal/Social Welfare Department, (3) Local body, (4) Private Aided, (5) Private Unaided, (6) Others, and (7) Un-recognized. We construct three aggregate categories - government run schools (1 and 2), private schools (4 and 5) and others (3 , 6, and 7). In addition to these features, the data report ongoing incentive schemes in various schools to increase enrollment. Various schemes running in schools before MGNREGA provide free uniforms, textbooks, stationery, and attendance fellowships. One concern with the DISE data is that since schools self-report the data, there is measurement error in the data.[13] We address the implications of measurement error in the data for our results in subsequent sections.

The district level characteristics are from the Census of India 1991 and 2001. These include total population, population growth rate, percentage of female population, literacy rate, female literacy rate, percentage of the Scheduled Castes and Scheduled Tribes population, and percentage of working population. Agricultural wages for 1996-97 and total output per agricultural worker for 1990-93 come from the Planning Commission's 2003 report.

Table 1 provides the summary statistics of outcome variables by phases of MGNREGA districts. Consistent with the roll-out criterion, the better-off Phase III districts have better educational outcomes. Conditional on being enrolled, 92 percent of the children pass exams in grade 5 in Phase III districts, whereas 88 percent do in Phase I districts.

[13]These data are collected using a district level administrative structure. School principals fill a standardized survey about the school. The data are manually checked at various levels for completeness, accuracy, and inconsistencies. States also implement checks. NEUPA has commissioned an external audit of the school data. These audits check 5 percent of the schools chosen randomly from at least 10 percent of the districts from each state. The auditors also visit the schools. These audits have established that the enrollment data reported by the principals are remarkably accurate.

In Phase III districts, 49 percent of students enrolled score more than 60 percent marks in grade 5 examinations, whereas in Phase I districts only 38 percent do. Similarly, these percentages for grade 7 examinations are 43 percent in Phase III districts and 36 percent in Phase I districts. Additional summary statistics about the schools and districts in different phases are presented in Appendix Tables S1 and S2 and the details are discussed in the online Supplemental Appendix.

4 Empirical Strategy

We use the timing of roll-out of the MGNREGA program across districts of India for identification. Phase I districts received the program in February 2006, Phase II in April/May 2007, and Phase III in April 2008. We use 2005 as the baseline year and include data from 2005-2008 in our analysis. Later we use data from 2003 for the districts we have it for to provide support to our identifying assumption.

4.1 Roll-out and Selection

The timing of the roll-out of the program was not randomly determined. Thus, a simple comparison of the districts across different phases is not likely to generate causal estimates of the program. In order to circumvent this issue, we compare outcomes within districts that received program in different phases over time. This allows us to control for time invariant differences in unobserved characteristics of districts that received the program in different phases. We also use within-school variation for identification by including school fixed effects to purge any time invariant school level characteristics that may be correlated with the treatment.

We further interact the three variables determining selection into the phase of roll-out with year indicators to control for trends in these variables. In addition, we include a rich set of district specific controls including: 2001 levels of total population, percentage

of rural population, population growth rate, overall literacy rate and female literacy rate interacted with year indicators. We also control for a state-specific time trend to control for state specific time-varying unobserved heterogeneity, such as discretionary state-level education funding. We allow for a differential trend for government and private schools over time by interacting school type with year indicators.

Our identifying assumption is that the outcomes in districts that received the program in different phases are not trending differentially prior to treatment after controlling for trending program criteria. We test this assumption on a sub-sample of states for which we have pre-treatment data. We show that growth in school enrollment in districts that received the program in different phases is similar prior to the program. We also show that the within-school results are invariant to including changes in enrollment from 2003 to 2005. We do not have data from 2003-2005 for 10 small states and union territories. We verify that excluding these 10 states in our empirical analysis does not influence the results to rule out selection into the sample.

4.2 Estimation Procedure

We use school level data from 1.13 million schools from 2005 to 2008 to test our hypotheses. Our empirical specification is as follows:

$$Y_{idst} = \alpha_0 + \alpha_1 MGNREGA_{dt} + \alpha_2\, X_{idst} + \alpha_3\, Z_{ds} * T_t + \alpha_4\, State_s * trend + T_t + I_{ids} + \epsilon_{idst} \quad (1)$$

where Y_{idst} is the outcome variable for school i in district d in state s in year t. $MGNREGA_{dt}$ is an indicator that takes value 1 if district d in state s has started the MGNREGA program in year t, and 0 otherwise; X_{idst} is a vector of school level controls including different kinds of incentives received by the students, and the characteristics of the teachers and infrastructure of the school i in district d in state s in year t; Z_{ds} is a vector of district-level controls for demographic characteristics, and these are interacted with year indicators

to control for trends in these characteristics starting at specific levels for which we have values; $State_s$ is a vector of state indicators, and is interacted with a linear time *trend* to control for state-specific trends and account for state spending priorities; T_t and I_{ids} are year- and school-fixed effects, respectively, and ϵ_{idst} is the idiosyncratic error term. We drop the MGNREGA phase indicators due to multi-collinearity in our school fixed effects model. We cluster errors at the district level to account for arbitrary correlation over time.

In order to examine the school choices by school type, we interact the introduction of MGNREGA with the type of school. The empirical model is as follows:

$$\begin{aligned} Y_{idst} &= \beta_0 + \beta_1 MGNREGA_{dt} + \beta_2\, P_{ids} * MGNREGA_{dt} + \beta_3\, G_{ids} * MGNREGA_{dt} \\ &\quad + \beta_4\, X_{idst} + \beta_5\, Z_{ds} * T_t + \beta_6\, State_s * trend + \beta_7\, Schooltype_i * T_t + T_t + I_{ids} + \epsilon_{idst} \end{aligned}$$

where Y_{idst} is the outcome variable for school i in district d in state s in year t. P_{ids} is an indicator equal to 1 for private schools and 0 otherwise and G_{ids} is an indicator which takes value 1 for government schools and 0 otherwise. The omitted category is others. We include the the interaction of the MGNREGA policy indicator with each of these type indicators to examine whether effects of the program differ by school type. $Schooltype_i$ are indicators for government and private schools, and these are interacted with year indicators to control for differential trends in different types of schools. Note that once we include the school fixed effects, indicators for school type (private and government) are not included as these are time invariant properties of schools. As before, we also drop the phase indicators due to multi-collinearity in the school fixed effects model.

The outcomes we examine are: enrollment, pass rate, pass rate conditional on taking the exams, pass rate of those who pass with more than 60 percent marks.[14]Note that we do not have age specific population data so we are unable to normalize our results

[14]In India, more than 60 percent marks are considered first division.

by this age-specific population. Instead, we control for trends in district-specific total population.[15]

There are three concerns that may confound the interpretation of our results. First, we may be spuriously attributing to MGNREGA the effects of other government programs aimed at influencing enrollment. The Government of India introduced two programs in the early 2000s to promote direct enrollment in schools. The first program, the Sarva Shiksha Abhiyan (SSA), was intended to provide universal access to elementary education. The second program was the Mid-day meal which provided cooked meals for children in attendance at schools. However, both programs were launched much earlier than the MGNREGA. The second concern might be that the increase in private schools driven by growth in the private school market, independent of the program, affects our results. We see significant declines in the enrollment in the private schools. Hence, an increase in number of schools cannot be causing this decline. Further, our estimates are robust to including state- specific trends, and school type by year fixed effects. Therefore, different trajectories of growth across states is not generating our results. In Appendix Figure S1 (in the supplementary online appendix, available with this article at the World Bank Economic Review website), we also show that phase wise trends in expansion of schools both for private and public schools were similar. Thus, an independent increase in demand for private schools is unlikely to be driving our results.

Finally, if MGNREGA attracts migrants into districts, then the results could be driven by changes in population. Across district migration in India is very low (Topolova, 2010). Further, if migration were responsible for the changes in enrollment, then we would expect similar sized effects for primary and upper primary grades and individual classes within these grades. In Tables 2 and 3, the size of the effect is much larger in primary school with no effect discerned in upper primary school. It seems implausible that households with children only in specific age groups would migrate into the MGNREGA

[15]We also get similar results using log specifications.

districts to find work.[16]

5 Results

5.1 Overall Enrollment

In order to evaluate the effect of MGNREGA on enrollment, we estimate equation 1 and present the results in Table 2. Column (i) presents the basic difference-in-difference specification with school and year fixed effects. This result is robust to controlling for state specific time trends as reported in Column (ii). Both specifications control for district level controls that influenced the roll-out timing. The study controls for the Scheduled Castes and Scheduled Tribes population from the Census of India 1991, agricultural wage in 199697, and output per agricultural worker in 199093, interacted with time indicators. This accounts for differential trends in districts with the backward district status that influenced selection into the program. In addition, we control for trends in districts level total population, percentage of urban population, population growth rate, overall literacy rate, and women's literacy rate in a similar manner.[17] The school-level controls include any attendance scholarships being offered at the time, uniform, books, stationery and other such subsidies offered to girls, the number of classrooms, the number of classrooms in good condition, availability of common toilets, girls toilets, drinking water facilities, electrification status, number of male teachers, and number of female teachers.

The coefficient in Columns (i) and (ii) is -2.23 and is statistically significant at the 5 percent level. Overall, enrollment in this period is increasing and thus this coefficient indicates that introduction of MGNREGA results in a smaller annual increase in school

[16]Also, anecdotal evidence suggests that beneficiaries use the employment guarantee in summer months. One concern might be that schools are already closed for vacation. However, schools in India generally close for only around 40 days and the timing varies spatially ranging from mid-May to end of June in North to mid-June to end-July in the South. Also, Imbert and Papp (2015) show that the program impacts rural wages in a general equilibrium framework. Given that, there is an incentive to substitute for adult labor year round.

[17]We observe that the effects are no different if we do not include these co-variates.

enrollment in treated districts. Hence, implementation of MGNREGA results in relative slower growth in enrollment, with 2 fewer children enrolled per school in the treated districts.

When split by primary and upper primary grades, it is clear that this effect is driven by primary classes where the magnitude is 2.23 (Columns (iv) and (v)). We do not find any change in the enrollment of children in upper-primary classes. Since these children are already past elementary school (which is free in case of government schools), it is possible that households do not want to withdraw these children from schools as they have invested in their schooling substantially.[18]

5.2 Effects on Enrollment by Type of Schools

In order to examine if the type of school that children attend is affected, we evaluate equation 2 and report the results in Table 3. In Table 3, we show the interaction of the MGNREGA dummy interacted with school types indicators. The excluded category is 'other types' schools. Columns (i), (iii), and (v) repeat the results of the estimation of equation 1 for overall enrollment, primary enrollment and upper-primary enrollment with additional controls for school type by year fixed effects.

Overall enrollment in government schools is unaffected, whereas enrollment reduces significantly for private schools (Column (ii)). The coefficient on the interaction term with the private school indicator is significant at the 5 percent level. This result is driven by primary schools (Columns (iii) - (vi)).[19] Since 66 percent schools in the data are government schools and only 13 percent are private schools, the decrease in enrollment

[18]However, it is also possible that households who are employed in MGNREGA sites are younger and do not have children beyond the primary grades. In our subsequent analysis, we do observe heterogenous effects on children in upper primary schools as well. Hence we do not think that participating household's demographic composition is driving these results.

[19]In the previous version of the paper, where we did not include the school type (private, public or others) by year fixed effects, the decreases in enrollment were being driven by public schools, and private school enrollment actually seem to have improved. However, we thank an anonymous referee for suggesting that enrollment in different school types could be trending differentially and we need to account for that by including the school type times year fixed effects.

per private school is much larger in magnitude. The effect of the program on overall enrollment is small in magnitude. Using the average number of government, private and other schools per district in the sample period, our results indicate that 9,824 children per district are not attending school due to the program.[20]

Our identifying assumption is that there are no pre-trends in enrollment in districts belonging to different phases prior to MGNREGA's implementation. DISE data is not available for all states prior to 2005, although major states are covered since 2003. We use data from 2003 to 2005 to check if there are differential pre-trends in enrollment by phases of MGNREGA roll-out.[21] In Appendix Table S4, we control for district-specific changes in enrollment from 2003 to 2005 (pre-treatment years) and allow this to vary over time by interacting with year indicators for the states for which we have pre-program data.[22] The overall effect on enrollment and enrollment by primary and upper primary are similar to those reported in Table 3.

Prior to the program implementation, we observe that the Phase III districts are better in levels. But the growth rate in enrollment is similar. Appendix Figures S2 and S3 show that between 2003 and 2005, the growth in enrollment and number of schools looks similar across districts in different phases. These two tests together show that pre-trends in enrollment are not biasing our results.[23]

As we discussed earlier, a concern with the DISE data is that there is measurement error in reporting. Since school headmasters provide the information, it could be inaccu-

[20]In results not shown, we do not find any differences in effects for girls versus boys.

[21]In the Appendix, we discuss that our main results are no different if we exclude or include the states for which we do not have DISE data prior to 2005, we are reassured that the sample for which we have data prior to 2005 is not systematically different. The results are reported in Appendix Table S3. Limited data for a few states is also available for 2001 and 2002 but the coverage is not as expansive. Since data for many states and many variables is not available, we do not use these years.

[22]The study lost 0.7 percent of the sample schools as new districts were carved in 2004 and we are unable to use their pre-trend data. The study also checks the consistency of results when excluding those schools without pre-trends in Appendix Table S3.3.

[23]Appendix S3 on robustness tests further substantiate our identification by showing in a year-by-year comparison of early versus late districts, that the decline in enrollment occurs in 2006 after the early phases are treated. Appendix Figure S4 depicts this in a graph. We also conduct a placebo test to rule out pre-trends in our results for children's employment to ensure credibility of our findings. The results are discussed in Subsection D3 in the online Supplemental Appendix.

rate as they may have incentives to inflate enrollment numbers . If the policy change does not change the reporting behavior of the headmasters differently in districts of different phases, our double differencing approach should yield unbiased estimates. However, if the policy change systematically changes the reporting behavior then our estimates could be biased. For our results to be generated by measurement error, the schools in the early phases districts would have to under-report enrollment and this would have to vary across public and private schools and primary and upper primary schools which is highly unlikely.

There is also a tremendous amount of heterogeneity in the quality of private schools. Hence, we examine if the quality of the private schools influences household decisions. Private schools are expensive but may not necessarily be of good quality. If we split the private schools in quartiles of the student-teacher ratio, we see that the drop in enrollment in private schools is driven by poor quality private schools which have very high student-teacher ratio. The interactions of highest two quartiles with thresholds of 1.35 teachers to 100 children and 2.3 teachers to 100 children with the NREGA commenced indicator are negative, large, and statistically significant. These results are presented in Table 4 and are very important from policy perspective. These findings indicate that households are responsive to the quality of the schools when making investment decisions about children's human capital. Low quality private schools could have lower returns to schooling or could be admitting more marginal students. Hence, when the opportunity cost of a child's time in school increases, parents withdraw their children from such low quality schools. This also highlights that the private schools are not always an optimal schooling choice for the households.

5.3 Schooling Outcomes

Absenteeism from school or devoting fewer hours to school work can influence performance outcomes even if the child is enrolled in school. Hence, we also examine effects on

schooling outcomes.For grades 5 and 7, the data reports whether a student passed the exam and if they passed with more than 60 percent marks.

In Table 5, we show that the passing rate in government schools falls by 1.8 percent for grade 7 students (Columns (ii) and (iv)) with the coefficient significant at the 10 percent significance level, whereas there is no effect on grade 5 (Columns (i) and (iii)). The program effects passing with more than 60 percent marks much more significantly. Both private and government school 7th graders do worse on this measure. This effect is almost twice as large for government schools than the private schools and the difference is statistically significant (Columns (vi) and (viii)).[24]

5.4 Mechanisms

In order to shed light on the mechanism, we examine the employment outcomes of children in our DID framework. The data on child labor come from the National Sample Survey Organization (NSSO) employment and unemployment surveys (rounds 2004-05 to 2008-09) and we employ earlier rounds to carry out a falsification check. The data asks individuals to identify their principal occupation in the last month. We examine two outcome variables: Child reports working (employed, self employed or unpaid family labor) and child reports doing chores (housework or free collection of goods). We restrict our sample to 206,321 non-disabled children aged between 5 and 15 from the two rounds and look at their reported principal activities. The indicator for 'working' equals to 1 if a child's reported principal activity is working in the household enterprises (paid or unpaid), as wage employee, or in other types of work, and 0 otherwise. The indicator for doing chores is 1 if a child's reported principal activity is attending to domestic duty or doing any other housework, and 0 otherwise.[25]

[24]The program does not affect the students' appearing at exams conditional on enrollment, as reported in Appendix Table S6.

[25]Other alternatives for principal activity include attending educational institution, seeking jobs, rentiers, pensioners, remittance recipients, and others.

The empirical model is as follows:

$$L_{idst} = \gamma_0 + \gamma_1 \, PhaseI * Post + \gamma_2 \, PhaseII * Post + \gamma_3 \, Z_{ds} * T_t + T_t + I_{ds} + \epsilon_{idst} \quad (2)$$

where L_{idst} is the reported labor outcome of child i in district d in state s at time t. $PhaseI$ and $PhaseII$ are the indicators for the respective phases; Z_{ds} is a vector of district-level controls for demographic characteristics, and is interacted with year indicators to control for trends in these characteristics; T_t and I_{ds} are year- and district-fixed effects, respectively, and ϵ_{idst} is the idiosyncratic error term. After the program, there is a 1.33 percent increase in the likelihood of child reporting working in Phase I districts relative to Phase III districts (Column (i) of Table 6). This is significant at the 5 percent significance level. In Phase II districts, this effect is 1.17 percent increase marginally significant at the 10 percent significance level. We cannot reject the equality of these two coefficients. In column (ii), the coefficient for doing chores is small and positive but indistinguishable from 0. Our findings indicate that child labor supply increases in response to the program.

6 Conclusion

We use the phased roll-out of MGNREGA to estimate the impact of employment based safety net programs on schooling and labor market outcomes of children. We find that fewer children enroll in schools in primary grades due to the introduction of the program and their likelihood of being employed increases. This is surprising especially because the schooling aspirations for children have become stronger in the country and large strides have been made in ensuring universal enrollment in primary schools. The drop in enrollment is driven by low quality private schools implying that school quality affects enrollment choices when the opportunity cost of attending school shifts.

We also find that among the enrolled, school pass rate with more than 60 percent

marks declines for grade 7 but not grade 5 students. Our findings have important policy implications. All in all, unless state or market institutions increase support to offset this affects, employment based safety programs can worsen the schooling outcomes of children.

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Table 1: Summary Statistics for Outcome Variables by MGNREGA Phases

		Phase I			Phase II			Phase III		
		Obs	Mean	Std.	Obs	Mean	Std.	Obs	Mean	Std.
Enrollment	Total	1,427,624	223.76	219.23	811,704	230.64	217.60	1,343,989	209.82	207.79
	Primary	1,232,837	219.40	198.22	690,974	228.09	197.33	1,116,974	201.57	190.42
	Upper-primary	390,848	113.01	117.09	231,646	113.02	70.65	490,679	102.12	103.60
	Government	1,006,795	233.43	224.016	576,487	242.60	221.815	783,388	194.43	181.14
	Private	133,146	293.19	281.59	94,408	266.93	262.3	243,666	291.22	284.06
	Others	287,683	158.72	144.31	140,809	159.30	145.04	316,935	185.54	182.38
Passing Rate Conditional on being enrolled	Grade 5	996,134	0.88	0.21	556,877	0.90	0.19	928,972	0.92	0.17
	Grade 7	284,491	0.86	0.23	170,904	0.87	0.21	388,192	0.88	0.20
Passing Rate Conditional on Appearing in Exams	Grade 5	1,023,758	0.95	0.16	578,720	0.96	0.14	947,591	0.96	0.12
	Grade 7	290,747	0.90	0.20	176,551	0.91	0.19	397,070	0.91	0.18
Passing 60 Conditional on being enrolled	Grade 5	997,423	0.38	0.34	558,074	0.44	0.34	930,690	0.49	0.35
	Grade 7	285,758	0.36	0.34	171,654	0.36	0.32	389,877	0.43	0.33
Passing 60 Conditional on Appearing in Exams	Grade 5	1,021,693	0.40	0.35	578,103	0.46	0.35	946,941	0.51	0.35
	Grade 7	290,652	0.37	0.35	176,552	0.37	0.33	397,220	0.44	0.33

Source: Authors' analysis based on data from the District Information System for Education (2005-06 to 2008-09).

Table 2: The Impact of Introduction of MGNREGA on Enrollment (2005-06 to 2008-09)

Dependent Variable: Total Enrollment					
	Overall Enrollment			Primary	Upper Primary
	(i)	(ii)	(iii)	(iv)	(v)
MGNREGA Commenced	-2.23**	-2.23**	-1.96**	-2.23**	-0.42
	(1.01)	(1.02)	(0.95)	(1.04)	(0.56)
Year- & School-fixed Effects	Yes	Yes	Yes	Yes	Yes
Controls for Backwardness [i)]	Yes	Yes	Yes	Yes	Yes
Incentives [ii)]	Yes	Yes	Yes	Yes	Yes
Teacher Characteristics [ii)]	Yes	Yes	Yes	Yes	Yes
School Infrastructure [ii)]	Yes	Yes	Yes	Yes	Yes
District Demographics [iii)]	-	Yes	Yes	Yes	Yes
State-specific Trends	-	-	Yes	Yes	Yes
Observations		3,583,317		3,053,180	1,113,283
Number of Schools		1,106,957		941,390	378,324

Source: Authors' analysis based on data from the District Information System for Education (2005-06 to 2008-09).

i) To account for the backward district status that influenced selection into the program, we control for the Scheduled Castes and Scheduled Tribes population from Census of India 1991, agricultural wage in 1996-97, and output per agricultural worker in 1990-93, interacted with year dummies.

ii) School Incentive programs include textbooks, stationary, uniforms, attendance scholarship, and other incentives; teacher characteristics include the number of male and female teachers; infrastructure includes the number of classrooms, classrooms in good condition, the existence of common toilet, women's toilet, electricity, and water facilities.

iii) District level demographic characteristcs include total population, percentage of urban population, population growth rate, overall literacy rate, and women's literacy rate from the Census of India 2001.

iv) Robust standard errors clustered at district level are reported in parantheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Heterogneous Impact of the Introduction of MGNREGA on Enrollment (2005-06 to 2008-09)

	Total Enrollment		Primary Enrollment		Upper Primary Enrollment	
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
MGNREGA Commenced	-1.96**	-1.44	-2.23**	-1.59	-0.42	0.15
	(0.95)	(1.26)	(1.04)	(1.24)	(0.56)	(0.83)
MGNREGA Commenced *Government School		0.39		0.74		-1.07
		(1.83)		(1.90)		(1.03)
MGNREGA Commenced *Private School		-8.21**		-12.49***		-0.38
		(3.38)		(4.10)		(1.46)
Gvt School*Year Dummies	-	Yes	-	Yes	-	Yes
Pvt School*Year Dummies	-	Yes	-	Yes	-	Yes
Year- & School-fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Backwardness [i]	Yes	Yes	Yes	Yes	Yes	Yes
Incentives [ii]	Yes	Yes	Yes	Yes	Yes	Yes
Teacher Characteristics [ii]	Yes	Yes	Yes	Yes	Yes	Yes
School Infrustructure [ii]	Yes	Yes	Yes	Yes	Yes	Yes
District Demographics [iii]	Yes	Yes	Yes	Yes	Yes	Yes
State-specific Trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,583,317		3,053,180		1,113,283	
Number of Schools	1,106,957		941,390		378,324	

Source: Authors' analysis based on data from the District Information System for Education (2005-06 to 2008-09).

i) To account for the backward district status that influenced selection into the program, we control for the Scheduled Castes and Scheduled Tribes population from Census of India 1991, agricultural wage in 1996-97, and output per agricultural worker in 1990-93, interacted with year dummies.

ii) School Incentive programs include textbooks, stationary, uniforms, attendance scholarship, and other incentives; teacher characteristics include the number of male and female teachers; infrastructure includes the number of classrooms, classrooms in good condition, the existence of common toilet, women's toilet, electricity, and water facilities.

iii) District level demographic characteristcs include total population, percentage of urban population, population growth rate, overall literacy rate, and women's literacy rate from the Census of India 2001.

iv) Robust standard errors clustered at district level are reported in parantheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Heterogneous Impact Within Private Schools by Teacher to Student Ratio in 2005-06 (2005-06 to 2008-09)

	Total Enrollment			Primary Enrollment			Upper Primary Enrollment		
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)
MGNREGA Commenced	-9.76***		-2.462	-12.44***		0.156	-1.60		-2.152
	(3.40)		(3.427)	(3.91)		(4.048)	(1.74)		(1.951)
MGNREGA Commenced*Lowest quartile for teacher to student ratio		-20.54**	-18.07*		-26.53***	-26.69***		-1.790	0.362
		(8.950)	(9.504)		(9.373)	(10.14)		(4.062)	(4.145)
MGNREGA Commenced*2nd lowest quartile for teacher to student ratio		-11.70***	-9.242**		-13.84***	-13.99***		-2.103	0.0493
		(3.957)	(3.974)		(3.955)	(4.470)		(2.695)	(2.728)
MGNREGA Commenced*3rd lowest quartile for teacher to student ratio		-3.864	-1.402		-3.212	-3.367		-1.657	0.495
		(3.906)	(2.950)		(4.241)	(3.447)		(2.002)	(1.769)
MGNREGA Commenced*Highest quartile for teacher to student ratio		-2.462			0.156			-2.152	
		(3.427)			(4.048)			(1.951)	
Each teacher to student ratio quantile *Year Dummies	-	Yes	Yes	-	Yes	Yes	-	Yes	Yes
Year- & School-fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Backwardness [i)]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Incentives, teacher, & infrastructure [ii)]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Demographics [iii)]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-specific Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		358,162			255,817			199,512	
Number of Schools		101,276			76,131			61,302	

Source: Authors' analysis based on data from the District Information System for Education (2005-06 to 2008-09).

i) To account for the backward district status that influenced selection into the program, the study controls for Scheduled Castes and Scheduled Tribes population from Census of India 1991, agricultural wage in 1996-97, and output per agricultural worker in 1990-93, interacted with year dummies.

ii) School Incentive programs include textbooks, stationary, uniforms, attendance scholarship, and other incentives; teacher characteristics include the number of male and female teachers; infrastructure includes the number of classrooms, classrooms in good condition, the existence of common toilet, women's toilet, electricity, and water facilities.

iii) District level demographic characteristcs include total population, percentage of urban population, population growth rate, overall literacy rate, and women's literacy rate from the Census of India 2001.

iv) Robust standard errors clustered at district level are reported in parantheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: The Impact of the Introduction of MGNREGA on Performance Outcomes (2005-06 to 2008-09, Unit: %)

	Pass/Enrollment		Pass/Appearing at Exam		Pass 60/Enrollment		Pass 60/Appearing in Exam	
	Grade 5	Grade 7	Grade 5	Grade 7	Grade 5	Grade 7	Grade 5	Grade 7
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
MGNREGA Commenced	0.39	-0.34	0.34	-0.06	0.42	3.52**	0.36	3.75**
	(0.42)	(0.63)	(0.34)	(0.62)	(0.73)	(1.73)	(0.74)	(1.78)
MGNREGA Commenced	-0.47	-1.84*	-0.43	-1.71*	-0.25	-5.98***	-0.28	-6.11***
*Govenment School	(0.67)	(1.01)	(0.58)	(0.98)	(0.88)	(1.83)	(0.89)	(1.89)
MGNREGA Commenced	-0.35	-0.54	-0.25	-0.43	0.85	-3.80**	0.85	-4.02**
*Private School	(0.53)	(0.65)	(0.37)	(0.56)	(0.87)	(1.70)	(0.87)	(1.74)
Gvt School*Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pvt School*Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year- & School-fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for Backwardness [i)]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Incentives, teacher, & infrastructure [ii)]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Demographics [iii)]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-specific Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,412,910	807,709	2,398,334	807,994	2,471,122	825,393	2,450,877	826,621
Number of Schools	834,812	311,403	833,173	314,233	839,023	313,675	836,656	316,481

Source: Authors' analysis based on data from the District Information System for Education (2005-06 to 2008-09).

i) To account for the backward district status that influenced selection into the program, the study controls for Scheduled Castes and Scheduled Tribe population from Census of India 1991, agricultural wage in 1996-97, and output per agricultural worker in 1990-93, interacted with year dummies.

ii) School Incentive programs include textbooks, stationary, uniforms, attendance scholarship, and other incentives; teacher characteristics include the number of male and female teachers; infrastructure includes the number of classrooms, classrooms in good condition, the existence of common toilet, women's toilet, electricity, and water facilities.

iii) District level demographic characteristcs include total population, percentage of urban population, population growth rate, overall literacy rate, and women's literacy rate from the Census of India 2001.

iv) Robust standard errors clustered at district level are reported in parantheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Linear Probability Regression on Child Labor (Age: 5 - 15, Unit: %)

Outcome Variables	Child reported working (employed, self-employed, or family business)	Child reported doing unpaid household services (housework or free collection of goods)	Child reported active (either (i) or (ii))
	(i)	(ii)	(iii)
Phase 1 x Post	1.33**	0.43	1.76**
	(0.63)	(0.56)	(0.89)
Phase 2 x Post	1.17*	0.36	1.53*
	(0.67)	(0.51)	(0.88)
District- and Year-fixed Effects	Yes	Yes	Yes
Controls for Backwardness [i)]	Yes	Yes	Yes
Observations	126,209	126,209	126,209
Number of Districts	438	438	438

Source: Authors' analysis based on data from the National Sample Survey Office, Schedule 10, Round 61 (2004-05) and Round 66 (2008-09).

i) To account for the backward district status that influenced selection into the program, the study controls for Scheduled Castes and Scheduled Tribes population from Census of India 1991, agricultural wage in 1996-97, and output per agricultural worker in 1990-93, interacted with year dummies.

ii) Robust standard errors clustered at district level are reported in parantheses. *** p<0.01, ** p<0.05, * p<0.1

Supplementary Appendix

S1. Implementation Details of Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGA)

Typical projects under MGNREGA are road construction, earthworks related to irrigation, water conservation, or other rural public projects (Azam 2012). Any households living in the rural area can apply to work, but they cannot choose what type of project to work on. To become a beneficiary of MGNREGA, adults residing in a rural household need to apply for a job card (free of cost) at the local Gram Panchayat where they reside.[1] Within 15 days of application, the Gram Panchayat issues the job card, which bears the photographs of all adult members of the household willing to work under MGNREGA. Meanwhile, a 33 percent participation rate for women is mandatory under MGNREGA (Ministry of Rural Development 2008).

While the wage is set by each state government, the central government is responsible for the entire cost of wages of unskilled manual workers and for 75 percent of the cost of materials and wages of skilled and semiskilled workers. On the other hand, the state governments bear the cost of material and wages of skilled and semiskilled workers, as well as the cost of the unemployment allowance (Ministry of Rural Development 2008). Wages are typically paid by piece-rate, but some areas also pay fixed daily wages. Daily earnings are below the set wage due to theft and leakage in the program.[2]

S2. Additional Summary Statistics

Table S2.1 provides the summary statistics for the outcome variables for the schools in the sample period. The average enrollment is 220.22 students per school, of these 114.8 are boys, and 106 are girls. Average enrollment in primary classes is higher at 214 students compared to 108 in upper-primary classes. The pass rate for enrolled students is approximately 90 percent for grade 5 and 87 percent for grade 7. Some children do not take exams, and the pass rate in grades 5 and 7 conditional on taking exams is 96 and 91 percent, respectively. Passing with marks of 60 percent or above is around 43 percent in grade 5, and falls to 39 percent for grade 7.

Table S2.1. Summary Statistics Outcome Variables (All Phases, All Years)

		Obs	Mean	Std. Dev.	Min	Max
Enrollment	Total	3,583,317	220.22	215.20	1	16155
	Primary classes	3,053,180	214.50	194.99	1	16145
	Upper-primary classes	1,113,283	108.73	112.48	1	3517
	Government schools	2,366,670	222.71	211.16	1	16155
	Private schools	471,220	286.86	279.26	1	13841
	Other schools	745,427	170.39	162.48	1	8040
Passing rate conditional on being enrolled	Grade 5	2,673,492	0.90	0.19	0	1
	Grade 7	898,816	0.87	0.21	0	1
Passing rate conditional on appearing in the exam	Grade 5	2,744,763	0.96	0.14	0	1
	Grade 7	921,451	0.91	0.19	0	1
Passing 60 conditional on being enrolled	Grade 5	2,486,187	0.43	0.35	0	1
	Grade 7	898,816	0.39	0.33	0	1
Passing 60 conditional on appearing in the exam	Grade 5	2,546,737	0.45	0.35	0	1
	Grade 7	921,451	0.40	0.34	0	1

Source: Authors' analysis based on data from the District Information System for Education (2005–06 to 2008–09).

1 A Gram Panchayat usually comprises of a group of villages, and is the lowest level of administration in the Indian government (Azam 2012).

2 See Niehaus and Sukhtankar (2013) for details.

Table S2.2. Comparison of District-Level Characteristics across MGNREGA Phases

	Year of Measurement	Phase I		Phase II		Phase III	
		Mean	Std Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Total population (1000 people)	2001	1,831	1,112	2,047	1,429	1,992	1,119
Population growth rate (%)	1991–2001	21.13	6.96	21.18	8.03	20.75	9.67
Overall literacy rate (%)	2001	47.16	10.45	52.51	12.39	58.23	10.32
Percentage of female population (%)	2001	48.68	1.24	48.31	1.27	48.04	1.98
Female literacy rate (%)	2001	43.55	12.55	49.32	15.32	58.46	14.17
Percentage of working population (%)	2001	42.25	6.65	40.01	6.94	40.35	7.12
Percentage Scheduled Castes and Scheduled Tribes population (%)	1991	38.42	20.74	31.27	21.63	25.76	20.87
Agricultural wages (Rs/person/day)	1996–97	32.14	9.58	37.72	9.84	46.44	18.48
Output per agricultural worker (Rs/worker)	1990–93	5,196	3,401	7,025	5,212	11,868	9,521

Source: Authors' analysis based on data from Census of India 2001 and Planning Commission (2003).

Table S2.2 compares the overall characteristics of the districts in the three phases of MGNREGA. While there is no difference in the population growth rate, the literacy rate is much higher in Phase III districts. The three criteria used to determine the roll-out confirm that Phase I districts are the most "backward." Average Schedule Castes and Tribes population at 38.4 percent is the highest, while agricultural wages and output per worker are the lowest.

Over this period, educational outcomes improved in all districts: Enrollment increased and proportion of repeaters declined. There is also a growth in number of schools. Hence, the data consist of an unbalanced panel of schools.

S3. Robustness Tests

Predata Subsample Selection Issue

Since the study is using a subsample of states from the main sample to control for pre-trends due to data availability limitations, it is shown that this subsample is not selected in any way that can confound the results. There are 10 states or Union Territories for which data are not available in years subsequent to 2003 but prior to 2005 and are thus used in the empirical analysis in the paper.[3] These states are thus not used, and the study replicates the analysis from table 2 and table 3 for only the states for which District Information System for Education (DISE) data are available since 2003. The results from this exercise are reported in tables S3.1 and S3.2 and are remarkably similar to those reported in tables 2 and 3. This test assures that selection into the sample does not confound the results.

Year-by-Year Comparison

The study runs a year-by-year difference-in-difference model comparing early versus late MGNREGA districts as in Imbert and Papp (2015) using the sample for which the 2003 data are available. The coefficients and the confidence intervals are plotted in fig. S3.4. A large decline in enrollment was observed in 2006, the year MGNREGA was introduced, and subsequently enrollment in early phase districts continues to be lower relative to the preprogram years.[4] In addition, it is shown here that the difference-in-difference

3 These states or Union territories are Andaman and Nicobar Islands, Arunachal Pradesh, Dadra and Nagar Haveli, Daman and Diu, Goa, Haryana, Jammu and Kashmir, Lakshadweep, Manipur, and Pondicherry.

4 Note that for 2003 and 2004, the study does not have several school-level control variables in the data. Specifically, there are no data on teacher characteristics and school infrastructure variables. Thus, the regression analysis in this specification excludes these variables.

Table S3.1. The Impact of Introduction of Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGA) on Enrollment (2005–06 to 2008–09)

	Dependent variable: total enrollment				
	Overall enrollment			Primary	Upper-primary
	(1)	(2)	(3)	(4)	(5)
MGNREGA commenced	−2.23**	−2.23**	−1.93**	−2.20**	−0.42
	(1.04)	(1.04)	(0.98)	(1.06)	(0.59)
Year- & School-fixed effects	Yes	Yes	Yes	Yes	Yes
Controls for backwardness [a]	Yes	Yes	Yes	Yes	Yes
Incentives [b]	Yes	Yes	Yes	Yes	Yes
Teacher characteristics [b]	Yes	Yes	Yes	Yes	Yes
School infrastructure [b]	Yes	Yes	Yes	Yes	Yes
District demographics [c]	–	Yes	Yes	Yes	Yes
State-specific trends	–	–	Yes	Yes	Yes
Observations		3,478,376		2,961,395	1,073,974
Number of schools		1,068,298		908,531	362,132

Source: Authors' analysis based on data from the District Information System for Education (2005–06 to 2008–09).

Note: Excluding States for which 2003–04 and 2004–05 data are unavailable. Excluded Jammu and Kashmir, Haryana, Arunachal Pradesh, Manipur, Daman and Diu, Dadra and Nagar Haveli, Goa, Lakshadweep, Pondicherry, and Andaman and Nicobar Islands, to form a consistent panel with figs. S3.2–S3.4. a) To account for the backward district status that influenced selection into the program, the study controls for Scheduled Castes and Scheduled Tribes population from Census of India 1991, agricultural wage in 1996–97, and output per agricultural worker in 1990–93, interacted with year dummies. b) School incentive programs include textbooks, stationery, uniforms, attendance scholarships, and other incentives; teacher characteristics include the number of male and female teachers; infrastructure includes the number of classrooms, classrooms in good condition, the existence of common toilet, women's toilet, electricity, and water facilities. c) District-level demographic characteristcs include total population, percentage of urban population, population growth rate, overall literacy rate, and women's literacy rate from the Census of India 2001. Robust standard errors clustered at district level are reported in parentheses. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

(DID) coefficients in 2004 and 2005, prior to the program roll-out, are small and statistically insignificant. This clearly demonstrates that there is no pre-trend that could be potentially biasing the estimates.[5] This further substantiates the study design.

Placebo Test for Investigating Pre-Trends in Children's Employment Outcomes

The study also runs a placebo falsification exercise to test if the increase in likelihood of children's employment post-treatment in Phase I and Phase II districts relative to Phase III districts is due to a pre-trend (results shown in table S3.5). The study uses 1998–99 and 2004–05 years and runs the same equation (3) DID empirical specifications as table 6. The results from this placebo test reveal insignificant effects on child employment (column 1) in Phase I and Phase II districts unlike the results reported in table 6. Hence, it is reasonably assured that the program induced an increase in the likelihood of employment for children. In column (2), however, it is found that the children in early phase districts were less likely to be doing chores. The coefficient for Phase I districts is negative and statistically significant at the 5 percent significance level, and for Phase II districts is negative and significant at the 1 percent significance level. Table 6, column (2), reports the respective coefficients estimated for the program period. These are positive, small in magnitude, and statistically insignificant. This could imply that relative to the earlier period, the likelihood of children doing household chores has increased in early phase districts. Accounting for this in a specification where the three waves of data are used (1998–99, 2004–05, and 2008–09) with two pre-periods in the DID estimation, the study does not find a statistically significant effect for domestic chores but does find a robust and statistically significant estimate for employment. Hence, the results for domestic chores should be interpreted with caution.

5 Also, the same patterns appear if 2002 is used as the reference year instead of 2003, but the number of districts for which the enrollment data are available is smaller and hence the 2002 data are not used.

Table S3.2. The Impact of the Introduction of MGNREGA on Enrollment (2005–06 to 2008–09)

	Total enrollment		Primary enrollment		Upper-primary enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
MGNREGA commenced	−1.91**	−1.345	−2.15**	−1.549	−0.43	0.391
	(0.97)	(1.300)	(1.06)	(1.304)	(0.61)	(0.837)
MGNREGA commenced		0.314		0.755		−1.408
*Government school		(1.903)		(1.975)		(1.078)
MGNREGA commenced		−8.183**		−12.43***		−0.491
*Private school		(3.387)		(4.183)		(1.466)
Gvt school*year dummies	–	Yes	–	Yes	–	Yes
Pvt school*year dummies	–	Yes	–	Yes	–	Yes
Year- & school-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls for backwardness [a]	Yes	Yes	Yes	Yes	Yes	Yes
Incentives, teacher, & infrastructure [b]	Yes	Yes	Yes	Yes	Yes	Yes
District demographics [c]	Yes	Yes	Yes	Yes	Yes	Yes
State-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
Controls for pre-trends [d]	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,469,995		2,953,317		1,072,039	
Number of schools	1,060,053		900,590		360,252	

Source: Authors' analysis based on data from the District Information System for Education (2003–04 to 2008–09).

Note: Controlling district-level pre-trends from 2003–05, excluding states for which 2003–04 and 2004–05 data are unavailable. Excluded Jammu and Kashmir, Haryana, Arunachal Pradesh, Manipur, Daman and Diu, Dadra and Nagar Haveli, Goa, Lakshadweep, Pondicherry, and Andaman and Nicobar Islands, to form a consistent panel with figs. S3.2–S3.4. a) To account for the backward district status that influenced selection into the program, the study controls for Scheduled Castes and Scheduled Tribes population from Census of India 1991, agricultural wage in 1996–97, and output per agricultural worker in 1990–93, interacted with year dummies. b) School incentive programs include textbooks, stationery, uniforms, attendance scholarships, and other incentives; teacher characteristics include the number of male and female teachers; infrastructure includes the number of classrooms, classrooms in good condition, the existence of common toilet, women's toilet, electricity, and water facilities. c) District-level demographic characteristcs include total population, percentage of urban population, population growth rate, overall literacy rate, and women's literacy rate from the Census of India 2001. d) The study also adds the controls for district-level pre-trends, which include the changes from 2003–04 and 2004–05, interacted with year dummies. Robust standard errors clustered at district level are reported in parentheses. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

Table S3.3. Heterogeneous Impact of the Introduction of MGNREGA on Enrollment (2005–06 to 2008–09)

	Total enrollment		Primary enrollment		Upper-primary enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)
MGNREGA commenced	−1.93**	−1.07	−2.20**	−1.34	−0.42	0.39
	(0.97)	(1.24)	(1.06)	(1.26)	(0.59)	(0.88)
MGNREGA commenced		−0.02		0.43		−1.32
*Government school		(1.85)		(1.94)		(1.09)
MGNREGA commenced		−8.41**		−12.74***		−0.43
*Private school		(3.38)		(4.18)		(1.48)
Gvt school*year dummies	–	Yes	–	Yes	–	Yes
Pvt school*year dummies	–	Yes	–	Yes	–	Yes
Year- & school-fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls for backwardness [a]	Yes	Yes	Yes	Yes	Yes	Yes
Incentives [b]	Yes	Yes	Yes	Yes	Yes	Yes
Teacher characteristics [b]	Yes	Yes	Yes	Yes	Yes	Yes
School infrastructure [b]	Yes	Yes	Yes	Yes	Yes	Yes
District demographics [c]	Yes	Yes	Yes	Yes	Yes	Yes
State-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,478,376		2,961,395		1,073,974	
Number of schools	1,068,298		908,531		362,132	

Source: Authors' analysis based on data from the District Information System for Education (2005–06 to 2008–09).

Note: Excluding States for which 2003–2004 and 2004–2005 data are unavailable. Excluded Jammu and Kashmir, Haryana, Arunachal Pradesh, Manipur, Daman and Diu, Dadra and Nagar Haveli, Goa, Lakshadweep, Pondicherry, and Andaman and Nicobar Islands, to form a consistent panel with figs. S3.2–S3.4. a) To account for the backward district status that influenced selection into the program, the study controls for Scheduled Castes and Scheduled Tribes population from Census of India 1991, agricultural wage in 1996–97, and output per agricultural worker in 1990–93, interacted with year dummies. b) School incentive programs include textbooks, stationery, uniforms, attendance scholarships, and other incentives; teacher characteristics include the number of male and female teachers; infrastructure includes the number of classrooms, classrooms in good condition, the existence of common toilet, women's toilet, electricity, and water facilities. c) District-level demographic characteristcs include total population, percentage of urban population, population growth rate, overall literacy rate, and women's literacy rate from the Census of India 2001. Robust standard errors clustered at district level are reported in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table S3.4. The Impact of the Introduction of MGNREGA on Performance Outcomes (2005–06 to 2008–09)

	Appearing at Exam/Enrollment	
	Grade 5 (1)	Grade 7 (2)
MGNREGA commenced (unit: %)	0.22	–0.17
	(0.23)	(0.19)
MGNREGA commenced	−0.25	−0.40
*Govenment school (unit: %)	(0.35)	(0.30)
MGNREGA commenced	−0.15	−0.07
*Private school (unit: %)	(0.34)	(0.29)
Gvt school*year dummies	Yes	Yes
Pvt school*year dummies	Yes	Yes
Year- & school-fixed effects	Yes	Yes
Controls for backwardness [a]	Yes	Yes
Incentives, teacher, & infrastructure [b]	Yes	Yes
District demographics [c]	Yes	Yes
State-specific trends	Yes	Yes
Observations	2,412,910	2,398,334
Number of schools	834,812	833,173

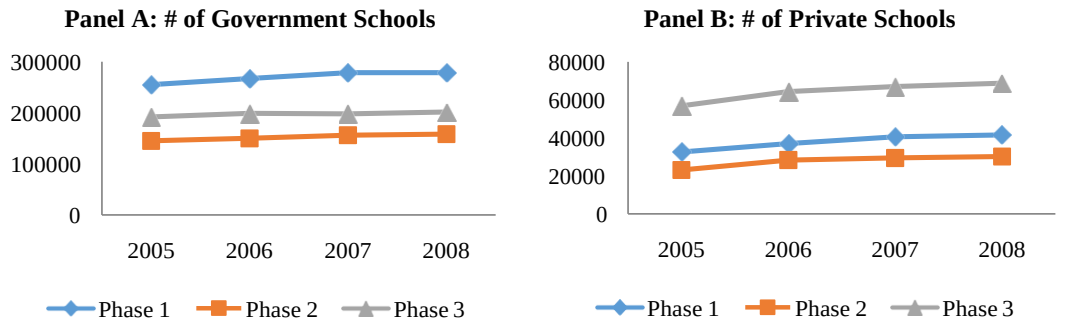
Source: Authors' analysis based on data from the District Information System for Education (2005–06 to 2008–09).
Note: a) To account for the backward district status that influenced selection into the program, the study controls for Scheduled Castes and Scheduled Tribes population from Census of India 1991, agricultural wage in 1996–97, and output per agricultural worker in 1990–93, interacted with year dummies. b) School incentive programs include textbooks, stationery, uniforms, attendance scholarships, and other incentives; teacher characteristics include the number of male and female teachers; infrastructure includes the number of classrooms, classrooms in good condition, the existence of common toilet, women's toilet, electricity, and water facilities. c) District-level demographic characteristcs include total population, percentage of urban population, population growth rate, overall literacy rate, and women's literacy rate from the Census of India 2001. Robust standard errors clustered at district level are reported in parantheses. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

Table S3.5. Placebo Test for Linear Probability Regression on Child Labor (Age: 5–15)

Outcome variables	Child reported working (employed, self-employed, or family business) (1)	Child reported doing unpaid household services (housework or free collection of goods) (2)	Child reported active (either (1) or (2)) (3)
Phase 1 × Post	0.40	−1.72***	−1.31
	(0.72)	(0.60)	(0.83)
Phase 2 × Post	0.33	−1.78***	−1.45**
	(0.60)	(0.52)	(0.73)
District- and year-fixed effects	Yes	Yes	Yes
Controls for backwardness [a]	Yes	Yes	Yes
Observations	155,916	155,916	155,916
Number of districts	438	438	438

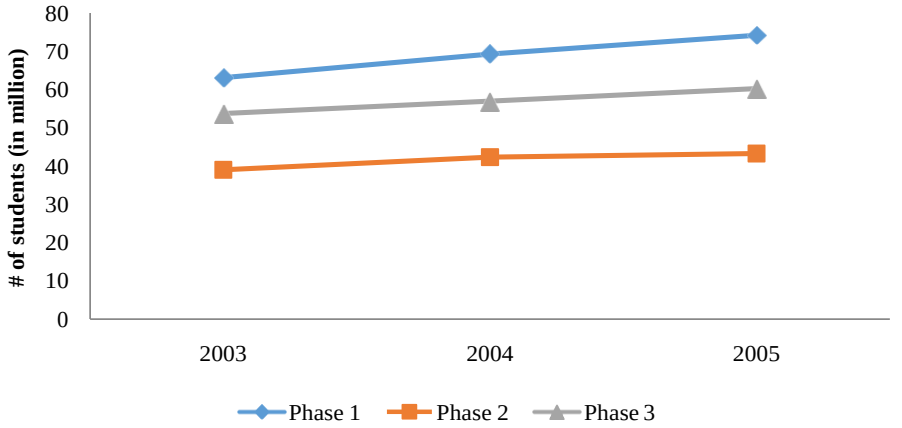
Source: Authors' analysis based on data from the National Sample Survey Office, Schedule 10, Round 61 (2004–05) and Round 66 (2008–09).
Note: a) To account for the backward district status that influenced selection into the program, the study controls for Scheduled Castes and Scheduled Tribes population from Census of India 1991, agricultural wage in 1996–97, and output per agricultural worker in 1990–93, interacted with year dummies. Robust standard errors clustered at district level are reported in parantheses. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

Figure S3.1. Phase-Wise Expansion in Different Types of Schools



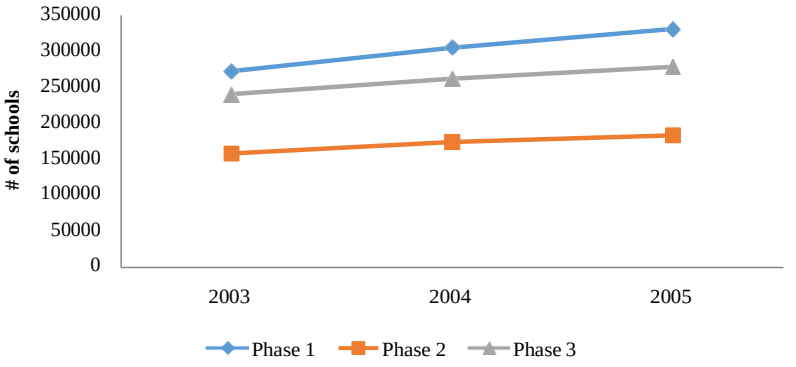
Source: District Information System for Education (2005–06 to 2008–09).

Figure S3.2. Total Enrollment



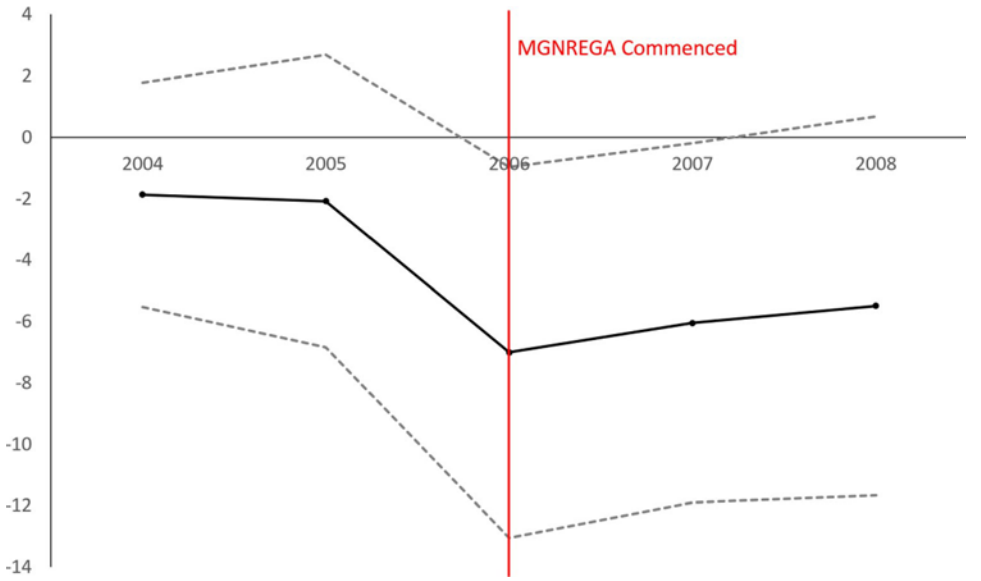
Source: District Information System for Education (2003–04 to 2005–06).
Note: The following 10 states or UTs: Andaman and Nicobar Islands, Arunachal Pradesh, Dadra and Nagar Haveli, Daman and Diu, Goa, Haryana, Jammu and Kashmir, Lakshadweep, Manipur and Pondicherry are excluded because the data in one or more years are not reported in DISE data.

Figure S3.3. Total Number of Schools



Source: District Information System for Education (2003–04 to 2005–06).
Note: The following 10 states or UTs: Andaman and Nicobar Islands, Arunachal Pradesh, Dadra and Nagar Haveli, Daman and Diu, Goa, Haryana, Jammu and Kashmir, Lakshadweep, Manipur and Pondicherry are excluded because the data in one or more years are not reported in DISE data.

Figure S3.4. Year-Wise Impact of MGNREGA



Source: District Information System for Education (2003–04 to 2008–09).
Note: The figure plots year-by-year DID coefficients (with 95 percent confidence interval) relative to baseline year 2003. MGNREGA was introduced in February 2006. A significant relative decline in enrollment in 2006 is observed in early MGNREGA districts relative to later ones. Subsequently, the enrollment is lower when compared to pre-program years. Prior to the program roll-out, the coefficients are smaller and statistically insignificant, ruling out differential pre-trends in prior years.

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