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Abstract

This paper estimates the effect of employment guarantee scheme on school attendance of 14-17 year old boys in India. Using data from a nationally representative household survey, we find that a household's work in the scheme reduces the probability that a 14-17 year old male household member attends school by 14%. Estimation using an older subsample who attended school before the implementation of the scheme shows that the negative effect is not driven by unobserved household heterogeneity that affects both school attendance and work in the scheme.

1. Introduction

Workfare has been widely used as a poverty-alleviation policy in both developed and developing countries. One of the earliest examples includes England's *Poor Law* of 1834 in which poor people were granted relief through residence in workhouses (Thane, 2000). In the context of developing countries, self-targeting feature of workfare is particularly attractive in absence of a cost effective administrative machinery to determine whether a relief claimant is in need or not (Besley and Coate, 1992). Whether workfare contributes to permanent reduction in poverty is still debated[1]. However, there is empirical evidence that workfare positively affects short-run outcomes like income and consumption (Jalan and Ravallion, 1999; Scandizzo et al., 2009; Ravi and Engler, 2012; and Azam, 2012).

India's recent workfare program under the National Rural Employment Guarantee Act (NREGA) guarantees 100 days of wage employment per year to every rural household. On an average, 50 million households have been provided employment every year since 2008-09 making it one of the largest workfare programs in the world (Ministry of Rural Development 2012). The primary objective of this Employment Guarantee Scheme (henceforth EGS) is poverty alleviation through employment generation. The secondary objective is to strengthen natural resource management - like water conservation, drought proofing, irrigation canals maintenance, land development, and rural road construction - to address the causes of chronic poverty. The EGS was first implemented in 200 districts of India in 2006-07. By April 2008, it was extended to cover the whole India. A burgeoning literature has already started to analyse the effect of this EGS on different aspects of rural economy, like market wage (Imbert and Papp 2013), aspiration of rural poor (Bhatia and Dreze 2006). However, there has been little work on the effect of the scheme on educational outcomes of households who

[1] See Devereux and Solomon (2006) for a review of developing country experiences.

work under this scheme. In this paper we study the effect of this EGS on the school attendance of youth in rural India.

Theoretically, a household's work in EGS can have two opposing effects on the school attendance of household members who are of school going age. Education being a normal good, the income effect would lead the household to invest more on education. The school attendance of members of households who do not work in the EGS increases as well when the EGS induced demand drives the market wage in upward direction. The substitution effect, due to EGS induced increase in market wage, would lead to withdrawal of household members from school and sent to work. Two features of the EGS in India make the substitution effect work differently for households who obtain work under EGS and households who do not obtain. One, work in the EGS must be performed by adult members of a household, implying that non-adult members cannot substitute school attendance for the EGS work. Two, a maximum of 100 days of work can be provided to a household in a year, implying that household's year-long labour market engagement cannot be the EGS. The substitution effect for the households who obtain work in EGS may mean that non-adult members (14-17 years) are withdrawn from school to perform the usual income generating work of adult member who opted for work in EGS. Relative strength of this latter effect may generate differential educational outcomes for youth of households who work in EGS vis-à-vis households who do not. This argument is consistent with the concern raised by Murgai et al. (2013) that advocates of the program often assume that workers remain idle in absence of the EGS.

In this paper we use data from a nationally representative survey of households, carried out in 2009-10, to estimate the effect of a household member's work in the employment guarantee scheme on the school attendance of younger male household members (14-17 years). We exploit the self-selection feature of the scheme to compare school attendance of the

households who applied for and received work in the EGS, with that of households who applied but didn't receive work in EGS. We find that a household member's work in the EGS reduces the probability that a 14-17 year old male member of the household attends school by 14%. We use an older subsample of youth (19-21 years) who attended the same level of schooling as 14-17 year olds before the implementation of the EGS. Estimation with this subsample shows that there is no significant difference in school attendance outcomes between households who later worked in EGS and those who applied but didn't obtain work. This implies that the effect of the EGS on school attendance of 14-17 year olds is not driven by unobserved household heterogeneity that affects both the chance to obtain work in the EGS and the school attendance outcomes. We also show that results are robust to the inclusion of local economic shocks that may affect both EGS participation and school attendance.

Rest of the paper is organised as follows. Section 2 provides details of the data and estimation strategy used in this paper. The next section presents the estimation results. The last section concludes the paper and provides policy implication of the results.

2. Data and estimation strategy

We use data from the 66th Round Employment and Unemployment Survey (2009-10) of National Sample Survey Office. The survey collects information on employment and unemployment of individuals from a nationally representative sample of households in India. In addition to employment status of household members, it also collects information on the educational attainment and current school attendance status of household members. We use both individual and household level information to estimate the effect of work in EGS on the school attendance of male household members who are 14-17 years old.

In order to work in the EGS a household must have a job card. Any adult member of the household with a job card can apply for work. The act mandates that work must be provided within 15 days after application (with wage fixed at the minimum wage for agricultural worker in a state). If work cannot be provide within 15 days, the applicant is entitled to unemployment benefit (fixed at 25% of the wage rate). Crucial to our estimation strategy is that not all households who applied for work in the EGS obtained work in the year 2009-10. Households self-select themselves to apply for work in the EGS. Some of them did not get work because of non-availability of public work under the scheme. Our analysis is based on the sample of households who applied for work in the EGS. We exploit the difference in the EGS work status among the applicant households for identification. The estimation model takes the following form

$$y_{ij} = \alpha + \beta x_{ij} + \gamma z_j + \delta EGS_j + (\mu_j + \varepsilon_{ij}) \qquad (1)$$

where y_{ij} is the educational outcome of individual i in household j, x_{ij} a set of individual characteristics, z_j a set of parental and household characteristics, EGS_j a dummy for households that got work in employment guarantee scheme, and $(\mu_j + \varepsilon_{ij})$ a composite error term with μ_j being unobserved household heterogeneity.

We use data for male youth (14-17 years old) to estimate the effect of EGS on their school attendance. Individuals in this age group are eligible to work as they have crossed the legal age limit protected by child labour law. However, they cannot work in the EGS as only adult members of a household are eligible to work in the EGS. Thus this age group corresponds to the trade-off in household decision between school attendance and non-EGS work of youth household members. We call this subsample "14-17 years (2009-10)". The dependant variable for this subsample is a dummy for school attendance.

A major concern with the above estimation is that if unobserved household heterogeneity affects both the chance of getting work in EGS and educational outcomes of household members, we are likely to find significant estimate of δ even when the EGS work does not have any effect on school attendance. In order to be confident that δ captures the effect of the EGS work and not that of unobserved household heterogeneity, we estimate the model using an older subsample consisting of household members who have attended school before the implementation of the EGS in 2006-07. Among individuals of the younger subsample (14-17 years in 2009-10), 85% are attending secondary or higher secondary education. In the older subsample we choose individuals who are likely to attend secondary or higher secondary education in 2005-06 (a year before the first implementation of the EGS). Individuals who are 19-22 years in 2009-10 were 14-17 years in 2005-06. Thus the older subsample consists of individuals who were 14-17 years old in 2005-06. We call this subsample "14-17 years (2005-06)". We observe their educational attainment in 2009-10. In this age group those who have attained secondary or more education must have attended school in 2005-06. Thus, the dependant variable for this sub-sample is a dummy variable for school attendance in 2005-06, inferred from the attainment of at least secondary education in 2009-10. We use the same set of variables, including *ex post* EGS work dummy, for the estimation using data from this older subsample. If δ appears to be significant in this estimation we would conclude that the work in EGS is associated with unobserved household heterogeneity that affects both school attendance and the chance to obtain work in the EGS three years later. On the contrary if δ is insignificant in this subsample, we would infer that estimate of δ in the younger subsample is not contaminated by the presence of unobserved household heterogeneity and stands for true effect of the EGS work on school attendance.

An additional concern in the above estimation is the possibility of area-specific shocks that affect both EGS participation and school attendance. India has a long tradition of using work

program as social security. If ESG is used as a relief measure by government in drought affected areas, households in affected areas will be more likely to receive work under EGS conditional on application. At the same time, school attendance will respond negatively to drought induced income shock[2]. In order to account for area-specific shock, we use district level rainfall as an additional control variable in the estimation model.

Descriptive statistics of these two subsamples are presented in Table 1. The school attendance in 2009-10 sample is much higher than that in 2005-06 sample. Part of the difference can be attributed to the economy-wide rise in school attendance of young individuals from 2005-06 to 2009-10. There is very little difference in the EGS participation in these two samples – 56% in 2009-10 sample and *ex-post* 57% in 2005-06 sample.

[2] See Jacoby and Skoufias (1997).

Table 1: Descriptive statistics – male 14-17 years (2009-10) and male 14-17 years (2005-06)

	14-17 years (2009-10)		14-17 years (2005-06)
	Proportion		Proportion
School attendance	0.710		0.645
EGS (*ex post* for "2005-06" subsample)	0.561		0.577
Age *§*	15.422 (1.084)		15.554(1.103)
Land owned (in hectare) *§*	0.996 (2.029)		1.163(2.757)
Caste			
General Caste	0.204		0.205
Scheduled Tribe	0.346		0.342
Scheduled Caste	0.214		0.213
Other Backward Caste	0.236		0.240
Religion			
Hindu	0.756		0.723
Muslim	0.116		0.131
Christian	0.085		0.100
Other religion	0.043		0.045
Number of observations	3729		2707
§ Mean value (standard deviation)			

Two subsamples are very similar in terms of age, household land ownership, caste and religion. Average age of individuals (whose school attendance is the focus of this study) is 15.5 years in both samples. Land ownership is slightly higher in the older sub-sample. There is considerable variation in terms of caste and religion in both samples.

3. Estimation results

Estimation results of equation (1) are reported in Table 2. For the subsample "14-17 years (2009-10)", the dependant variable is a dummy for school attendance in 2009-10. For the older subsample "14-17 years (2005-06)", the dependant variable is a dummy for school attendance in 2005-06.

A probit model is used for both subsamples. Two specifications of the model are estimated for each subsample. The first specification includes EGS dummy, age of the individuals, parental education, land ownership, caste religion and district fixed effects. The second specification incorporates an additional variable on district level rainfall. We prefer to do separate estimations with rainfall variables, as the number of observations decline due to non-availability of rainfall data for some districts. Estimation results are presented in Table 1. Marginal effects and their standard errors are reported for each specification of the model.

Table 2: Estimates of school attendance of 14-17 year old male – 2009-10 and 2005-06

	14-17 years (2009-10)						14-17 years (2005-06)					
		(i)			(ii)			(i)			(ii)	
	Marginal effect		Std. error	Marginal effect		Std. error	Marginal effect		Std. error	Marginal effect		Std. error
EGS	-0.137	***	0.034	-0.152	***	0.040	-0.009		0.051	-0.047		0.060
Age	-0.183	***	0.013	-0.203	***	0.016	-0.070	***	0.017	-0.078	***	0.021
Father's education (No education omitted)												
Primary	0.192	***	0.035	0.161	***	0.040	0.210	***	0.050	0.200	***	0.059
Secondary	0.378	***	0.044	0.404	***	0.054	0.723	***	0.067	0.735	***	0.079
More than secondary	0.478	***	0.074	0.387	***	0.089	1.247	***	0.160	1.363	***	0.204
Mother's education (No education omitted)												
Primary	0.096	***	0.039	0.087	**	0.044	0.139	**	0.059	0.208	***	0.072
Secondary	0.149	***	0.060	0.154	***	0.075	0.262	***	0.097	0.341	***	0.124
More than secondary	0.059		0.154	-0.043		0.183	0.001		0.274	0.140		0.347
Land owned (in hectare)	0.018	**	0.008	0.019	**	0.009	0.059	***	0.013	0.057	***	0.014
Caste (General Caste omitted)												
Scheduled Tribe	-0.061		0.057	-0.090		0.063	-0.383	***	0.088	-0.343	***	0.105
Scheduled Caste	-0.076		0.048	-0.052		0.055	-0.251	***	0.076	-0.241	***	0.087
Other Backward Caste	0.061		0.045	0.073		0.052	-0.197	***	0.072	-0.227	***	0.084
Religion (Hindu omitted)												
Muslim	-0.157	***	0.052	-0.212	***	0.062	-0.404	***	0.081	-0.516	***	0.098
Christian	-0.073		0.107	-0.098		0.126	0.138		0.148	0.094		0.170
Other religion	-0.087		0.096	-0.085		0.110	0.024		0.139	0.092		0.172
Dristrict level annual rainfall (deviation from 1951-2000 average)				0.004		0.005				-0.008	*	0.004
District fixed effects			Yes			Yes			Yes			Yes
Log likelihood			-1723.731			-1283.873			-1281.300			-955.250
Number of observations			3729			2789			2707			2033

, ** and * stand for significance at 10%, 5% and 1% level, respectively.*

Estimation results from specification (i) for the subsample "14-17 years (2009-10)" shows that a household's work in the EGS reduces the probability of school attendance of its 14-17 year old members by 14%. The effect is highly significant after controlling for age, household characteristics and district fixed effects. Estimates of marginal effects of other variables have expected sign. Higher age is negatively, higher parental education and land holding positively associated with the probability of school attendance. There is no significant difference in school attendance among individuals from different castes who applied for work in the EGS. Among the religious groups, Muslims are less likely to attend school than other religions. In specification (ii), we introduce an additional control for district specific shock measure by rainfall deviation from long-run average rainfall of a district. For this subsample the variable is constructed by average annual rainfall (over 1951-2000) of a district from the rainfall of that district in 2009. Estimation results do not change much due to the inclusion of this additional control variable. The rainfall deviation in the year 2009 does not have any significant effect on school attendance. The effect of EGS on school attendance increases marginally, from 14% to 15%. We check the robustness of our result using other ways to incorporate district level rainfall data, such as annual rainfall in 2009, annual rainfall in 2008, deviation of 2008 rainfall from long-run average and logarithmic value of them. The results do not change substantially. Moreover, the effect of EGS on school attendance always remains around 14%.

Now turning to the estimates from specification (i) for "14-17 years (2005-06)" sample, we find that *ex post* work in the EGS is not significant. This confirms our hypothesis that obtaining work in EGS is not associated with unobserved household heterogeneity that affects educational outcomes. This is consistent with the design of the EGS - universal coverage, voluntary participation. Thus, obtaining work is solely determined by institutional capacity to provide work within 15 days from the application for work. Other results for this

subsample are very similar to those of "14-17 years (2009-10)" subsample except that individuals from non-general castes are less likely to attend school. This difference in caste results for older and younger individuals is consistent with Hnatkovsk et al. (2013) who find gradual convergence in educational outcomes of different castes since 1980s. The specification (ii) includes district level rainfall deviation in year 2005 from the long-run average rainfall. We do not observe any substantial change in the estimates. The *ex post* EGS work remains insignificant. The rainfall deviation in 2005 is marginally significant, indicating that higher rainfall deviation is associated with lower school attendance. Again we check the robustness of our result using other rainfall variables. The EGS remains insignificant in all specifications.

The negative impact of EGS on school attendance is consistent with the finding of Datt and Ravallion (1994) that other household members take up the displaced productive activity when someone joins a workfare program in India. Our results suggest that boys who are of working age but not eligible to work in EGS may replace the household member who opts for work in EGS with negative implication for their schooling.

4. Conclusion

Using data from households who applied for work in the EGS, we estimate the effect of a household's work in EGS on the school attendance of its younger members (male 14-17 years old). We find that a household's work in the EGS reduces the probability of school attendance of its younger members by 14%. Our estimation strategy confirms that the negative effect of the EGS is not caused by unobserved household heterogeneity that affects both a household's chance to obtain work in the EGS and educational outcome of its members. We also provide evidence that the result is not driven by area-specific shocks that

affect the probability of receiving work in EGS and the probability of school attendance in opposite directions. The results indicate that a well-intended employment guarantee scheme can have perverse effect with long term consequence for the beneficiaries and the economy as a whole. From a policy perspective, it is important to modify the design of EGS in India to arrest the negative effect on school attendance. An integrated approach like PROGRESA in Mexico that combines education, nutrition and health interventions in one package can be more successful in poverty alleviation.

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