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Does a food for education program affect school outcomes? The Bangladesh case

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Abstract The Food for Education (FFE) program was introduced to Bangladesh in 1993. This paper evaluates the effect of this program on school participation and duration of schooling using household survey data collected in 2000. Using propensity score matching combined with difference-in-differences methodologies, we find that the program is successful in that eligible children on average have 15% to 26% higher school participation rates, relative to their counterfactuals who would have been eligible for the program had they lived in the program-eligible areas. Conditional on school participation, participants also stay at school 0.7 to 1.05 years longer than their counterfactuals.

Keywords Education · Program evaluation

JEL Classification J38 · I28

1 Introduction

Education is an important form of human capital investment. Yet, not all children have a chance to go to school. Some children from poor households

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are likely to be poor in the future because their parents are unable to invest in their education.

Bangladesh introduced a Food for Education program (FFE) in July 1993. The main feature of the program is to provide a free monthly foodgrain ration contingent on the family being judged as poor and having at least one primary-school-age child attending school that month. The program is aimed at the alleviation of both current and future poverty. The novelty of this program is its commitment to long-term poverty alleviation via investment in children's education and the use of an in-kind foodgrain ration to also benefit short-run food and nutrition security. Our main focus is to assess whether poor households, who are eligible for the FFE program, are more likely to send their children to school (school participation) and keep them there longer (duration of schooling) than they otherwise would have done.[1]

Previous evaluations of the FFE program have indicated that it has had a significant effect on primary school enrolments. In a sample survey of Bangladesh schools in 1996, Alam et al. (1999) found that FFE schools had 53% higher enrolments in grade I than non-FFE schools and 30% higher enrolments in grade IV. There are two other studies evaluating the effects of the FFE program on children's education. Using an instrumental variable (IV) approach, Ravallion and Wodon (2001) and Ahmed and del Ninno (2002) compare those who are treated with the rest of the population and find that the treatment (either receiving the FFE foodgrain ration or the amount of grain received through the FFE program) has a positive and significant impact on school participation rates.

The current paper differs from the previous studies in the following ways: First, previous studies evaluated the average treatment effect on the population while we evaluate the effect of the intention to treat on the treated. We believe that the latter is a more salient effect to evaluate as it has more policy relevance. Second, our control group is those who would have been eligible had they lived in regions where the FFE program was introduced rather than those who did not receive the treatment (including those who would not have been treated) as in Ravallion and Wodon (2001) and Ahmed and del Ninno (2002). Third, our study recognises possible heterogenous treatment effect and uses propensity score matching (PSM) combined with difference-in-differences methodologies to estimate the effect of FFE eligibility on the eligible group. Finally, in addition to the effect of FFE on the outcome of school participation, it also estimates the impact of program eligibility on children's completed duration of schooling, an outcome previous studies have not

[1]Many other developing countries have also introduced similar programs, such as the Mexican PROGRESA and Oportunidades programs, Brazil's Bolsa Eschola Program, Colombia's school vouchers program and The Philippines' and other countries' early childhood development programs. Although these programs differ in design, evaluation studies normally find them to be effective with positive and significant impacts (see, for example, Skoufias and McClafferty 2001; Angrist et al. 2002; Bourguignon et al. 2003; Schultz 2004; Coady and Parker 2004; Behrman et al. 2005, 2007).

investigated. Our results reveal that the average effect of program eligibility on school attendance is significantly larger than the effect of receiving the grain subsidies as presented in previous studies.

The paper is structured as follows. The next section introduces background details on the operation of the FFE program, the survey and the data used in the analyses. Section 3 describes the evaluation strategy. Section 4 presents the evaluation results. Conclusions are given in Section 5.

2 Background, survey design and the data

Bangladesh is a developing country, and up to the mid-1980s, rural education had been neglected. In the late 1980s and early 1990s, the government of Bangladesh realised the importance of education and identified the development of human capital as a primary strategy for reducing poverty.

In 1993, the FFE program was introduced. Its aim was to use targeted food transfers to encourage poor families to enrol children in primary school and to keep them there. The expectation was that the program would have three benefits: to enhance human capital and, hence, reduce long-term poverty; to provide nutritional gains to poor families and to improve the targeting of government food subsidy programs, thereby reducing the large leakages from the foodgrain rationing program. The program started as a large-scale pilot program, and by 2000, it covered some 17,811 primary schools (27% of the total) and 2.1 million students (13%). FFE covered government schools and four of the eight categories of non-government schools.[2] The annual program expenditure of around $US 77 million represented 20% of the total expenditure on primary education in 1997/1998, up from 4.7% in 1993/1994 (Ahmed and del Ninno 2002). The cost per student beneficiary was about $US 0.10 per day in 2000.

The FFE program delivers a free monthly foodgrain ration contingent on the family being judged as eligible (meeting at least one of the four targeting criteria) and having at least one primary-school-age child attending school that month. The local Primary Education Ward Committee and the School Management Committee jointly prepare the list of beneficiaries.

If one primary-school-age child from an eligible family attends school, the household is entitled to receive 15 kg of wheat or 12 kg of rice per month. To receive the maximum of 20 kg of wheat or 16 kg or rice, the household must send more than one child and all primary-school-age children to school.[3] The enrolled children must attend 85% of classes in a month to receive a grain ration and attendance records are kept by teachers and submitted monthly

[2] Of the 66,235 primary schools in Bangladesh, 62% are government and 38% non-government.

[3] According to the survey information, the sample households on average consume about 21 kg cereals per week. Hence, the subsidy received from the FFE program is almost equivalent to one quarter of the monthly supply of cereal products for an average household.

to the Thana (local government) offices. They, and the School Management Committee, then arrange with the Ministry of Food for the grain to be delivered to a nominated warehouse for collection by the beneficiary family using a ration card. The family can consume the grain and/or sell it.[4]

The FFE program uses a two-step targeting mechanism. *First*, two to three Unions (districts) that are economically backward and have a low literacy rate are selected from each of the 460 rural Thanas (regions). All government, registered non-government, community (low-cost) and satellite primary schools, and one Ebtedayee Madrasa (religion-based) primary school, in these selected Unions are covered by the FFE program. *Second*, within each selected Union, households with primary-school-age children become eligible for FFE benefits if they meet at least one of the following four targeting criteria as assessed by the School Management Committees: (1) a landless or near-landless household that owns less than half an acre of land, (2) the household head's principal occupation is day labourer, (3) the head of the household is female and (4) the household earns its living from a low-income artisan occupation.

Three factors may prevent eligible households from receiving the food ration: First, only primary school students enrolled in FFE schools can receive the food ration. Students from eligible households enrolled in non-FFE schools cannot receive a food ration. Second, enrolled children must attend 85% of classes in a month. Third, only a maximum of 40% of students in each FFE school, including those who are not eligible, can receive the grain ration. Thus, if some schools have more than 40% of all the students who are eligible, some of these students will not receive a ration. In this situation, the decision as to who should receive the ration is made by the headmaster and teachers, and may change over time. If a child from a FFE-eligible household enrolls in a FFE school but does not receive the food ration in 1 year, he/she could receive a ration in following years if others drop out. Teachers endeavor to select the poorest households when they are faced with potentially eligible households beyond the 40% figure. The extent to which they succeed is not clear. However, the survey data indicate that, on average, the household income of students who receive the ration is 12% lower than eligible students in FFE schools who do not receive the ration, suggesting that teachers do target the poorest when faced with an excess demand.

In this paper, we evaluate the average effect of FFE program eligibility on children's school outcomes: school enrollment and completed duration of schooling by using PSM combined with difference-in-differences methodology. Assuming conditional independence (we will discuss this issue in more detail in Section 3), the PSM method has the advantage of matching the eligible group with more appropriate counterfactuals. In the case of violation of the conditional independence assumption, the difference-in-differences method

[4]Due to concerns about the loss in teaching time for food distribution, the Government in February 1999 relieved teachers of this responsibility and instead assigned the task to private dealers.

Table 1 Distribution of household (HH) program participation between FFE and non-FFE Unions

	FFE Unions		Non-FFE Unions	
	No. of HH	No. of children aged 6–13	No. of HH	No. of children aged 6–13
Eligible households	209	399		
Non-eligible households	191	336	200	343

Source: authors' own calculation from IFPRI-FMRSP sample survey database as described in Ahmed and del Ninno (2002)

gives us additional power to minimise possible contamination from selection on unobservables.

The data are from a survey of schools, households, communities and food grain dealers conducted by the IFPRI-FMRSP (Food Management and Research Support Project) in September–October, 2000. The sample includes 600 households from 60 villages in 30 Unions and 10 Thanas, including both FFE and non-FFE Unions.[5] Table 1 indicates the distribution of households and primary-school-aged children. There are 400 households from FFE Unions and 200 from non-FFE Unions. Within FFE Unions, 209 households with 399 children of primary school age (aged 6 to 13)[6] are program eligible households and 191 households with 336 primary-school-aged children are non-eligible. In the non-FFE Union sample, there are 200 households with 343 primary-school-aged children.

As discussed earlier, not all children from eligible households participated in the FFE program (Table 2). For children from eligible households in FFE Unions, around 14% are not in school, and 6% are attending non-FFE schools.[7] In addition, 95 eligible children (24%) attending FFE schools did not receive the foodgrain ration. This may be due to either the 85% school

[5]This survey was designed and conducted for the purpose of evaluating the FFE program. Detailed information on the survey is presented in Ahmed and del Ninno (2002). The sampling follows four steps. First, 10 Thanas are randomly selected with probability proportional to their population. Second, two FFE Unions and one non-FFE Unions per Thana were selected at random. Third, two villages from each Union were randomly selected. Fourth, 10 households that had at least one primary-school age child were randomly selected in each village. Ravallion and Wodon (2001) use the 1995–1996 Bangladesh nationwide Household Expenditure Survey.

[6]How to define "primary school age" is a difficult issue. According to Ahmed and del Ninno (2002), primary school starts at age 6 and finishes at age 10. However, many children start school late and some may repeat grades. As a result, the majority of 11 to 13-year-olds in our sample are still in primary school. Including those who have never gone to a school, only 11% of 11-year-olds have finished primary school, and these proportions for 12- and 13-year-olds are 22% and 39%, respectively. At age 14, this ratio increases to 60%. Thus, following Ahmed and del Ninno (2002) we define our primary school sample as aged 6 to 13 years.

[7]There are a few households (3%) in the sample of the FFE Unions that participated in a stipend program. The program consists of a small cash subsidy to poor households whose children attend school. The subsidy is only a fraction of that in the FFE program. In our analysis, we exclude observations that are receiving the stipend subsidy to insure that the estimated FFE program participation effect is not confounded by other factors.

Table 2 Primary-school-aged children program participation status within FFE Unions

	FFE-eligible households		FFE-non-eligible households	
	Freq.	%	Freq.	%
Total no. of children	388	100	319	100
Not attending school	55	14.18	102	31.97
Attending non-FFE school	22	5.67	93	29.15
Attending FFE school and receive ration	216	55.67	0	0

Source: authors' own calculation from IFPRI-FMRSP sample survey database as described in Ahmed and del Ninno (2002)

attendance rule and/or the operation of the rule that a maximum of 40% of the students in each FFE school can participate in the program at any one point in time.[8]

Table 3 presents summary statistics of variables relevant to selection rules. The first panel compares eligible and non-eligible households within FFE Unions. Incomes of non-eligible households are more than double that of eligible households. The non-eligible households have almost three times the land holdings of eligible households and 11% fewer household heads are labourers. These differences are statistically significant. The proportion of households headed by females is slightly higher for the eligible households than for non-eligible households, as is the proportion of household heads who are illiterate, though neither difference is statistically significant. These data suggest that, within each FFE union, the degree of compliance with the FFE eligibility criteria is quite high.

Panel 2 of Table 3 compares the total sample of households from FFE Unions with the total sample of households from non-FFE Unions. FFE Unions are slightly less affluent than non-FFE Unions, with the average annual household incomes being Tk13,082 and Tk14,333, respectively. In addition, FFE Unions have slightly smaller land holdings, and a higher percentage of household heads being a female, a labourer, and illiterate. None of these differences, however, is statistically significant.

We also compare the average difference in the two outcome variables, school participation and duration of schooling, for children from these groups. These data are presented at the bottom of each panel of Table 3. We find that, for the primary-school-age children (6–13 years of age), neither the average school participation rate nor the average duration of schooling differs significantly between the FFE and non-FFE Unions. This does not imply that the FFE program has no impact on schooling outcomes because some of the children from the FFE Unions are from non-FFE eligible households.[9] When

[8]From the sample survey, which asks each household the number of days children were absent from school in the month before the survey date, we find that about 60% of the 95 eligible children not receiving the subsidy were absent more than 15% of school days.

[9]Moreover, there are other household and individual characteristics which might affect schooling outcomes between FFE and non-FFE Unions that are not controlled for.

Table 3 Household (HH) characteristics for FFE-eligible and non-eligible groups

	FFE-eligible HH vs non-eligible HH in FFE Unions				FFE Unions vs non-FFE Unions			
	FFE HH	NFFE HH	Diff.	T-ratio	FFE U	NFFE U	Diff.	T-ratio
Household income	8339	18271	−9931	**−6.04**	13082	14333	−1251	−0.84
	(9457)	(20863)			(16690)	(17447)		
	209	191			400	200		
Total land	51.09	148.72	−97.63	**−5.49**	97.89	99.58		
	(70.85)	(232.82)			(175.79)	(193.24)	−1.69	−0.10
	202	186			388	192		
Proportion of household heads as labourer	0.28	0.17	0.11	**2.78**	0.23	0.21	0.02	0.49
	(0.45)	(0.37)			(0.42)	(0.41)		
	209	191			400	200		
Male household heads	0.86	0.91	−0.05	−1.53	0.88	0.91		
	(0.35)	(0.29)			(0.33)	(0.29)	−0.03	−0.95
	209	191			400	200		
Proportion of HH heads illiterate	0.28	0.27	0.02	0.34	0.28	0.26		
	(0.45)	(0.44)			(0.45)	(0.44)	0.02	0.39
	209	191			400	200		
Age 6–13 school participation	0.86	0.7	0.16	**5.35**	0.79	0.8	−0.01	−0.23
	(0.35)	(0.46)			(0.41)	(0.40)		
	399	336			735	343		
Age 6–13 school duration	3.33	2.87	0.46	**2.73**	3.12	3.07	0.04	0.22
	(2.06)	(2.36)			(2.22)	(2.29)		
	390	325			715	336		
Age 14–18 school participation	0.42	0.45	−0.04	−0.55	0.44	0.61	−0.18	−4.22
	(0.50)	(0.50)			(0.50)	(0.49)		
	101	99			200	122		
Age 14–18 school duration	5.62	6.93	−1.31	**−2.56**	6.26	6.99	−0.73	**−2.50**
	(3.52)	(3.37)			(3.50)	(3.30)		
	93	89			182	119		

Source: authors' own calculation from IFPRI-FMRSP sample survey database as described in Ahmed and del Ninno (2002)
Mean values are presented in the first row for each variable, standard deviations are in the second row in parentheses, and sample sizes are presented in the third row

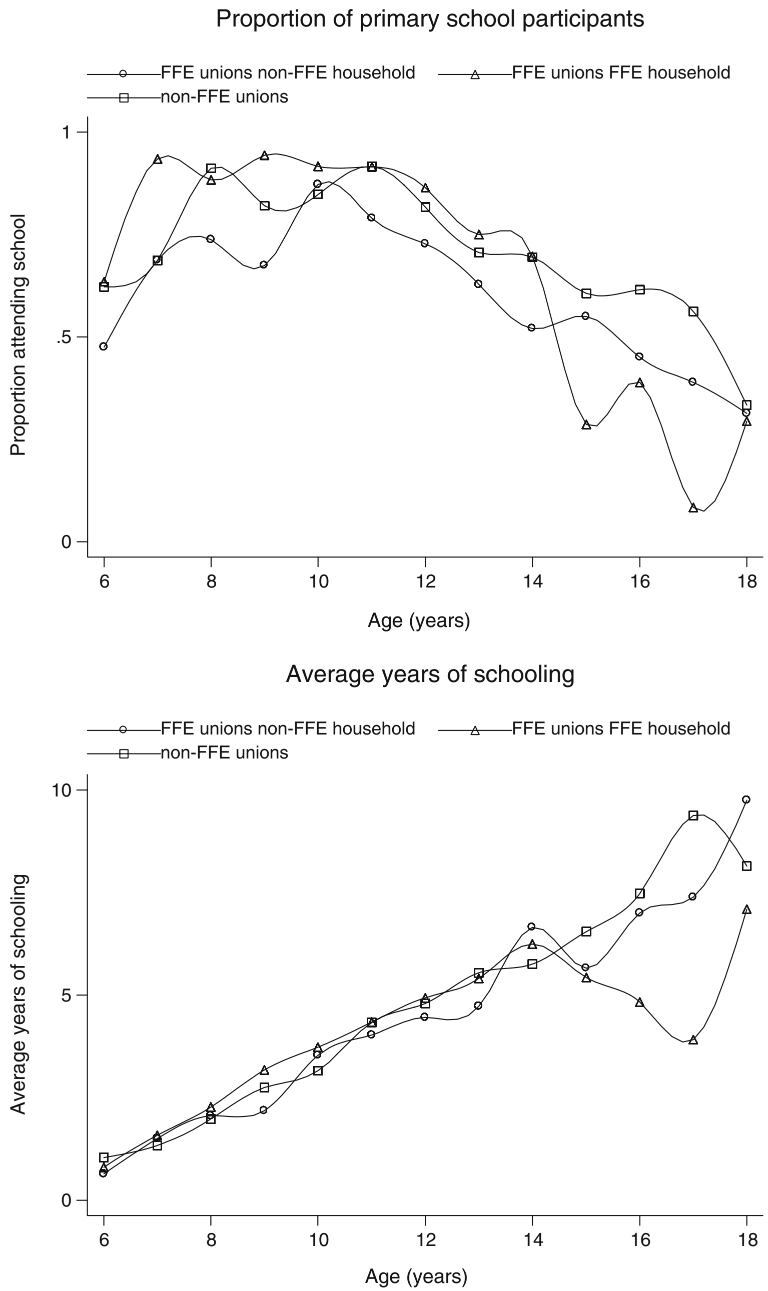

Fig. 1 Primary school participation and years of schooling by age and participation status

comparing the mean difference in the two outcome variables for secondary-school-aged children (14–18 years of age), who are not subject to the FFE program in either FFE or non-FFE Unions, we find that both the average school participation rate and the average duration of schooling are significantly higher in the non-FFE Unions than in the FFE Unions. It could be that, without the FFE program, the primary school participation rate for the FFE Unions would have been lower than the non-FFE Unions as well. Indeed, when comparing the schooling outcome variables between the FFE-eligible households and FFE non-eligible households in the FFE Unions (bottom of panel 1), we find that, on average, primary-school-age children from the FFE-eligible households of the FFE Unions have a significantly higher school participation rate and stay at school longer than children of the same age from the non-eligible households. On the contrary, their older siblings on average stay at school for less time than children of the same age from the non-eligible households.

We plot these outcome variables for the three groups by age in Fig. 1, which confirms that children between the ages of 6 and 13 from the FFE-eligible households in FFE Unions are the most likely group to attend school (top panel of Fig. 1) and have, on average, more years of schooling (bottom panel of Fig. 1) relative to both children from non-eligible households in FFE Unions and from households in the non-FFE Unions. The figure also shows that, for children above 13 years of age, the proportion who attend school and their average years of schooling are both lower for children from FFE-eligible households than their counterfactuals in the other two groups.

The above simple mean comparisons suggest that the effect of the FFE program on school participation and duration of schooling for primary-school-age children is positive and quite significant. These comparisons, however, do not take into account any differences in household and personal characteristics between different groups. In the following sections, more rigorous evaluations are conducted.

3 Evaluation strategy

Previous studies of the FFE program evaluate the effect of the amount of grain-ration received on school attendance (Ravallion and Wodon 2001; Ahmed 2000; Ahmed and del Ninno 2002). In this study, however, we focus on the effect of eligibility on schooling outcomes ("intention to treat"). The FFE program has two special features, which are (1) not every eligible child, but only those who are at school, can receive the grain ration, and (2) not every eligible child at school receives the grain-ration due to the maximum 40% rule. In this setting, if one is interested in the extent to which the program increases school attendance, the effect of eligibility should be the most relevant evaluation to conduct for the following reasons.

First, if FFE-eligible households can only receive the grain ration when their child(ren) go to school, the effect of the treatment measured as "receiving the grain-ration", by definition, is positively determined by the treatment and there is a reverse causality between the treatment and the outcome (being at school determines whether one can receive a grain-ration or not).

Second, the decision to attend school is made knowing that not attending school implies a zero probability of receiving the ration, whereas the probability of receiving a ration by attending school is very high. Thus, "eligibility to receive the grain-ration" induces children to go to school even if they may not receive the ration. Every child who goes to school, in response to their eligibility, should be counted as an effect of the program. The important impact of the program to be estimated, therefore, is the link between eligibility and schooling response.

Third, from the perspective of policy makers, perhaps the most important policy instrument available to them is whether to make the household eligible or not, but not whether the child will take up or not. Thus, the effect of "eligibility" is the most important parameter to estimate for policy makers (Rouse 1998; Katz et al. 2001; Bettinger and Slonim 2006).

Fourth, the estimation of the effect of "eligibility" requires fewer restrictions than the estimation of the effect of "receiving the grain-ration". Those who were eligible but did not receive the grain-ration may have done so due to various reasons, such as family and individual unobservable characteristics which deter them from going to a school, or perhaps they respond to unobservable characteristics of the schools and teachers who do not allow an eligible child to obtain the grain-ration (similar arguments can be found, for example, in Bettinger and Slonim 2006). Without information on these unobservables, it is difficult to construct a control group which would satisfy the "conditional independence assumption" required for the evaluation (see discussion below).[10]

Having explained the decision to evaluate the effect of eligibility rather than receiving a grain ration (as in Ravallion and Wodon 2001 and Ahmed and del Ninno 2002), we are now in a position to set up the problem.[11] Our purpose is to evaluate the *treatment* effect of FFE program eligibility on the treated (the eligible) on an outcome variable, Y. Assume this outcome variable depends

[10]With regard to the outcome of schooling duration, the eligibility (Intention to Treat) should also be the most relevant evaluation to conduct. In addition to points 3 and 4 listed above, which are applicable to any evaluation, we also know that one of the rules of the FFE program is that the subsidies given at school can be rotated among eligible children when those who are eligible exceed the 40% limit per school (see discussion in Section 2). This rule implies that children who initially do not receive a food subsidy but remain in school may eventually receive a subsidy providing the child is from an eligible household. Thus, staying on at school may be affected not only by whether the household is receiving a food-grain subsidy or not, but also by whether the child is eligible or not. This implies that there are more children who stay longer at school because of their eligibility status than those who do because they receive a grain-subsidy.

[11]As we evaluate the effect of eligibility on the two school outcomes, hereafter, we use treatment and eligibility interchangeably.

on a set of exogenous variables, X,[12] and on a treatment (eligibility), d. The evaluation problem can be expressed as:

$$Y_i = X_i\beta + d_i\alpha_i(X_i) + U_i, \tag{1}$$

where α_i measures the impact of the eligibility for individual i with characteristics X_i, β defines the relationship between X and Y, while U_i is the error term.

If assignment into the FFE program eligible group *within the FFE Unions* is based on observable characteristics, we may assume that identification comes from selection on observables. If so, the eligibility dummy variable d_i should be uncorrelated with the error term U_i. Then, using a sample of households in the FFE Unions, the simple regression estimation of Eq. 1 should provide a consistent estimate of the treatment effect, α, providing that (1) α is homogenous across the eligible and non-eligible groups and across individuals with different Xs, in other words, α has no subscript i, and (2) X includes all the variables affecting both eligibility and outcomes in the absence of the FFE program (Rosenbaum and Rubin 1985; Rubin 1978; Blundell and Costa Dias 2000; Ravallion and Wodon 2001).

However, three issues may prevent us from using OLS estimation to get a consistent estimate of the effect of eligibility on our outcomes. *First*, there may be non-compliance of the program assignment criteria, which may be associated with unobservable characteristics, which in turn is related to the outcome variable Y. If this is the case, then d is related to U, and OLS estimation of Eq. 1 will produce a biased estimate of the program effect even if α is homogenous. An advantage of using "eligibility" rather than "receiving the grain-ration" as the treatment may be that it reduces the problem of non-compliance since there is less reason to believe that non-compliance would occur in the process of assigning households into eligible groups.[13]

Second, OLS regression assumes a linear relationship between Xs and Y. In other words, it assumes that the effect of FFE eligibility is constant across individuals with different levels of X (homogenous α), which may not be plausible. For example, giving a very poor family 12 kg grain may induce them to send their child to school, but the same amount of grain may have less effect on a less poor family. This functional form problem may become very important when the treatment and comparison groups are not similar in characteristics, or in other words, lack of common support. When this happens, OLS conceals the problem as it does not quantify the extent to which the two groups are dissimilar in Xs.

[12] It is important to know whether the program selection criteria are observable to the program administrators or not. However, we could not find any written document which can verify this. Nevertheless, we were able to obtain confirmation from researchers (A. Ahmed and C. de Ninno) in IFPRI, who helped to design and introduce the program in Bangladesh, that the information was available to the administrators but it is not clear the extent to which the administrators verified the information.

[13] Unless the process of the assignment is affected by human errors, which we cannot rule out.

Third, using FFE-eligible and non-eligible households in FFE Unions to conduct the evaluation assumes that it is possible to find appropriate counterfactuals for the eligible group in the ineligible group, which is impossible. Unless the treatment effect is homogenous, failure to find the right counterfactuals or satisfy the common support condition will generate biased estimates of the treatment effect (Heckman et al. 1996, 1997). It is most likely that the treatment effect, α, is heterogenous between those who are eligible and those who are not within the FFE Unions. Thus, assuming that, given Xs, the outcomes of non-eligible individuals would have been the outcomes for eligible individuals had they not been treated would be a too strong assumption.

Fortunately, our data include not only households from the FFE Unions, but also households from non-FFE Unions.[14] This aspect of the data, together with the use of the PSM method, allows us to address the possible problems associated with non-compliance, lack of common support and heterogenous treatment effects (Rosenbaum and Rubin 1985; Rubin 1978; Blundell and Costa Dias 2000; Dehejia and Wahba 2002). PSM does assume selection on observables, but the assumption of selection on observables is not defined over the sample of eligible and non-eligible households within the FFE Unions, but between *eligible households in the FFE Unions* and *potentially would-have-been eligible control groups in the non-FFE Unions*. Even though, on average, the households from the non-FFE Unions are slightly more affluent than households from the FFE-eligible households in the FFE Unions, some households within non-FFE Unions may in fact satisfy the selection criteria and would have been eligible for the program had they lived in the FFE Unions. Thus, these households can serve as a valid counterfactual group, and we can assume that the selection of households into the treatment (i.e. living in the FFE Unions) is exogenous (the decision of where to live was made long before the FFE program was introduced). Furthermore, the use of households in non-FFE Unions ensures that there is enough common support between the treatment and control groups. PSM ensures that only those with very similar weighted Xs (propensity scores) in the treatment and control groups are compared.

Nevertheless, selection at the Union level is not random, and poor and less literate Unions are more likely to be selected, as discussed in Section 2. Thus, some unobservable regional effect could be correlated with both the treatment, d, and the outcome variable, Y. Previous studies have found that non-random program placement may bias the evaluation results (see, for example, Rosenzweig and Wolpin 1986). This indicates that a simple matching method may not solve the potential endogeneity problem at the Union level, but matching combined with difference-in-differences will solve the problem. Below, we provide a detailed discussion of our analytical strategy.

[14]In addition, the survey not only includes primary school children, but also secondary school children. The advantage of these data availabilities will be discussed later.

We first estimate a probit model of whether a household is eligible for the program from the sample of households in the FFE Unions. Using the estimated probit results, we then predict propensity scores of the potential eligibility probability for households in the non-FFE Unions. Those in the non-FFE Unions who have the same or a similar probability of being eligible for the program are then used as the counterfactuals for their eligible counterparts in the FFE Unions. To illustrate, assume Y_i is the value of the outcome for individual i from a eligible household, and Y_i' is the value of the outcome for the counterfactual, then the effect of the treatment on the treated, α_i, can be defined as:

$$\alpha_i = E\left(Y_i - Y_i' \mid P(X),\ d = 1\right) \tag{2}$$

Note that, as counterfactuals are from different regions, the simple matching method cannot distinguish the influences of region, such as differences in the macro-economic environment and other unobservable factors. Thus, we may actually obtain:

$$\alpha_i + R = E\left(Y_i - Y_i' \mid P(X),\ d = 1\right), \tag{3}$$

where R is the regional effect. However, utilising the richness in our data, we are able to separate the effect of the treatment, α, from the effect of the region, R. There are two ways to control for the possible regional differences. *First*, we can match children from non-FFE eligible households in the FFE Unions with their counterfactuals in non-FFE Unions (those who would not have been eligible for the program had the FFE program implemented in these Unions). As neither of these two groups participated in the program, the difference between them would be a pure regional difference. Thus, matching children from the eligible group in the FFE Unions with their counterparts from the non-FFE Unions and matching children from the non-eligible households in the FFE Unions with their counterparts from Non-FFE Unions leads to Eqs. 4 and 5 below, respectively:

$$\left(Y_{iFFE_1} - Y'_{iNFFE_1}\right) = \alpha_i + R, \tag{4}$$

$$\left(Y_{iFFE_0} - Y'_{iNFFE_0}\right) = R, \tag{5}$$

The difference between Eqs. 4 and 5 can distil the regional effect (both observables and unobservables), R, and results in a more accurate estimate of the treatment effect, α_i. In the estimation section, this is referred to as "Diff-in-Diffs 1".

The *second* option to control for the regional effect is to use children who are beyond primary-school-age (i.e. 14 to 18 years or secondary school), who are not eligible for the FFE program even if they are from FFE eligible households. The differences in schooling outcomes are evaluated between primary-school-age children who are from eligible households in FFE Unions and would-have-been eligible households in non-FFE Unions and between secondary-school-age children who are from eligible households in FFE

Unions and would-have-been eligible households in non-FFE Unions. The difference-in-differences between these two estimators can also be used to eliminate the effect of region on outcomes, referred to as "Diff-in-Diffs 2". This method, however, requires that the regional effect on primary school attainment is the same as that on secondary school attainment and that there is no spill over effect of the program participation into secondary school children in the treated group. Although it is very unlikely that these assumptions can be satisfied, the comparison may nevertheless add to our understanding of the impact of the program.

4 PSM with difference-in-differences estimators

To estimate propensity scores, a probit model of whether a child is from a program eligible household is estimated for a sample of children from FFE Unions.[15] The dependent variable is whether the household is eligible for the program and the independent variables are age, age squared and gender of the child, whether the child is a sister or brother of the household head as opposed to being his/her child or grandchild,[16] mother's and father's years of schooling, whether the household head is a labourer or not, the gender of the household head, and a group of household composition variables including number of male and female children in a household, number of primary-school-aged children in a household, and household size. In addition, we also include household total income, total land holding, total health expenditure, housing wealth, and other wealth. Further, to capture the community facility effect, we include distance between the home and the nearest primary school, the nearest bus stop, the nearest shop and distance between home and the nearest drinking water. Finally, dummy variables indicating the region (Thana) of residency are also used.[17] The estimated coefficients are then used to predict the probability of a child being in the program eligible group for children from both FFE and non-FFE Unions. Since the program was introduced 6 to 7 years before the data were collected, it is important to make sure that the matching characteristics are not affected by the program. Thus, we also estimate the propensity score equation excluding household income, number of children (fertility) and household wealth variables.

Figure 2 presents the distribution of predicted propensity scores for the groups of primary-school-age children from eligible and non-eligible households in the FFE Unions compared to those from the non-FFE Unions. Panels A and B of the figure present the propensity scores with the full set of the

[15] The results are available from the following website: http://econrsss.anu.edu.au/~meng/.

[16] This may affect whether a child is sent to school or not as household heads may treat their own children or grandchildren differently from their brothers or sisters.

[17] The reason we use Thana rather than Union is because, when matching across FFE and non-FFE Unions, the Union dummy variables are orthogonal to program participation, and this makes the matching impossible.

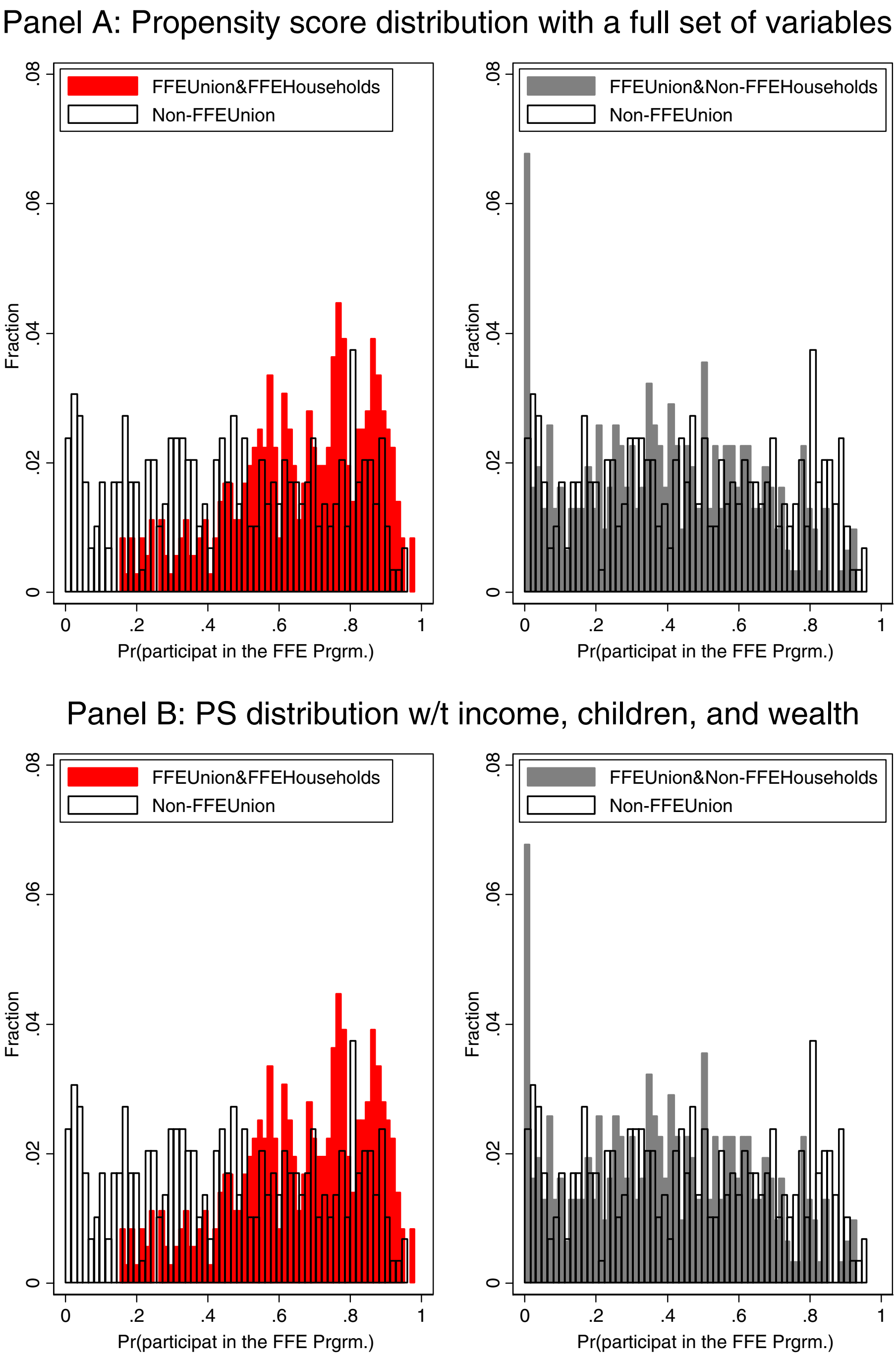

Fig. 2 Distribution of propensity score using sample of children aged 6 to 13 years

control variables and those without income, children and wealth variables, respectively. The figure indicates that, at the right tail of the distributions, where most individuals from the eligible group locate, there is a higher density of households from non-FFE Unions than from non-FFE eligible households in FFE Unions. The mean predicted probability of being eligible for the treatment group is 0.66; for non-FFE Unions, it is 0.46, while for non-FFE eligible households in FFE Unions, it is 0.39.[18] These suggest that, had the program been introduced in the non-FFE Unions, many households there would have been eligible to participate in the program, and hence, non-FFE Unions potentially provide an appropriate common support condition for the eligible group in the FFE Unions. At the same time, we also observe that at the lower end of the propensity score distribution, there is a similar density of households from the non-FFE households in the FFE Unions and from non-FFE Unions. Thus, we may be able to divide households from the non-FFE Unions into pseudo-eligible and non-eligible groups by matching their propensity scores with both the eligible and non-eligible groups in the FFE Unions so as to obtain a "Diff-in-Diffs 1" estimate.

It is possible that the PSM leads to the same children in the non-FFE Unions being matched both to the eligible and non-eligible groups in the FFE Unions. The overlapping of the matching will cause biased estimation of the treatment effect, assuming a heterogenous treatment effect. To avoid this, we first match eligibles from the FFE Unions with would-have-been eligibles from the non-FFE Unions, and then exclude the latter group before matching the remainder (would-not-have-been eligibles) in the non-FFE Unions to the non-eligibles in the FFE Unions. Later, we also test the sensitivity of this matching order.

The matching method used is "nearest neighbour matching" with replacement. This approach matches each treated unit with a single control unit which has the closest propensity score. Treated units for which no control unit is found within the maximum absolute distance specified are dropped. The distance is specified by setting a caliper width. As different caliper widths result in different numbers of treated units without a matching unit, the parameters being estimated will be different. To test robustness, we present results for two different caliper widths.

Our results are reported in Table 4. They show that, for the total sample relative to the non-FFE Union would-have-been eligible group, primary-school-aged children in the FFE Union eligible group are 12 percentage points more likely to attend school. Comparing primary-school-age children from non-eligible households in the FFE Unions with their counterparts in the non-FFE Unions, however, results in a negative difference of 9 percentage points, indicating that primary-school-age children from the non-eligible group in the FFE Unions are much less likely to go to school than their counterparts in the non-FFE Unions. The difference-in-differences 1 measure indicates that

[18]Comparable figures obtained from excluding income, children and wealth as control variables are 0.64, 0.47 and 0.43, respectively.

Table 4 PSM combined with difference-in-differences on primary school participation rate

Diff-in-Diffs 1	Eligibles in FFE Unions vs would-have-been eligibles in non-FFE Unions		Non-eligibles in FFE Unions vs would-not-have-been eligibles in non-FFE Unions		Difference-in-differences	
	Caliper = 0.01	Caliper = 0.005	Caliper = 0.01	Caliper = 0.005	Caliper = 0.01	Caliper = 0.005
Total sample						
Effect	0.12	0.12	−0.09	−0.09	0.21	0.21
Std err	0.05	0.05	0.06	0.06		
T-ratios	2.60	2.65	1.52	1.51	2.80	2.81
Matched treated	357	353	265	197		
Matched controls	154	155	102	87		
Total treated	389	389	319	319		
Total controls	306	306	152	152		
% of treated matched	91.77	90.75	83.07	61.76		
% of controls matched	50.33	50.65	67.11	57.24		
Male sample						
Effect	0.06	0.04	−0.12	−0.11	0.18	0.15
Std err	0.06	0.06	0.08	0.09		
T-ratios	0.96	0.65	1.47	1.26	1.76	1.41
Matched treated	159	126	123	83		
Matched controls	80	76	54	50		
Total treated	188	188	159	159		
Total controls	165	165	89	89		
% of treated matched	84.57	67.02	77.36	52.20		
% of controls matched	48.48	46.06	60.67	56.18		
Female sample						
Effect	0.17	0.17	−0.06	−0.09	0.23	0.26
Std err	0.07	0.07	0.10	0.10		
T-ratios	2.48	2.40	0.62	0.88	1.98	2.10
Matched treated	183	135	84	68		
Matched controls	73	68	43	41		
Total treated	208	208	160	160		
Total controls	141	141	73	73		
% of treated matched	87.98	64.90	52.50	42.50		
% of controls matched	51.77	48.23	58.90	56.16		

the average effect of program eligibility on the primary school attendance is 21 percentage points. This difference is statistically significant at the 1% level.[19]

Dividing our sample into males and females, the matching results show that the difference in school participation rates for boys between the eligible group in the FFE Unions and their counterfactuals in non-FFE Unions is small (4 to 6 percentage points) and not precisely estimated, while the difference between the non-eligible group in the FFE Unions and their counterparts in the non-FFE Unions is around negative 11 to 12 percentage points. Eliminating regional effects, the difference-in-differences 1 estimates result in a 15 to 18 percentage points improvement in school participation for boys. For girls, the school participation rate for the eligible group in FFE Unions is statistically significant and 17 percentage points higher than that of their counterfactuals from non-FFE Unions, with a negative 6 to 9 percentage points difference between the non-eligible group in the FFE Unions and their counterparts in the non-FFE Unions. The difference-in-differences 1 estimation, hence, indicates a 23 to 26 percentage point improvement in school participation for girls. We also investigate excluding income, children and wealth from the control variables in estimating propensity scores. The results are reported in Table 5, which show a consistent pattern of the effect but the magnitudes are smaller.

The above analysis is based on the mean treatment effect on the treated. Examining Fig. 1 reveals that the effects of the program may vary depending on the age of the children. Previous related research in developing countries finds that exposure to programs at different ages may have a differential impact (see, for example, Behrman et al. 2004, 2005; Armecin et al. 2006). We therefore separate our sample into two age groups to examine the program impact on children aged 6 to 9 and 10 to 13 years. The results, presented in Table 6, indicate that the effect for the younger age group is small and statistically insignificant, while the effect for the older age group is double that for the younger group and it is statistically significant.

We conducted various sensitivity test to examine the robustness of our estimates (see Table 9). The upper panel of Table 9 tests our decision on the matching order (i.e. first match the eligibles in the FFE Unions with their counterfactuals in the non-FFE Unions and then exclude matched ones from the non-FFE Union sample before matching the remainders to the non-eligible group in the FFE Unions). In this test, we match non-eligibles in FFE Unions

[19]There might be an issue related to the timing of the introduction of the program. As the program was first introduced in 1993 and our data were collected in 2000, it is possible that some schools in 2000 had only just introduced the program while others had been in the program for 6 years. However, our data show that 54% of the program schools in the sample were introduced to the program within the first year. Another 25% and 21% of the program schools introduced the program in the second and third years, respectively. We, therefore, assume that the timing issue could be ignored.

Table 5 PSM combined with difference-in-differences on primary school participation rate—sensitivity test (excluding income, children, and wealth from the PPS estimation)

Diff-in-Diffs 1	Eligibles in FFE Unions vs would-have-been eligibles in non-FFE Unions		Non-eligibles in FFE Unions vs would-not-have-been eligibles in non-FFE Unions		Difference-in-differences	
	Caliper = 0.01	Caliper = 0.005	Caliper = 0.01	Caliper = 0.005	Caliper = 0.01	Caliper = 0.005
Total sample with no income or children						
Effect	0.08	0.08	–0.07	–0.07	0.15	0.16
Std err	0.04	0.05	0.06	0.06		
T-ratios	1.75	1.81	1.10	1.24	1.93	2.09
Matched treated	355	354	268	256		
Matched controls	151	150	100	99		
Total treated	389	389	319	319		
Total controls	306	306	155	156		
% of treated matched	91.26	91.00	84.01	80.25		
% of controls matched	50.65	50.98	64.52	63.46		
Total sample with no income, children, and wealth						
Effect	0.08	0.08	–0.08	–0.05	0.16	0.13
Std err	0.04	0.04	0.06	0.06		
T-ratios	1.91	1.96	1.20	0.72	2.07	1.70
Matched treated	340	335	276	244		
Matched controls	170	170	96	96		
Total treated	389	389	319	319		
Total controls	306	306	136	136		
% of treated matched	87.40	86.12	86.52	76.49		
% of controls matched	50.65	50.98	70.59	70.59		

Table 6 PSM with diff-in-diffs by age group

Diff-in-Diffs 1	Full set of controls			Excluding income, children, and wealth as controls		
	Eligible in FFE Unions vs would-have-been eligibles in non-FFE Unions	Non-eligibles in FFE Unions vs would-not-have-been eligibles unmatched in non-FFE Unions	Difference-in-differences	Eligible in FFE Unions vs would-have-been eligibles in non-FFE Unions	Non-eligibles in FFE Unions vs would-not-have-been eligibles unmatched in non-FFE Unions	Difference-in-differences
6–9 years old						
Effect	0.04	−0.04	0.08	0.05	−0.08	0.13
Std err	0.06	0.10		0.06	0.07	
T-ratios	0.78	0.42	0.75	0.89	1.09	1.42
10–13 years old						
Effect	0.14	−0.06	0.20	0.12	−0.14	0.26
Std err	0.07	0.09		0.07	0.09	
T-ratios	2.01	0.69	1.75	1.58	1.64	2.27

with households in non-FFE Unions first and then exclude the matched and matching the remainders with the eligibles in the FFE Unions. We find that changing the matching order generates a larger effect of the program eligibility on children's school participation, increasing it from 21% to 24–26%. We also use the full sample of children from non-FFE Unions, including those who are matched with the eligible group, to match with children from non-eligible households in FFE Unions (see the lower panel of Table 9). The results also show a larger effect than the effect revealed in Table 4. These tests suggest that our results are robust to alternative matchings.[20]

We also estimate "Diff-in-Diffs 2". The results are reported in the upper panel of Table 7.[21] They show that the treatment effects are 15 to 17 percentage points, although not statistically significant. To some extent, this may relate to the small matched sample size used for the older age group. For the total eligible group in FFE Unions and their counterparts in the non-FFE Unions, only 39 and 28 children aged 14 to 18 are matched, respectively, when the caliper is set equal to 0.01, and 28 and 20 children, respectively when the caliper is set to 0.05. In addition, as discussed earlier, using "Diff-in-Diffs 2" imposes two strong assumptions: a common regional effect for different age groups and no spill over effect of the FFE program for secondary-school-aged children.

The violation of the assumption of common regional effect may cause an overestimation of the program participation effect if the regional effect is larger for secondary school participation than for primary school participation. This seems plausible, as children of secondary-school-age may have more and better employment opportunities than their primary-school-aged counterparts and, hence, in poorer and less educated regions, demand for education may be lower, which, in turn, may generate the outcome of lack of secondary school provision in poorer regions.

[20]The matching in this study is performed using the stata "psmatch" command. It allows us to set calipers to test the sensitivity of the results with regard to different distances of matching. In addition, using "psmatch", we are able to identify "would-have-been" eligibles (i.e. those who are from the non-FFE Unions and are matched with eligible households in FFE Unions). With this identification, we can exclude these "would-have-been" eligibles when we match non-eligible households in FFE Unions with the remainders in the non-FFE Unions (or "would-not-have-been" eligibles). However, using "psmatch", we are unable to obtain unbiased estimates and robust standard errors (see Abadie and Imbens 2004, 2006). Although "nnmatch" generates unbiased estimates and robust standard errors, it is impossible to identify the matched sample ("would-have-been" eligibles) in the non-FFE Unions in order to exclude them from the matching with the "would-not-have-been" eligibles in the FFE Unions. Without this exclusion, we are unable to conduct difference-in-differences analysis. Hence, we kept our main analysis using "psmatch". Nevertheless, we test the sensitivity of our results vs the results using "nnmatch" by comparing results for the sample of eligible group in the FFE Unions with the full sample of children from non-FFE Unions (both "would-have-been" and "would-not-have-been" eligibles) using "psmatch" and the results using "nnmatch" (see Table 10). These results show that the magnitude of the results differ only slightly, while they are both positive and statistically significant, suggesting that the "nnmatch" (bias-adjusted) results do not alter our overall conclusions.

[21]Note that the estimated propensity score in this matching uses a sample of children aged 6 to 18 years. The propensity score distribution for this estimation is reported in Fig. 3.

Table 7 PSM combined with difference-in-differences on primary school particiaption rate—sensitivity test (using older children as a control group)

Diff-in-Diffs 2	Eligibles in FFE Unions vs would-have-been eligibles in non-FFE Unions, aged 6–13		Eligibles in FFE Unions vs would-have-been eligibles in non-FFE Unions, aged 14–18		Difference-in-differences	
	Caliper = 0.01	Caliper = 0.005	Caliper = 0.01	Caliper = 0.005	Caliper = 0.01	Caliper = 0.005
Total sample						
Effect	0.09	0.08	−0.08	−0.07	0.17	0.15
Std err	0.05	0.05	0.13	0.16		
T-ratios	1.82	1.56	0.57	0.46	1.16	0.94
matched treated	203	157	39	28		
matched controls	122	106	27	20		
total treated	388	388	88	88		
total controls	306	306	93	93		
% of treated matched	52.32	40.46	44.32	31.82		
% of controls matched	39.87	34.64	29.03	21.51		
Diff-in-Diffs 2	Eligibles in FFE Unions vs would-have-been eligibles in non-FFE Unions, aged 6–13		Eligibles in FFE Unions vs would-have-been eligibles in non-FFE Unions, aged 14–18; excluding treated when they were young		Difference-in-differences	
	Caliper = 0.01	Caliper = 0.005	Caliper = 0.01	Caliper = 0.005	Caliper = 0.01	Caliper = 0.005
Test for spill-over effect						
Effect	0.09	0.10	−0.18	−0.13	0.27	0.23
Std err	0.05	0.05	0.15	0.17		
T-ratios	1.89	1.79	−1.14	−0.80	1.66	1.31
matched treated	206	156	17	15		
matched controls	123	107	14	13		
total treated	388	388	44	44		
total controls	306	306	93	93		
% of treated matched	53.09	40.21	38.64	34.09		
% of controls matched	40.20	34.97	15.05	13.98		

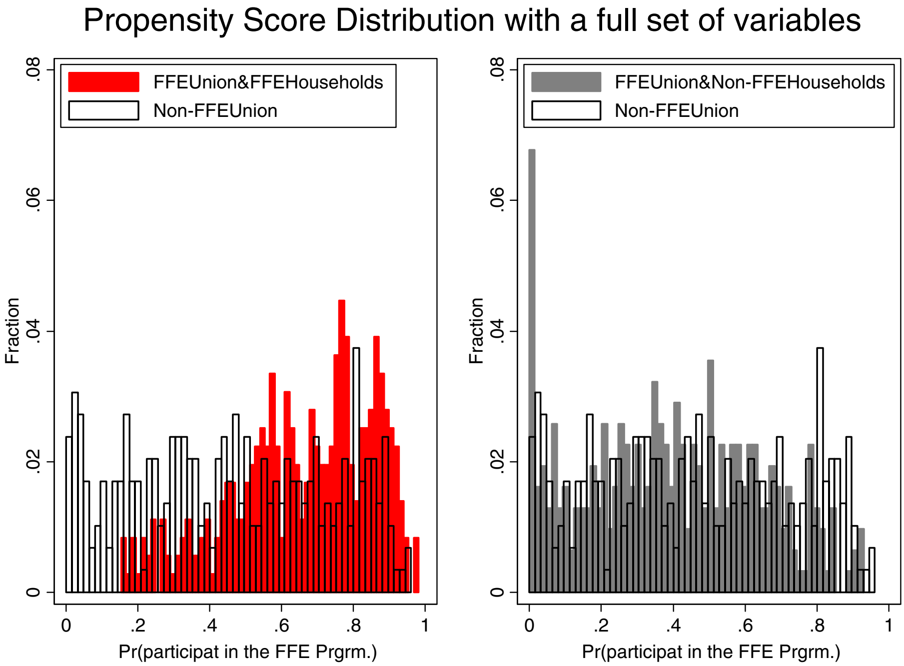

Fig. 3 Distribution of propensity score using sample of children aged 6 to 18 years

The violation of the no-spill-over-effect assumption may cause an under-estimation of the effect of the treatment if the spill-over effect of the FFE program on secondary school participation is positive. Given that the program had been in operation for more than 7 years when the survey was conducted, it is very likely that many children of secondary school age had been participants of the program when they were younger. The effect of spill over, however, is an empirical question, which may be tested. In our data, information on the time the first child of the household entered the FFE program is available. Using this information, we are able to exclude children who are aged 14 to 18 and who participated in the FFE program when they were in primary school. Excluding this sample of children (half of the children aged 14 to 18 from the eligible households in FFE Unions), we find that the difference-in-differences estimation in the lower panel is much larger than indicated in the upper panel of Table 7 (23 to 27 percentage points vs 15 to 17), suggesting that there is a spill-over effect, and the under-estimation caused by this effect is quite large. However, as the extent to which the assumption of a common regional effect may bias the results upward is not clear, we are unable to tell how close the effect estimated here is to the real effect of eligibility on school participation.

In addition to the school participation outcome, we also evaluate the effects of the FFE program on the duration of schooling, conditional on ever attending a school. As most of the children in the sample are still at school, the data on schooling duration is right-censored. The hazard model deals with this problem. To obtain an estimate of the completed duration of schooling, we first estimate a piecewise constant hazard model of school duration[22] and then use the estimated results to predict the completed schooling duration for each individual still at school, and hence, has a right censored dependent variable.[23]

To investigate the effect of FFE program eligibility on completed duration of schooling, the matching combined with difference-in-differences method is also employed and the results are reported in Table 8. The upper panel of the table presents the results of "Diff-in-Diffs 1" using combined male and female samples (matching on both propensity score and gender). It shows that, on average, the eligible group has 0.53 to 0.54 of a year longer schooling than their would-have-been eligible counterparts in non-FFE Unions, while children of non-eligible households in FFE Unions have 0.15 to 0.17 of a year less schooling than children from the would-not-have-been group in non-FFE Unions. The difference-in-differences estimates indicate an average effect of eligibility of 0.7 year more schooling. The middle and lower panels of Table 8 report the same results for male and female samples, separately. For the male sample, we find a 0.8 to 0.9 of a year average effect of eligibility, whereas for the female sample, the effect is slightly larger, ranging from 0.9 to 1.05 years. All effects are highly significant (Tables 9 and 10).

The results for "Diff-in-Diffs 2" estimation are reported in Table 11, which indicates an average effect of eligibility of 1 to 1.1 years more schooling. Once again, we think that using the older group to eliminate the regional effect is less reliable for the same reasons mentioned earlier, though the results are similar to the "Diff-in-Diffs 1" estimation.

In summary, we find that the effect of FFE eligibility on both school attendance and the duration of schooling are quite large, and this is especially true for girls. The result that FFE eligibility has a larger effect on girls than boys is interesting. Our conjecture is that, in a developing country where preference for boys is strong, poor households may prefer to send their sons to school rather than their daughters, and hence, had there been no FFE program, boys of the poor households would have been sent to school anyway. Thus, the FFE program actually provides incentives for parents to send their daughters to

[22] A piecewise-constant model is an exponential hazard rate model where the constant rate is allowed to vary within pre-defined time-segments. The model is specified as: $\lambda(t_i) = e^{\beta' X_i}\lambda_0(t_i)$. Independent variables, X, included are the same as those included in the estimation of the propensity score model.

[23] The results from the estimated duration model are available from the following website: http://econrsss.anu.edu.au/~meng/. Note that complete durations for the same individuals vary depending on the assumptions made by the researcher. When there are only primary-school-age children in the sample, the assumption is that nobody will continue schooling after primary school, whereas if there are children of secondary-school-age in the sample, the assumption is that nobody will continue schooling beyond secondary school. The calculated completed schooling in the latter case should be much longer than for the former. In this paper, we assume that nobody will continue schooling beyond secondary school rather than primary school.

Table 8 PSM combined with difference-in-differences on completed duration of schooling

Diff-in-Diffs 1	Eligibles in FFE Unions vs would-have-been eligibles in non-FFE Unions		Non-eligibles in FFE Unions vs would-not-have-been eligibles in non-FFE Unions		Difference-in-differences	
	clp=0.01	clp=0.005	clp=0.01	clp=0.005	clp=0.01	clp=0.005
Total sample						
Effect	0.53	0.54	−0.17	−0.15	0.70	0.70
Std. err.	0.08	0.08	0.13	0.13		
T-ratio	6.74	6.67	1.30	1.15	4.66	4.50
Matched treated	291	251	174	156		
Matched controls	144	137	95	93		
Total treated	388	388	319	319		
Total controls	307	307	162	169		
% of treated matched	75.00	64.69	54.55	48.90		
% of controls matched	46.91	44.63	58.64	55.03		
Male sample						
Effect	0.43	0.38	−0.36	−0.50	0.79	0.89
Std. err.	0.15	0.17	0.20	0.22		
T-ratio	2.87	2.28	1.80	2.33	3.17	3.24
Matched treated	85	67	74	68		
Matched controls	57	50	55	53		
Total treated	181	181	159	159		
Total controls	165	165	108	108		
% of treated matched	46.96	37.02	46.54	42.77		
% of controls matched	34.55	30.30	50.93	49.07		

Table 8 (continued)

Diff-in-Diffs 1	Eligibles in FFE Unions vs would-have-been eligibles in non-FFE Unions		Non-eligibles in FFE Unions vs would-not-have-been eligibles in non-FFE Unions		Difference-in-differences	
	clp=0.01	clp=0.005	clp=0.01	clp=0.005	clp=0.01	clp=0.005
Female sample						
Effect	0.90	0.95	0.00	−0.10	0.90	1.05
Std. err.	0.22	0.24	0.35	0.42		
T-ratio	4.08	4.00	0.00	0.23	2.16	2.16
Matched treated	121	99	55	44		
Matched controls	70	62	37	30		
Total treated	207	207	160	160		
Total controls	142	142	71	79		
% of treated matched	58.45	47.83	34.38	27.50		
% of controls matched	49.30	43.66	52.11	37.97		

(1) clp refers to caliper width used to match observations
(2) Completed durations are predicted durations from the schooling duration hazard model estimated for children aged 6 to 18. The results of the estimated hazard model are available upon request from the authors
(3) Std. Errs and t-ratios reported may be biased because complete durations are predicted from a hazard model

Table 9 Sensitivity tests for Table 4

Matching non-eligibles first	Non-eligibles in FFE Unions vs would-not-have-been eligibles in non-FFE Unions		Eligibles in FFE Unions vs would-have-been eligibles in non-FFE Union		Difference-in-differences	
	Caliper=0.01	Caliper=0.005	Caliper=0.01	Caliper=0.005	Caliper=0.01	Caliper=0.005
Total						
Effect	−0.14	−0.14	0.13	0.11	0.26	0.24
Std err	0.05	0.05	0.07	0.07		
T-ratios	2.82	2.82	1.84	1.59	3.12	2.93
Matched treated	310	310	333	283		
Matched controls	159	159	80	79		
Total treated	319	319	388	388		
Total controls	307	307	147	47		
% of treated matched	97.18	97.18	85.82	72.94		
% of controls matched	51.79	51.79	54.42	53.74		
Matching eligibles and non-eligibles from non-FFE Unions separately with over-lappped matched sample	Eligibles in FFE Unions vs would-have-been eligibles in non-FFE Unions		Non-eligibles in FFE Unions vs would-not-have-been eligibles in non-FFE Unions		Difference-in-differences	
	Caliper=0.01	Caliper=0.005	Caliper=0.01	Caliper=0.005	Caliper=0.01	Caliper=0.005
Total						
Effect	0.12	0.12	−0.14	−0.14	0.26	0.26
Std err	0.05	0.05	0.05	0.05		
T-ratios	2.61	2.61	2.82	2.82	3.82	3.84
Matched treated	355	338	310	310		
Matched controls	155	155	159	159		
Total treated	389	389	319	319		
Total controls	306	306	307	307		
% of treated matched	91.26	86.89	97.18	97.18		
% of controls matched	50.65	50.65	51.79	51.79		

Table 10 Sensitivity tests, PSMATCH vs NNMATCH

	Eligibles in FFE Unions vs their matched would-have-been eligibles in non-FFE Unions		Non-eligibles in FFE Unions vs their matched would-not-have-been eligibles in non-FFE Unions		Difference-in-differences	
	Caliper=0.01	Caliper=0.005	Caliper=0.01	Caliper=0.005	Caliper=0.01	Caliper=0.005
psmatch						
Effect	0.12	0.12	−0.14	−0.14	0.26	0.26
Std err	0.05	0.05	0.05	0.05		
T-ratios	2.61	2.61	2.82	2.82	3.82	3.84
nnmatch						
Effect	0.12		−0.10		0.22	
Std err	0.05		0.05			
T-ratios	2.57		2.17		3.36	

Table 11 Effect of eligibility on complete duration of schooling, using older children as a control group

Diff-in-Diffs 2	Eligibles in FFE Unions vs would-have-been eligibles in non-FFE Unions, aged 6–13		Eligibles in FFE Unions vs would-not-have-been eligibles in non-FFE Unions, aged 14–18		Difference-in-differences	
	clp=0.01	clp=0.005	clp=0.01	clp=0.005	clp=0.01	clp=0.005
Total sample						
Effect	0.53	0.54	−0.54	−0.44	0.07	0.98
Std err	0.08	0.08	0.48	0.52		
T-ratios	6.74	6.67	1.12	0.84	2.21	1.86
Matched treated	291	251	43	36		
Matched controls	144	137	26	23		
Total treated	388	388	88	88		
Total controls	307	307	93	93		
% of treated matched	75.00	64.69	48.86	40.91		
% of controls matched	46.91	44.63	27.96	24.73		

clp refers to caliper width used to match observations school. Schultz (2004) also found that the Mexican PROGRESA program had a larger effect on girls than boys.

The results obtained in this study are considerably larger than those found in Ahmed and del Ninno (2002), who observe a 8.4% difference in schooling participation for children from FFE beneficiary households. Although Ahmed and del Ninno (2002) use the same data as this paper, there are two important differences between their study and ours. First, they estimate the effect of receiving the grain ration on schooling participation while we estimate the effect of eligibility on schooling outcomes. Second, they use an IV estimate where a dummy variable indicating whether the household is a FFE beneficiary household or not is used to measure the treatment and the control group includes everybody who did not receive a grain ration, including non-eligibles in the FFE Unions and would-not-have-been eligibles in the non-FFE Unions. Their instrument is whether the household is living in the FFE Union or not. This creates two possible problems. One is that their control group is contaminated by both non-eligibles from the FFE Unions and would-not-have-been eligibles from the non-FFE Unions. The other problem is that their regression-based estimation does not fully take into account the non-linear treatment effect across different Xs.[24]

Our findings of the effect of the FFE program on school enrollment and duration of schooling of children in Bangladesh are also higher than the findings of the effect on the same outcomes of two conditional cash transfer programs, namely the Mexico PROGRESA program (Skoufias and McClafferty 2001; Schultz 2004; Coady and Parker 2004) and the Red de Proteccion Sol (RPS) in Nicaragua (Maluccio and Flores 2005). PROGRESA is found to have a 1 percentage point increase in primary school enrolment (Schultz 2004), and 5–8 percentage points increase in secondary school enrolment for boys and 11–14 percentage points for girls, and a 0.7-year increase in duration of schooling. The RPS increased primary school enrollment by 13 percentage points after 2 years. Although the cash equivalent benefit for the FFE program is much less than that for both PROGRESA and RPS,[25] the larger effect of the FFE program relative to that of PROGRESA and RPS may be related to many

[24] Ravallion and Wodon (2001), using 1995–1996 Household Expenditure Survey data, estimated the effect of receipt of the grain stipend on school participation. They find that every 114 kg of grain stipend received annually increases children's school participation rate by 18 to 19%. Given that the information on the average grain stipend received by each child is not provided in their paper, it is hard for us to compare their results with ours. Nevertheless, as they used exactly the same methodology as Ahmed and del Ninno (2002), we expect that their estimation is also an under-estimate of the "true" effect.

[25] Coady and Parker (2004) calculate that the average household benefit from PROGRESA is around 255 pesos in 2002, which accounts for about 22% of the monthly income of beneficiary families. RPS supplemented the average participant household per capita expenditure by around 18% (Maluccio and Flores 2005). Based on Ahmed and del Ninno (2002), we calculate the cash equivalent of the average FFE benefit to be around 4% to 5% of household expenditure, which should be even lower as a share of income.

other factors. However, a detailed comparative study of the different programs is beyond the scope of this paper.

5 Conclusions

This paper has evaluated the effect of the FFE program implemented in Bangladesh. Although the FFE program has several objectives, our main focus has been its impact on primary school participation and school duration. We found that, on average, the FFE program increased the school attendance rate of the treated group by 15 to 26 percentage points and increased their duration of schooling by 0.7 to 1.05 years.[26]

The treatment effect of the FFE program is larger for girls than for boys. This may have some positive long-term effects on future gender differentials in labour market outcomes.

The results obtained in this study are considerably larger than those found in previous evaluations of the Bangladesh FFE program. The main difference comes from the methodology used between this study and that of the previous studies. We argue that the PSM combined with difference-in-differences estimation used in this study provides a more accurate estimate of the average treatment effect on the treated than the IV estimate used in previous studies. In addition, an interesting result that emerges from this study is the larger effect of the FFE program on schooling outcomes relative to the effect of the two conditional cash transfer programs PROGRESA in Mexico and the RPS program in Nicaragua. To what extent initial enrolment rates influenced the differential outcomes in these three studies requires further examination, as does the effects of the in-kind vs cash transfers and the proportionate increases in household incomes they represent. We believe that further comparative studies into this area may be warranted.

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[26]The results obtained in this study are conditional on some assumptions. These assumptions include: (1) whether a household lives in the FFE or non-FFE Union is exogenous, and (2) children enrolled in schools which introduced the program in the first year (1993–1994) would have the same effect as those who enrolled in schools which introduced the program in the second or the third year. The violation of these assumptions may bias the results, and hence, some caution should be born in mind when interpreting our results.

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