Evaluating the Food for Education Program in Bangladesh*

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Abstract

The Food for Education (FFE) program was introduced to Bangladesh in 1993 and has been operating for more than 8 years. This paper evaluates the effect of this program on school participation and duration of schooling using household sample survey data collected in 2000. Various evaluation methodologies are employed. We found that the program is successful in that the participating children on average have 20 to 30 per cent higher school participation rates, relative to their counterfactuals who did not participate in the program. Conditional on school participation, participants also stay at school 0.5 of a year to 2 years longer than their counterfactuals. Using estimated earnings functions from the Bangladesh Household Income and Expenditure survey, these combined education effects of the FFE program would represent an increase in lifetime earnings of between 7 and 16 per cent if the participant is going to work in the rural sector, and 13 to 25 per cent if in the urban sector. These increases would bring large numbers of households above the poverty line.

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1 Introduction

Education is one of the most important forms of human capital investment. Yet, not all children have a chance to go to school. The fact that parents make decisions for their children implies that some children from poor households are likely to be poor in the future if their parents are unable to invest in their education.

The government of 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 a one-stone-two-birds strategy aiming at alleviation of both current and future poverty. The novelty of this program relative to other poverty alleviation programs is its commitment to a long-term poverty alleviation via investment in children's education. Thus, to evaluate the effectiveness of this program, our main focus is on whether the poor households who are in the FFE program are more likely to send their children to school (school participation) and keep them there (duration of schooling) than their non-FFE counterparts.

The paper is structured as follows. The next section introduces some background details on the FFE program, the survey and the data. Section 3 lays out the evaluation strategy and possible control groups. Sections 4 and 5 presents the evaluation results. Section 6 investigates the long-run effect of the FFE program. Conclusions are given in section 7.

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: enhance human capital and hence reduce long-term poverty, provide nutritional gains and improve the targeting of government food subsidy programs. From its beginnings in 1993 as a large-scale pilot program, by 2000 the FFE covered some 17,811 primary schools (27 per cent of the total) and 2.1 million students (13 per cent). Of the 66,235 primary schools in Bangladesh, some 62 per cent are government and 38 per cent non-government. The FFE covered government schools and four of the eight categories of non-government schools. The FFE program expenditure of around $US 77 million represented 20 per cent of total expenditure on primary education in 1997/98, up from 4.7 per cent in 1993/94 (Ahmed and del Ninno 2002). The cost per beneficiary student is currently about $US 0.10 per day. Food aid donors such as the U.S. also provided a significant portion of the grain for the program.

Previous evaluations of the FFE program have indicated that it has had a substantial effect on enrolments in primary school. In a survey of Bangladesh schools in 1996 Alam et al. (1999) found FFE schools had increased enrolments of 19-30 per cent prior to the introduction of the FFE compared to 2-5 per cent in non-FFE schools. After the introduction of the FFE the comparable figures were 13-14 per cent and -1-6 per cent, respectively. In cross-sectional comparisons in 1996 FFE schools had 53 per cent higher enrolments in Grade I than non-FFE schools and 30 per cent higher in Grade IV. In a different survey Ahmed and del Ninno (2002) found that in FFE schools enrolment changed by 35 per cent after the introduction of FFE, whereas in non-FFE schools the change was less than 3 per cent. Using household survey data for 2000, Ahmed and del Ninno (2002) found that in FFE Unions primary school enrolments in FFE beneficiary households were 20 percentage points higher than in non-beneficiary households in the same Unions. Using probit and Tobit analyses, they found that the probability of a child from an FFE household going to school increased by about 8 per cent.

These studies, however, adopted simple evaluation methodologies. To better understand the effect of the FFE program, we conduct a program evaluation based on a more rigorous evaluation methodology in this paper. To do so, understanding how the FFE program works is important.

The FFE program provides 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 Primary Education Ward Committee and the School Management Committee jointly prepare the list of beneficiaries. The family can either consume the grain and/or sell it.

If one primary-school-age child from a designated poor family attends school the household is entitled to receive 15 kg of wheat or 12 kg of rice per month. To be eligible for 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. The enrolled children must attend 85 per cent of classes in a month to be eligible and records on this are kept by the teachers and submitted monthly 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. 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.

The FFE program uses a two–step targeting mechanism. First, 2 to 3 Unions (districts) that are economically backward and have a low literacy rate are selected from each of the 460 rural Thanas. 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:

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 a female;
4. The household earns its living from low-income artisanal occupations.

Although in FFE Unions households which satisfy the above selection criteria should be eligible for participation, three important conditions may prevent them from receiving the food subsidy: The first is that only primary-school students can participate. In other words, those who are not at school or who are enrolled in high school are not eligible. Second, only students who enrolled in FFE schools can receive the food subsidy. Some students from eligible households may enroll in Non-FFE schools, and hence, cannot receive a food subsidy. Third, only a maximum of 40 per cent of students in each FFE school can receive the subsidy. Thus, if some schools have more than 40 per cent of the students who are eligible, some of these students do not receive a subsidy. In this situation, the decision on who should receive the subsidy is made by teachers, and it may change over time. In other words, if a child from a FFE-eligible household enrolls in a FFE school but does not receive food subsidy for one year, he/she could receive a subsidy in following years if others drop-out. Teachers endeavour to select the least poor households when they are faced with potentially eligible households beyond the 40 per cent figure. It is not clear the extent to which they succeed.

The data used in this study 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 to October, 2000[1]. The sample includes 600 households from 60 villages in 30 Unions and 10 Thanas. The sampling follows four steps. First, 10 Thanas are randomly selected with probability proportional to their size (PPS), based on Thana level population data. Second, two FFE Unions and one non-FFE Union per Thana were selected at random. Third, two villages from each Union were randomly selected with PPS, using village-level population data from the 1991 census. Fourth, 10 households that had at least one primary-school age child were randomly selected in each village from the census list of households.

Table 1 indicates the distribution of households and primary school aged children among FFE/Non-FFE households and FFE/Non-FFE Unions in the sample. In total 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) are participating households and 191 households are non-participating with 336 primary-school-aged children. In the non-FFE Union sample, the number of primary school aged children is 343.

As discussed before, not all children who are from eligible households actually participated in the FFE program. Table 2 presents the distribution of children aged 6 to 13 (primary-school-age-children) among four status categories. For children from eligible households in FFE Unions, around 14 per cent of them are not at school, and 6 to 8 per cent are attending non-FFE schools,

[1] Detailed information on the surveys are presented in Ahmed and Ninno (2002).

depending on whether those who participated in the stipend program are included or not [2]. In addition, there are 95 eligible children (24 per cent) attending FFE schools but who did not receive the food subsidy. This is due to the operation of the rule that a maximum of 40 per cent of the students in each FFE school can participate in the program.

3 Evaluation strategy and possible control groups

Our purpose is to evaluate the effect of FFE on an outcome variable, Y. Assume this outcome variable depends on a set of exogenous variables, X, and on treatment, d. The evaluation problem, thus, can be written as:

$$Y_i = X_i\beta + d_i\alpha + U_i \tag{1}$$

where α measures the impact of treatment for individual i. β defines the relationship between X and Y, while U_i is the error term.

Because assignment into the FFE program is based on observable characteristics, that is the Union one lives in and the four targeting criteria for households within the participating Unions, one can assume that the identification of this model comes from selection on observables. Hence it is probable that the participation dummy variable d is uncorrelated with the error term U and the OLS estimation of equation 1 should provide a consistent estimate of the treatment effect, α, providing that X includes all the variables affecting both participation and outcomes in the absence of participation. This is the so-called conditional independence assumption. This assumption insures that given X, the non-treated outcomes are what the treated outcomes would have been had they not been treated (Rosenbaum and Rubin, 1985; Rubin, 1978; Blundell and Costa Dias, 2000).

Nevertheless, there are limitations associated with the OLS estimate, which may result in inconsistent estimation of the treatment effect. First, OLS estimation imposes the assumption of linear functional form, which may cause inconsistent estimation due to missing non-linear

[2] The stipend program only operates in non-FFE Unions and 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.

terms in the error term which could relate to the treatment. Second, the estimate of equation 1 assumes that in the control group, there are individuals who have exactly the same X vectors as their treated counterparts, which may or may not be the case. This is the so-called common support condition. Failure to satisfy the common support condition will cause biased estimates of the treatment effect (Heckman, Ichimura, Smith, and Todd, 1996 and Heckman, Ichimura, and Todd, 1997). In our example, suppose income is the only conditioning variable and all low-income households are selected into the treatment group and high income households are not. The linear estimation of equation 1 then will not be able to disentangle the effect of income from the effect of participation.

A recent development in the program evaluation literature offers a solution. The propensity score matching method (Rosenbaum and Rubin, 1985; Rubin, 1979; Blundell and Costa Dias, 2000; and Dehejia and Wahba, 2002) solves the linearity problem and addresses the common support problem explicitly. Like the linear regression estimate, propensity score matching assumes selection on observables. The basic idea of matching is to construct counterfactuals for participants who have similar characteristics, X, and then compare the outcomes for those treated and their counterfactuals. The challenge in a simple matching model is how to construct counterfactuals when there are too many Xs, which makes it impossible to find matches in every cell. This is the so-called dimensionality problem.

Propensity score matching assumes matching on X is the same as matching on $P(X)$, where $P(X) = Pr(d = 1 \mid X)$. Thus, all the demensions in X can be summarised into a predicted probability of being treated. Those in the non-treated group who have the same or similar probability of participating would be used as the counterfactuals for their treated counterparts. To illustrate, assume Y_i is the value of the outcome for a treated individual i, and Y_i' is the value of the same outcome for its counterfactural, and the effect of treatment on the treated, α, is defined as:

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

As discussed above, both linear regression and propensity score matching assume selection on observables. This assumption is very strong. In addition, if counterfactuals are from different time periods or different regions, the simple matching method may not be able to tease out the effects associated with the time or region, such as differences in the macro-economic environment and other unobservable effects. However, if matching is combined with the difference-in-differences method, there is scope for selection on unobservables (Blundell and Costa Dias, 2000). The idea of difference-in-differences is very simple. If an outcome for the treated group is Y_{it_0} prior to the treatment and Y_{it_1} after the treatment, one may identify the effect of treatment as $\alpha_i = Y_{it_1} - Y_{it_0}$ assuming that no changes in personal characteristics or other personality changes, which may affect the outcome, occurred during the period. However, the difference between the two outcomes may be also attributable to the change in macro-economic environment. If this is the case, then estimated α_i is not the pure treatment effect.

To deal with this problem, one can identify a group of counterfactuals which experienced exactly the same changes in the macro-economic environment and would have been treated in the program, but did not. If the outcomes for this group in the pre- and post- program period are Y'_{it_0} and Y'_{it_1}, respectively, the pure effect of the treatment can be written as $\alpha_i = (Y_{it_1} - Y'_{it_1}) - (Y_{it_0} - Y'_{it_0})$. Combining propensity score matching with difference-in-differences, the effect of the treatment on the treated is then defined as:

$$\alpha_i = E[(Y_{it_1} - Y'_{it_1}) - (Y_{it_0} - Y'_{it_0}) \mid Pr(X),\ d = 1] \tag{3}$$

Equation 3 may generate a better estimate of the effect of treatments, which teases out both observable effect and time or regional effects on the outcome.

Before discussing our possible choice for control groups, we need to define the treatment in our study first. Two possible candidates may be used: being eligible for the program or actually participating in the program (receiving food-grain subsidies). Previous studies often use the second definition (Ahmed, 2000; Ahmed and del Ninno, 2002). We believe that the choice depends on the outcome one is interested in evaluating. In this paper, two outcomes to be evaluated are: whether households who are eligible to participate in the program are more likely to send their children to school or not, and whether their children are more likely to stay at school longer if they do attend. With regard to the first outcome, the treatment should be defined as the eligibility of a household to participate in the program but not whether the child is actually receiving food-grain subsidy for attending a school. This is because the decision to attend school is made by knowing that not attending school implies a zero probability of receiving the subsidy, whereas the probability of receiving a subsidy by attending school is very high, even though not a certainty. With regard to the second outcome, the length of time spent at school, the treatment should also be the eligibility. As we discussed earlier, the subsidies given at school are rotated among eligible children when those who are eligible exceed the 40 per cent limit per school. This rule implies that children who initially do not receive a food subsidy but remain in school, may eventually receive a subsidy. Thus, staying on at school may be part of the treatment effect.

Based on the survey design discuseed in the previous section, there might be several possible control groups in this study. First, households from FFE Unions but who do not meet the eligibility criteria for the FFE program. This group lives in the same macro-economic environment as the group of households who are eligible, but as the selection of eligibility depends on the four criteria mentioned before, this group of households suffers from the problem of lack of the common support condition relative to the treated group. This may not be a valid control group. Hereafter, this group is referred to as control group 1

Second, non-FFE Unions may constitute a good control group. Even though the households from the non-FFE Unions are likely to be more affluent than households from the FFE Unions on average, some households within these Unions may in fact satisfy the selection criteria and would have been eligible for the program had they lived in the FFE Unions. Thus, it may serve as a more valid control group than the non-eligible households in the FFE Unions. This group will be referred to as control group 2 in our empirical analysis. The possible drawback in using this group is that the macro-economic environment of the treatment and control groups is different. However, if we use propensity score matching combined with difference-in-differences, this problem may be eliminated. The idea is to estimate a probit equation for assignment into the program for households within the FFE union and use the results from this estimation to predict the probability of program assignment for households in the non-FFE union. The propensity score matching, then, allows us to match the treated and non-treated groups in the FFE unions with their respective counterfactuals in the non-FFE unions and to estimate $(Y_{iFFE_1} - Y'_{iNFFE_1}) = \alpha + R$ and $(Y_{iFFE_0} - Y'_{iNFFE_0}) = R$, where Y_{iFFE_1} and Y_{iFFE_0} refers to outcomes of the treated and non-treated groups in the FFE union, respectively, while Y'_{iNFFE_1} and Y'_{iNFFE_0} are outcomes of those who are from the Non-FFE union but whose propensity scores are matched to observations from the treated and non-treated groups in the FFE union, respectively. R is the regional or macro-economic enviromental effects. The difference between the two estimators can tease out the difference in the regional effect or the local macro-economic environment, R, and leave us with a purer estimate of the treatment effect, α.

The third possible control group is children who are beyond of primary-school-age (i.e. 14 to 18 years) and hence are not eligible to receive a food-grain subsidy. The differences in outcomes are evaluated between children of primary-school-age from FFE-eligible and the non-eligible households, and children of beyond primary school age from the FFE-eligible and non-eligible households. The difference-in-differences between the two estimators can be obtained satisfying the common support condition and the effect of age on outcomes can be eliminated. This may enable us to obtain a purer treatment effect, α.

This situation can be illustrated by the following example. Suppose the outcome Y is determined by two variables, age and income, in addition to the treatment effect, α. Suppose that income indicates the common support condition. Assume that we have four groups of children and their outcome functions are determined as follows:

$$Y_1 = \gamma_L Age_L + \beta_L INC_L + \alpha \qquad (\textit{prmry sch age chld from FFE HH}), \tag{4}$$

$$Y_2 = \gamma_L Age_L + \beta_H INC_H \qquad (prmry\ sch\ age\ chld\ from\ non-FFE\ HH), \tag{5}$$

$$Y_3 = \gamma_H Age_H + \beta_L INC_L \qquad (\textit{chld age } 14 \textit{ to } 18\ from\ FFE\ HH), \tag{6}$$

$$Y_4 = \gamma_H Age_H + \beta_H INC_H \qquad (\textit{chld age } 14 \textit{ to } 18\ from\ non-FFE\ HH), \tag{7}$$

where the subscripts H and L indicate high and low while Age and INC indicate age and income. The outcomes Y_1 and Y_2 are outcomes for children aged 6 to 13 from the program eligible and non-eligible households, respectively, and Y_3 and Y_4 are outcomes for children aged 14 to 18 from the program eligible and non-eligible households, respectively. The difference between Y_1 and Y_2 can control for the age effect on Y, leaving the income effect, $\beta_L INC_L - \beta_H INC_H$, and the treatment effect, α; the difference between Y_3 and Y_4, also controls for the age effect and leaves the income effect, $\beta_L INC_L - \beta_H INC_H$. The difference-in-differences estimator, $(Y_1 - Y_2) - (Y_3 - Y_4)$, therefore, can tease out the left over income effect and leave the treatment effect, α.

Table 3 presents some summary statistics of variables which determines selection rules for households from FFE and Non-FFE Unions as well as eligible versus ineligible households within the FFE Unions. The data indicate that the non-FFE Unions are slightly more affluent than FFE Unions, with average annual household incomes for the two groups being Tk14,334 and Tk13,082, respectively. In addition, the average land holding, percentage with household heads who are labourers, and percentage with females as household heads between the FFE and non-FFE Unions only differs slightly between the two groups. FFE Unions have slightly smaller land holdings, and a higher percentage of both labourer and female-headed households.

Within FFE Unions, however, the differences between non-eligible and eligible households are much larger. Incomes of the former are more than double the latter. The non-eligible households have almost three times the land holdings of the eligible households, the differences in the proportions of labourer and female-headed households are not as stark, although still significant.

In conclusion, these summary statistics indicate that, with respect to the selection criteria, the participating households from the FFE Unions resemble households from non-FFE Unions more than they resemble non-eligible households from the FFE Unions. This suggests that the common support condition may be better met in the sample of households drawn from the non-FFE Unions. In other words, many households in the non-FFE Unions might have been eligible for participation in the program had they lived in the FFE Unions.

We also plot the outcomes to be evaluated by treatment status and by age in Figure 1. Children between age 6 and 13 who live in FFE Unions and are from program-eligible households are the most likely group to attend school (bottom panel of Figure 1) and have, on average, more years of schooling (top panel of Figure 1) relative to both children from non-eligible households in FFE Unions and from households in the non-FFE Unions. Interestingly, 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 the least for children from FFE eligible households in FFE Unions compared to the other two groups.

4 Regression analysis

We first estimate a standard probit regression model on children's school attendance and a piecewise constant hazard model on duration of schooling.[3] For each of these models, two different specifications are estimated. The first is the standard regression as presented in equation 1 for a group of children who are aged 6 to 13. The coefficient α measures the treatment effect. The second regression includes the sample of children aged 6 to 13 years as well as those aged 14 to 18 years to obtain a difference-in-differences measure of the treatment effect. This model is specified below[4]:

$$Y_i = X_i\beta + d_i\alpha + Z_i\gamma + (d_i * Z_i)\delta + U_i \tag{8}$$

where Z is a dummy variable indicates children of primary school age (aged 6 to 13). Recall that only households with primary school children are entitled to FFE grain subsidies. Those households whose children are not at primary school are not eligible for subsidies, even if they are from otherwise program-eligible households. Thus, the difference between coefficients α and δ provides a difference-in-differences estimator of the effect of the treatment on school participation and schooling duration, which can eliminate both the lack of common support problem and age-related differences in school participation[5]

The independent variables included in the vector X 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

[3] The probit model is specified as $Prob(Y = 1) = \Phi(\beta\prime X)$ and the piecewise constant hazard model is specified as:$\lambda(t_i) = e^{\beta\prime X_i}\lambda_0(t_i)$.

[4] Note that although both equations 1 and 8 are specified in a linear form for simplicity, they are estimated as probit and piecewise constant hazard models in the empirical work.

[5] See section 4 for a more detailed explanation.

child or grandchild, number of male and female children aged 0 to 18 in a household, household size, mother and father's years of schooling, whether the household head is a labourer or not, the gender of the household head, total household income, household land holding, number of rooms in the household, value of the housing, distance between home and the nearest primary school, and a group of dummy variables indicating the region of residency. The two outcomes evaluated are whether a child is attending school or not, and the number of years a child has been in school.[6]

Two control groups are used. We first compare the effect of the treatment on school participation for children from FFE eligible households living in FFE Unions with children from Non-FFE eligible households living in FFE Unions (first control group) and then with children from Non-FFE unions (second control group). There are deficiencies associated with both control groups as mentioned before. Nevertheless, the difference-in-differences estimation presented in equation 8 should, to some extent, minimise these.

The results of estimated equations 1 and 8 on school participation are presented in Table 4. They are mostly consistent across the two models for each control group. The following discussion will, therefore, focus on the results from equation 8, except when we discuss the effect of the FFE program.

We observe a strong inversed U-shaped relationship between school enrolment and children's age for the estimations using both control groups. This coincides well with Figure 1. Boys are less likely to participate school than girls, though the difference is marginally significant when using the first control group and not significant when using the second control group. Father's schooling level has a strong positive effect on children's school attendance when using both control groups, whereas mother's schooling level contributes positively when using the second control group and negative but insignificantly when using the first control group. The relationships between the dependent variable and the land holding, occupation of household head, and household income are not consistent between the two samples. Using the first control

[6] In Ahmed and del Ninno (2002) a vector of village level variables is also included. These variables, however, are not obtained directly from the survey but are the sample means calculated for each village by the authors. We believe that these variables may not contribute significantly to either program participation or the outcomes of interest and hence do not include them in our estimations.

group, the land holding and the occupation of household head have a statistically significant effect in the expected direction on children's school attendance, while the household income variable is statistically insignificant.[7] For the second control group, however, household income has a significant positive effect on school participation, but there were no statistically significant effects of occupation and land holding.

In summary, most of the results are quite intuitive, suggesting that very young and older children are less likely to be at school and that poor households are less likely to send their children to school. In addition, more educated parents are more likely to send their children to school. However, the result that girls are more likely than boys to go to school is not a common finding in a developing country, especially in a South Asian country (World Bank, 2001), even though it is consistent with the finding presented in Ahmed and del Ninno (2002).

Turning to our main interest of the effect of the treatment on school participation, we find that the school participation rate of those treated is 16.2 per cent higher than their non-treated counterparts for the control group 1 sample of children aged 6 to 13 (Table 4). When using the second control group, this difference reduced to 6.4 per cent. These results, however, could be biased due to a lack of the common support condition between the treated and the control group 1, and a lack of a common macro-economic environment between the treated and the control group 2, as discussed in the previous section.

These biases may be partly eliminated by using the difference-in-differences estimator, which is presented in the second and fourth columns of Table 4. The coefficients on "treated group" and its interaction term with the "aged 6 to 13" dummy in columns indicated "equation 8", suggest the differences in the school participation rate between the treated and the control groups 1 and 2 for children aged 14 to 18, and children aged 6 to 13 years, respectively. The difference between these two coefficients in columns two and four can unearth both the lack of a common support condition (the comparison with the control group1)/the lack of a common macro-economic environment (the comparison with the control group 2), and the impact of differences in age on

[7] The insignificant result on household income may be related to the high correlation between land holding and household income (the correlation coefficient is 0.6). When we exclude the land holding variable from the regression, the effect of household income becomes positive and statistically significant, a t-value of 2.07. In addition, the occupation of the houshold head also becomes more statistically significant, with a t-value of 2.02.

outcomes. We found that for children aged 6 to 13, the school participation rate for children from program-eligible households is 22 and 16 per cent higher than their counterparts from non-program-eligible households using the first and second control groups, respectively. For children aged 14 to 18, however, this difference is minus 4 and 8 per cent, respectively, though neither is statistically significant. Thus, our difference-in-differences measure of the effect of program participation on school participation is 26 and 24 per cent for using the first and second control groups, respectively, and the χ^2 tests show that these differences are statistically significant at the 3 to 4 per cent level. Thus, the difference-in-differences estimators are 10 to 18 percentage points higher than the simple-difference estimators.

Table 5 reports the simple-difference and difference-in-difference measures of un-adjusted and adjusted program participation effects on the duration of schooling, conditional on ever attending school using the first (top panel) and second (bottom panel) control groups. The unadjusted measures are raw differences, while the adjusted measures are differences calculated using predicted complete duration from estimated results of equations 1 and 8 in a piecewise constant hazard model.

The differences between the estimations of the school participation equation (Table 4) and the schooling duration equation (Table 5) are two fold. First, the participation equation is estimated using a probit model, while the schooling duration equation is estimated using a piecewise-constant hazard rate model. As most of the children in the sample are still at school, the data we have on duration of schooling is right-censored. The hazard model deals with this problem. Second, the schooling duration equation evaluates schooling duration conditional on school participation. Thus, children who have never been to school are excluded from the sample.

We use results from the estimated hazard model to predict the complete schooling duration for each individual and then calculate the mean schooling durations for children aged 6 to 13 and 14 to 18 years who are from participation and non-participation households. These mean values and standard deviations are then used to calculate the effect of program participation on duration of schooling using simple-difference and difference-in-differences methods.

The unadjusted differences presented in the first part of each panel of Table 5 show that, conditional on school participation, there is little difference in duration of schooling between children aged 6 to 13 of the treatment group and the first and second control groups. However, children aged 14 to 18 from the first and second control groups on average stay at school 0.83 and 0.67 of a year longer than children of the treatment group, respectively, and the differences are statistically significant. When the unadjusted difference-in-differences are calculated, we observe that primary school children from the treatment group actually stay at school 0.82 and 0.72 of a year longer than their counterparts from the two control groups, respectively.

The adjusted differences are presented in the second part of each panel of Table 5. Note that the adjusted mean years of schooling durations are much longer than the actual (unadjusted) durations. This is because they are predicted complete durations. Controlling for all the variables included in Table 4, children from the treatment group on average stay in school 0.19 and 0.111 of a year longer than their counterparts from the two control groups, respectively, using the predicted duration from equation 1. These differences are statistically significant. When children aged 14 to 18 are included in the sample we found that children aged 6 to 13 from the treatment group stay in school 0.54 and 0.23 of a year longer than their counterparts from the two control groups, while these differences for children aged 14 to 18 years are negative. Using the difference-in-differences measure, we observe that the treated group on average stays in school 0.83 and 1.58 years longer than their two respective counterfacturals.

The estimations from this section show that on average the FFE program increases the primary school participation rate by between 24 and 26 per cent, depending on the control group used. For those who go to school, the program also increases their duration of schooling by between 0.8 and 1.6 of a year. These effects are quite significant. However, these estimates may still suffer from the problems of assuming linear specification of the models and lack of common support. The next section will try to solve these issues by using the propensity score matching method.

5 Propensity score matching and difference-in-difference estimators

To estimate propensity score, a probit model of whether a child is from an eligible program household is estimated for a sample of children from FFE Unions. The dependent variable is whether the household is eligible for the program and the independent variables include all the variables included in Table 4.[8] The estimated results 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.

Figure 2 presents the distribution of predicted propensity scores (predicted probabilities) for the groups of children from the treatment and the two control groups. The figure indicates that at the higher end of the distribution, where most individuals from the treatment group locate, there are higher density of individuals from the second control group than from the first control group, indicating that the second control group potentially provides a better common support condition to the treatment group. The mean predicted probability of being in the treatment group for the treated group is 0.64, for the second control group is 0.50, while for the first control group, it is 0.41. This suggests that had the program been introduced in the non-FFE Unions, many households there would have been eligible to participate in the program. At the same time, we also observe that at the lower end of the propensity score distribution, there are more matched cases between the first and second control groups than between the first control group and the treatment group. These seem to suggest that within the non-FFE unions, there are some households who would have been in the treatment group and others would have been in the control group had the program been introduced there. Thus, we may be able to divide households from the non-FFE unions into pseudo treatment and control groups by matching their propensity scores with both the treatment group and the first control group.

As discussed earlier, because children of the second control group live in different Unions than the treatment group and the first control group, if we take the simple difference between the treatment and second control group, the differences associated with their geographic variation

[8] When we match across FFE and non-FFE Unions the dummy variables for Thana rather than for Union are used. This is because when matching across FFE and non-FFE Unions, the Union dummy variables are orthoganal to program participation and, hence, makes the matching impossible.

may contaminate the estimated treatment effect. Thus, in this section, we use the difference-in-differences measure of the matched treatment group and the second control group and the matched first and second control groups to estimate more accurately the effect of the treatment on the treated.

The matching method used in this study is "nearest neighbour matching", which matches each treated unit with a single control unit with 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.

We first match the treated group with control group 1. As these children all live in the FFE Unions, no regional differences should exist with this matching. Nevertheless, as discussed earlier, children of control group 1 are from households that are not eligible for the program and, hence, the lack of common support problem is more severe for this group. The result of this matching is reported in the first column in the first panel of Table 6. It shows that the treatment effect is 18 per cent if the caliper width is set at 0.01 and reduces to 15 per cent when we seek more accurate matching (reducing the caliper to 0.005). The difference, however, is not large. These effects are very similar to that estimated using regression analysis, where a 16 per cent difference is observed when comparing children aged 6 to 13 from the treated group with their counterparts from control group 1 (see Column 1 in Table 4). The slight difference may be related to the lack of a common support condition introduced by the regression analysis.

We then pursue another estimation strategy. We use the second control group, which has a much better support condition than the first control group. Here the problem we have, though, is the difference in the regional macro-economic environment between the two groups, as they are from different Unions, and the FFE Unions are economically and educationally more backward. To deal with this issue we employ propensity score matching combined with difference-in-differences. We match primary-school-aged children from the treated group with their counterparts from control group 2, and the estimated effect here has two components, namely the program participation effect and the effect of regional differences. To disentangle these two effects, we first use secondary-school-aged children as another control group. Children aged 14 to 18 from the treated group are matched with their counterfactuals from the second control group. As children of this age group in both treated and control groups are not participants of the program, we expect that the differences between the school participation rates of these groups should be a pure regional difference. Taking difference-in-differences of these two estimates should provide a more accurate treatment effect.

The results of this difference-in-differences measure are reported in the second panel of Table 6.[9] These results show that, relative to control group 2, primary-school-aged children in the treated group are more likely to attend school and the difference is 8 to 9 percentage points. Comparing secondary school aged children in the treated group with their counterparts in control group 2, however, results in a large negative difference of 19 to 22 percentage points, indicating that secondary-school-aged children in the treated group are less likely to go to school than their counterfactuals in the control group 2. The difference-in-differences measure indicates that the treatment effect may be of the order of 28 to 31 percentage points.[10]

This difference-in-differences measure, however, implies the following assumptions. First, the regional effect on primary school attainment is the same as that on secondary school attainment. Second, there is no spill over effect of the program participation into secondary school children in the treated group. The violation of the first assumption may cause an overestimation of the program participation effect if the regional effect is larger for the secondary school participation than for the primary school participation. This seems very possible as children of secondary school age have more and better employment opportunities than their primary-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.

The violation of the second assumption may cause an under-estimation of the effect of

[9] Note that the propensity scores obtained for this matching are from estimation of the program participation equation specified the same way as described before. However, to be able to match children in the secondary school age group, the program participation equation is estimated using a sample of children aged 6 to 18 years. The propensity score distribution for this estimation is reported in Appendix A.

[10] These results are comparable to that reported in the fourth column of Table 4, where a difference-in-differences measure of 24 percentage points is found. Once again the difference between the propensity score matching and regression results may indicate bias in the regression analysis.

program participation if the spill over effect of program participation on secondary school participation is positive. Given that the program had been in operation for seven 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 can 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 participated in the FFE program when they were in primary school. Excluding this sample of children, we find that the difference-in-differences estimation is much larger than what is indicated in Table 6, suggesting that there is a spill over effect and the under-estimation caused by this effect is quite large, around 0.42. However, as it is not clear to us which one of the two effects dominate we are unable to tell if the effect estimated here is under or over-estimation of the real treatment effect.

Finally, we also take the difference-in-differences measure of the matched difference between primary school children of the treatment group and the control group 1 and between that of the control groups 1 and 2 to control for the regional effect. These results are reported in the third panel of Table 6. It shows that among matched cases, primary-school-aged children from the treated group on average have 9 per cent higher probability of participating at school than their counterfactuals from the second control group. This effect is marginally significant. When comparing the primary school children from control groups 1 and 2, we find that the former is 12 per cent less likely to participate at school than the latter. The difference-in-differences measure indicates a treatment effect of 21 per cent. Unlike the difference-in-differences measure presented in the second panel of Table 6, this measure does not suffer from the two strong assumptions associated with the previous measure because we only look at children of primary school age. We, therefore, believe that this measure may be more accurate.

We also conducted the same exercise for duration of schooling and the results are reported in Table 7. The evaluated program participation effect comparing the treated group with control group 1, results in a very small and insignificant result. The difference-in-differences measure using secondary-school-age children to disentangle the regional effect results in an estimated difference of 2 to 2.3 years in completed duration of schooling. Using control groups 1 and 2 to tease out the regional effect generates an estimate of 1.4 to 1.5 years if the hazard model is estimated with both primary and secondary school children and 0.6 of a year with primary school children only. Once again, we think the latter result may be more reliable for the same reason mentioned earlier .

The results obtained in this study indicate a much larger effect of FFE program participation on the primary school participation rate compared to the study by Ahmed and del Ninno (2002). In their paper they found a program participation effect of 8.4 per cent. The difference between our findings and theirs may be due to the following reasons. Firstly, in their study the control group used includes everyone from the non-FFE treatment households, both control groups 1 and 2. Secondly, the evaluation strategy adopted in their study is very different from ours. They assume that participation into the program is endogenous and, hence, utilised the Heckman selectivity bias model to deal with the endogeneity problem. In this study we assume that selection into the program is based on observable characteristics, which is more realistic. Thirdly, the evaluation conducted in Ahmed and del Ninno (2002) is based on regression analysis while this study adopts propensity score matching and difference-in-differences methodologies.

6 Evaluating the long run effect

To evaluate the long run effect of the FFE program, we mainly focus on the effect of education on earnings. If the program makes children more likely to attend school and to stay at school longer, what will be the long-term earnings effect on these children? We explore this issue by estimating the effect of education on earnings for the current labour force in Bangladesh. The following equation is estimated:

$$\ln W_i = X_i\beta + \eta edu_i + \epsilon_i \tag{9}$$

where W_i is hourly earnings for individual i, X_i is a vector of exogenous variables which affect earnings, including age and its squared term, a dummy variable for married individuals, a dummy variable for males, and a vector of regional dummy variables. The variable edu_i may take two values, whether the individual i has ever been to school or not, and given that he/she has been to school, the number of years of schooling he/she attended. These two measures corresponded directly to the outcomes of the FFE program evaluated in this paper, namely, the probability of attending school and the duration of schooling.

The data used to estimate equation 9 is from the Bangladesh Household Income-Expenditure Survey, 2000, which was conducted by the Bangladesh Bureau of Statistics. The sample comprises 7,445 households and 38,563 individuals. Among them only 6661 are wage and salary earners and only 6635 reported their earnings and working hours. This is the sample used in estimating equation 9. Our sample comprises 2656 and 3979 individuals whose main work is in the urban and rural sectors, respectively. 33.3 per cent of the former and 60.6 per cent of the latter are illiterate. The earnings equation is estimated for male and female and urban and rural samples separately. Because the FFE program is only implemented in rural areas, our main focus is the effect of education on earnings in the rural sector. Nevertheless, rural-urban migration is a valid choice for any individual and hence we also estimated the effect of education on earnings in the urban sector.

Table 8 reports the results from OLS estimation of equation 9. The top two panels show the results for rural and urban samples with edu_i measured as a dummy variable for no schooling and the bottom two panels present the results for rural and urban samples with edu_i measured as number of years of schooling for the sample of individuals who have ever been to school. There are three columns in the table, the first is for the total sample and the second and third columns are for the male and female samples, respectively. The results from the top two panels show that, individuals who are illiterate earn on average 20 and 46 per cent less than individuals with any schooling for the total rural and urban samples, respectively. The effect of illiteracy has a larger effect for females than for males in both rural and urban sectors and the difference is larger in rural than urban sectors. In the rural sector, illiteracy reduces hourly earnings by 15 per cent for males and 69 per cent for females. The effects in the urban sector are 38 and 69 per cent for male and female workers, respectively.

The World Bank (1998 p.60) estimates that in 1995-96 rural household heads with some primary education in Bangladesh would have an increased per capita consumption (a proxy for total income, not hourly earnings as in Table 9) of 6 per cent. For those in urban areas, the comparable figure was 13 per cent. Wage earnings provide about half of total incomes for the poorest households (World Bank 2002 p. vii). Our estimates are somewhat higher than the World Bank estimates. However this could be due both to the difference in the dependent variables used between our study and that of the World Bank and increasing returns to education over the five year interval in the data used.

The bottom two panels of Table 8 reveal that, conditional on school participation, each additional year of schooling increases earnings of the total rural and urban samples by 4.9 and 6.0 per cent, respectively. Once again the effect is larger for the female sample than for the male sample. For those with rural jobs, every additional year of schooling increases earnings of male and female workers by 4.5 and 9.0 per cent, whereas for those with urban jobs the additional year of schooling increase earnings of male and female workers by 5.3 and 10.7 per cent, respectively. These are comparable to the estimates made by the World Bank (2002 p.101) of 5 to 8 per cent using the same data but a different specification.

Equipped with these estimates and assuming the rate of return to education in Bangladesh will not change in the near future, we are now in the position to calculate the long-run earnings effect of the FFE program for the program participants. From Tables 6 and 7, we have the effect of FFE program participation on school enrolment and duration of schooling, while Table 8 reveals the effect of school attendance and years of schooling on earnings. Multiplying these two sets of results provides an estimate the typical effect of FFE program participation on future earnings. These results are presented in Table 9.

The first row of Table 9 indicates the returns to attending school, which are the estimates of returns to no schooling from the top two panels of Table 8, with the opposite sign. The second and third rows multiply the first row and the lower and higher bounds of the estimated effects of program participation on school attendance from Table 6 (0.21 and 0.28) to estimate the effects of the FFE program on earnings through increased school attendance. These effects are calculated to be between 4 and 6 per cent for the total sample if our program participants are not moving to the urban area and 10 to 13 per cent if they possess urban jobs. The effect is much larger for females than for males. If they possess rural jobs, the effect will be 3 to 4 per cent for males and 14 to 19 per cent for females. If they acquire urban jobs, the effect will be 8 to 11 per cent for males and 14 to 19 per cent for females.

The fourth row of Table 9 presents the returns to each additional year of schooling for those who have ever attended school, from the bottom two panels of Table 8. The fifth and sixth rows multiply the fourth row by the lower and higher bounds of the estimated effect of the program participation on duration of schooling from Table 7 (0.57 and 2.01 years). These two rows provide estimates of the effects of the FFE program on hourly earnings through increased years of schooling, which are calculated to be between 3 and 10 per cent for the total sample if they take rural jobs and 3 and 12 per cent if they hold urban jobs. Once again, the effect is much larger for females than for males. With rural jobs, the effects on males and females are between 3 and 9 per cent and between 5 and 18 per cent respectively. With urban jobs, the effects on males and females are between 3 and 12 per cent and between 6 and 21 per cent respectively.

Adding these participation and duration effects together, the last two rows of Table 9 reveals the total effect of the FFE program on the future earnings of the treated. This effect is between 7 and 16 per cent if the participant is going to work in the rural sector and 13 to 25 per cent if he/she is going to work in the urban sector. These are fairly large increments in future earnings.[11] Note that these estimates are based on the rate of return to education using 2000 data. In most developing countries, as economies grow, returns to education often increase. If that happens, the effect of the FFE program on future earnings of the program participants should be even higher.

From the BHIES survey data for 2000 the mean annual wage of men was about $US 365 and that of women $US 195 if they were in rural employment. If in urban employment the comparable figures were $US 593 and $US 265. The incremental effects of FFE participation on the male rural workers would hence amount to additional earnings of between $US 22 and $US 48 per year, while for women the range would be $US 39-72 (Table 10). The increments

[11] These are comparable to the effects of a comprehensive education, health and nutrition program PROGRESA aimed exclusively at poor mothers in Mexico, as reported by Skoufias and McClafferty (2001). They estimated the monetary enrolment grants of the program resulted in 0.7 years of additional schooling (10 per cent increase) of children on average. Taking into account that higher schooling is associated with higher levels of income, their estimations are that children will have lifetime earnings that are 8 per cent higher due to the educational benefits of PROGRESA.

for a male urban worker would be between $US 65 and $US 125 per year, and for a female $US 56-109.[12]

The annual public cost of the FFE program per beneficiary student per year in 1997-98 was $US 36 (calculated from Ahmed and del Ninno 2002 pp. 43-44). Hence the lower bound estimates of the incremental earnings effects above, which are based on additional schooling of 0.57 years, would cost the Government about $US 20. The upper bound incremental earnings would cost about $US 72, based on 2.01 additional years of schooling needed to achieve this.

Ravallion and Wodon (2000) have estimated that participation in the FFE program only results in a partial displacement of child labour by schooling. Only about one-third of the extra school attendance comes at the expense of the opportunity costs of children's work.[13] Valuing the extra time at school by the average boy's and girl's wages of about $US 98 and $US 61 per year respectively, implies the private opportunity costs of school attendance for 0.57 of a year for a boy would be less than $US 2, while for 2.01 years it would be less than $US 6. Comparable figures for a girl are $US 1 and $US 2. These one-time up front private costs of participation pale in comparison to the incremental annual earnings resulting from the additional education (Table 10). When these private costs are added to the public costs above, the total national costs for these two enhanced durations of schooling would be around $US 22 and $US 78 for boys and $US 21 and $US 74 for girls, respectively.

In Table 10 the above estimated public and private costs and benefits of the FFE program are summarized and the internal rates of return calculated. The internal rates of returns on the total national investment in the FFE program are attractive at more than 14 per cent per year. For boys the range is 14.1-25.1 per cent per year and for girls 17.0-24.3 per cent. Private rates of return are around double these and those of girls in the FFE program exceed boys by 6-14

[12] These benefits exclude the nutritional benefits from the additional foodgrains provided under the FFE program. del Ninno and Dorosch (2002) estimate that the marginal propensity to consume wheat from the FFE in Bangladesh is quite high at between 0.21 and 0.41 for the poorest households. These were considerably higher than the marginal propensities to consume wheat from current income. Hence small in-kind transfers result in significantly higher wheat consumption than do increases in cash income. Ahmed (2000) found that in his sample survey, FFE program households significantly increased food and calorie intake at the household level, although he offers no statistical tests. He also states that none of the targeted programs has made any noticeable improvements in nutritional status, as measured anthropometrically. Alam et al. (1999) also could not detect significant anthropometric differences in their sample of beneficiary and non-beneficiary FFE households.

[13] Parker and Skoufias (2000) had an estimate of this effect of 25 per cent for PROGRESA in Mexico.

percentage points.

These analyses imply that the FFE program represents both an extremely wise economic investment for the government in terms of economic growth, but also a powerful tool of poverty alleviation, especially for women. It is interesting however that the average rates of return on additional years of schooling resulting from the FFE program declines as schooling duration increases. This seems consistent with the review of 100 studies of rates of return to education by Psacharopoulos (1994), which showed that the average rate of return to investments in primary education was 18.4 per cent, whereas that to higher education was only 10.9 per cent.

7 Conclusions

This paper evaluates the Food for Education 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 21 to 28 per cent, and increased their duration of schooling by 0.57 of a year to 2.1 years. Further calculation suggests that the public internal rates of return of the FFE program are more than 14 per cent per year and the private rates are more than 30 per cent. Hence it would seem that the FFE program not only has a significant effect on school participation and schooling duration of the poor children but also is both publicly and privately an attractive investment.

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Table 1: Distribution of households among FFE/non-FFE households
and FFE/non-FFE Unions

	FFE Union		Non-FFE Union	
	No. of HH	No. of children aged 6-13	No. of HH	No. of children aged 6-13
Participating households	209	399		
Non-Participating households	191	336	200	343

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

Table 2: Distribution of primary school aged children in different status in FFE Unions

Including stipend program participants as attending Non-FFE schools	FFE HH		Non-FFE HH	
	Freq.	%	Freq.	%
Total	399	100.00	336	100.00
Not attending school	55	13.78	102	30.36
Attending Non-FFE school	33	8.27	110	32.74
Attending FFE school and receive	216	54.14	0	0.00
Attending FFE school not receive	95	23.81	124	36.90
Excluding stipend program participants	FFE HH		Non-FFE HH	
Total	388	100.00	319	100.00
Not attending school	55	14.18	102	31.97
Attending Non-FFE school	22	5.67	93	29.15
Attending FFE school and receive	216	55.67	0	0.00
Attending FFE school not receive	95	24.48	124	38.87

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

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

	FFE Union						Non-FFE Union	
	Total		FFE HH		Non-FFE HH			
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
HH income	13082	16691	8290	9447	18222	20781	14334	17448
Total land	97.9	175.8	51.2	71.1	147.5	231.9	99.6	193.2
% of HH head as labour	22.75		28.5		16.58		21.11	
% of Female as HH head	12.0		14.0		9.8		9.0	
No. of Households	400		207		193		200	

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

Table 4: OLS estimations of equations 1 and 8

	Control group 1 FFE Union: FFE participating and non-participating HH				Control group 2 FFE Union FFE-participating HH & non-FFE Union			
	Equation 1		Equation 8		Equation 1		Equation 8	
	dF/dx	Std. Err	dF/dx	Std. Err	dF/dx	Std. Err.	dF/dx	Std. Err.
Program participating HH (1)	0.162	**0.035**	-0.041	0.065	0.064	**0.026**	-0.075	0.054
Participating HH*Age 6-13 (2)			0.218	**0.064**			0.160	**0.056**
Dummy for age 6-13			0.000	0.080			-0.002	0.066
Child's gender male=1	-0.043	0.036	-0.058	**0.036**	-0.021	0.029	-0.029	0.032
Child's age	0.355	**0.059**	0.202	**0.034**	0.332	**0.050**	0.188	**0.032**
Child's age^2	-0.018	**0.003**	-0.010	**0.002**	-0.017	**0.003**	-0.009	**0.002**
No. of male children in HH	-0.015	0.028	-0.014	0.028	-0.003	0.022	0.004	0.023
No. of female children in HH	-0.004	0.031	0.005	0.031	-0.037	0.025	-0.020	0.028
Child as siblings of the HH head	-0.086	0.163	-0.184	0.147	-0.011	0.095	-0.033	0.106
HH size	-0.014	0.016	-0.020	0.016	-0.005	0.014	-0.014	0.015
Gender of HH head male=1	-0.024	0.219	0.022	0.221	-0.195	0.172	-0.192	0.188
Time for going to nearest school	0.000	0.001	0.000	0.001	-0.002	**0.001**	-0.003	**0.002**
Mother's years of schooling	-0.020	**0.011**	-0.008	0.011	0.013	**0.008**	0.015	**0.009**
Father's years of schooling	0.026	**0.007**	0.023	**0.006**	0.012	**0.005**	0.011	**0.005**
Total household income*10^{-4}	0.006	0.013	0.011	0.013	0.026	**0.011**	0.042	**0.012**
HH head being a labourer	-0.076	**0.044**	-0.074	**0.044**	0.005	0.030	0.033	0.033
Total land holding of the HH*10^{-3}	0.219	0.154	0.251	**0.127**	-0.005	0.088	0.075	0.114
Value of the housing*10^{-4}	0.012	0.008	0.005	0.005	0.003	0.007	0.017	0.010
Regional dummy	Yes		Yes		Yes		Yes	
obs. Probability	0.77		0.71		0.83		0.76	
pred. Probability	0.85		0.79		0.88		0.82	
no. of obs.	676		907		719		935	
Pseudo R2	0.25		0.29		0.18		0.24	
Coeff. difference between (1) and (2)			0.26				0.24	
chi^2 of difference between (1) and (2)			4.16				4.79	
Prob>chi2			0.04				0.03	

Table 5 Treatment effect on duration of schooling for primary school-age children(years)

Control Group 1	Treatment group in FFE Union		Control group in FFE Union		Difference	
Unadjusted differences	Mean	SD	Mean	SD	Mean	T-ratio
6 to 13 years of age	3.65	2.06	3.64	1.89	-0.02	-0.10
14 to 18 years of age	6.75	2.61	7.59	2.80	-0.83	-1.91
Diff-in-Differences					0.82	1.75
Adjusted differences						
Equation 1 aged 6-13	8.69	0.82	8.50	0.49	0.19	3.51
Equation 8 aged 6-13	13.08	1.11	12.55	1.74	0.54	4.26
Equation 8 aged 14-18	9.76	2.96	10.05	2.81	-0.29	-0.62
Diff-in-Diffs (equation 8)					0.83	1.70
Control Group2	FFE HH in FFE Union		non-FFE Union		Difference	
Unadjusted differences	Mean	SD	Mean	SD	Mean	T-ratio
6 to 13 years of age	3.64	1.89	3.59	2.07	0.05	0.32
14 to 18 years of age	6.75	2.61	7.43	2.85	-0.67	-1.64
Diff-in-Differences					0.72	1.64
Adjusted differences						
Equation 1 aged 6-13	8.66	0.68	8.55	0.50	0.11	2.21
Equation 8 aged 6-13	13.02	1.12	12.79	1.34	0.23	2.20
Equation 8 aged 14-18	9.56	3.04	10.92	2.52	-1.35	-2.98
Diff-in-Diffs (equation 8)					1.58	3.58

Table 6 Results from propensity score matching combined with difference-indifferences on primary school participation rate

	Aged 6 to 13					
Trtd. vs. Cntrl 1	clp=0.01	Clp=0.005				
Effect	0.18	0.15				
Std. Err.	0.05	0.06				
no. of matched treated	209	162				
number of matched contol used	137	117				
Discarded controls	179	226				
Discarded treated	182	202				
proportion matched	0.49	0.39				
	Aged 6 to 13		Aged 14 to 18		Diff-in-diffs	
Trtd. vs. Cntrl 2, 6/13 vs. 14/18	clp=0.01	Clp=0.005	clp=0.01	clp=0.005	clp=0.01	clp=0.005
Effect	0.08	0.09	-0.19	-0.22	0.28	0.31
Std. Err.	0.05	0.05	0.14	0.15		
t-ratio					16.09	14.10
no. of matched treated	215	174	36	27		
number of matched contol used	138	120	27	22		
Discarded controls	173	214	52	61		
Discarded treated	168	186	66	71		
proportion matched	0.51	0.42	0.35	0.27		
	Treated vs. Cntrl 2		Cntrl 1 vs. Cntrl 2		Diff-in-diffs	
Trtd vs. Cntrl2, Cntrl1 vs. Cntrl2	clp=0.01	Clp=0.005	clp=0.01	clp=0.005	clp=0.01	clp=0.005
Effect	0.09	0.08	-0.12	-0.13	0.21	0.21
Std. Err.	0.05	0.05	0.05	0.06		
t-ratio					54.75	46.82
No. of obs	220	179	181	145		
number of matched contol used	133	116	130	106		
Discarded controls	168	209	138	174		
Discarded treated	173	190	176	200		
proportion matched	0.51	0.43	0.50	0.40		

Table 7 Results from propensity score matching combined with difference-in-differences on duration of schooling

	aged 6 -13					
Treated and cntrl 1	clp=0.01	clp=0.005				
Effect	0.06	0.07				
Std. Err.	0.11	0.11				
no. of matched treated	179	150				
number of matched contol used	118	106				
discarded controls	215	244				
discarded treated	201	213				
proportion matched						
	aged 6-13		aged 14-18		Diff-in-diffs	
Treated and cntrl 2	clp=0.01	clp=0.005	clp=0.01	clp=0.005	clp=0.01	clp=0.005
Effect	0.83	0.84	-1.18	-1.41	2.01	2.25
Std. Err.	0.17	0.18	0.69	0.78		
t-ratio					21.50	18.31
no. of matched treated	222	185	31	22		
number of matched contol used	133	117	24	19		
discarded controls	166	203	57	66		
discarded treated	179	195	71	76		
proportion matched	0.51	0.43	0.30	0.22		
	Treated and control 2		control 1 and control 2		Diff-in-diffs	
Trtd vs. Cntrl2, Cntrl1 vs. Cntrl2 (1)	clp=0.01	clp=0.005	clp=0.01	clp=0.005	clp=0.01	clp=0.005
Effect	0.83	0.84	-0.57	-0.51	1.40	1.35
Std. Err.	0.17	0.18	0.15	0.16		
t-ratio					116.9	94.5
no. of matched treated	222	185	209	162		
number of matched contol used	133	117	143	117		
discarded controls	166	203	110	157		
discarded treated	179	195	169	195		
proportion matched	0.51	0.43	0.56	0.44		
	Treated and control 2		control 1 and control 2		Diff-in-diffs	
Trtd vs. Cntrl2, Cntrl1 vs. Cntrl2 (2)	clp=0.01	clp=0.005	clp=0.01	clp=0.005	clp=0.01	clp=0.005
Effect	0.40	0.42	-0.18	-0.14	0.57	0.56
Std. Err.	0.10	0.11	0.10	0.10		
t-ratio					75.62	64.85
No. of obs	222	185	209	162		
number of matched contol used	133	117	143	117		
discarded controls	166	203	110	157		
discarded treated	179	195	169	195		
proportion matched	0.51	0.43	0.56	0.44		

Table 8 Effect of education on hourly earnings

			Ever went to school			
	Total		Males		Females	
Rural jobs	Coef.	Std. Err.	Coef.	Std. Err	Coef.	Std. Err
Constant	0.601	0.071	1.241	0.068	0.837	0.213
Dummy for no schooling	-0.204	0.019	-0.155	0.019	-0.689	0.086
Age	0.046	0.004	0.049	0.004	0.051	0.012
Age^2	-0.001	0.000	-0.001	0.000	-0.001	0.000
Dummy for married	0.150	0.029	0.109	0.034	0.136	0.075
Dummy for males	0.695	0.028				
Regional dummies	Yes		Yes		Yes	
Number of observations	3979		3485		494	
Adjusted R^2	0.3365		0.2416		0.2165	
	Total		Males		Females	
Urban jobs	Coef.	Std. Err.	Coef.	Std. Err	Coef.	Std. Err
Constant	0.075	0.102	0.515	0.105	0.649	0.298
Dummy for no schooling	-0.461	0.027	-0.378	0.027	-0.689	0.080
Age	0.084	0.005	0.103	0.006	0.083	0.018
Age^2	-0.001	0.000	-0.001	0.000	-0.001	0.000
Dummy for married	0.092	0.038	0.014	0.043	-0.046	0.092
Dummy for males	0.831	0.033				
Regional dummies	Yes		Yes		Yes	
Number of observations	2656		2140		516	
Adjusted R^2	0.4859		0.3962		0.276	
			Years of schooling			
	Total		Males		Females	
Rural jobs	Coef.	Std. Err.	Coef.	Std. Err	Coef.	Std. Err
Constant	0.564	0.148	0.912	0.142	-0.171	0.755
Years of schooling	0.049	0.005	0.045	0.005	0.090	0.027
Age	0.051	0.008	0.050	0.009	0.077	0.048
Age^2	-0.001	0.000	-0.001	0.000	-0.001	0.001
Dummy for married	0.099	0.061	0.093	0.066	0.105	0.239
Dummy for males	0.321	0.068				
Regional dummies	Yes		Yes		Yes	
Number of observations	1247		1152		95	
Adjusted R^2	0.25		0.2461		0.1682	
	Total		Males		Females	
Urban jobs	Coef.	Std. Err.	Coef.	Std. Err	Coef.	Std. Err
Constant	-0.166	0.126	0.485	0.134	-0.219	0.459
Years of the schooling	0.060	0.004	0.053	0.004	0.107	0.016
Age	0.077	0.007	0.076	0.008	0.066	0.032
Age^2	-0.001	0.000	-0.001	0.000	-0.001	0.000
Dummy for married	0.019	0.046	0.041	0.050	-0.144	0.134
Dummy for males	0.587	0.044				
Regional dummies	Yes		Yes		Yes	
Number of observations	1556		1352		204	
Adjusted R^2	0.46		0.3891		0.447	

Table 9 Calculated long-run earnings effect of the program participation

	Rural Jobs			Urban Jobs		
	Total	Males	Females	Total	Males	Females
Return to no school	-0.20	-0.16	-0.69	-0.46	-0.38	-0.69
21% increase in school attendance low estimate	0.04	0.03	0.14	0.10	0.08	0.14
28% increase in school attendance high estimate	0.06	0.04	0.19	0.13	0.11	0.19
Return to years of schooling given school attendance	0.05	0.04	0.09	0.06	0.05	0.11
0.57 year increase in schooling low estimate	0.03	0.03	0.05	0.03	0.03	0.06
2.01 years increase in schooling high estimate	0.10	0.09	0.18	0.12	0.11	0.21
Total increase in hourly earnings low estimate	0.07	0.06	0.20	0.13	0.11	0.21
Total increase in hourly earnings high estimate	0.16	0.13	0.37	0.25	0.21	0.41

Table 10. Estimated private and national benefits/costs of FFE program per beneficiary student[a]

Benefits and costs	**Effects of FFE program**			
	Males		**Females**	
	Lower bound	**Upper bound**	**Lower bound**	**Upper bound**
Initial *public* costs ($US)	20	72	20	72
Initial *private* costs ($US)	2	6	1	2
Initial total *national* costs ($US)	22	78	21	74
Annual incremental earnings benefits after age 18 years in *rural* employment ($US)	22	48	39	72
Annual incremental earnings benefits after age 18 years in *urban* employment ($US)	65	125	56	109
National internal rate of return with *rural* employment (% per year)	17.2	14.1	21.6	17.0
National internal rate of return with *urban* employment (% per year)	25.1	20.5	24.3	19.9
Private internal rate of return with *rural* employment (% per year)	36.3	33.5	48.9	48.1
Private internal rate of return with *urban* employment (% per year)	47.0	42.5	52.8	52.5

Note: [a] This is based upon adult earnings increments beginning to occur 12 years after the completion of primary school and continuing for a further 27 years, a public FFE cost of $US 36 per year of schooling and a private child opportunity attendance cost of $US 2.6 per school year from foregone earnings of boys and $US 1 for girls.

Figure 1 Primary school participation and years of schooling
by age and participation status

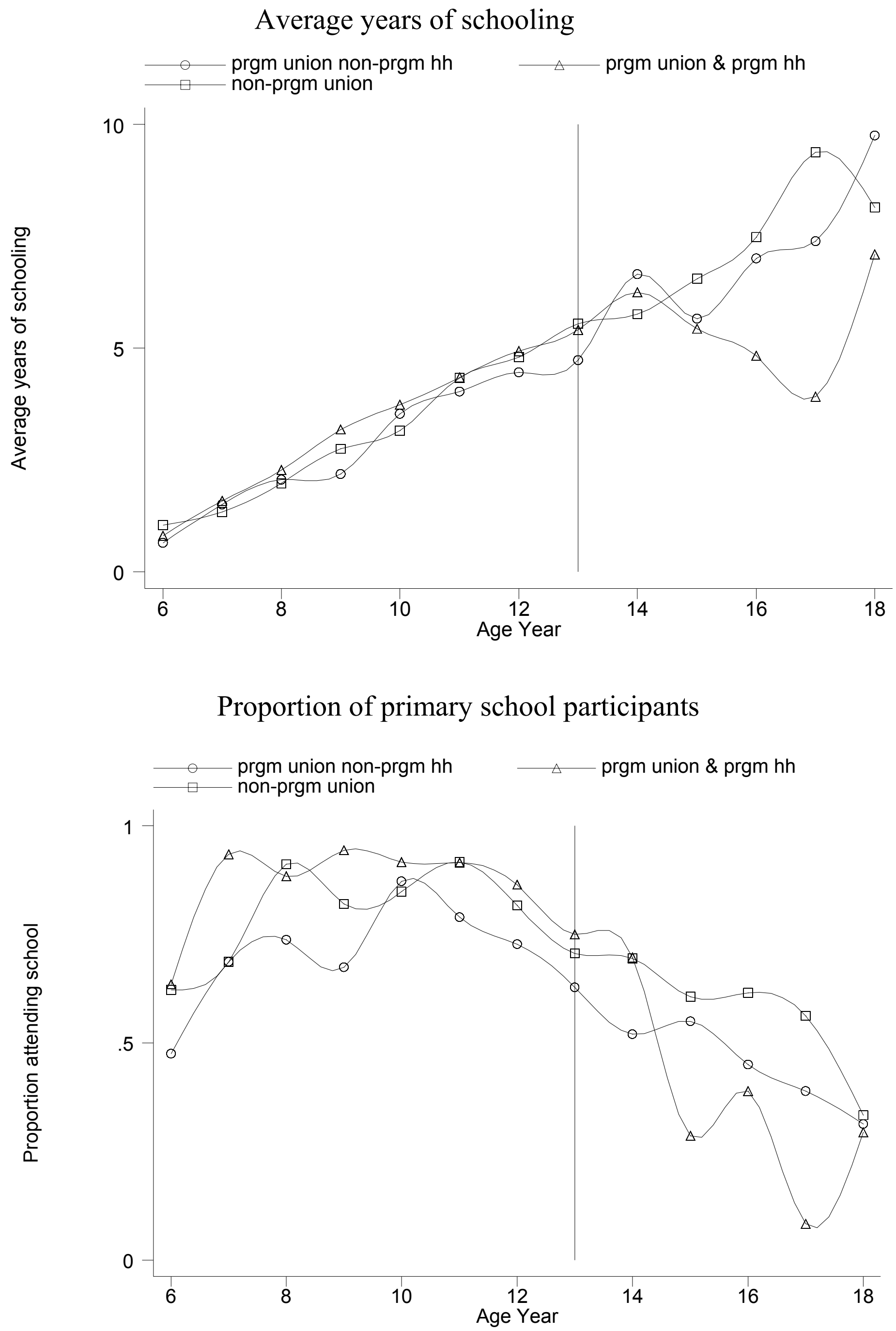
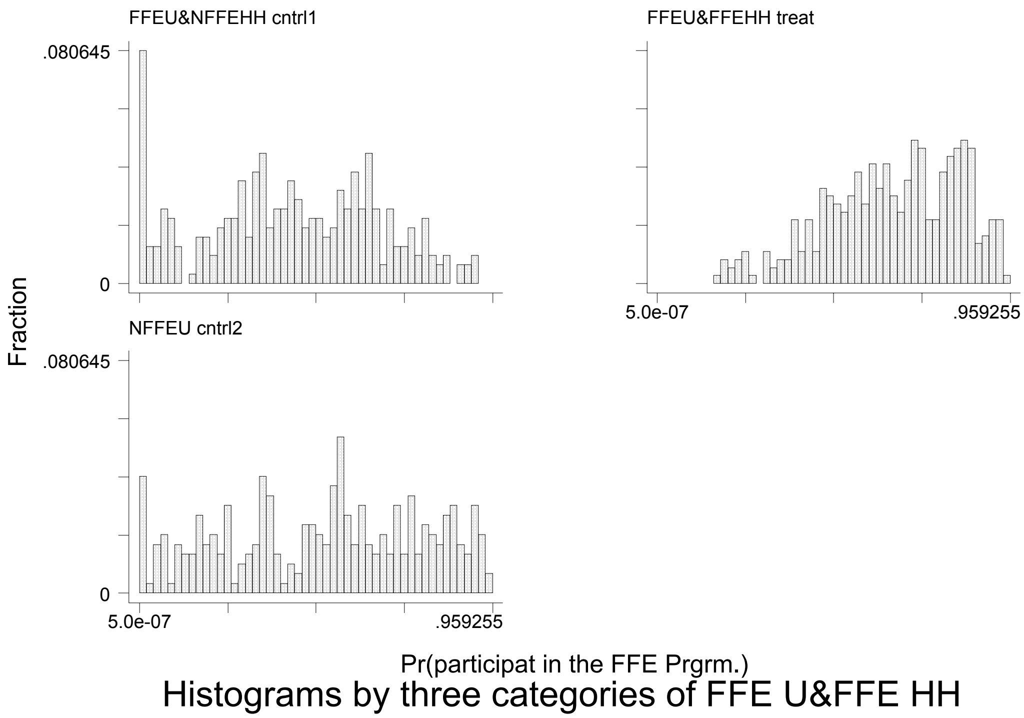


Figure 2 propensity scores for the three groups using sample of aged 6 to 13 years



Appendix A

Figure A propensity scores for the three groups using sample of aged 6 to 18 years