Does Child Labor Displace Schooling?

Evidence on Behavioral Responses to an Enrollment Subsidy

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1. Introduction

Schooling typically raises future earnings. Yet one finds relatively low enrollments amongst currently poor families. A common explanation is that schooling competes with labor-intensive jobs for children (wage labor, employment in family enterprises, or collection activities). By this view, the low current incomes of their families keeps poor children out of school and thus perpetuates their poverty into the next generation.[2]

If this is right, then policy reforms that promote labor-intensive production — the comparative advantage of most low-income countries — are a mixed blessing for the poor; trade liberalization may well attract poor children out of school prematurely. Pro-growth trade policies may then come at a cost to human development, and possibly future growth, in poor countries. A recent study of child labor in a city in western India concluded that: "The prevalence and absolute expansion of child labor in a period and region of relatively high growth of aggregate output indicates that the nature of economic growth is flawed" (Swaminathan, 1998: 1526).

One proposal has been to ban child labor in developing countries, as it had been by the late nineteenth century in most present-day developed countries. It is recognized that a ban could come at a cost to the short-term welfare of the poor,[3] though a proper assessment would have to take account of general equilibrium effects, particularly on the labor market.[4] However, the

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[2] This assumes that parents cannot borrow to finance schooling or enter binding contracts with their children. On the implications of contract enforcement problems for understanding why child labor exists see Baland and Robinson (1998).

[3] Heywood (1988) describes the resistance to bans on child labor in late nineteenth century France.

[4] Famously, Simonde de Sismonde argued that child labor lowered parents' wages and so "their activity has not produced an improvement in the incomes of the poor" (Heywood, 1988, p.222). Also see Basu and Van (1998) who argue that there can be multiple equilibria in the labor market such that one equilibrium entails child labor with low wages while the other has higher wages but no child labor.

enforceability of a ban on child labor is a moot point. While child labor bans were eventually introduced in most of Europe during the nineteenth century, enforcement was very difficult, and it has been argued that other factors (rising incomes, and technological change) were more important in reducing child labor (Nardinelli, 1980; Heywood, 1988). In most developing countries, it is far from obvious how a ban could be enforced, especially in rural areas.

Observations such as these have led to a search for other ways to reduce child labor while keeping the advantages to the poor of labor-intensive growth in developing countries. One seemingly appealing option is to make schooling more attractive to parents, and this has been favored by a number of observers.[5] An obvious policy instrument for this purpose is a targeted enrollment subsidy. Motivated by a desire to reduce both current and future poverty, cash or in-kind transfers targeted to poor families — but conditional on their kids staying at school — have recently become popular in developing countries. If there is substitution between child labor and schooling then such programs will reduce child labor. Of course, the foregone income of participants could then be high (though presumably less than the value of the subsidy) — raising concerns about the efficiency of such programs as a means of reducing current poverty.

Both the arguments that "child labor reflects bad growth", and that "making schools cheaper can turn it into good growth" assume that child labor displaces schooling. This paper tests that assumption. To do so we study the effects on schooling and child labor of an enrollment subsidy in Bangladesh. We measure the extent of substitution by seeing how much the extra schooling induced by the subsidy comes out of child labor. We do not evaluate the program, but only use it as means of identifying how much child labor displaces schooling.

[5] Including Grootaert and Kanbur (1995), World Bank (1995, Chapter 11; 1999), Psacharopoulos (1997), and Grootaert and Patrinos (1998).

The next section compares our approach to alternatives suggested by the literature. Section 3 outlines our theoretical model, demonstrating that an enrollment subsidy increases schooling, but need not reduce child labor. We then turn to our data and econometric model in section 4. Our main empirical results can be found in section 5, and section 6 concludes.

2. Testing for Substitution between Child Labor and Schooling

Children are a current economic resource for poor parents. It is common to find children doing productive work of some sort in poor rural economies. Cain (1977) found that children in a Bangladeshi village were economically active from the age of six, and that boys were net producers by 15. Jacoby and Skoufias (1997) found that child labor helps smooth the incomes of rural Indian families, consistent with poorly developed credit and risk markets.

However, there are ways that poor families can protect the schooling of working children, because there are other things that children do besides school and work. To allow double shifts (given limited school building and other facilities), primary school days of four hours or so per child are common. Longer school days than this also create logistic problems of feeding the children. Public primary ("vernacular medium") schools in Bangladesh are open about 120 days a year and the school day entails 3-4 hours of class time (about 17 hours per week, with slightly less at junior grades, and slightly more at private schools). A survey for Bangladesh found that boys (5-14) in rural areas classified as being in the workforce (including work on the family farm or non-farm enterprise) did an average of 26 hours work per week; the corresponding average for girls was 20 hours (BBS, 1996, Table 6.10). So one cannot assume that the time these children spend working must come at the expense of formal time at school, although there may be displacement of informal (after-school) tutorials or homework.

How might one measure the effect of child labor on schooling? A common method is to compare the educational attainments of children who work with those who do not. From such comparisons, Psacharopoulos (1997) concludes that child labor leads to two years less schooling on average (using data for Bolivia and Venezuela). This suggests that child labor entails a large cost to childrens' future welfare. However, the possibility of selection bias through the choices made by parents clouds such comparisons. The parents of children who currently work may well send their kids to school less than do other parents even when work is not an option.

If one could do an experiment that created an exogenous increase in schooling, then one could simply see how much child labor fell as a consequence. An exogenous decrease in the price of schooling would qualify for such an experiment. An important element in the price of schooling is the wage rate for child labor. However, this is also the price of leisure (assuming that parents are free to allocate their children's time; we return to that assumption later). Thus disentangling the own price effect from the cross-price effect is problematic using wage data.[6]

One might look for other indicators of school price. The presence of a school in the village of residence is one possibility (Rozenzweig, 1982). Another is the distance or travel time to the nearest school (Grootaert, 1998) or average out-of-pocket expenditures on schooling in the area of residence (Cartwright, 1998). On *a priori* grounds it is unclear just how well such variables measure the price of schooling, and the usual concerns arise about attenuation bias due to the use of weak proxies. The endogeneity of both access and average school expenditures also raises concerns, since these measures may well pick up spurious geographic effects. The existing evidence for effects of school price on child labor is mixed, although it is recognized in the literature that this may be due to poor indicators of school price (Grootaert and Patrinos, 1998).

We follow a different route. We examine how parents' choices between sending their kids to school versus work in rural Bangladesh are affected by the Food-for-Education (FFE) program. The program aims to keep the children of poor rural families in school. In 1995-96, 2.2 million children participated (13% of total enrolment). Participating households receive monthly food rations as long as they send their children to primary school. Targeting is done in two stages. First economically backward areas are chosen by the center. Second, community groups — exploiting idiosyncratic local information — select participants within those areas.

From the 1995-96 Household Expenditure Survey (HES) done by the Bangladesh Bureau of Statistics (BBS), the mean amount of rice received under the FFE program was 114 kg per year per participating household. Based on the same survey, we estimate that the average price of rice paid by the poor in 1996 was 12.5 Tk per kilo in rural areas. That translates in an average monetary value for the FFE stipend of 119 Tk per month. A separate BBS survey in 1996 found that the average monthly income of boys in paid work was 464 Tk while it was 291 Tk for girls (BBS, 1996, Table 5.11, p.53). Given that there are on average about two children of primary-school age in participating households, the value of the FFE stipend is about 13% of the average monthly earnings of boys and 20% of that for girls.[7]

To receive the stipend, children must attend at least 85 percent of all classes each month. The headmaster of the school monitors school attendance and the food distribution is made within the school each week. The schools submit estimates of their grain needs to the local district headquarters, which then takes charge of transport, distribution, and handling.

The FFE stipend is a pure discount on the price of schooling to parents. Our data include

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[6] To add to the difficulty, wages for child labor are rarely collected (or at least in the same surveys for which other relevant data are required), or are badly measured.

[7] These are probably underestimates, since there is anecdotal evidence that FFE rations are sold to

both participants and non-participants, and we allow for the purposive targeting of the stipend. We can thus use the existence of this program as a quasi-experiment to test whether child labor substitutes for schooling. The test also throws direct light on the effectiveness of an enrolment incentive in reducing child labor, as is often recommended in policy discussions.

3. The Effect of a School Stipend on Child Labor

In this section we provide a rudimentary model of parents' decisions about how to allocate their children's time. The model is no more complex than is needed to demonstrate formally the argument in the previous section that there can be no presumption that cheaper schooling will reduce child labor. We assume that parents care about current consumption and their children's schooling, which may give pleasure in its own right, but will presumably also make parents directly better off in the future, via transfers from their adult children. Parents also attach value to their children's leisure and/or domestic labor within the home.

The effects of a program such as FFE will depend in part on what constraints parents face in allocating their children's time. Schooling is not compulsory in most low-income countries, and so there is no constraint requiring a minimum amount of schooling. There is a maximum in public and NGO schools. When this is binding, the FFE stipend becomes an ordinary targeted transfer payment. The program may still likely to reduce child labor, but only via the income effect, assuming that children's leisure is a normal good for parents). However, since the program exists as a response to low school attendance amongst the poor, we will not assume that the constraint on maximum school attendance is binding.

If there is underemployment of child labor then this will also constrain parents' choices. If

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buy cheaper grain, suggesting that FFE rice has a higher price that we have assumed.

the wage rate for child labor is inflexible downwards then a small increase in the stipend will have no effect on child labor — the extra time at school will come out of leisure.

However, it is hard to see what would generate downward inflexibility in child wages in this setting. There are no child labor unions to our knowledge. Adverse effects of low adult wages on nutritional status and (hence) productivity can yield downward wage inflexibility (under the well-known Efficiency Wage Hypothesis). However, while income pooling may not be complete within the household, it is plain that children do share in the family's total resources, in which case the link from children's own wages to their nutritional status will be weak.[8]

Thus it would seem reasonable to assume that parents in this setting are free to determine how their children's time is allocated. In making that choice, let parents' utility be:

$$U = U(C, S, H; Z) \tag{1}$$

where the household's current consumption is C, S is the child's school attendance, and H is the child's leisure. We assume that U is strictly quasi-concave in C, S and H. We allow for heterogeneity by including a vector of exogenous household and local geographic variables Z. The child's total time available (T) can be devoted to schooling, leisure (H), or wage labor (L):

$$S + H + L = T \tag{2}$$

In addition to income from child labor or the FFE stipend, the household obtains an income Y from other sources, which we assume to also be a function of Z. (The latter will include the parents' education and landholding.) So the budget constraint is:

$$C = wL + bS + Y(Z) \tag{3}$$

where w is the wage rate for child labor, and b is the monetary value of the food received under

[8] A survey by the Bangladesh Bureau of Statistics in 1996 found that 83% of children in rural areas paid their earnings to their parents (BBS, 1996, Table 5.12, p. 54).

the FFE program. The stipend is zero if the household is not selected for FFE participation.

Parents maximize (1) subject to (2) and (3) with respect to *C*, *S*, *H* and *L*, given *w*, *b*, *Z* and *T*. This is equivalent to maximizing (1) with respect to *C*, *H*, and *S*, subject to:

$$C + (w\text{-}b)S + wH = wT + Y(Z) \qquad (4)$$

This makes clear that *w-b* is the price of schooling. With no other constraints on time allocation, the parents' choice equates the MRS between consumption and schooling with school price *w-b*, and it equates the MRS between consumption and leisure with the price of leisure, *w*. The derived demand function of parents for their children's schooling and leisure are then:

$$S = S(w\text{-}b,\ w,\ wT + Y(Z),\ Z) \qquad (5)$$

$$H = H(w,\ w\text{-}b,\ wT + Y(Z),\ Z) \qquad (6)$$

The supply of child labor is then determined as a residual using (2). The corresponding utility-compensated demand functions minimize the full expenditure, $C+(w\text{-}b)S+wH$, needed to attain a given level of utility, and so are given by:

$$S = S^*(w\text{-}b,\ w,\ U,\ Z) \qquad (7)$$

$$H = H^*(w,\ w\text{-}b,\ U,\ Z) \qquad (8)$$

The effect of an increase in the stipend reveals how time allocation varies with the price of schooling. Using the Slutsky decomposition, the effect on the supply of child labor is:

$$\frac{\partial L}{\partial b} = \frac{\partial S^*}{\partial (w-b)} + \frac{\partial H^*}{\partial (w-b)} - S\frac{\partial (H+S)}{\partial (wT + Y(Z))} \qquad (9)$$

If this is negative then child labor is a substitute for schooling. Quasi-concavity of *U* implies that the first term on the RHS of (9) — the utility compensated own-price effect on demand for schooling — is negative. The third term in (9) is negative, assuming that schooling and leisure are a normal good in total (i.e., that $H+S$ is increasing in full income at given *w* and *b*). The second

term is the utility-compensated cross-effect of the price of schooling on demand for children's leisure, or (equivalently, by symmetry of the Slutsky matrix) the effect of the price of leisure on schooling. The sign of this effect is ambiguous. It will be positive if schooling and leisure are (utility-compensated) substitutes. A sufficient condition for the program incentive to reduce child labor is that schooling and leisure are complements.

So it is unclear on theoretical grounds whether a reduction in the price of schooling generated by a higher stipend will reduce child labor; the extra time spent at school may well come out of children's leisure. And, by the same token, if the substitution effects between schooling and leisure are strong enough, child labor will not come at much cost to longer-term prospects of children escaping poverty. Our empirical work tests these effects.

4. Data and Estimation Methods

We test whether work is a substitute for schooling using a household survey for Bangladesh, with a matched community survey. We use data from BBS's 1995-96 national Household Expenditure Survey (HES). We only use the rural sample in the HES, since the FFE program is not found in urban areas. The HES included questions on FFE participation. The survey did not include time use. We measure the incidence of child labor according to survey responses to the question: "What was your normal activity last week?". A child is deemed to be in the labor force if the answer was "employed", "employed but not working", "household work", or "seeking work". By this definition, 11.8% of boys and 12.1% of girls aged 5-16 in the sample were classified as being in the workforce. It is likely that this understates the extent of child labor, either because of deliberate under-reporting, or because relatively small amounts of part time work are not deemed to constitute the child's "normal activity".

Our theoretical model assumed an interior solution. This is reasonable since it is likely that most, if not all, children in rural Bangladesh work at least a few hours each month. However, our data only allow us to test for effects on whether a child's "normal activity" is being in the workforce. This will presumably entail that the child works more than some number of hours, though we do not (of course) know what that number is. (It is very unlikely that parents will report working as their children's "normal activity" if the number of hours worked is "low", but how low we cannot say.) Clearly, these data do not allow us to capture any effects of the program on small amounts of child labor, though presumably it should not be hard to accommodate modest amounts of part-time work while still keeping children at school.

We assume that a child is reported to be in the workforce if the amount of work done exceeds some latent critical value, ?. Actual labor supply differs from ? by an amount pX + e, for a vector of observed variables X, with corresponding parameters p, and an additive random error term, e, that is taken to be a normally distributed innovation error. The probability of the child being reported as normally in the labor force is then the value of F(pX) where F is the distribution function of e. Thus we estimate a probit on the observed binary variable (taking the value one if the child's normal activity is to be in the workforce, and zero otherwise). We use the data for all children in rural areas aged 5-16, and we estimate separate probits for boys and girls.

In modeling school attendance we follow reasonably standard practices in the literature. A question in the HES asked: "What is your current educational status".[9] We estimate a probit for the answers to this question, for all children aged 5-16 years who have not completed primary school. (We use a wide age interval because the average time to complete primary school in rural Bangladesh is nine years; World Bank, 1996.)

We find that 74% of boys in the sample were recorded as "currently attending school", and 75% of girls. Of the 1295 children not at school (685 boys), 704 (378 boys) were not classified as being at work in the "normal activity" question either. So the data do not suggest that the majority of those children not at school are normally working instead.

The extent of household participation in the FFE program is measured by the actual quantity of foodgrains received under the program. Apart from program participation, the explanatory variables in the regressions include household size and family structure variables, the education levels of the father and the mother, the land ownership, the age of the kid and the religion, and a number of village level variables on school access and quality.

Household participation in the program is treated as endogenous. There are two levels of purposive targeting: selection of the village, and selection of the household. The community module provides independent information on whether the village participates in FFE. Given that geographic placement is based on explicit targeting criteria, it is reasonable to treat this aspect of the purposive targeting as a problem of selection-on-observables. However, that is not plausible for individual placement, given that the program is designed to exploit idiosyncratic local information. Our estimation method assumes that household-level participation depends on unobservables (notably the idiosyncratic information on household-level circumstances held by those allocating the program within villages) but village level placement can be accounted for adequately by a set of observed control variables at village level. This is consistent with what we know about how the program was targeted in practice (section 2).

In addition to the common vector of explanatory variables *X* (which includes the FFE stipend), the probits for schooling and child labor included the residuals from a first stage tobit

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[9] Alternatively we could have used the "normal activity" question, for which "student" is one

regression for explaining the FFE stipend. This first stage regression included X and a dummy variable for village participation. To be a valid instrumental variable, village participation must not affect child labor or schooling controlling for the variables in the second-stage regressions. Under this identifying assumption and with normally distributed errors, we can consistently estimate the coefficient on the FFE stipend in the probits for child labor and schooling as long as we control for the residuals from the first-stage regression.[10] The coefficient on the residuals also provides an exogeneity test.

Given the way the program is targeted, village-level participation is a suitable instrument for participation at the individual level provided one includes a set of geographic control variables for village placement (Ravallion and Wodon, 1998). The set of geographic controls include distances to school; the type of school (governmental, private, NGO); a series of school quality variables reported in the community survey; land distribution; irrigation intensity; road quality; electrification; distance and time to thana and district headquarters and to Dhaka; distance to various facilities (health care, Banks, government agencies); incidence of natural disasters; attitudes to women's employment, education and family planning; average schooling levels of the head and spouse; majority religion of the village; and population size of the village. These were (jointly) very good predictors of program placement. A probit regression of whether the village had the program on the geographic control variables gave a pseudo-R^2 of 0.55 (Chi-square of 91.7 which is significant at the 0.5% level, with 166 observations).

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possible response. However, we decided that the educational status question would be more reliable.

[10] Datt and Ravallion (1994, Appendix 1) prove consistency for a more general simultaneous tobit model (generalizing Smith and Blundell, 1986, to allow for a censored endogenous variable). The consistency proof for our case is a minor variation. If the FFE stipend was continuous (rather than censored) then our estimation method would be the same as that proposed by Rivers and Vuong (1988).

5. Results

Table 1 gives the probits for child labor and schooling. The FFE stipend has a significant negative effect on children's labor force participation, and it has a strong opposite effect on the probability of being at school. Exogeneity of the stipend is rejected (at the 5% level) for boys' work and (at almost the 10% level) for girls' schooling. At the mean of the sample, an extra 100 kilos of rice increases the probability of a boy going to school by 0.17, and 0.16 for a girl. The FFE stipend appears to be about right for achieving full school attendance for kids at the current average attendance rate (about 0.74) and receiving the average stipend (114 kilos).

The displacement of child labor is smaller than the gain in schooling. An extra 100 kg. of rice reduces the incidence of child labor by 0.04 and 0.02 for boys and girls respectively (31% and 18% of mean child-labor incidence). So, for boys selected for the program, lower incidence of child labor accounts for about one quarter of the increase in school enrolment; for girls, it accounts for one eighth.

Recall that the average wage for boys is 464 Tk/month, and 291 Tk for girls. Then the stipend's effect on child labor (Table 1) implies an average foregone income from FFE participation of 16.7 Tk/month for boys and 6.4 Tk for girls. A typical FFE family has two children of primary-school age. For a family with one school-age boy and one school-age girl, the foregone income from FFE participation is then 19% of the estimated monetary value of the FFE stipend of 119 Tk/month (section 2).

So there is a large net transfer benefit to poor households from the program. There is of course also a benefit over time, through higher schooling. Wodon (1999) finds that completing primary school in rural Bangladesh increases expected per capita consumption by nine percent (controlling for a range of individual and household characteristics). And there are likely to be

other benefits from the higher school attendance induced by the program, including through better health-care and greater ability to participate in society. A complete evaluation would have to also consider the costs, of course. For example, unless there is excess capacity (which seems unlikely) or a sufficient contemporaneous investment on the supply-side, the higher enrollments due to the program will create congestion in schools, lowering the quality of education.

The effects of household demographic variables are generally weak. Children from larger households are neither more nor less likely to be in the workforce, or at school. A higher share of working-age adult males in the family reduces child labor by boys. This suggests greater pressure for boys to earn income when in families where there are fewer adult male earners. There is a negative effect of female headship on girls' schooling.

There are very strong effects of parental education on children's child labor and schooling. Higher parental education is associated with lower incidence of child labor and higher school attendance rates. There are qualitatively similar effects of maternal education, although they are not as large in magnitude or as significant statistically.

Finally, owning more land decreases girls' child labor, but not boys. Parents with larger holdings may well have larger demand for boy's labor time in helping to supervise hired labor — an activity that is unlikely to be seen as appropriate for girls in rural Bangladesh.

6. Conclusions

We have tried to determine if children sent to work in rural Bangladesh are caught in a poverty trap, such that the extra current income to poor families from child labor comes at the expense of the children's longer term prospects of escaping poverty through education. Concerns about the effects of child labor on schooling have often been raised in development-policy

debates, including in recent discussions of the welfare effects of labor-intensive growth fuelled by trade liberalization.

The poverty trap argument depends critically on the substitution possibilities between children's leisure and schooling. On a priori grounds it would not seem difficult for parents to assure that a child in Bangladesh working for (say) 20 hours per week can still attend all primary school classes. Nor does it seem that the majority of children who have not finished primary school, but are not at school, are normally working. Casual observations and the descriptive statistics available from surveys do not seem to offer much support for the poverty trap idea.

To explore the question more deeply, we have used a targeted school stipend to identify how much child labor substitutes for schooling. We find strong effects on school attendance of the incentive provided by Bangladesh's Food-for-Education program. A stipend with a value considerably less than the mean child wage was enough to assure nearly full school attendance amongst participants. This impact on schooling is likely to be socially beneficial from a number of points of view.

Our results suggest that the enrollment subsidy also reduced the incidence of child labor. However, this effect only accounts for a small proportion of the increase in school enrolment; the reduction in the incidence of child labor by boys (girls) represents about one quarter (eighth) of the increase in their school enrollment rate. Parents are clearly substituting other uses of their children's time, so as to secure the current income gain from access to the program with modest impact on earnings from their children's work.

Our tests are limited in a number of respects. Work may well displace time for doing homework or attending after-school tutorials; we have not been able to identify such effects with the data available. There may also be other welfare losses to children from work (such as when this entails exposure to an unsafe working environment). And there may well be other gains (such

as when the skills leant from working enhance the returns from schooling). However, our results do lead us to question the seemingly common view that child labor is a major factor perpetuating poverty in this setting by keeping children from poor families out of school.

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Table 1: *Impact of the Food-for-Education Program on child labor and schooling*

	Work by boys			Work by girls			Schooling of boys			Schooling of girls		
	d*F*/d*X*	Std. Err.	P>|z|	d*F*/d*X*	Std. Err.	P>|z|	d*F*/d*X*	Std. Err.	P>|z|	d*F*/d*X*	Std. Err.	P>|z|
FFE stipend	-0.036	0.011	0.000	-0.022	0.007	0.000	0.172	0.034	0.000	0.160	0.033	0.000
FFE residuals to correct for endogeneity	-0.004	0.002	0.016	0.000	0.001	0.676	-0.005	0.006	0.355	-0.009	0.006	0.108
Household characteristics												
Log household size	-0.007	0.009	0.404	-0.003	0.006	0.618	0.008	0.032	0.790	-0.022	0.030	0.461
Share boys 5 to 9	-0.003	0.033	0.934	-0.004	0.023	0.876	0.021	0.120	0.863	-0.105	0.114	0.359
Share girls 5 to 9	-0.052	0.035	0.125	0.004	0.021	0.831	0.189	0.123	0.125	-0.179	0.114	0.116
Share boys 10 to 16	-0.012	0.029	0.684	-0.041	0.021	0.030	-0.063	0.104	0.548	0.005	0.100	0.959
Share girls 10 to 16	-0.052	0.033	0.102	-0.044	0.023	0.034	0.002	0.109	0.989	-0.100	0.105	0.340
Share adults male 17 to 40	-0.113	0.040	0.002	-0.056	0.025	0.010	0.243	0.129	0.060	-0.047	0.119	0.692
Share adults female 17 to 40	-0.044	0.041	0.288	-0.049	0.029	0.060	0.132	0.146	0.367	0.218	0.142	0.126
Share adults male above 40	-0.022	0.040	0.578	0.003	0.028	0.915	-0.036	0.145	0.805	-0.156	0.143	0.275
Share adults female above 40	-0.057	0.043	0.188	-0.003	0.027	0.919	0.179	0.148	0.227	-0.088	0.146	0.548
Female household head	-0.013	0.014	0.462	0.001	0.014	0.934	0.049	0.063	0.470	-0.193	0.099	0.023
No spouse, married	0.009	0.025	0.684	0.012	0.019	0.442	-0.070	0.077	0.330	0.040	0.058	0.518
No spouse, single	0.056	0.052	0.096	0.009	0.016	0.464	-0.023	0.068	0.724	0.026	0.053	0.638
No spouse, div./widowed	0.024	0.031	0.331	0.010	0.022	0.574	-0.251	0.096	0.003	-0.038	0.077	0.599
Age of the child	0.029	0.013	0.058	0.014	0.007	0.079	0.248	0.030	0.000	0.293	0.030	0.000
Age of the child squared	0.000	0.001	0.701	0.000	0.000	0.779	-0.014	0.002	0.000	-0.016	0.002	0.000
Non-Muslim	0.008	0.012	0.413	-0.004	0.005	0.435	-0.053	0.039	0.148	-0.057	0.036	0.089
Education of father and mother												
Father below class 5	-0.015	0.006	0.013	-0.011	0.004	0.003	0.095	0.022	0.000	0.089	0.020	0.000
Father class 5 (primary completed)	-0.023	0.006	0.001	-0.015	0.004	0.000	0.104	0.025	0.001	0.119	0.020	0.000
Father class 6 to 9 (secondary school)	-0.021	0.006	0.001	-0.019	0.005	0.000	0.111	0.024	0.000	0.140	0.018	0.000
Father higher level	-0.028	0.007	0.000	-0.019	0.005	0.000	0.197	0.018	0.000	0.192	0.012	0.000
Mother below class 5	-0.013	0.007	0.094	-0.008	0.005	0.179	0.112	0.024	0.000	0.092	0.025	0.004
Mother class 5 (primary completed)	-0.022	0.007	0.010	-0.015	0.004	0.001	0.099	0.030	0.006	0.088	0.029	0.016
Mother class 6 to 9 and higher	0.020	0.016	0.170	0.005	0.010	0.632	0.033	0.047	0.489	0.094	0.048	0.052
Land ownership												
0.05 to 0.49 acres	0.006	0.009	0.439	-0.005	0.004	0.297	0.016	0.027	0.544	0.051	0.023	0.033
0.50 to 1.49 acres	-0.011	0.008	0.194	-0.011	0.004	0.015	0.100	0.026	0.001	0.119	0.021	0.000
1.50 to 2.49 acres	-0.003	0.010	0.799	-0.010	0.004	0.070	0.137	0.024	0.000	0.122	0.022	0.000
2.50 acres or more	-0.003	0.010	0.800	-0.008	0.005	0.114	0.124	0.027	0.000	0.125	0.023	0.000

Source: Regressions by the authors using 1995-96 Household Expenditure Survey for Bangladesh. The values of d*F*/d*X* are estimated at the sample means. Sample sizes: 2441 boys and 2323 girls. Pseudo R2 of 0.38 for work for boys, 0.44 for work for girls, 0.20 for schooling for boys, and 0.26 for schooling for girls. The excluded categories for dummy variables are male household head, spouse present, illiterate father, illiterate mother, landless household, and Muslim household. The regressions also included about 60 variables describing schools and communities likely to influence program placement at village level. The column P>|t| denotes the significance level. The residuals used to correct for endogeneity were obtained from a first-stage tobit for the FFE stipend which included village-level selection for the program as an instrumental variable; the latter was highly significant in the first-stage regression as were many other variables, and generally with expected signs. The first stage regression was estimated on a sample of 3625 household.