Child Labour and Schooling Responses to Access to Microcredit in Rural Bangladesh

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Abstract

Microcredit has been shown to be effective in reducing poverty in many developing countries. However, less is known about its effect on human capital formation. In this paper, we develop a model examining the relation between microcredit and child labour. We then empirically examine the impact of access to microcredit on children's education and child labour using a new and large data set from rural Bangladesh. We address the selection bias using the instrumental variable method where the instrument relies on an exogenous variation in treatment intensity among households in different villages. The results show that household participation in a microcredit program may increase child labour and reduce school enrolment. The adverse effects are more pronounced for girls than boys. Younger children are more adversely affected than their older siblings and the children of poorer and less educated households are affected most adversely. Our findings remain robust to different specifications and methods, and when corrected for various sources of selection bias.

JEL: H43, I21, J13, J24, L30, O12

Keywords: Microcredit, child labour, school enrolment, instrumental variable, treatment effect.

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1. Introduction

Microcredit programs have expanded rapidly in recent decades in the developing world. It has reached more than 20 million borrowers in Bangladesh, which accounts for about 60 percent of the country's poor rural households (World Bank 2006). The United Nations (UN) declared 2005 as the International Year of Microcredit, and urged multilateral donor agencies and developed countries to support the microfinance movement to achieve its Millennium Development Goal of halving poverty by 2015. International donors, lending agencies, national governments are now allocating tens of millions of dollars for microcredit programs each year. There has been renewed pledge from policy makers and practitioners to expand such programs and to increase their outreach to reduce poverty. The success and popularity of microcredit over the past decades are evidenced by the fact that there are more than 7000 microfinance institutions today, serving millions of poor people, and that microcredit has proved to be an important instrument in helping "large population groups find ways in which to break out of poverty" (The Norwegian Nobel Committee's press release in awarding the Nobel Peace Prize for 2006 to Grameen Bank and its founder Muhammad Yunus.).

If access to microcredit helps reduce poverty, then one might surmise that it could also improve investment in children's education. This is because underdeveloped credit markets coupled with low household income (Ranjan 1999; Baland and Robinson 2000; Doepke and Zilibotti 2005) or lack of access to credit are often considered major factors responsible for inadequate education for children in developing countries (Jacoby and Skoufias 1997; Ranjan 2001; Dehejia and Gatti 2005; Edmonds 2006). Access to credit can have a positive effect on children's education through a number of channels. First, to the extent that credit may increase the borrower's income, the income effect may positively affect the demand for children's schooling (Behrman and Knowles 1999). Second, the vulnerability of rural households to adverse exogenous shocks may force them to pull their children out of school in times of need, hampering sustained school enrolment for their children. Loans from microcredit organizations (MOs) can assist consumption smoothing (Pitt and Khandker 1998; Khandker 2005; Islam 2007), thereby reducing the likelihood that children are withdrawn from school in response to adverse shocks. Third, several studies have demonstrated that women have stronger preferences than men for their children's education (Pitt and Khandker 1998; Behrman and Rosenzweig 2002). Since women are the dominant group of borrowers from MOs, microcredit may positively affect children's schooling through empowering women. These preferences toward schooling may also be influenced by mandatory adult training programs conducted by MOs. Though MOs in general do not have any direct declared objective of improving children's education, they do educate members about the potential benefits of sending children to school. For example, Grameen Bank members need to memorize sixteen decisions, one of which is, 'we shall educate our children'.

On the other hand, microcredit may also have unintended consequences on children's education for several reasons. First, microcredit loans often require establishment of household enterprise, which requires extra labour to work in it. For example, if a household uses microcredit loans to purchase livestock, it will require labour to take care of the animals, which can increase the demand for child labour. Second, the amount of loan is not large enough to hire external labour, which may compel the household to resort to child labour.[1] Third, the loan repayment period is short and interest rate is high, making the household myopic, which may induce parents to heavily discount the future return on their children's education.[2] In order to service the loan, it may be necessary to supplement household income, at least temporarily, with the proceeds from child labour. Therefore, the additional activities made possible by access to microcredit and the factors related to servicing the terms of microcredit loan may adversely affect children's education. Children may need to be employed directly in the newly created or expanded household enterprises, or as carer for their siblings, or in farm and livestock duties and other household chores.

While various empirical studies have found that microcredit can increase the household's income and consumption (Pitt and Khandker 1998; Kaboski and Townsend 2005; Islam 2007; Karlan and Zinman 2008a), there is less evidence on the impact of microcredit on human capital formation, and the limited evidence that exists is far less conclusive than the effect of microcredit on alleviating poverty. One strand of empirical studies reports that access to credit can help reduce child labour and increase schooling in developing countries.[3] For example, Jacoby (1994) finds that unequal access to credit is an important source of inequality

[1] Loan size varies but is typically between US$40 to $150. However, members may take larger loans after repaying their first loan. Loans are made for any profitable and socially acceptable income generating activities such as poultry, livestock, sericulture, fisheries, rural trading, rural transport, paddy husking, food processing, small shops and restaurants.

[2] Typical interest rates on microcredit loan are above 30% on a reducing-balance basis and most MOs require that households start repaying the loans four weeks after obtaining credit. The effective interest rates are even higher because of commissions and fees charged by microcredit organizations. The frequency of repayments, and the systems adopted to collect repayments also raise the effective interest rates.

[3] See Belly and Lochner (2007) for the empirical literature on borrowing constraints and schooling in the context of developed countries.

in schooling investment in Peru. Dehejia and Gatti (2005) find a negative association between child labour and access to credit across various countries. Jacoby and Skoufias (1997) observe that, in India, the incidence of child labour increases as access to credit becomes more difficult.[4] The second strand of literature finds ambiguous results. Wydick (1999) reports that the relation between access to microcredit and children's schooling is not unambiguously positive in the case of Guatemala. He finds that a child is more likely to work in a household enterprise when the household borrowing is used for capital equipment instead of working capital. A similar conclusion is drawn in Maldonaldo and Gonzalez-Vega (2008), who find that households demand more child labour if they cultivate land and operate labour-intensive microenterprises. Based on microcredit programs in Bangladesh, Pitt and Khandker (1998) find that girls' schooling is positively affected when women borrow from Grameen but not so when they borrow from other microcredit programs. Finally, Yamauchi (2007) finds that investment in household enterprise may not necessarily eliminate child labour or promote children's education in rural Indonesia while Hazarika and Sarangi (2008) report that, in rural Malawi, children tend to work more in households that have access to microcredit.

Given the limited and conflicting evidence summarized above, the purpose of this paper is to examine the impact of household participation in microcredit programs on *both* children's schooling and child labour using a new, large, nationally representative and unique data set constructed from various microcredit programs in Bangladesh. Our results show that participation in microcredit programs adversely affects children's schooling and exacerbates the problem of child labour. The results overwhelmingly indicate that girls are more likely to be affected adversely, although the effect on boys is ambiguous. It is also shown that younger children, who are more exposed to the program, are more likely to be put to work and less likely to attend school as their parents take out microcredit.

We also estimate the treatment effect by gender of participants, by household income proxied by the level of education obtained by parents, and by household land ownership. Although the adverse effect does not differ much whether credit is obtained by women or men, we find some evidence for gender preferences: the adverse effect on girls' schooling tends to be smaller when credit is obtained by mother than when it is obtained by father. The adverse effect decreases in household income and asset ownership, implying that children of poorer households are more likely to be caught in a vicious poverty cycle. Our empirical findings

[4] Similar results are reported in Beglee, Dehejia and Gatti (2005) for Tanzania, and in Edmonds (2006) for South Africa.

remain robust to different specifications and methods, and when corrected for various sources of selection bias.

The adverse effect of microcredit on children's schooling and child labour is likely to be due to several specific aspects of microcredit loans discussed earlier. Indeed we also find evidence showing that participation in microcredit programs increases the likelihood that children are taken out of school to work for their parents in household enterprises set up with microcredit. Overall our results suggest that care needs to be taken in assessing the effectiveness of microcredit programs. On one hand, successful microcredit programs can alleviate poverty and contribute to rural economy. On the other hand, they can alter parents' incentives in a way that adversely affects children's schooling, which could exacerbate poverty in the longer term. In addition, the adverse effect that falls unequally on girls would reduce the effectiveness of policies to promote gender equality in education in developing countries. In sum, microcredit programs need to be complemented by other policies to tackle the multiple goals of poverty reduction, human capital formation, and social development.

The rest of the paper is organized as follows. Section 2 provides a simple theoretical model examining the relation between microcredit and child labour and shows that access to microcredit can result in increased child labour if the credit cannot be used to hire external labour and the required returns on investment are high. Section 3 describes our data and presents descriptive statistics. Section 4 discusses issues related to our empirical methodology while Section 5 reports the main empirical findings. Section 6 provides the results from additional robustness check. Section 7 concludes the paper.

2. A Model of Microcredit and Child Labour

Child labour contributes to the household's current consumption at the cost of reduced consumption for children in the future. Therefore, if the household's current consumption is too low (and marginal utility too high) relative to discounted marginal utility of children's future consumption and the household is unable to borrow against future earnings to increase its current consumption, then the household would resort to child labour to increase current consumption. The corollary is that children's education will benefit if the household can increase its current consumption without resorting to child labour. In this section, we present a simple model to understand whether microcredit offers such opportunities.

Our basic model follows Baland and Robinson (2000). A household consists of parents and a child. Parents live for two periods indexed by $t = 1,2$. In each period, parents have a unit of time endowment, which can be supplied inelastically to market activities or used in the household enterprise. Parents' time endowment is worth $e \geq 1$ efficiency units of labour. At $t = 1$, the child also has a unit of time endowment. Parents decide how to allocate the child's time between child labour and human capital accumulation. If l_c is the fraction of child's time that is allocated to work, then $1 - l_c$ is the fraction allocated to schooling, and the child's human capital at $t = 2$ measured in efficiency units of labour is $h(1 - l_c)$ where h is twice-differentiable, strictly increasing, and strictly concave with $h(0) = 1$. The labour market is assumed competitive in each period, and wage per efficiency unit of labour is normalized to one. Parental utility function takes the form

$$U(c_1, c_2, w(c_c)) \equiv u(c_1) + \beta u(c_2) + \delta w(c_c), \tag{1}$$

where c_t is the household's consumption at $t = 1,2$, c_c is child's consumption at $t = 2$, and both u and w are twice-differentiable, strictly increasing, and strictly concave. In the above, $\beta \in (0, 1)$ is a discount factor for the household's second-period consumption and $\delta \in (0, 1)$ is a parameter measuring the extent to which parents are altruistic to their child.[5] Unlike Baland and Robinson, we do not consider the possibility of parents' savings or bequests since the low level of current consumption is the main reason for child labour.

We first look at the case where the household does not have access to microcredit and is unable to establish a household enterprise. In this case, parents' entire time endowment and

[5] In Baland and Robinson (2000), the parental utility function has the same form in both periods and there is no discounting. Because child labour contributes to parents' income only in the first period, parents' first-period consumption is larger than their second-period consumption if bequests are non-negative and savings are zero. Since the parental utility function is strictly increasing and concave, this implies that, with zero savings, the marginal utility from the first-period consumption is less than the marginal utility from the second-period consumption. Thus it follows that zero savings can never be optimal for parents: if savings are set equal to zero for some reason (although it cannot be optimal as argued above), then the marginal utility from the first-period consumption is less than that from the second-period consumption so that the laissez-faire level of child labour is actually *below* the efficient level, contrary to Baland and Robinson's Proposition 3 (p. 670). A simple way to rectify the problem is either to assign different parental utility functions in the two periods or to introduce discounting. We choose the second option.

part of child's time are devoted to market activities.[6] Denoting child labour by l_c, consumption in each period is $c_1 = e + l_c$, $c_2 = e$, and $c_c = h(1 - l_c)$. The household's problem is to choose l_c to maximize the objective function in (1) subject to constraint, $l_c \in [0, 1]$. Denote the solution by l_c^* and use the asterisk for optimal consumption in each period. Following Baland and Robinson, we focus on an interior optimum $l_c^* \in (0, 1)$, of which the first-order condition is

$$l_c^* : u'(c_1^*) - \delta w'(c_c^*) h'(1 - l_c^*) = 0. \tag{2}$$

Next we move to the case where the household can choose to participate in the microcredit program. As discussed previously, credit is typically used for capital expenditure necessary to establish a household enterprise for quick generation of additional income. Running the household enterprise requires labour and, not being able to hire external labour, the household needs to reallocate its labour between market activities and the household enterprise.

To simplify matters, we assume that the household enterprise generates income only in the first period. Since child labour is relevant only in the first period, this simplification is at no loss of generality. The income from the household enterprise is given by $\pi(k, l)$ where k is the amount of credit to be paid back at $t = 2$ at interest rate ρ, l is the amount of labour put into the household enterprise, and π is twice-differentiable, strictly increasing in both arguments, and strictly concave with $\pi(k, l) = 0$ if $k = 0$ or $l = 0$. Since we rule out employment of external labour, we have $l \equiv e l_{ah} + l_{ch}$ where l_{ah} is the amount of adult labour and l_{ch} is the amount of child labour employed in the household enterprise. Denote the amount of adult labour in market activities by l_{am} and the amount of child labour in market activities by l_{cm}. Then we must have $l_{am} + l_{ah} = 1$ and $l_{cm} + l_{ch} \leq 1$. With the household enterprise, consumption in each period is $c_1 = e l_{am} + l_{cm} + \pi[k, e(1 - l_{am}) + l_{ch}]$, $c_2 = e - (1 + \rho)k$, and $c_c = h(1 - l_{cm} - l_{ch})$.

[6] Since households eligible for microcredit typically own a small piece of land (less than half an acre), it is reasonable to assume that household labour is mostly devoted to market activities, rather than to family farm.

The household's problem is now to choose $(l_{am}, l_{cm}, l_{ch}, k)$ to maximize the objective function in (1) subject to constraints, $l_{am} \in [0, 1]$, $l_{cm} \geq 0$, $l_{ch} \geq 0$, $l_{cm} + l_{ch} \leq 1$, $k \geq 0$. To make the comparison with the case without household enterprise meaningful, we focus on the interior optimum for child labour, which will be true if, for example, δ is not too small. Thus the constraint $l_{cm} + l_{ch} \leq 1$ is slack. Denoting the solution by $(\hat{l}_{am}, \hat{l}_{cm}, \hat{l}_{ch}, \hat{k})$ and the second partial derivative of π evaluated at the solution by π_l, the first-order conditions with respect to labour variables are given by

$$\hat{l}_{am} : e(1-\pi_l)u'(\hat{c}_1) \begin{cases} \leq 0 \text{ if } \hat{l}_{am} = 0, \\ = 0 \text{ if } \hat{l}_{am} \in (0,1), \\ \geq 0 \text{ if } \hat{l}_{am} = 1, \end{cases} \tag{3}$$

$$\hat{l}_{cm} : u'(\hat{c}_1) - \delta w'(\hat{c}_c)h'(1-\hat{l}_{cm}-\hat{l}_{ch}) \begin{cases} \leq 0 \text{ if } \hat{l}_{cm} = 0, \\ = 0 \text{ if } \hat{l}_{cm} \in (0,1), \end{cases} \tag{4}$$

$$\hat{l}_{ch} : \pi_l u'(\hat{c}_1) - \delta w'(\hat{c}_c)h'(1-\hat{l}_{cm}-\hat{l}_{ch}) \begin{cases} \leq 0 \text{ if } \hat{l}_{ch} = 0, \\ = 0 \text{ if } \hat{l}_{ch} \in (0,1). \end{cases} \tag{5}$$

Needless to say, participation in the microcredit program should be individually rational for the household. That is, the household should be better off with microcredit than without it if it decided to participate in the program. It is then easy to see that a necessary condition for beneficial microcredit is $\pi_l \geq 1$, or the marginal return on labour from the household enterprise should be larger than that from market activities. To see this, suppose $\pi_l < 1$. Then from (3), we have $\hat{l}_{am} = 1$, or entire adult labour should be in market activities. Also from (4) and (5), we have $\partial U / \partial l_{cm} > \partial U / \partial l_{ch}$. Thus if optimal child labour in market activities is positive, then child labour in the household enterprise should be zero. In sum, if $\pi_l < 1$, then the solution to the household's problem is the same as the one in the absence of microcredit. Thus in what follows, we focus on the case $\pi_l \geq 1$. In this case, we have $\hat{l}_{am} = 0$ and, from (4) and (5) again, child labour should necessarily be employed only in the household enterprise. At the interior solution, the first-order condition (5) holds with equality:

$$\hat{l}_{ch} : \pi_l u'(\hat{c}_1) - \delta w'(\hat{c}_c) h'(1 - \hat{l}_{cm} - \hat{l}_{ch}) = \pi_l u'(\hat{c}_1) - \delta w'(\hat{c}_c) h'(1 - \hat{l}_{ch}) = 0 . \tag{6}$$

We now turn to our main question of how microcredit affects the extent of child labour. Let us first observe that if the household decides to participate in the microcredit program, its utility should be at least as large as when it does not participate. Since participation in microcredit reduces the household's second-period consumption, it should necessarily be that either the first-period consumption increases or the child's second-period consumption increases, the latter being equivalent to reduced child labour. Otherwise, the household can always return to the situation without microcredit. Rearranging (2) and (6) leads us to

$$\delta[w' h'(1 - \hat{l}_{ch}) - w' h'(1 - l_c^*)] = \pi_l u'(\hat{c}_1) - u'(c_1^*). \tag{7}$$

Since w and h are strictly concave, it follows from (7) that $\hat{l}_{ch} \geq l_c^*$ or child labour increases with microcredit if and only if $\pi_l \geq u'(c_1^*)/u'(\hat{c}_1)$. Note also that $\pi_l \geq u'(c_1^*)/u'(\hat{c}_1)$ is consistent with $\pi_l \geq 1$, a necessary condition for beneficial microcredit. This is because, when child labour increases, the only way the household can benefit from microcredit is to increase the first-period consumption ($\hat{c}_1 \geq c_1^*$), from which $u'(c_1^*)/u'(\hat{c}_1) \geq 1$ follows since u is strictly concave. The above condition has a ready interpretation: beneficial microcredit increases child labour if the marginal return on labour from the household enterprise is sufficiently large. In this case, parents divert more child labour to the household enterprise than when they did not have access to microcredit. Needless to say, child labour need not increase in this case if parents can hire external labour at market wage of one. On the other hand, if $1 \leq \pi_l < u'(c_1^*)/u'(\hat{c}_1)$, then microcredit can actually reduce child labour. As this case shows, inability to hire external labour alone is not sufficient for an increase in child labour; if the return on child labour from the household enterprise is not sufficiently large relative to the return on schooling, then parents would continue to send their children to school. However, since repayment of the loan typically requires high returns on investment, one could argue that the case of $\pi_l \geq u'(c_1^*)/u'(\hat{c}_1)$ is more likely. In sum, access to microcredit can result in increased child labour if the credit cannot be used to hire external labour and the required returns on investment are high.

3. The Program, Data and Descriptive Statistics

3.1. Background: Schooling and Child Labour in Bangladesh

Bangladesh has achieved rapid progress in child schooling in recent years. The gross primary enrolment rate increased from 72 percent in 1990 to 96 percent in 2000**.** This has been made possible due to government's various stipend programs for children in primary and secondary schools in all rural areas of Bangladesh. However, the Bangladesh Household Income and Expenditure Survey 2000 reports the net primary enrolment rate of only 65.4 percent and the primary school completion rate of 66.3 percent in 2000.[7] The Bangladesh Child Labour Survey 2002-03 estimates that 6.4 million children aged 5-17 work in rural areas compared to 1.5 million in urban areas. Most of the child labour is in agriculture. Nearly 50 percent of primary school students drop out before they complete grade five. Among the poorest quintile of households, the share of family income contributed by child labourers reaches nearly 50 percent (Salmon, 2005). Child labourers aged 5-14 constitute about 12 percent of the country's labour force (Rahman et al. 1999) of which 73.5 percent are boys and 26.5 percent are girls.

Despite the persistence of child labour, considerable progress has been made in increasing equitable access, reducing dropout rates and implementing quality enhancement measures in primary education. Access to primary education has increased steadily over the past two decades. A compulsory primary education law was adopted in 1990, and the compulsory primary education program was extended nationwide in 1993 although the law is not strictly enforced. Incentives to attend primary school have been introduced with the distribution of textbooks and provision of "food for education"—the latter was converted to a cash stipend in 2002. Primary education in rural areas is provided through government schools, madrasas (Islamic schools) and NGO-run non-formal primary schools.

3.2. The Program and Data

The data were collected by the Bangladesh Institute of Development Studies (BIDS) on behalf of the Palli Karma-Sahayak Foundation (PKSF) (Rural Employment Support Foundation)

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[7] Gross primary school enrolment rate is the total number of pupils enrolled in primary school, regardless of age, expressed as a percentage of the population in the theoretical age group for primary education. Net primary school enrolment rate is the total number of pupils in the theoretical age group for primary education who are actually enrolled in primary school expressed as a percentage of the total population in that age group.

with support from the World Bank.[8] This survey is the largest and the most comprehensive of the existing microcredit programs in Bangladesh. Its geographic coverage is spread evenly across Bangladesh, and the sub-district (thana) level comparisons reveal that selected sub-districts are not different from the average (Zohir et al. 2001). The data cover 13 MOs of different sizes in terms of operations and membership. These MOs were selected to constitute a nationally representative data set for the entire microcredit program in Bangladesh. The most notable MOs studied in this paper are ASA and Proshikha, the third and fourth largest MOs, respectively, in Bangladesh. All 13 MOs follow the Grameen Bank-style lending procedure and typically give access to microcredit to households owning less than a half-acre of land.

The survey includes 13 districts covering 91 villages spread over 23 sub-districts in Bangladesh. A census of all households in the 91 villages was conducted before the survey was administered in early 1998. The actual targeting of survey households involves two stages: (1) the selection of the villages where MOs operate; (2) the selection of treated households within the selected villages. The non-participants from the program villages who are observationally similar were also selected as the control group. Participation in a credit program was defined in terms of current membership reported during the census. From the village census lists of households, 34 households were drawn from each program and non-program village. Because the census found a large number of ineligible households in program villages, the sample was drawn to maintain the proportion of eligible and ineligible households of about 12:5. The sample size within program and control villages was also determined accordingly.

3.3. Descriptive Statistics

The original survey consists of 3026 households. In this paper, we consider the subset of 2034 households who have at least one child aged 7-16 at the time of the survey. This represents a total of 4277 children of which 2658 belong to treatment households and the remainder to the control group. Our sample contains both male and female borrowers but the former account for only 12 percent of all borrowers (and 133 households) representing 281 children. Among all children, 54.2 percent are boys.

[8] The PKSF is the apex organization for microfinance. The microlending community regards it as a regulatory agency, and it exercises authority over the MOs.

The household level questionnaire includes primary and secondary activity of each child. We define "child labourer" as anyone aged 7-16 who performs any economic activity (i.e., if a parent answers 'employed', 'household work', or 'employed but not working'). A child is considered to be in school if he/she is currently enrolled in school and attended school in the last month of the survey period. By this definition 77.4 percent of girls aged 7-16 in the sample were classified as being in school and 10.4 percent in work. The corresponding figures for boys are 71.3 percent and 15.7 percent, respectively. Other children are reported to be neither working nor in school, and possibly many of them are helping parents with household work. So there may well be under-reporting of child labour.[9] The results by participation status are reported in Table 1. School enrolment is lower and child labour higher among children of the treatment group. We find a statistically significant difference in school enrolment and child labour between boys of treated and untreated households, but no such difference exists for girls. However, the difference in school enrolment between girls and boys is larger in the treatment group.

--- Table 1 goes about here. ---

Figure 1 plots school enrolment of children by age for both sex groups. Children at high-school age (12-16 years old) are less likely to be enrolled in school because of drop-outs. At primary-school age, the proportion of children aged 7-8 enrolled in school is lower than their older counterpart (9-11 years old), indicating that there are a considerable number of children who start schooling at a later age. The difference between treatment and control groups in school enrolment is larger for boys. Girls aged 7-11 have a similar rate of enrolment in both treatment and control groups, but after age 13, girls in the control group tend to have a lower enrolment rate. On average, children at primary-school age have a higher enrolment rate compared to their older siblings, the latter more likely to drop out from school and go to work. Overall, a higher proportion of children from treated households are in work (Figure 2).

--- Figures 1 and 2 go about here. ---

Table 1 also provides other descriptive statistics for child and household demographics and village characteristics. It shows that the average age of children is 11.5 years for both groups

[9] In our sample, only one percent of children are reported to be both in school and in work, so we ignore these cases. It is usual in rural areas of Bangladesh that parents arrange a modest amount of part-time work for their children while still keeping them at school (see, for example, Ravallion and Woodon 2000).

of households. There is no difference between treatment and control groups in the gender composition of children. The treated group has slightly more members in the household than the non-treated group. For each household in the survey, there is an average of four children below 18 years of age. Non-treated households tend to be better educated, a little older but smaller in household size. Descriptive statistics not reported in Table 1 shows that a total of about one quarter of mothers did not go to school at all. More than a quarter of our sample has a secondary school in their locality, and primary schools exist in most of the villages. Compared to program villages, control villages are more likely to have primary and secondary schools, telephone office and local government (UP) offices. On the other hand, program villages have superior health facilities and are located relatively closer to the nearest sub-district. However, most of these differences are not statistically significant at the conventional level. Thus we do not think that the differences between the program and control villages render the possibility of non-random program placement an issue of concern. Nonetheless, we control these characteristics plus a wide variety of village-level variables in our regression to take into account of possibility of non-random program placement at the village level.

4. Empirical Methodology

In estimating the impact of microcredit on children's school enrolment and child labour, we follow a standard methodology (Wydick 1999; Ravallion and Wodon 2000; Edmonds 2006). Let S_i be a binary variable that denotes whether child i (i) works (S_i =1) or not (S_i =0) and (ii) attends school (S_i =1) or not (S_i=0). We estimate the impact of participation in microcredit programs on children's schooling/work with the following equation:

$$S_{ijkl} = \beta_{0l} + \beta_1 X_{ijkl} + \beta_2 Z_{kl} + \beta_3 Credit_{jkl} + \varepsilon_{ijkl} \qquad (8)$$

where the subscripts index child (i), household (j), village (k), and district (l). X is a vector of child- and household-specific covariates, and Z is a vector of village-specific covariates. β_{0l} captures fixed effects. 'Credit' is a continuous treatment variable defined by the amount of microcredit borrowed by the household. It is equal to zero if a household did not participate in a microcredit program. The error term ε_{ijkl} is assumed to be i.i.d. Using equation (8) we can use the probit model to estimate the probabilities of child labour or school enrolment attributable to participation in microcredit programs.[10]

Estimating equation (8) directly is problematic, however. First, programs may be placed in specific villages and hence program placement may not be random. Selection for placement could be influenced by biases in favour of high-income villages – because they may have higher participation rates – or by official bias in favour of poorer villages. However, given that programs are placed by central decision and that there are hundreds of MOs, it is reasonable to assume that village level program placement is a problem of "selection-on-observables". The survey covers a wide range of village level variables. So we can account for the non-random program placement by a set of control variables at the village level, which are included in the vector Z. We also use district level fixed effects to remove any unobserved heterogeneity across different geographical areas. Since we have 13 MOs, each from a different district, this fixed effect also captures the differences between the MOs. Thus, we tackle the potential problem of non-random program placement using both geographical and MO-level fixed effects and village-level observed covariates.[11] It is to be noted that we adopt an estimation strategy different from that used by Pitt and Khandker (1998). We do not use village fixed effects. Rather we use village-level pre-program characteristics to control non-random program placement. Village fixed effects could give us biased results if the programs are placed based on certain shocks (e.g, floods) at the village level. We control any unobserved heterogeneity using geographical (district level) and MO-level fixed effects.[12]

Second, households self-select into the program but not all of them are able to obtain microcredit. Generally only eligible poor households receive microcredit, the eligibility being typically determined based on the amount of land-holding. However, other factors that influence whether a household has access to microcredit could also affect outcomes for

[10] It is possible to use a bivariate probit model to estimate child labour and schooling simultaneously. However, the number of children who are both in school and in work or who do neither is very small in our sample. Thus the work versus schooling is nearly a dichotomous decision and therefore we do not adopt a bivariate probit model.

[11] Probit estimates with fixed effects give rise to inconsistent coefficients of the fixed effects. However, when the number of observations per fixed effect is at least 8, we can consistently estimate the fixed effects (Heckman 1981). We have at least 250 observations per district and so the model is consistently estimated. For the same reason, we do not estimate parental fixed effects which can eliminate unobserved time-invariant household-level variables or permanent heterogeneity. Instead we consider clustering at the household level.

[12] Fixed effects would eliminate village-level omitted characteristics, but differences in initial conditions also matter for program placement. See Keane and Wolpin (2002) for a similar analogy for problems using state-level fixed effects to estimate the welfare impacts in the US, and the resulting bias in the estimates. See also Morduch (1999) for pitfalls using village fixed effects in Pitt and Khandker (1998). However, our qualitative conclusion is not affected even if we use village fixed effects or separate fixed effects for target and non-target populations in each village. Using different fixed effects only changes the size, not the sign, of the coefficient estimates.

children of that household. One such factor could be household income or wealth. For example, MOs may be more willing to provide credit to households that operate non-farm enterprises because the use of credit is less fungible in such households. Microcredit loans often require that family enterprises be established because they provide less opportunity for misuse of the loan. Poor households that operate an enterprise are also more likely to employ their children in that enterprise, and thus less likely to send them to school. Such negative correlation between credit access and schooling introduces a conservative bias in the coefficients. Thus we need to consider the endogeneity of participation in microcredit programs at the household level. The endogeneity problem implies that selection into treatment is on the basis of unobserved characteristics ε_{ijkl} in equation (8). This implies potential non-zero correlation between ε_{ijkl} and $Credit_{jk}$. Consequently, impact estimates that use a simple probit/linear probability model (LPM) may not reflect the program's causal effect on children's school enrolment or child labour.

To account for self-selection into the program, we consider a source of exogenous variation. The MOs set the eligibility criteria for participating in the program. A household is eligible if it does not own more than a half-acre of land. The land ownership criterion is mainly used as a targeting mechanism to identify the poor. Since poverty does not exclusively depend on land ownership, however, the administrator, local loan officer or branch manager sometimes take into account other socio-economic conditions of a household. Consequently there are some ineligible households that receive microcredit. Although these households are a distinct minority (70 percent of the treatment group in our sample is eligible), the participation in the program based on eligibility is probabilistic since the program eligibility criterion is not strictly followed. Thus our approach in estimating the treatment effect is similar to the use of fuzzy regression discontinuity design (see Van der Klaauw 2002), which we implement using an IV approach.

It is clear that a household is more likely to receive microcredit when a microcredit program is already available in a village. Therefore as an instrument for the actual receipt of microcredit, we may consider the eligibility status interacted with an indicator for presence of program in a given village.[13] Instead of using this instrument directly, however, we utilize an unexploited

[13] Pitt and Khandker (1998) and Islam (2007) use this instrument for participation in a credit program in Bangladesh, and discuss the plausibility of this instrument in detail. Morduch (1998) questions the validity of using this instrument, but in response to Morduch's critique, Pitt (1999) argues at length that the eligibility criterion satisfies the conditional exogeneity and exclusion restriction.

exogenous source of variation in the treatment intensity based on a household's exposure to the program in different villages. As shown in Figure 3, treated households in different villages appear to borrow different amounts. Intensity of treatment varies widely in different villages, depending on how long a microcredit program has been available in the village. In our sample, the earliest a program was made available in a village was 1980 and the latest a program became available in another village was 1997. As shown in Figure 3, the amount of credit a household borrows largely depends on how long the program has been available in the village. So we use the instrument, $I = M_k \times E_j \times N_k$ where M_k is a binary variable that equals 1 if village k has a microcredit program, E_j is a binary variable that equals 1 if household j is eligible (i.e., owns less than half-acre of land), and N_k is the number of years a microcredit program has been available in village k.[14] With controls for village and fixed effects, identification requires that there be no contemporaneous village-level unobservables that are correlated with program placement and child labour/schooling. The equation for the demand for credit then assumes the form:

$$Credit_{jkl} = \alpha_{0l} + \alpha_{1jk}(M_k \times E_j \times N_k) + \alpha_2 X_{jk} + \alpha_2 Z_k + \xi_{jkl} \tag{9}$$

where X now includes only household-specific covariates since participation in microcredit programs is determined at the household level.

--- Figure 3 goes about here. ---

The probit estimates are obtained using the two-stage procedure where the second-stage regression uses the value of credit from the first-stage credit demand equation (9), which is estimated by a standard Tobit model. The use of estimated variable (instrumented credit) in a non-linear specification may lead to bias but this bias is of second order and thus very small (Train et al. 1987). We also estimate the second stage using ordinary least squares (OLS) estimations of LPM. Additionally, because of the non-random nature of our sample we use inverse-propensity score weights in the standard fashion for all the estimators (Hirano, Imbens and Rider 2003). This involves attaching an estimated weight to each observation in one sample that corresponds to the probability of observing a similar observation in the other

[14] We also used $M_k \times E_j$ only as an instrument and obtained qualitatively similar results. We also experimented with instruments that include separate dummies for year of microfinance placement in villages. Again the results turn out to be similar.

sample. With normalization, we attach a weight of one to each treated household, and to each comparison group member a weight of *p/(1-p)*, where *p* is the estimated propensity score.[15]

A potential problem with interpreting these results when using credit as the treatment variable is that the reported amount of credit is subject to misreporting or other types of measurement error since households may forget or not report the amount correctly.[16] This measurement error is likely to impart attenuation bias to the estimated coefficients. However, we do not think the problem of measurement error is serious in our case since we are using instrumented credit variable as the treatment variable. Nonetheless, we also use a binary treatment indicator, i.e., whether or not a household is currently a member of a microcredit program or not, which is unlikely to be measured or reported with error. It can also serve as a robustness check of our main results. It should be noted, however, that the use of binary treatment indicator raises another issue as dummy endogenous regressors with limited dependent variables raise some econometric problems. Angrist (2001) advocates using simple IV estimators as an alternative because they require weaker assumptions and are often sufficient to answer questions of interest in empirical studies. We therefore estimate the treatment effect also by using a LPM in the second stage of the IV regression.[17]

To adjust for clustering at the village level we first use the cluster-correlated Huber-White covariance matrix estimator. Donald and Lang (2007) have pointed out that asymptotic justification of this estimator requires a large number of aggregate units. Monte Carlo simulations (Bertrand, Duflo and Mullainathan 2004) suggest that, when the number of primary sampling units (PSUs) is less than 50, this estimator performs poorly, leading to excessive rejection of the null hypothesis of no effect. Fortunately, with 91 PSUs in our sample we can potentially overcome the problem by using cluster-consistent standard errors. The cluster-adjustment works well for binary outcomes and nonlinear models such as logit and probit models, provided that the number of clusters is large (Angrist and Lavy 2002).[18]

[15] The estimated difference in covariate after adjusting propensity scores is lower than the unadjusted difference between treatment and control groups. However, our qualitative conclusion remains unchanged with or without weighting.
[16] See, for example, Karlan and Zinman (2008b) for problems with self-reported credit data.
[17] When all independent variables are discrete (as is the case with most of our variables), the LPM is fairly general, and fitted probabilities lie within the interval. In addition, the LPM has also the advantage of allowing straightforward interpretation of the regression coefficients. Moreover, we compute Huber-White standard error to take into account the heteroscedastic error term of LPM.
[18] Alternatives to cluster-adjusted standard errors include the hierarchical linear modelling, two-step procedure by Donald and Lang (2007) and the Bell and McCaffrey's (2002) biased reduced linearization estimator for micro data.

Secondly, children of the same household are likely to be similar in a wide variety of characteristics. It follows that there may be large intra-household correlations. Moreover, the data were collected by using households as the survey unit. Therefore we also estimate standard errors clustering at the household level as there is usually more than one school age child within a household.

Checking the Validity of the Instrument:

Before presenting our main findings, we discuss if our chosen instrument is a suitable one. The first-stage regression of equation (9) using a standard Tobit model shows that the instrument is highly statistically significant with t-statistic of 8.5. The coefficient estimate is positive and also economically significant, implying that our instrument is significantly related to the demand for credit. We also estimate the participation decision equation by regressing a binary indicator for participation on an indicator of interaction between eligibility and program village dummies (plus all controls). The results are stronger with t-statistic of 12. The regression using basic controls and no controls results in stronger coefficient estimates. Since we have a single instrument for the credit variable, we cannot test the exogeneity of the instrument as in an over-identified model. The remaining concern is whether the instrument satisfies the exclusion restriction, i.e., whether eligibility affects child labour or school enrolment only through participation in the credit program or the amount of credit borrowed. Although the exclusion restriction is not directly testable, we address this concern in a number of ways. First, we estimate a reduced form regression to examine the effect of loan eligibility on school enrolment/child labour. The results indicate that that there is no effect of eligibility or program placement on school enrolment and child labour. We also estimate an equation in which credit is instrumented but instrument eligibility enters the second-stage regression directly (and naturally in the first stage regression). By definition of IV, the instrument should be uncorrelated with the outcomes of interest through any channels other than their effects via the endogenous regressors. Therefore, once the credit is instrumented, eligibility itself should have no effect on schooling or child labour when both instrumented credit and eligibility status are entered as controls for child labour/school enrolment equation. The results do not indicate any significant effect of eligibility in any of the specifications.[19] Finally we stress that our identification strategy does not depend exclusively on the eligibility rule since we also exploit the variation in credit demand among households in different villages based on the availability of program in different villages.

[19] The detailed results of the first-stage regression are available upon request.

5. Empirical Findings

This section reports our empirical findings where the estimated value of credit from the first-stage regression (equation (9)) is used as the regressor in the second-stage estimation (equation (8)). We estimate the impacts of credit extended to women and men separately.[20] This is to see how the gender of participants in the microcredit program affects schooling and work decision for their children. As mentioned earlier, Pitt and Khandker (1998) and Behrman and Rosenzweig (2002) report that women tend to show a stronger preference than men for educating their children. Pitt, Khandker and Cartwright (2006) also find that women's participation in microcredit programs helps to improve women's empowerment. If microcredit empowers women, then it may also increase the relative chance of girls' schooling. If parents have differential preferences for the education of their daughters and sons, then education outcomes could be different for boys and girls, which could be determined by a household production function (Rosenzweig and Schultz 1982). We thus estimate the results separately for boys and girls by credit given to both women and men using three sets of control variables: "no controls" (excluding the X and Z variables), "basic controls" (some household and child demographic variables, and village controls), and "full controls" (the full set X and Z variables). The list of the full controls is chosen from a larger set of controls by selecting those that were most significant. In identifying the set of control variables we first consider the variables (e.g., household and village characteristics) that the MOs use to select a household and that are likely to determine household demand for credit. We then include a number of regressors to take into account the number of siblings, family composition that can potentially determine the children's schooling or work status. The final set of covariates included in X and Z is listed in the Appendix.

Table 2 reports the estimates of the second-stage regression using the LPM and probit models under different covariate specifications. Columns (1) and (4) represent the treatment effects without any controls. The estimates in column (1) can be considered the Wald estimates, representing the difference in the probability of child labour between children of microcredit participants and non-participants divided by the amount of credit borrowed by the participating households. The Wald estimates in Table 2 show that credit is associated with

[20] Though credit is given to both women and men in different villages, credit groups are never mixed by gender. Households do not have choice over which gender is to participate since MOs select one or the other gender, but not both.

higher probabilities of child labour for girls but lower probabilities of child labour for boys, regardless of whether credit is obtained by men or women. However, the coefficients are not statistically significant. Moreover the Wald estimates are likely to suffer from the omitted variable bias since parental decisions on schooling and child labour are likely to be influenced by household demographic and socio-economic characteristics.

To address the above issues, we consider two sets of control variables, basic control and full control variables, as discussed in Section 4. In Table 2, the results from the LPM model are reported in columns (2) and (3) and those from the probit model are reported in columns (5) and (6). In columns (3) and (6), the full set of controls is included, which is our preferred specification. All coefficients are estimated as marginal effects calculated at the mean. The results in columns (3) and (6) portray a clear picture. Microcredit significantly increases the probability of child labour for girls. For boys, there is some indication that microcredit reduces the probability of child labour especially when credit is obtained by women. Overall, the impact of microcredit on child labour is positive and significant. The qualitative results are independent of whether credit is obtained by men or women. For example, microcredit increases the probability of child labour for girls by 7.9% according to the probit model and 13.7% according to the LPM model. The probability increases by 8.4% and 14.3% respectively when women are borrowers. For boys, women's credit has a marginal negative effect on child labour. Table 2 also shows that girls are affected more adversely, and boys more favourably, when credit is obtained by men than by women, although these estimates are not statistically significant. A Hausman-like test does not support the difference in treatment effect between men and women borrowers. Finally the magnitude of the estimated coefficients increases as we move from basic controls to full controls. The overall finding is that microcredit clearly increases the likelihood of child labour for girls while the impact on boys is less clear.

--- Table 2 goes about here. ---

Table 3 reports the effect of microcredit on school enrolment. The results overwhelmingly indicate that access to microcredit negatively affects children's school enrolment. This is true across all regression models and regardless of whether credit is obtained by men or women. The negative effect is especially pronounced for girls although, for boys, it is statistically insignificant. For example, microcredit decreases the probability of school enrolment for girls by 22.6% according to the LPM model and 19.2% according to the probit model. We also find that the negative effect on girls' school enrolment is larger when microcredit is obtained by men than by women: in the probit model, the probability changes from 19.4% to 22.8%. The negative effect on boys' school enrolment, while statistically insignificant, is larger when women are borrowers. One might surmise that this could be an indication of gender preference by parents. However, Hausman-type tests do not reject the equality of the coefficients between the sexes of the borrower. Once again, the magnitude of the estimated coefficients increases as we move from basic controls to full controls, suggesting that a fuller picture requires the analysis of how a household's socio-economic characteristics affect child labour and school enrolment. We turn to this below.

--- Table 3 goes about here. ---

Table 4 shows how the probabilities of children's school enrolment and child labour are associated with other control variables. The results are mostly consistent with previous studies. For controls at the household level, children's school enrolment is positively associated with education attained by any adult member of the household, the household head's education level, and the male head of the household, while it is negatively associated with the number of younger siblings and the age of the household head. Presence of a mother in the household has a positive but statistically insignificant effect on schooling. For controls at village level and beyond, children's school enrolment is positively related to presence of secondary school or college, and infrastructure such as health facility and brick-built road. Interestingly, presence of grocery market and bus stand has a negative effect on children's schooling. A primary school in the village does not have any statistically significant effect on school enrolment or child labour. This may reflect the fact that almost all villages have a primary school. Similarly rice prices do not have any effect on either school enrolment or child labour possibly because the geographical variation in rice prices is very small. The sign of the adult male wage coefficient in the child labour equation is positive but statistically and economically insignificant, suggesting that adult male and child labour are imperfect substitutes.[21]

--- Table 4 goes about here. ---

[21] According to Basu and Van (1998), if children and adults are substitutes in production (the "substitution axiom"), the prevalence of child labour depresses adult wages —a condition under which a ban on child labour may be desirable. Our results indirectly suggest that this might not be the case. Moreover, when we regress adult male wages on child labour, we find a positive coefficient (t-ratio=1.53), indicating that the substitution axiom does not hold in our case.

The results reported in Tables 2 and 3 do not change qualitatively if we change the treatment variable. Table 5 shows the treatment-on-treated effect using a binary participation indicator as the treatment variable. The estimated effect using two-stage least squares (2SLS) is identical to the indirect least squares estimate obtained from taking the ratio of the reduced-form coefficients, because we are estimating a just identified equation. The results are qualitatively similar to the previous estimates which used credit as participation variable. Girls' education continues to be affected adversely by parental participation in microcredit programs whether credit is obtained by men or women. In probit results, for example, we find that women's microcredit borrowing increases the probability of girls' child labour by 13.7% and decreases the probability their school enrolment by 44.4%. The magnitude of the impact estimates is similar in case the borrower is a man. The corresponding coefficient estimates for child labour for boys are not statistically significant and have mixed signs. Overall, binary participation measures generate considerably larger coefficient estimates for girls. However, these results are only indicative as they do not take into account the variation of treatment intensity, and treat the program effect to be the same for all children in the treatment group.

--- Table 5 goes about here. ---

The standard errors reported in the above tables are corrected for clustering at the village level and weighted by the propensity score to take into account the choice-based sampling. The standard errors in square brackets take into account intra-sibling correlations within a household. Both standard errors are typically of similar magnitude. Since they do not differ much, we report below the regression results only with the clustered standard error at the village level. We also experimented with the two-step procedure discussed by Donald and Lang (2007). In our case this amounts to estimating village fixed effects (household fixed effects when considering intra-sibling correlation) in an equation like (8), and then regressing the estimated fixed effects on instrumented credit and other village covariates (household covariates). Since the estimation results are similar, they are not reported for the sake of brevity. In what follows, we report the results of impact estimates separately by various control variables.

5.1. Impact Estimates by Children's School Age

Table 6 reports the impact estimates for children aged 7-12 (primary school age) and 12-16 (secondary school age: up to grade 10). As before, we use the binary treatment status indicator as the participation variable. The results show that the adverse effect of microcredit on children at the primary school age is mostly significant regardless of the gender of borrowers and children. Girls at the primary school age are especially adversely affected than boys, and more so when credit is obtained by men. For example, the probability of their school enrolment decreases by 33% when credit is obtained by women and by 41% when credit is obtained by men. For children at the secondary school age, microcredit has a mixed effect. Women's credit has a statistically significant negative impact on girls' schooling while men's credit also has a negative but statistically insignificant effect, possibly due to smaller sample size of male participants. For boys at the secondary school age, microcredit increases their likelihood of school enrolment although coefficient estimates are not statistically significant. Overall, microcredit adversely affects younger children more than their older siblings, and girls more than boys, irrespective of the gender of the borrower.

--- Table 6 goes about here. ---

5.2. Impact Estimates by Household Income

Household income plays an important role in determining child labour and school enrolment (Basu and Van 1998; Edmonds 2005; Bhalotra 2007; Belly and Lochner 2007). Poorer families are more likely to take their children out of school in times of need. Poverty is associated with increased level of parental stress, depression and poor health — conditions which might adversely affect parents' ability to nurture their children. Impact estimates by household income also allow us to examine the hypothesis implicit in Basu and Van's (1998) 'luxury axiom' that parents send their children to work and keep them from school only if household income falls below a certain (subsistence) level. However, we cannot treat income as exogenous. Income is endogenous because the amount of credit borrowed by the household directly affects household income. If the participation in microcredit programs has a positive effect on household income, then including income as an explanatory variable would underestimate the actual effect of the program. Moreover, children's contribution to household income also makes the income variable endogenous. Since children working on the family farm are not paid a wage, their contribution cannot be deducted from total income. Even if we could observe income from child labour, the endogeneity problem would not be resolved by simply subtracting it from the total household income if the labour supply of different household members is jointly determined. Income is endogenous for another reason: children living in poorer families may have adverse home environment or face other problems. Such omitted variables may continue to affect their schooling or child labour even if family income may increase.

There are mainly two approaches in dealing with the endogeneity issue: fixed effects estimation (Blau 1999) and instrumental variable technique. While the fixed effects estimation should eliminate any bias from permanent differences in family or children, it may exacerbate bias due to unobserved temporary family shocks (Dahl and Lochner 2005). In the absence of appropriate instruments for income in our context, we use parental education as a proxy for permanent income. If education has a positive return, families with more educated parents are expected to have more income. Clearly parental education is not affected by program participation or child labour supply. We use three categories of parental education: *Low* refers to those households where the highest level of education obtained by parents is primary (0-4 years of schooling) or less; *Middle* refers to households where the highest level of education obtained by parents is more than primary but less than a high school degree (5-10 years of schooling), and *High* includes households where one of the parents obtained at least a high school degree (11 or more years of schooling). We adopt the following functional form:

$$\begin{aligned} Y_{ijkl} = {} & \delta_{0l} + \delta_1 X_{ij} + \delta_2 Z_k + \delta_3 (Credit_{jk} \times Low_{jk}) + \delta_4 (Credit_{jk} \times Middle_{jk}) \\ & + \delta_5 (Credit_{jk} \times High_{jk}) + \nu_{ijkl} \end{aligned} \tag{10}$$

where we incorporate the household's permanent income by interacting the three categories of parental education with the amount of credit borrowed. These interaction terms capture the differences in slope across different levels of education within the treatment group.

Equation (10) is unidentified since the number of endogenous regressors exceeds the number of instruments. Therefore, we need additional instruments that are correlated with the interaction terms between credit and different education categories. Since credit is interacted with education dummies all the predicted values will be closely correlated. In the absence of suitable identifying instruments, we use estimated credit from the first-stage and interact the education dummy variables with the estimated credit variable. Our equation thus becomes:

$$Y_{ijkl} = \lambda_{0l} + \lambda_1 X_{ij} + \lambda_2 Z_k + \lambda_3 (\hat{D}_{jk} \times Low_{jk}) + \lambda_4 (\hat{D}_{jk} \times Middle_{jk}) + \lambda_5 (\hat{D}_{jk} \times High_{jk}) + \upsilon_{ijkl} \tag{11}$$

where $\hat{D}$ is the credit demand estimated from equation (9).

Figure 4 shows how children's school enrolment and child labour vary as the level of parental education changes. The graphs show that there is a positive relationship between children's school enrolment and parental education and a negative relationship between child labour and parental education. Households in the control group tend to have a higher level of children's school enrolment and lower incidence of child labour.

--- Figure 4 goes about here. ---

Table 7 reports the impact estimates based on different levels of parental education. A clear picture emerges. Households with the lowest parental education are those with the largest and significant adverse effect of microcredit on children's schooling. For example, the probit estimates imply that the probability of children's school enrolment decreases by 29.3% in these households while that of child labour increases by 9.7%. For households with medium to high levels of parental education, the impact is largely insignificant, although there is some indication that girls are adversely affected by microcredit in households with medium level of parental education. Given our interpretation of parental education as a proxy for household income, these results indicate that microcredit to the poorest of the poor households neither alleviates the problem of child labour nor improves children's schooling. These households engage their children more in work in order to generate immediate returns from their microenterprise projects. An additional observation is that, while statistically insignificant, the likelihood of children's school enrolment is positive in households with high education. Figure 4 also shows that children are more likely to be sent to school as household income proxied by parental education increases. Taken together, these results indirectly support Basu and Van's (1998) 'luxury axiom'.

--- Table 7 goes about here. ---

5.3. Microcredit, Household Income and Child Schooling

The results in the previous section have shown that children's schooling is less likely to be adversely affected if they come from relatively less poor or more educated family. Microcredit given to high income households actually reduces the probability of child labour and improves children's schooling. To the extent that microcredit can increase household income, one might argue that it could help poor households to graduate out of poverty, thereby improving children's education in the long term. Conversely, if escape from poverty proceeds only very slowly, then microcredit may intensify the problem of child labour and worsen human capital formation. We examine this issue below based on the regression coefficients in Table 7.

Consider the difference between the coefficients for the low and middle income groups in Table 7. Since the coefficient estimates measure the difference in probabilities between treated and untreated households in different income groups, the difference in estimated coefficients between two treated groups can be interpreted as the difference-in-difference estimate of the impact of household income. Then the first column in Table 7 suggests that a 10 percent increase in credit given to middle-income households reduces the probability of children's schooling by about 2.6 percent less than if it had been given to low-income households. A similar calculation for child labour indicates that a 10 percent increase in credit given to middle-income households increases the probability of child labour by 0.6 percent less than if it had been given to low-income households. While the adverse effect of microcredit is reduced when household income increases from low to middle, the adverse effect remains nonetheless. Microcredit can improve children's schooling and reduce child labour only for high-income households: a 10 percent increase in credit given to high-income households improves children's schooling by 0.8 percent and reduces child labour by 0.4 percent than if it had been given to middle-income households. Given the modest increase in income due to participation in microcredit programs,[22] however, it seems reasonable to conclude that child labour remains an issue to be tackled by MOs and policy makers rather than by letting the households graduate out of poverty. With microcredit alone, it would require a substantial amount of time for households to break out of poverty.

5.4. Impact Estimates by Land Ownership

In many rural areas in developing countries, land is often the most significant asset the household owns. If land can be used as collateral for general-purpose loans, then land ownership may have a positive effect on children's schooling. In this case, more land implies more household wealth, and the possible positive relationship between land ownership and children's schooling can be considered a confirmation of the positive relationship between household wealth and children's schooling. However, microcredit is mainly to be used to set up a household enterprise and the purchase of external labour is not often possible. Moreover households in the treated group have microcredit as the main source of loans. Therefore we do

[22] See, for example, Islam (2007) and references therein.

not expect a positive relationship between land ownership and children's schooling. Rather we may expect land ownership to have a negative effect since adult labour may need to be shifted from family farms to the household enterprise, increasing the need for child labour in family farms.

To examine this, we divide households into two groups: those with less than a half acre of land (poorer households) and those with more than a half acre of land (less poor households).[23] Although land ownership of less than a half acre of land is the eligibility criterion for microcredit, there were some households with larger land who still obtained microcredit. Table 8 reports the impact estimates by land ownership. The results show that microcredit has different effects in the two groups of households. In poorer households, it decreases the likelihood of school enrolment for girls while decreasing the likelihood of child labour for boys. In less poor households, the result is reversed. Although it is not clear why less poor households tend to keep boys at work when they obtain microcredit, we surmise that less poor households engage boys more in agriculture activity, while households with marginal landholding engage girls more in the household enterprise.

--- Table 8 goes about here. ---

Many of the results are statistically insignificant possibly because of relatively small sample size. However, overall results also show that our earlier findings were not driven by pre-existing differences in the characteristics between treatment and control groups. It is to be noted that poorer treated households have observed characteristics that are very similar to their non-treated counterparts.[24] Once again, our results show that poor households tend to put girls to work and keep them away from school when they obtain microcredit.

6. Additional Robustness Check

6.1. Potential Identification Issue: Causal Effect or Selection Bias?

[23] Household land ownership is less likely to be affected by microcredit. There is not enough evidence in the data that shows a different pattern of buying and selling land after becoming a member of a MO. Since microcredit is mainly provided for non-agricultural purposes, households are not entitled to buy land using the credit. Also, there is no evidence that households sell land to become eligible for microcredit.
[24] Descriptive statistics for this sub-group is not reported here, but similar results are available in Islam (2007).

Section 5 has reported on how microcredit affects children's schooling and child labour under the assumption that the differences in schooling and child labour between the treatment and control groups are not due to underlying differences in household characteristics. It could be argued, however, that households from program villages that are less likely to send their children to school are more likely to participate in microcredit programs. If this is the case, then our estimates would be picking up effects that are attributable to pre-existing differences in household characteristics between the treatment and control groups, and not to the participation in microcredit programs. In addressing the issue of possible selection bias, we first note from Table 1 that many of the household characteristics are not statistically different between the treatment and control groups. Any remaining differences have been accounted for by using propensity score weights, which also significantly reduce the differences between the two groups. Possible selection bias resulting from unobservable variables has been further addressed using the IV strategy. Nonetheless there may still remain some potential confounders that would violate the exclusion restriction. Given our efforts to control for confounders, the risk of such distortions does not seem large.

In order to substantiate the above claim, we conduct additional robustness checks using alternative approaches. We first use regression-adjusted years of education for older siblings: children who are 16-20 years old. This group of children is less likely to be affected by their parents' microcredit since they would have completed secondary school or dropped out before their parents obtained microcredit. We find no statistically significant difference between the children of treatment and control groups (t-ratio = 0.7). This result is also consistent with our finding that older children are less adversely affected by microcredit, presumably because their schooling period is less exposed to microcredit and younger children can work in the household enterprise instead of their older siblings. Furthermore, the opportunity cost of using child labour in the household enterprise increases with children's age. If they are at the advanced stage in school, much of the investment in schooling that has already been made would be forfeited or, if they are engaged in market work, their higher wage earnings would need to be sacrificed.

Next, we also control selection bias using an alternative method. We consider corrections for endogeneity using reduced form residuals that lead to a control function method of accounting for both selection and endogeneity.[25] This is also important if the impact of microcredit varies across households. In that case, IV/2SLS may not estimate the average treatment effect of credit. There are, however, different approaches to estimate control functions, and not all these procedures produce consistent estimates of the treatment effect. We adopt the procedure suggested by Vella (1993), which correctly identifies the treatment effect parameter in our context.[26] We first obtain generalized residuals using either Tobit (for credit as the treatment variable) or probit (for binary treatment indicator) for the reduced form first-stage equation, and then use the estimated residuals as an additional regressor in the second stage.[27] The results, available upon request from the authors, are similar to those reported before.

6.2. Alternative Measures of Children's Educational Attainment

In this sub-section, we consider the impact of microcredit participation on various alternative measures of children's educational attainment. While the previous measure of school enrolment has the advantage of capturing the current status of school age children, it does not measure the achievement of those who are not in school at the time of the survey. Two alternative measures are the number of years of schooling completed and the 'education gap'. In Bangladesh, the age at which children are expected to start school does not vary cross the country. The age at which a child is legally supposed to go to school is also the same. Therefore, we can construct a variable 'education gap' to measure the achievement in terms of grade completion for a given age. The education gap can be defined as:

Education Gap = *max*{0, Expected education – Actual Education}, where

$$\text{Expected Education} = \begin{cases} 0 \text{ if age} \leq 6 \\ \text{age} - 6 \text{ if } 7 \leq \text{age} \leq 16 \end{cases}$$

For example, if a child successfully stayed in school as expected, the gap is zero. If a child encountered problems such as late entry, failed grades, or drop-outs, then the gap is a positive number. If a child never attended school, then the gap is the level of expected education at that age. Finally, following Patrinos and Psacharopoulos (1997), educational attainment is

[25] The control function approach estimates the average treatment effect by controlling directly for the correlation between the error term and the outcome of equations with the treatment variable. It treats selection bias as an omitted variable problem and augments the outcome equation by a term to control for this omission. The traditional example is the Heckman's sample selection model that augments the outcome equation by an estimate of the Mills ratio.

[26] Garen (1984) suggests a linear control function estimator to correct for endogeneity. However, Garen's approach is appropriate when the dependent variable in the first stage can take a value over a continuous range and it should be uncensored. Similarly the two-stage conditional maximum likelihood approach of Rivers and Vuong (1988) is not applicable as the approach also requires that the credit variable be continuous (see Vella 1993, Ravallion and Wodon 2000).

[27] This model is identified even without the exclusion restrictions because of the non-linearity of the residuals.

obtained by defining a grade-for-age dependent variable as 100*[Education Grade/Expected Education], where education Grade is the number of years a child successfully completed in school.

The control function estimates using the above educational achievement measures are reported in Table 9. The results are based on OLS regressions in the second stage for each of these measures.[28] The negative coefficients for grade completion and grade-for-age, and the positive coefficient for education gap all imply that participation in microcredit program adversely affects children's grade achievement. Once more, girls are more adversely affected than boys: coefficient estimates for girls are larger than for boys and statistically significant at the 1 percent level. Women's participation in microcredit reduces girls' education by about 3 years of schooling while the corresponding decrease for boys' education is about 0.2 years of schooling. Men's participation has a larger negative impact on girls' grade completion—a reduction of 3.8 years in schooling compared to the girls from the control group. The effects on boys' school achievement are not statistically significant in general. The results from all three measures of educational achievement are similar, which is not particularly surprising since the three measures are likely to be highly correlated.[29]

--- Table 9 goes about here. ---

6.3 Are Children Really Working in Household Enterprises?

A possible explanation for the adverse effect of microcredit on children's schooling is that microcredit increases demand for labour in household enterprises set up with microcredit, which may cause children's time to be diverted away from school into household enterprises. We examine this issue below. We classify a child's current status into five different categories based on the detailed occupational information collected during the survey. They are (i) self-employment activity (in household enterprise), (ii) agricultural activity, (iii) day labour, (iv) service-related activity, and (v) student (enrolled in school). We run a multinomial logit model where the parameter of interest is the coefficient corresponding to the instrumented credit

[28] We use OLS instead of conventional Tobit, because in the first stage we estimate credit demand using Tobit. Using Tobit in the second stage then creates further difficulty in consistently estimating coefficients unless the first stage is exactly correctly specified (Angrist 2001). This is not the case if we use OLS.
[29] The coefficient of the residual from the first stage provides an exogeneity test and most of the results reject the exogeneity of credit. Adding squared or higher-order terms of the control function does not change the sign of the coefficient, and the magnitude of the coefficients becomes stable. The higher order terms are not statistically significant either.

variable obtained from equation (9). Table 10 reports the odds-ratios and corresponding marginal effects of the treatment variable.[30] The results show that, for children from treated households, the odds of being in self-employment activities instead of being in school are more than doubled. The corresponding marginal effect indicates that children from treated households are 26.6 percent more likely to work in self-employment activities than those from non-treated households. The odds-ratio is higher and negative for agricultural activity. However, the corresponding marginal effect is economically insignificant. The rest of the coefficient estimates (day labour and service-related activities) are not statistically significant. Finally, the marginal effect for the student status implies that children from treated households have a 26.6 percent lower chance of being enrolled in school than those from non-treated households. Overall, these results support the explanation that children from treated households are more likely to work in household enterprises set up with microcredit.

--- Table 10 goes about here. ---

7. Summary and Conclusion

This paper has studied the impact of access to microcredit on children's education and child labour using a new, large and nationally representative data set from various microcredit programs in Bangladesh. The results overwhelmingly indicate that household participation in microcredit programs has adverse effects on children's schooling, which are especially pronounced for girls. Younger children are more adversely affected than their older siblings and the children of poorer and less educated households are affected most adversely. It appears that children taken out from school are more likely to work in household enterprises that are set up with microcredit than in other types of work. Overall our results suggest that care needs to be taken in assessing the effectiveness of microcredit programs. While microcredit programs can alleviate poverty and contribute to rural economy in the short term, they can also result in unintended consequences of adversely affecting children's schooling, which could exacerbate poverty in the longer term. An additional concern relates to the gender-asymmetric impact of access to microcredit. Government policies aimed at rectifying

[30] We conducted a Hausman-like test to examine whether the maintained assumption of independence of irrelevant alternatives (IIA) is appropriate in our case. The results do not reject the null hypothesis that IIA holds, hence the multinomial logit model is suitable for the data.

gender imbalance in education may turn out to be less effective in the presence many active microcredit programs.

A number of policies can be adopted to mitigate the adverse effect on child labour and schooling so that microcredit can benefit both current and future generations (Wydick 1999). At the level of microcredit organizations, the gestation period between actual loan disbursement and the start of repayment can be extended. This allows many households to invest in suitable investment projects where they may find a greater balance between employing children at household enterprises and sending them to school. Reduced interest rates and longer repayment periods can also help households to become less myopic. In addition, increases in the size of credit allowing employment of external labour can take the burden off from households to resort to child labour. The latter suggests that microcredit organizations may eventually need to look further and consider financing rural enterprises at the village level rather than at the household level. These measures that are directed at microcredit organizations alone are by no means sufficient in reducing child labour and improving child schooling. They need to be complemented by policies that directly target children's education, of which the essence is to increase the return on education perceived by parents. In sum, microcredit programs need to be complemented by other policies to tackle the multiple goals of poverty reduction, human capital formation, and social development.

Appendix

Control Variables Included in the Regression:

Basic Control:

Child characteristics: Age, age squared, sex*

Household characteristics: landholding (less than one acre, one acre to 2 acres, 2 acres to less than 5 acres, more than 5 acres), mother's education, maximum education attained by any member of the household, age of father, sex of household head.

Village characteristics: Presence of primary school, secondary school.

*Sex is included when combined regression is run.

Full Control: Basic Control plus

Child characteristics: Sex*age, first-born, second-born, third-born, fourth-born, fifth or higher born, number of younger siblings, number of elder sisters.

Household characteristics: Number of children aged 0-6, 7-15, education of father (low (0-4 years), middle (5-10 years), high (11 and above)), age of mother, presence of mother.

Village characteristics: religious school, distance to the nearest school, child wage, adult wage, presence of brick-built road, presence of hospital, post office, grocery market, bus stand, distance to the nearest sub-districts, price of rice.

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Table 1: Descriptive Statistics

Variables	Treatment (I)	Control (II)	Difference III=(I-II)
Child Characteristics (7-16 years old)			
Child in work (in percentage)			
Boys	16.9	13.8	**3.0**
Girls	11.0	9.3	1.7
Child in school (in percentage)			
Boys	69.3	74.7	**-5.5**
Girls	76.6	78.9	-2.3
Age of child (in years)	11.497	11.494	0.003
Sex of child (percentage of girls)	55.7	55.6	0.1
Household Characteristics			
Mother age (in years)	37.66	38.14	-0.48
Mother schooling (years of education)	1.09	1.54	**-0.45**
Father age (in years)	45.85	46.80	**-0.95**
Father schooling (years of education)	2.64	3.20	**-0.56**
Household size	6.56	6.48	-0.08
Number of children			
0-6 years	0.81	0.79	0.02
6-16 years	2.79	2.66	**0.13**
Maximum education by any household member			
Male borrower (years of education)	4.78	5.29	**-0.50**
Female borrower (years of education)	4.17	4.57	**-0.40**
Amount of land (in decimals)	64.7	91.2	**-26.6**
Village Characteristics	Program village (I)	Control village (II)	Difference III=(I-II)
Primary school (%)	86.25	90.91	-4.66
Secondary school (%)	27.27	31.25	-3.98
Union health centre (%)	17.5	10	7.5
Distance to nearest sub-district (km)	7.14	11.91	**-4.77**
Presence of r grocery market (%)	22.5	18.2	4.3
Presence of bus stand (%)	15	9.1	5.9
Presence of post office (%)	20	18.2	1.8
Presence of telephone office (%)	6.3	9.1	-2.8
Presence of UP office (%)	13.8	18.2	-4.4

Notes: The third column presents the difference between columns (1) and (2). Differences that are statistically significant at less than five percent are marked bold.

Table 2: Impact Estimates of the Participation in Microcredit Program on Child Labour

	LPM			Probit		
	No Control	Basic Control	Full Control	No Control	Basic Control	Full Control
	(1)	(2)	(3)	(4)	(5)	(6)
Women and Men's credit						
All	-0.0146	0.0252	0.0801	-0.0144	0.0192	0.0541
	(0.0291)	(0.0290)	(0.0441)+	(0.0287)	(0.0219)	(0.0023)+
	[0.0228]	[0.0276]	[0.0418]+	[0.0225]	[0.0203]	[0.0304]+
Boys	-0.0433	-0.0121	0.0034	-0.043	-0.0136	-0.0106
	(0.0357)	(0.0383)	(0.0583)	(0.0354)	(0.0297)	(0.0418
	[0.0320]	[0.0370]	[0.0526]	[0.0316]	[0.0280]	[0.0383
Girls	0.0161	0.0502	0.1367	0.0158	0.0372	0.0794
	(0.0373)	(0.0394)	(0.0594)**	(0.0363)	(0.0241)	(0.0336)**
	[0.0309]	[0.0395]	[0.0609]**	[0.0301]	[0.0239]	[0.0351]**
Women's Credit						
All	-0.0134	0.0335	0.087	-0.0133	0.0276	0.0558
	(0.0285)	(0.0289)	(0.0456)+	(0.0281)	(0.0213)	(0.0302)+
	[0.0231]	[0.0284]	[0.0426]**	[0.0228]	[0.0203]	[0.0302]+
Boys	-0.042	-0.0029	0.0129	-0.0417	-0.0018	-0.0093
	(0.0353)	(0.0369)	(0.0590)	(0.0349)	(0.0261)	(0.0377)
	[0.0325]	[0.0377]	[0.0861]	[0.0320]	[0.0257]	[0.0258]+
Girls	0.0172	0.0611	0.1426	0.0168	0.0442	0.0835
	(0.0370)	(0.0400)	(0.0610)**	(0.0361)	(0.0241)+	(0.0348)**
	[0.0312]	[0.0409]	[0.0626]**	[0.0304]	[0.0245]+	[0.0363]**
Men's Credit						
All	-0.0187	0.0252	0.0774	-0.0184	0.0199	0.064
	(0.0426)	(0.0503)	(0.0746)	(0.0420)	(0.0323)	(0.0378)+
	[0.0363]	[0.0452]	[0.0681]	[0.0358]	[0.0293]	[0.0373]+
Boys	-0.0671	-0.0285	-0.0239	-0.0664	-0.0233	-0.0112
	(0.0550)	(0.0643)	(0.0912)	(0.0542)	(0.0435)	(0.0456)
	[0.0506]	[0.0603]	[0.0841]	[0.0497]	[0.0405]	[0.0426]
Girls	0.0345	0.0685	0.1507	0.0339	0.0416	0.1008
	(0.0570)	(0.0690)	(0.1094)	(0.0556)	(0.0285)	(0.0421)**
	[0.0497]	[0.0654]	[0.1040]	[0.0483]	[0.0273]	[0.0439]**

Notes: All the results are the marginal effects of instrumented credit variable using IV regressions. The regressions include child, household, village characteristics and district fixed effects (except the first and fourth columns). 'Basic control' is the subset of 'full control' and includes some household and child demographic variables. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986), while those in brackets are corrected for clustering at the household level. The coefficients and the standard errors are multiplied by the average credit borrowed by the respective group of households. All the estimates are also weighted propensity scores. Coefficients with + are significant at the 10%, those with ** at the 5%, and those with * at the 1%.

Table 3: Impact Estimates of the Participation in Microcredit Program on Children's School Enrolment

	LPM			Probit		
	No Control	Basic Control	Full Control	No Control	Basic Control	Full Control
	(1)	(2)	(3)	(4)	(5)	(6)
Women and Men's Credit						
All	-0.0924	-0.0915	-0.1588	-0.0925	-0.0959	-0.1623
	(0.0468)+	(0.0488)+	(0.0688)**	(0.0470)**	(0.0493)+	(0.0273)**
	[0.0327]*	[0.0410]**	[0.0609]*	[0.0330]*	[0.0416]**	[0.0622]*
Boys	-0.0658	-0.0686	-0.0756	-0.0657	-0.0938	-0.0661
	(0.0513)	(0.0569)	(0.0825)	(0.0516)	(0.0613)	(0.1148)
	[0.0432]	[0.0500]	[0.0768]	[0.0434]	[0.0547]+	[0.1049]
Girls	-0.1208	-0.0938	-0.2261	-0.1211	-0.0765	-0.1918
	(0.0596)**	(0.0600)	(0.0905)**	(0.0600)**	(0.0553)	(0.0850)**
	[0.0432]*	[0.0568]+	[0.0837]*	[0.0437]*	[0.0527]	[0.0782]**
Women's Credit						
All	-0.0908	-0.0969	-0.1717	-0.0908	-0.1032	-0.1733
	(0.0452)**	(0.0475)**	(0.0694)**	(0.0454)**	(0.0476)**	(0.0685)**
	[0.0332]*	[0.0424]**	[0.0620]*	[0.0334]*	[0.0427]**	[0.0632]*
Boys	-0.0655	-0.0752	-0.0965	-0.0654	-0.1043	-0.1237
	(0.0504)	(0.0549)	(0.0846)	(0.0507)	(0.0593)+	(0.0863)
	[0.0441]	[0.0515]	[0.0780]	[0.0443]	[0.0561]+	[0.0839]
Girls	-0.1179	-0.1017	-0.2296	-0.1182	-0.0842	-0.194
	(0.0590)**	(0.0603)+	(0.0921)**	(0.0594)**	(0.0549)	(0.0863)**
	[0.0436]*	[0.0583]+	[0.0853]*	[0.0441]*	[0.0538]	[0.0798]**
Men's Credit						
All	-0.0834	-0.0629	-0.1423	-0.0833	-0.0747	-0.1607
	(0.0664)	(0.0814)	(0.1108)	(0.0665)	(0.0790)	(0.1067)
	[0.0515]	[0.0654]	[0.0984]	[0.0516]	[0.0634]	[0.0954]+
Boys	-0.0295	-0.0153	-0.0194	-0.0293	-0.0482	-0.0781
	(0.0807)	(0.0980)	(0.1372)	(0.0803)	(0.1015)	(0.1356)
	[0.0681]	[0.0803]	[0.1223]	[0.0678]	[0.0838]	[0.1240]
Girls	-0.1422	-0.0966	-0.2635	-0.1434	-0.0867	-0.2278
	(0.0873)	(0.1027)	(0.1496)+	(0.0886)	(0.0900)	(0.1217)+
	[0.0683]**	[0.0900]	[0.1422]+	[0.0692]+	[0.0804]	[0.1161]**

Notes: All the results are the marginal effects of instrumented credit variable using IV regressions. The regressions include child, household, village characteristics and district fixed effects (except the first and fourth columns). 'Basic control' is the subset of 'full control' and includes some household and child demographic variables. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986), while those in brackets are corrected for clustering at the household level. The coefficients and the standard errors are multiplied by the average credit borrowed by the respective group of households. All the estimates are also weighted propensity scores. Coefficients with + are significant at the 10%, those with ** at the 5%, and those with * at the 1%.

Table 4: Effects of Control Variables on School Enrolment and Child Labour

	School Enrolment		Child Labour	
	LPM	Probit	LPM	Probit
Control Variables	(1)	(2)	(3)	(4)
Age of child	0.257	0.271	-0.096	-0.005
	(0.023)*	(0.022)*	(0.018)*	(0.014)
Age of child squared	-0.012	-0.013	0.006	0.001
	(0.001)*	(0.001)*	(0.001)*	(0.001)**
Number of younger siblings	-0.015	-0.015	0.023	0.014
	(0.008)+	(0.009)+	(0.006)*	(0.003)*
Number of older sister	0.016	0.018	-0.011	-0.01
	(0.006)*	(0.007)*	(0.004)*	(0.004)*
Sex of household head	0.128	0.119	-0.103	-0.071
	(0.054)**	(0.067)+	(0.032)*	(0.032)**
Whether mother is present in the family	0.039	0.019	-0.051	-0.026
	(0.051)	(0.060)	(0.042)	(0.034)
Highest education of any member	0.038	0.039	-0.02	-0.013
	(0.003)*	(0.004)*	(0.002)*	(0.002)*
Age of household head	-0.004	-0.004	0.004	0.002
	(0.001)*	(0.001)*	(0.001)*	(0.001)*
Education of household head				
(0-4 years of schooling)	0.064	0.033	-0.051	-0.017
	(0.031)**	(0.049)	(0.025)**	(0.030)
(5-9 years of schooling)	0.044	0.004	-0.016	0.008
	(0.024)+	(0.042)	(0.017)	(0.025)
Years of mother's schooling	-0.002	0.004	0	-0.004
	(0.003)	(0.006)	(0.003)	(0.003)
Village Characteristics:				
Presence of primary school	-0.02	-0.024	0.008	0.007
	(0.032)	(0.029)	(0.025)	(0.014)
Presence of secondary school or college	0.052	0.058	-0.011	-0.011
	(0.019)*	(0.018)*	(0.014)	(0.009)
Presence of religious school	0.026	0.022	-0.008	-0.002
	(0.034)	(0.034)	(0.028)	(0.018)
Presence of health facility	0.041	0.04	-0.017	-0.01
	(0.020)**	(0.023)+	(0.018)	(0.013)
Presence of brick-built road	0.066	0.065	-0.048	-0.031
	(0.027)**	(0.027)**	(0.017)*	(0.011)*
Presence of grocery market	-0.082	-0.09	0.033	0.026
	(0.021)*	(0.025)*	(0.012)*	(0.010)*
Presence of bus stand	-0.072	-0.072	0.058	0.043
	(0.031)**	(0.042)+	(0.020)*	(0.022)+
Distance to nearest sub-district (in km)	-0.002	-0.001	0.001	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Adult male wage	-0.001	-0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Rice price	0	0.001	0.006	0.003
	(0.011)	(0.011)	(0.008)	(0.005)
Number of observations	4277	4277	4277	4277
R-squared	0.22		0.23	

Notes: Regressions also include dummies for birth-order, dummies for land-holding, presence of post-office and instrumented credit variable. All the coefficient estimates are the marginal effects. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using the propensity score weighting scheme. Coefficients with + are significant at the 10%, those with ** at the 5%, and those with * at the 1%.

Table 5: Impact Estimates Based on Binary Participation Measure on School Enrolment and Child Labour

	Child is in school		Child is in work	
	LPM	Probit	LPM	Probit
Women's Credit				
Boys	-0.1690	-0.1878	0.0193	-0.0120
	(0.1374)	(0.1463)	(0.0956)	(0.0590)
	[0.1240]	[0.1353]	[0.0879]	[0.0567]
Girls	-0.4782	-0.4438	0.2569	0.1369
	(0.1359)*	(0.1322)*	(0.0946)*	(0.05376)**
	[0.1309]*	[0.1281]*	[0.0944]*	[0.05375]**
Men's Credit				
Boys	-0.1190	-0.1634	0.0097	-0.0325
	(0.1744)	(0.1769)	(0.1175)	(0.0663)
	[0.1636]	[0.1702]	[0.1176]	[0.0679]
Girls	-0.5828	-0.5153	0.3019	0.0842
	(0.1782)*	(0.1521)*	(0.1357)**	(0.04224)**
	[0.1800]*	[0.1571]*	[0.1313]**	[0.03974]**

Notes: All the results are the marginal effects of instrumented binary treatment indicator variable using IV regressions. The regressions include full control using child, household, village characteristics and district fixed effects. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using the propensity score weighting scheme. Coefficients with ** are significant at the 5%, and those with * at the 1%.

Table 6: Impact Estimates Based on Children's Age Group

	Child is in school		Child is in work	
	Age 7-12	Age 12-16	Age 7-12	Age 12-16
Women's credit				
Boys	-0.3274	0.1978	0.0491	-0.1784
	(0.1557)**	(0.2952)	(0.0413)	(0.2156)
Girls	-0.3295	-0.5991	0.0785	0.2818
	(0.1507)**	(0.2224)*	(0.0401)+	(0.1416)**
Men's credit				
Boys	-0.2329	0.0491	0.0051	-0.1673
	(0.1612)	(0.3816)	(0.0312)	(0.2666)
Girls	-0.4131	-0.6178	0.0382	0.1077
	(0.1877)**	(0.7428)	(0.0398)	(0.2457)

Notes: All the results are the probit marginal effects of instrumented binary treatment indicator variable using IV regressions. The regressions include full control using child, household, village characteristics and district fixed effects. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using the propensity score weighting scheme. Coefficients with + are significant at the 10%, those with ** at the 5%, and those with * at the 1%.

Table 7: Impact Estimates Based on Parental Education

Education of Household Head	Child is in school			Child is in work		
	All children	Boys	Girls	All children	Boys	Girls
Probit						
Low	-0.2931	-0.2235	-0.3477	0.0966	0.0220	0.1373
	(0.0705)*	(0.0839)*	(0.0878)*	(0.0317)*	(0.0410)	(0.0346)*
Middle	-0.0333	0.0415	-0.0920	0.0319	-0.0783	0.1019
	(0.0736)	(0.0911)	(0.1009)	(-0.0338)	(-0.0546)	(0.0367)*
High	0.0472	0.0887	0.0322	-0.0080	-0.1087	0.0649
	(0.1115)	(0.1446)	(0.1220)	(0.0470)	(0.0730)	(0.0472)
LPM						
Low	-0.2756	-0.1938	-0.3479	0.1302	0.0471	0.1972
	(0.0670)*	(0.0797)**	(0.0854)*	(0.0424)*	(0.0526)	(0.0524)*
Middle	-0.0452	0.0420	-0.1191	0.0617	-0.0653	0.1574
	(0.0652)	(0.0797)	(0.0894)	(0.0400)	(0.0585)	(0.0510)*
High	0.0046	0.0668	-0.0454	0.0082	-0.0925	0.1011
	(0.0914)	(0.1223)	(0.0936)	(0.0530)	(0.0769)	(0.0635)

Notes: *Low* refers to households where the highest level of education obtained by parents is primary (0-4 years of schooling) or less; *Middle* refers to households where the highest level of education obtained by parents is more than primary but less than a high school degree (5-10 years of schooling), and *High* refers households where one of the parents obtained at least a high school degree (11 or more years of schooling). All the results are the marginal effects of instrumented credit interacted with education dummies using IV regressions. The regressions include full control using child, household, village characteristics and district fixed effects. Standard errors in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using propensity score weighting scheme. Coefficients with ** are significant at the 5%, and those with * at the 1%.

Table 8: Impact Estimates Based on Land Ownership

	Child is in school		Child is in work	
	LPM	Probit	LPM	Probit
Poorer Households				
Boys	0.136	0.115	-0.085	-0.078
	(0.119)	(0.139)	(0.073)	(0.044)+
Girls	-0.231	-0.238	0.098	0.089
	(0.123)+	(0.128)+	(0.098)	(0.070)
Less Poor Households				
Boys	-0.331	-0.328	0.260	0.451
	(0.844)	(0.848)	(0.696)	(0.476)
Girls	0.017	0.073	-0.007	0.019
	(0.824)	(0.591)	(0.568)	(0.057)

Notes: Poorer household are those who own less than a half acre of land and less poor households own more than a half acre of land. The regressions include full control using child, household, village characteristics and district fixed effects. The coefficients and the standard errors are multiplied by the average credit borrowed by the respective group of households. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using propensity score weighting scheme. Coefficients with + are significant at the 10%, those with ** at the 5%, and those with * at the 1%.

Table 9: Impact of the Microcredit Program on Children's School Achievement

	Boys			Girls		
Female Borrower	Grade Completion	Education Gap	Grade-for-age	Grade Completion	Education Gap	Grade-for-age
Treatment effect	-0.196	-0.092	-21.647	-2.953	2.752	-48.393
	(0.617)	(0.611)	(12.912)+	(0.738)*	(0.696)*	(16.089)*
Control function	0.158	0.143	22.286	2.963	-2.755	49.405
	(0.624)	(0.617)	(13.283)+	(0.745)*	(0.705)*	(16.256)*
Male Borrower						
Treatment effect	-0.423	0.043	-23.727	-3.773	3.39	-72.497
	(0.846)	(0.817)	(17.603)	(0.961)*	(0.923)*	(21.299)*
Control function	0.327	0.047	25.084	3.537	-3.199	69.099
	(0.787)	(0.765)	(16.855)	(0.958)*	(0.921)*	(21.373)*

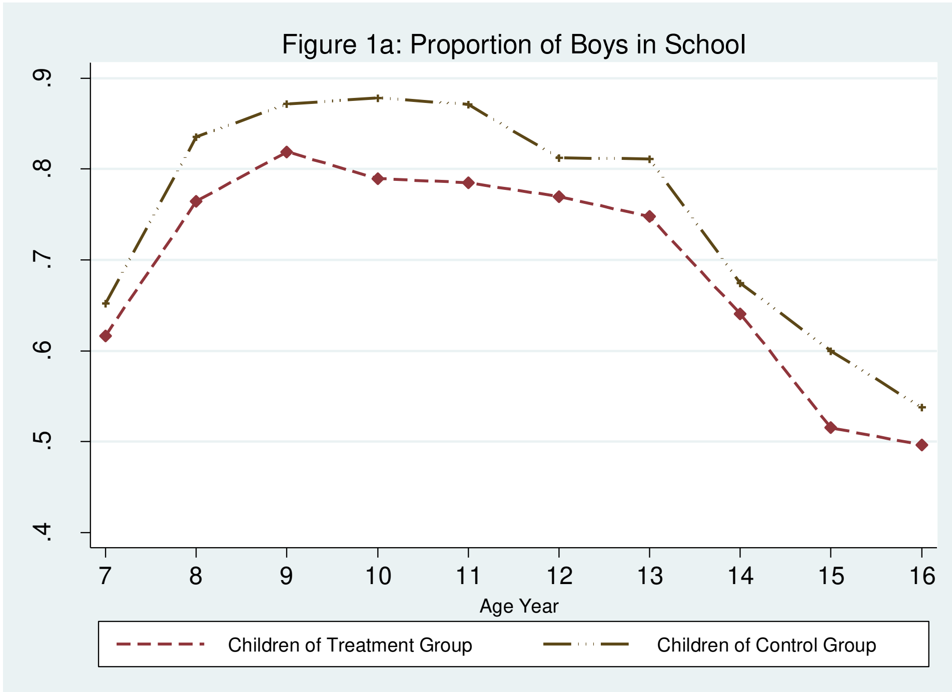
Notes: All the results are estimated using the control function method. The regressions include full control using child, household, village characteristics and district fixed effects. The coefficients and the standard errors of treatment effects are multiplied by the average credit borrowed by male and female borrowers. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using propensity score weighting scheme. Coefficients with + are significant at the 10%, and those with * at the 1%.

Table 10: Multinomial Logit Model for Children's Work/School Status

Child Occupation	Coefficient	Marginal Effect
Self-employment activity	2.03	0.266
	(0.71)*	(0.095)*
Agriculture	-6.48	0.000
	(1.73)*	(0.0000)*
Day labourer	1.33	0.0000
	(1.07)	(0.001)
Service-related activity	2.42	0.0000
	(4.50)	(0.0000)
Enrolled in school		-0.266
		(0.096)*

Notes: The regressions include full control using child, household, village characteristics and district fixed effects. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986). Coefficients with * are significant at the 1%.

Figure 1: Proportion of Children in School



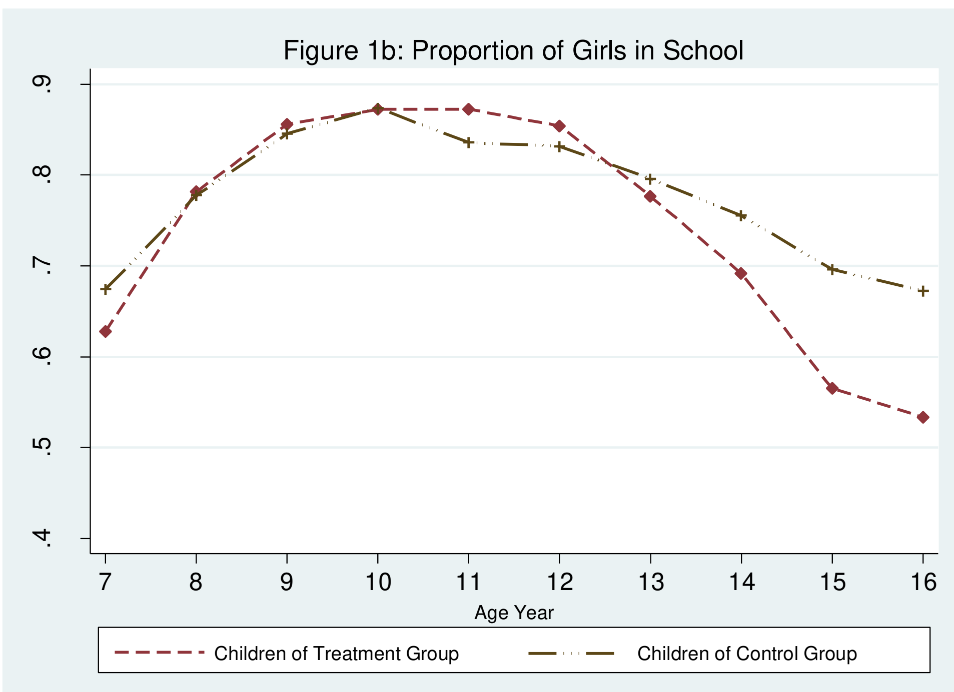
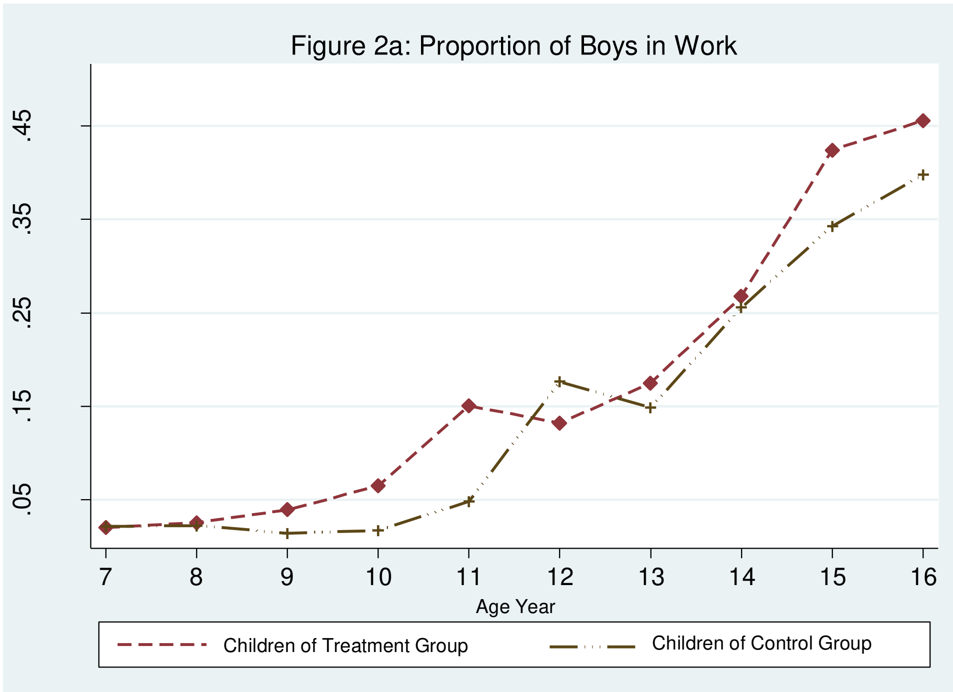


Figure 2: Proportion of Children in Work



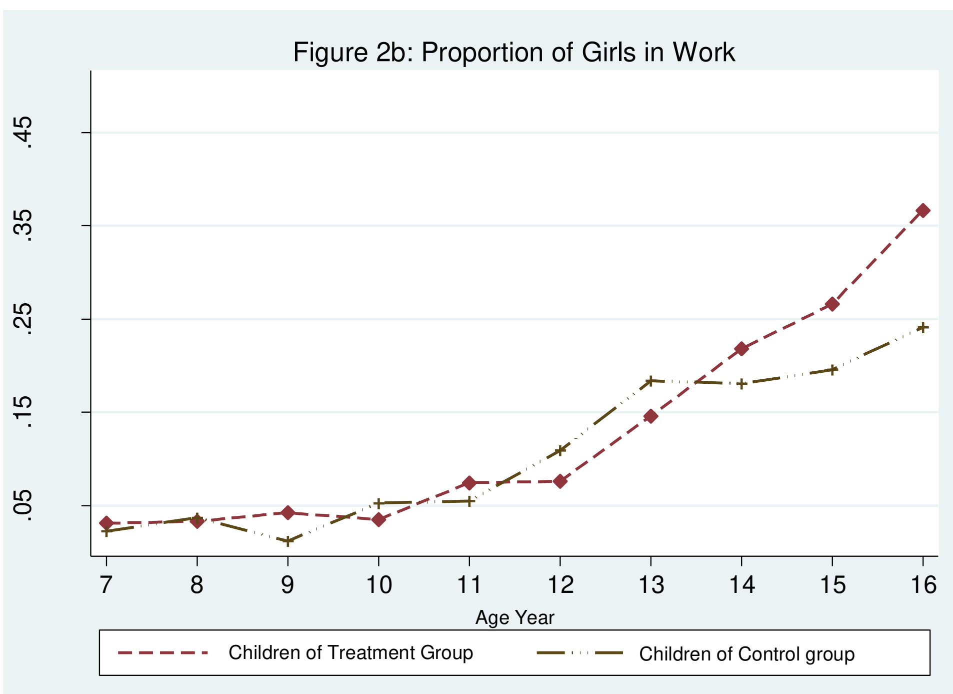
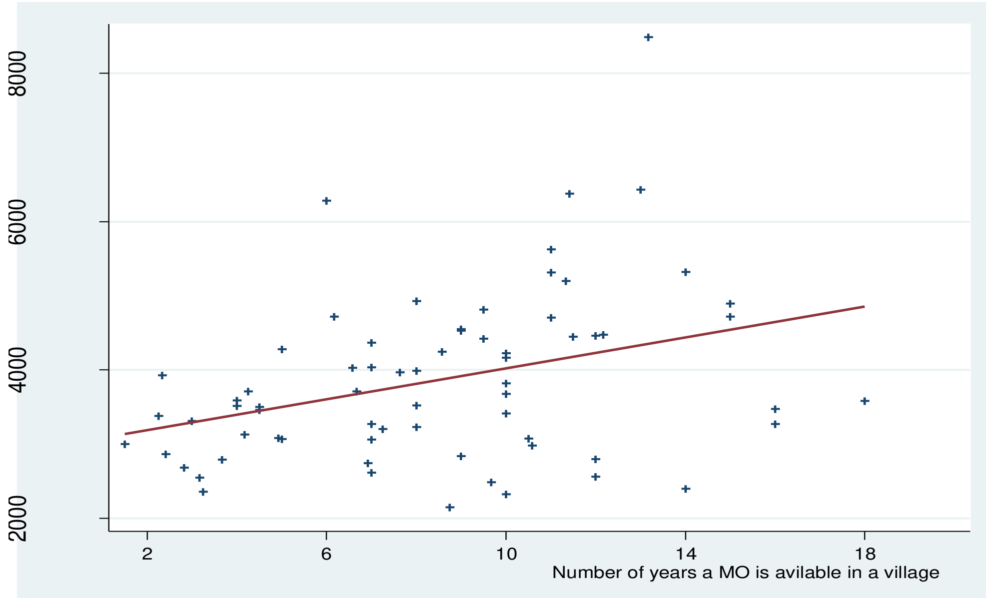
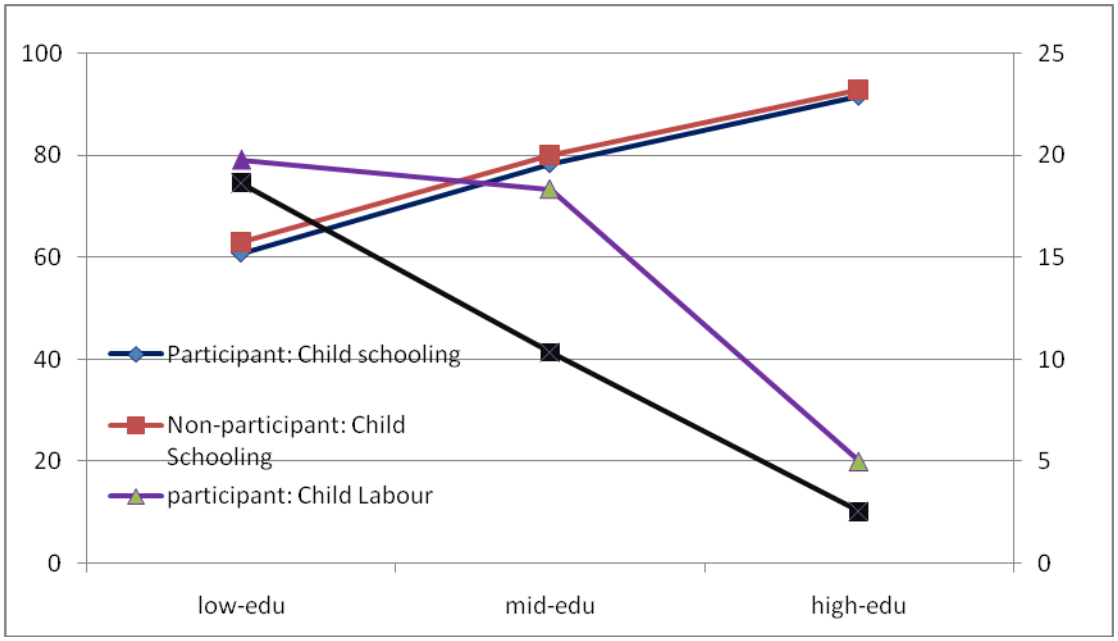


Figure 3: Yeas of Microfinance Program in a Village and the Amount of Credit Borrowed by Households



Notes: Average credit per household in a village is the amount of credit borrowed (in taka) by all households divided by the number of participating households in the program village. Number of years a MO is available in a village is the period from which microcredit has been first avilable in the program village.

Figure 4: School Enrolment and Child Labour at Different Levels of Parental Education



Note: *Low-edu* refers to those households where the highest level of education obtained by parents is primary (0-4 years of schooling) or less; *Mid-edu* refers to households where the highest level of education obtained by parents is more than primary but less than a high school degree (5-10 years of schooling), and *High-edu* includes households where one of the parents obtained at least a high school degree (11 or more years of schooling).