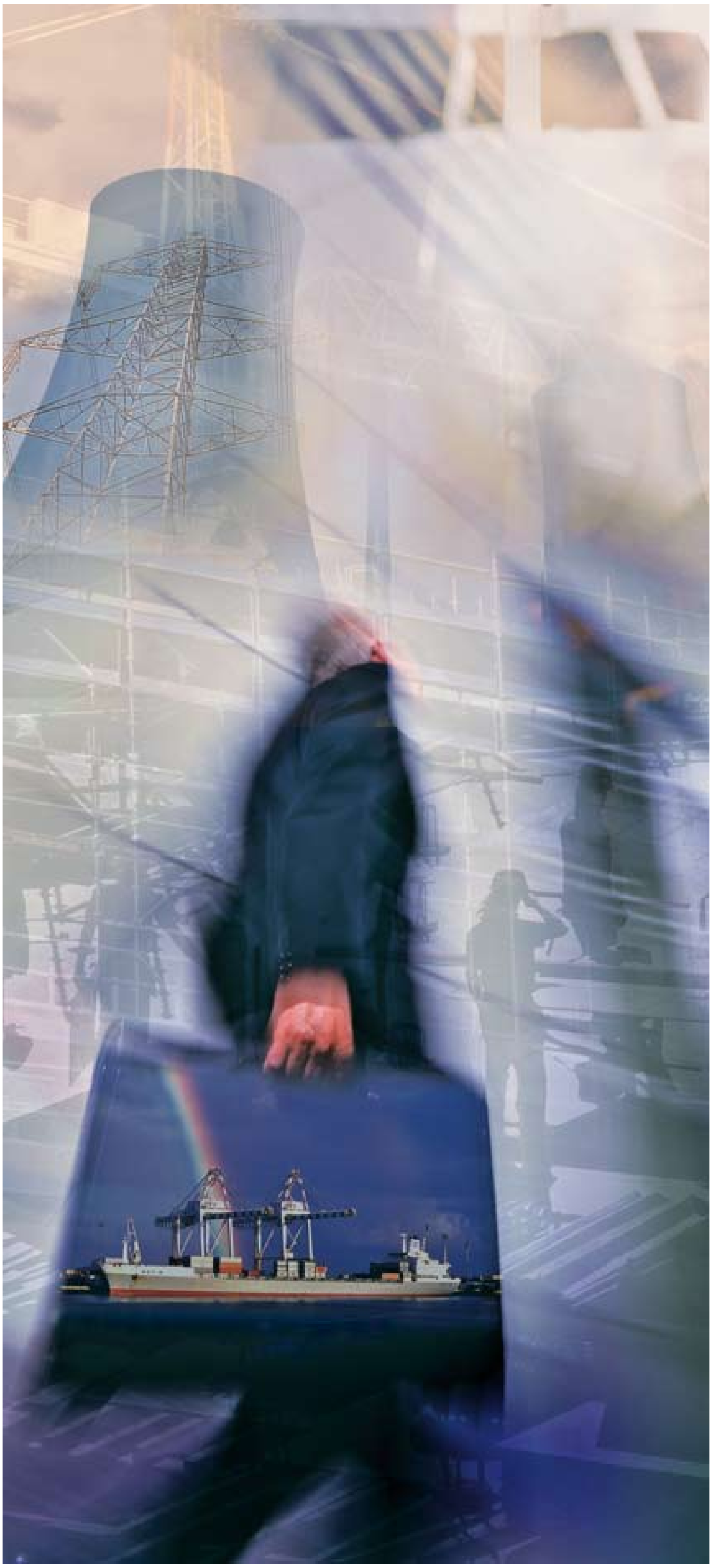

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CHILD LABOUR AND SCHOOLING RESPONSES TO ACCESS TO MICROCREDIT IN RURAL BANGLADESH

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Abstract

Microcredit is found to improve household welfare by increasing income, consumption, and reducing poverty in many developing countries. However, little is known about its effect on human capital formation. This paper evaluates, using a large survey data from Bangladesh, the impact of access to microcredit on school attendance and work of children in rural Bangladesh. It uses a novel source of exogenous variation in treatment intensity among households in different villages to estimate the causal effect of access to microcredit. The empirical results indicate that household's participation in a microcredit program may exacerbate child labour situation and reduce school enrolment. The effects are more pronounced on girls than boys. Younger children, in particular, can be strongly affected. The estimated impacts also vary by income, education and asset holding of households with the children of poorer and less educated households being more adversely impacted by a household's microcredit borrowing.

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

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

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1. Introduction

Low-income households in many developing countries often keep their children out of school to make an immediate contribution to household earnings. These households face borrowing constraints which raise the marginal cost of attending school (Becker 1993), or they send their children to work, even in absence of liquidity constraints, if the return to education is lower (Baland and Robinson 2000). In either case, schooling of children is likely to decrease which can cause a far-reaching 'dynastic trap' problem because a child who acquires less education will grow up to also be poor as an adult, thus likely sending his or her children to work in turn. For most part, imperfections or underdevelopment of credit markets (Ranjan 1999) or a lack of access to credit is considered as one of the major factors responsible for inadequate child schooling in developing countries (see, for example, Jacoby and Skoufias 1997; Ranjan 2001; Dehejia and Gatti 2005; Edmonds 2006).

Microcredit organizations have been offering credit for income generating activities of the rural population otherwise without access to formal credit. It is found to improve household welfare by increasing consumption, income and reducing poverty in many developing countries (see, for example, Pitt and Khandker 1998; Kaboski and Townsend 2005; Islam 2007; Karlan and Zinman 2008). However, there is less evidence of the impact of microcredit on human capital formation, and they far less conclusive than its effectiveness in alleviating poverty. For example, Wydick (1999) finds that the access to microcredit and children's schooling is not unambiguously positive in case of Guatemala. Pitt and Khandker (1998) find mixed results on schooling of children of microfinance household. They find when women borrow from Grameen, schooling of girls increases, but it does not do so when they borrow from the other programs. Hazarika and Sarangi (2008) find that children's propensity to work in rural Malawi increases in household access to microcredit.

This paper offers a conceptual framework of the relationship between access to microcredit and school attendance and child labour. It then empirically examines the impact of household's participation in a microcredit program on school attendance and demand for child labour. As has been widely recognized, one of the reasons why children may be sent to work instead of attending school is the fact that the net return to human capital investment is too low compared to its opportunity cost. This might be explained by large direct and indirect costs of schooling or by the return to schooling being close to zero because of low quality of the education. Another reason might be restrictions due to the presence of credit constraints on the investment in human capital. Credit-constrained poor households face the dire choice of sending their children to work to smooth out consumption (Jacoby and Skoufias 1997) or simply to survive: the reason why children are a current economic source for poor parents in developing countries. However, the availability of credit means that parents do not need to depend on their children's income. Parents' ability to borrow to finance their current investment or to continue income generating activities play an important role in investing children's education. Considerable evidence indicates that poor households in rural area in developing countries are credit constrained. Households are often neglected or denied credit by formal banking sector due to the collateral requirements. But they do not require any physical collateral to get loans from a microcredit organization. However, the nature of investment for microcredit loans and its repayment methods require quick returns from investment. Therefore, additional productive activities made possible by access to microfinance may alter the household preference towards child schooling. Children may directly be employed in the newly created or expanded micro enterprises, or indirectly, in child care or in farm and livestock duties and other household chores.

This paper uses a new, large, nationally representative and unique cross-sectional data set of treatment and control groups of microcredit programs from Bangladesh. Examining the causal effect of microcredit on children's schooling and work is made difficult by the non-random program placement and self-selection of participants into the program. Microcredit programs are available in certain villages and microcredit organizations (MOs) typically select the households who satisfy the eligibility criterion. Households are self-selected into the program. As a result, differences in educational outcome of children between treatment and control groups may reflect the underlying differences in characteristics of the two groups rather than the impact of credit on children's education and work. We address this selection bias using instrument variable method. We use a novel source of exogenous variation in treatment intensity among households in different villages as the identifying instrument. We estimate the treatment effect by combining regression adjustment with weighting based on propensity score (Rosenbaum and Rubin 1983) – an approach suggested by Robins and Rotnitzky (1995). We also seek to provide credible estimates using an alternative econometric methodology: control function approach which does not rely exclusively on the exclusion restriction.

Overall, the results suggest that the microcredit loans adversely affect both child schooling and the demand for child labour. The results overwhelmingly support the fact that girls are more likely to be affected adversely due to microcredit. The effects on boys are ambiguous: there is some evidence that microcredit can help to improve their child labour situation. Younger children are found to be more likely to engage in work and they are also less likely to go to school due to their parent's participation in microcredit program. These results are important from the policy perspective because policy to promote gender equality in education in developing countries may turn out to be less effective due to microcredit - contrary to what policy makers believe. We estimate the heterogeneous treatment effects by allowing the treatment effects to vary by gender of participants and by income and asset ownership of households. The results show that income or education of household matters in receiving the benefits, or to minimize the adverse consequences of microcredit programs. The results suggest that children of poorer households are more likely to be caught in a vicious poverty cycle. The effects do not differ much between men and women credit borrower. However, there is some evidence that women's participation can minimize the girl's probability to work. On the other hand, men's participation has the opposite effect – boys are more likely to go to school and work less. The empirical findings hold across different specifications and methods, and when corrected for various sources of selection bias.

2. Research Context

2.1 Access to Microcredit, and Schooling and Child Work

Theoretically, it is possible for microfinance, which requires investment in household enterprises, to have both positive and negative effects on children's schooling and work. Access to microcredit might raise demand for child labour to fulfill the household's basic income-generating requirements, take care of younger siblings, thus facilitating the labour efforts of more productive household members. If, for example, a household purchases livestock using loans, then it requires labour to take care of it, and increased amount of raw materials to manufacture craft boxes. Such type of investment can augment the demand for labour, which in turn can increase the demand for child labour. Moreover, since microcredit does not offer enough flexibility to hire labour because of the size of the loan, families are often forced to use child labour as there is a greater degree of substitutability between adult labour and child labour at the household level.

The literature has also identified few channels through which access to credit, in general, (but not necessarily microcredit) may affect human capital formation. First, if credit has any effect on the income level of the borrower, it may influence the demand for schooling through this (Behrman and Knowles 1999). Second, the vulnerability of rural households to adverse exogenous shocks forces them to engage in risk-coping strategies that may require pooling their children out of school and do not allow sustained enrollment overtime. In order to cope with such higher risk, poor households frequently adopt diversified production plans and employment and migration strategies, even if these actions entail lower average incomes (Morduch 1995). In addition, households smooth income by using financial savings, selling assets, taking children out of school, and developing informal insurance and credit arrangements (Jacoby and Skoufias 1997).

Access to loans from MOs might reduce the probability of children being withdrawn from school in response to adverse shocks as it can aid consumption smoothing. Evidence also indicates that microcredit can enhance income and increase consumption expenditure (Pitt and Khandker 1998, Islam 2007). Several studies have also demonstrated that women show a stronger preference for educating their children (Pitt and Khandker 1998; Behrman and Rosenzweig 2002). Since women are mostly the borrowers from MO, it might change their power to influence household schooling decisions. These preferences about schooling may be influenced by the mandatory adult training programs conducted by MOs. Though MOs, in general, do not have any direct professed objective of increasing school attendance (except some NGOs who offer direct schooling) they educate members about the potential benefits of sending children to school.[1] Microfinance could conceivably then have a positive effect on schooling through increases in income and knowledge of the borrower.

However, loans from MOs also require establishing a family enterprise, which enhances the demand for labour. This makes the microcredit loans fundamentally different from the traditional sources of loans. Typically the size of such loan does not allow household to hire labour. So, parents face decision whether to send their children to school or to work at the 'micro enterprise' as they require repaying the microcredit loan in a very short period of time after the loan is sanctioned.[2] Thus, small gestation gap between receiving loan and initial installment for repayment is likely to enhance employment of children in the microenterprise. This raises the marginal cost of attending school, and parents face a dire choice as the financial returns to schooling are also very low. Moreover, poorer households have a higher marginal utility from current consumption and discount the future very heavily. So, in case a parent's choice between child schooling and work, the expected return from investing in self-employment activities relative to returns from schooling are important.

There is a growing literature on the influence of the demand for child labour on schooling outcomes (see Edmonds 2007 for an excellent survey). However, there are relatively few studies, except those mentioned above, of households' participation in microcredit program and the impact on their children's schooling and work. So, this paper mainly links two branches of the literature. One

[1] For example, Grameen Bank members need to memorize sixteen decisions, one of which is –'we shall educate our children'.
[2] Most of the MOs require that households start repaying the credit upon instalment after four weeks of getting credit.

branch deals with effects of credit constraints on schooling, and the other analyzes aspects of child labour due to the presence of family farm in developing country.

The empirical studies in developing countries tend to conclude that access to credit improves child labour situation and increases schooling.[3] For example, Jacoby (1994) finds that unequal access to credit is indeed an important source of inequality in schooling investment in Peru. Jafarey and Lahiri (2002) show that access to credit is likely to reduce child labour and increase schooling. Guarcello, Mealli and Rosati (2003) find that credit rationing is associated with higher child labour in Guatemala. Dehejia and Gatti (2005) use cross-country data, and find negative associations between child labour and access to credit. Jacoby and Skoufias (1997) find that the incidence of child labour worsens with the lack of access to credit. Beglee, Dehejia and Gatti (2005) find that access to credit offsets the effect of income shocks on child labour in Tanzania. Edmonds (2006) finds inability of households to borrow against future income forces household to send children to work and reduce the school attendance in South Africa. On the other hand, the child labour problem may worsen even with increase in incomes if they are associated with expanding economic activity or increased employment opportunities for children within their households (Edmonds 2007). For example, Mueller (1984) documents a positive correlation between household productive capital and child labour in rural Botswana. Wydick (1999) finds that the probability of child working at household enterprise is higher when household borrows funds to purchase capital equipment instead of strictly for working capital purposes. Maldonado (2005) finds that households that cultivate land and operate labour-intensive microenterprises demand more child labour. Bhalotra and Heady (2003) show that farm size has a positive effect on children's hours of work and a negative effect school attendance, particularly girls. Menon (forthcoming) finds that credit obtained for investment purposes is likely to reduce the likelihood of schooling of children in rural Pakistan. Ravallion and Woodon (2000) find that the government "food for education" program in Bangladesh increased schooling but without much decline in the incidence of child labour, despite an increase in current income in the form of food subsidy.

2.2 Conceptual Framework

We consider here a generic Becker (1991) type household decision model. Assume that households derive direct utility from schooling of children independent of its financial return. Assume that

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

parents take decisions regarding their children and that they have a utility function defined over a set of commodities of the following form:

$$U = U(x, z, l_c, l_a, s) \tag{1}$$

where x is the household's consumption of market goods; z is the home-produced consumption good produced by the household enterprise; l_c and l_a are the leisure for children and adults, respectively; s is the child's school attendance. The utility function $U : \Re^5_+ \to \Re$ is assumed to be continuous, (weakly) monotonic and strictly quasi-concave. z can be produced at the household enterprise using both adult labour, L_a and child labour, L_c:

$$z = z(K, L_a, L_c) \tag{2}$$

where K is the capital. Assume that the production function $z : \Re^3_+ \to \Re_+$ is strictly monotonic, strictly concave, bounded from above and all the Inada conditions apply. The household enterprise is faced with the following borrowing constraint-

$$wL_h + rK \leq \Omega \tag{3}$$

where w is the wage rate (exogenously given), L_h is the hired labour (in practice L_h=0 in household enterprise, which is substituted by own labour); r is the rental rate of capital; Ω is the amount of working capital (Ω is the maximum amount of money a household can borrow from microfinance institution, Ω=0 for those who cannot borrow).[4]

The child's total time available T_c can be devoted to schooling, s, leisure, l_c, or work L_c: $T_c = s + l_c + L_c$. Similarly the adult's total available time, T_a, can be devoted to leisure, l_a, or wage labour, L_a: $T_a = l_a + L_a$. Assume all children are altruistic in the sense that they return all the payments to the parents (as is the social norm in most developing countries). Assume that child labour and adult labour are perfect substitutes.[5] If household earns income from two sources: the production at household enterprise and household's earnings from activities outside home. The household's budget constraint is given by the following equation-

$$Y + P_z z(K, L_a, L_c) = P_x x + (b - c)s + wL_h + rK \tag{4}$$

[4] The model makes the simplifying assumption that labour markets are perfectly competitive. This implies that any household member seeking work can find one at the going wage rate.
[5] Our results do not change if we consider child labour as a fraction of adult labour.

where Y is the exogenously given income of household which is earned through market activities, P_x and P_z are the prices of x and z, respectively, c is the cost of attendance to school, b is the discounted present value of the returns to schooling so that $(b-c)=\beta$ measures the discounted net benefit of schooling.

Substituting the borrowing constraint (it is binding at the optimal), the budget constraint can be re-written as-

$$Y + P_z z(K, L_a, L_c) = P_x x + \beta s + \Omega \tag{5}$$

Here we deliberately choose not to embellish the model with side relations other than (4a), incorporating, for example, fertility or child survival prospects or child quality and quantity trade-off, on the decision of child school attendance.

The first-order conditions yield the following system of demand equations (in reduced form):

$$g = g(P_x, P_z, w, \beta, Y, \Omega) \qquad \text{where } g = x, s, z, l_c, l_f, l_m. \tag{6}$$

Comparative static results can be obtained by differentiating (6) with respect to Ω and we can get an expression such as $\delta s/\delta\Omega$. So, we can obtain the relationship between microcredit and schooling. Essentially, the relationship involves substitution effect and income effect and the sign of $\delta s/\delta\Omega$ would depend on the relative magnitude and sign of both effects.

We can differentiate the time budget constraints and get -

$$\frac{\partial s}{\partial \Omega} = \frac{\partial s}{\partial l_a}\frac{\partial l_a}{\partial \Omega} + \frac{\partial s}{\partial l_c}\frac{\partial l_c}{\partial \Omega} + \frac{\partial s}{\partial L_a}\frac{\partial L_a}{\partial \Omega} + \frac{\partial s}{\partial L_c}\frac{\partial L_c}{\partial \Omega} \tag{7}$$

Clearly the sign of the all terms are not determined *a priori*. The effect of capital on child's schooling will depend on the relative magnitude and sign of the different terms of equation (7). Several characterizations here can be made concerning children. An increase in father's wage raises the implicit price of his leisure and will lead to substitution toward the child's education if child's education and the father's leisure are substitutes. If child quality is normal good, an increase in income through an increase in wage income is expected to contribute positively to children's education. To the extent that child and adult work are substitutes, child leisure and education may decline when mother involves more time to the production of consumption good. An increase in the child's wage or that of demand for child labour may work through different channels to alter the amount of education. The impact of an increase in child's wage also depends on whether leisure and education are complements or substitutes. If leisure and education are complements then the rise in the cost of leisure will induce a decline in the demand for education. However, if they are substitutes, a rise in the wage or production of consumption good will raise the demand for education. Therefore in order to determine the net effect we have to weigh the income and substitution effects.

The household demand framework indicates that the effects on schooling due to participation in microfinance are ambiguous. All depend on the different magnitude and signs of the terms associated with the Slutsky decomposition. However, one can empirically test incorporating household, individual, community and geographic characteristics in a regression framework as suggested by Becker (1993) that such preferences can be determined by a family's demographics, income and the like.

3. The Program, the Data and the Descriptives

3.1 Background: Schooling/child work in Bangladesh

Bangladesh has achieved rapid progress in child schooling in recent years. The gross primary enrolment rate, which was 72% by 1990, increased to 96% by 2000**.** The Bangladesh Household Income and Expenditure Survey (HIES) 2000 indicates a net primary enrolment rate of only 65.4% in 2000. At the same time, the primary school completion rate was 66.3%. The HIES 2000 shows that nearly 4.7 million children aged 6-10 years (out of a total population of 18.8 million in that age group) do not attend school in Bangladesh. According to the Bangladesh child labour survey 2002-03, the number of children aged 5-17 working in rural areas are estimated at 6.4 million as against 1.5 million in urban areas. Most of the child workers work in the agriculture related work. Nearly 50 percent of primary school students drop-out before they complete grade five. Salmon (2005) finds that among the poorest quintile of households, the share of family income contributed by child workers reach nearly 50 percent. Child labourers between ages 5-14 constitute about 12 percent of the country's labour force (Rahman et al. 1999). Among child workers 73.5 percent are boys and 26.5 percent are girls. 93.3 percent of working children in the age group 5-17 operate in the informal sector.

Despite the problem of child labour, there has been great deal of progress in increasing equitable access, reduction of dropout rates and implementation of a number of 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 for all children to attend primary school have been introduced through distribution of textbooks and provision of "food for education"—the latter has been converted to a cash stipend since 2002. Primary education in rural areas consists of government school and madrasas (Islamic school) and NGO-run non-formal primary schools. There are also now female secondary school stipend programs available for all rural areas in Bangladesh.

3.2 The Program and the Data

The data was collected by the Bangladesh Institute of Development Studies (BIDS) on behalf of Palli Karma-Sahayak Foundation (PKSF) (Rural Employment Support Foundation) with support from the World Bank.[6] It covers 13 MOs of different sizes in terms of operations and the number of members. These MOs were selected such that they represent a nationally representative data set for the entire microcredit program in Bangladesh. Of the MOs we study in this paper, most notable are ASA and Proshikha. ASA and Proshika are the third and fourth largest MOs, respectively in Bangladesh. This survey is the largest and the most comprehensive of the existing microfinance programs in Bangladesh. The geographic coverage of the survey was spread evenly over Bangladesh, and the sub-district level comparisons reveal that selected sub-district were not different from the average (Zohir et al. 2001). All 13 MOs follow the Grameen Bank-style lending procedure and typically give access to microcredit to households having less than half-acre of land.

The survey was primarily designed to evaluate the impact of overall microfinance programs in Bangladesh. The information available from the survey is sufficiently extensive to allow us to see the impact on children's schooling and work. The survey collected detailed information in three areas: (1) household socio-economic data; (2) village level data; and (3) the amount of loan and the reasons for participation/non-participation in the credit program and the use of the loans. This information was collected for both treatment and control households from both program and control villages. 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 first before the survey was administered in early 1998. The actual targeting of survey households involves two stages: (1) the selection of the villages where microfinance organization operates; (2) the selection of treated households within the selected villages. The non-participants from the program villages were also selected as control group since there were only few (eleven) control villages available. Participation in a credit program was defined in terms of current membership– as reported during the census. From

[6] PKSF is the apex organization for microfinance and the microlending community regards it as a regulatory agency and it exercise it authority over the MOs.

the village census list of households 34 were drawn from each program and non-program village. Because the census found a large number of ineligible households in program village the sample was drawn accordingly so as 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.[7]

3.3 Descriptive Statistics

The original survey consists of 3026 households. But, in this paper, we consider households who have at least one child of age 7-16 during the time of survey. This represents a total of 4277 children in 2034 households from both treatment and control groups; 2658 children are from treatment households. There are both men and women borrowers in our sample, with former accounts only 12% of all borrowers. 281 children belong to 133 treated households with men participants. Among all children, 54.2 percent are boys. The household level questionnaire includes primary and secondary activity of each child. We define "child worker" as anyone of age 7-16 who performs any economic activity (i.e., if a parent's answer was '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 one month. By this definition 77.4 percentage of girls aged 7-16 in the sample were classified as being in the school and 10.4 percent in the work, while 71.3 percent of boys of the same age are in school and 15.7 percent are in work. Other children are reported to be neither working nor in school, and possibly many of them are helping parents in household work. So there may well be under-reporting of child work. The results by participation status are reported in Table 1. School enrolment is lower and child work is higher among children of treatment group. We find statistically significant difference in school enrolment and child work between boys of treated and untreated households, but no such difference exists for girls. School enrolment of both boys and girls is lower in treatment group. However, girl-boy difference in school enrolment is larger in treatment group implying that girl's school attendance is relatively higher in treatment sample.

We plot the school enrolment of children of both groups by age in Figures 1a and 1b. The figures show that high-school age children (12-16 years old) are less likely to be enrolled in school. Among the primary school age children, proportion of children of age 7 and 8 enrolled are lower, however, increasing as age increases. The figures indicate that there are a considerable proportion of children who start schooling at a later age. They also show that the gap between school enrolment of treatment and control group is higher for boys. Girls of younger age have the similar rate of

[7] The sample size and its ratio between participating and non-participating households are different in few villages because of the absence of required number of appropriate households in each group.

enrolment, but after age 13 they tend to diverge between treatment and control groups: treatment group has lower participation rate in school. On average, primary-school-age children have higher school participation rate while their older siblings are less likely to participate in school and more likely to go to work (see Figure 2a-2b). Overall, there are a higher proportion of children in work from treated households.

Table 1 also provides descriptive statistics for child and household demographics and village characteristics. It shows that the average age of children is 11.5 years for both treated and non-treated household. There is no difference in terms of gender composition of children in the treatment and control groups. About a quarter of mothers did not go to school at all. The treated group has slightly more members in the household than non-treated group. There is an average of (below 18 years) four children per household in the survey. Non-treated households tend to be better educated, a little older but fewer members. More than a quarter of our sample has secondary school in their locality and in most of the villages there are primary schools. Presence of both primary and secondary schools are slightly lower in program village compared to that of control village. However, program villages have higher health facilities and relatively closer to the nearest sub-districts. The differences between the program and control villages in terms of presence of bus stand, post office, telephone office and local government office are not statistically significant.

In the next section, we outline the empirical strategy. Before that we note the following. We have argued that the effect of microfinance on child labour or schooling is ambiguous. It can also be deduced that, in some cases, we may have an interior solution where children both work and go to school due to microcredit. The survey includes information on a child's primary or secondary activity. We find only 1 percent of the children are both in school and work. It is likely that this understates the extent of child work, especially helping parents at the household work despite attending school.[8] Because of the shortcomings of data, we do not estimate the results where children both work and go to school. Our data also do not allow us to test the effect on number of hours work. It is very difficult to obtain the hours of worked data as most of the children in rural area of Bangladesh work as an informal basis. We also do not focus on hours spent going to school as there is rarely part-time schooling system in rural area of Bangladesh.

[8] It is usual in rural area of Bangladesh that parents engage a modest amount of part-time work for their children while still keeping them at school (see, for example, Ravallion and Woodon 2000).

4. Empirical Methodology

In modelling school attendance or child work, we follow reasonably standard practices in the literature (see Wydick 1999; Ravallion and Wodon 2000; Edmonds 2006). Let S_i is a binary variable that denotes whether child *i* (i) works or not; and (ii) attends school or not. We posit the following relationship to estimate the impact microcredit program participation on children's education/work:

$$S_{ijkl} = \beta_{0l} + \beta_1 X_{ij} + \beta_2 Z_k + \beta_3 credit_{jk} + \varepsilon_{ijkl} \tag{8}$$

where the subscripts index child (*i*), households (*j*), village (*k*), district (*l*) . *X* is a vector of child and household specific covariates, and *Z* is a vector of village specific covariates. B_{0l} is fixed-effects. 'Credit' is the amount of microcredit borrowed by the household, is capturing the program participation. The coefficient β_3 gives us the estimate of the treatment effect on the treated. The error term ε_{ijkl} is assumed to be i.i.d. We can then estimate employment or school attendance probabilities due to credit program participation using equation (8) with the probit model.[9]

However, estimating equation (8) with a *simple* probit model is problematic. First, as mentioned above, there is non-random program placement —programs are placed in certain villages. The choices of program placement could be due to, say, if high-income villages are more likely to secure participation in the program or if officials selecting villages for participation in the program bias the selection in favour of poorer villages. However, given that the program placement is done centrally and there is hundreds of MOs working in Bangladesh 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 (included in the vector *Z*). We also use district level fixed effects to remove any unobserved heterogeneity across different geographic areas. Since we have 13 MOs, each from a different district, this fixed effect also captures the differences across the microfinance institution. So, we tackle the non-random program placement using both fixed effects and village level observed covariates.[10]

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[9] It is possible to use a bivariate probit model to analyse the decision of child work and schooling simultaneously. However, the number of children who both attend school and work and who do neither are very small in our data. Thus the work versus schooling decision is nearly a dichotomous decision, and so simple probit model is suitable in our context. We model school attendance and child work separately since in many settings a sizeable group of children are neither in school nor reported to be working. Our intention here is to keep the modelling process as simple as possible, and we do not complicate our estimation strategy by employing multinomial logit or probit model. Even if we model idleness of children as one of the utility maximizing decision and apply multinomial choice model, our conclusions do not alter.

[10] Probit estimates with fixed effects give rise to inconsistent coefficients of the fixed effects. However, when the number of observations per fixed effects 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

Second, households are self-selected into the program, and not all the households willing to join in a program can obtain microcredit. It is generally the poorer households (defined typically by the amount of land-holding) who have access to microcredit in their own villages. There are also factors that influence whether a household has access to microcredit also influencing outcomes for children of that household. One such factor could be income or wealth of the household. For example, MOs may be more willing to provide credit to households that operate non-farm enterprises because the use to which credit is put less fungible in such households. The microcredit loans often require establishing family enterprise, petty business. But 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 would result in a conservative bias in the coefficients. Hence we need to consider the endogeneity of microcredit program participation at the household level. In terms of equation (8) the enodogeneity problem implies that selection into treatment is on the basis of unobserved characteristics ε_{ijkl}. This implies that there is potential non-zero correlation between ε_{ijkl} and $Credit_{jk}$. That is, $Credit_{jk}$ may be potentially endogenous. So impact estimates using simple probit/linear probability model (LPM) may not reflect the program's causal effect on children's schooling or work.[11]

To estimate the impact of credit on child labour and school attendance, we need a source of exogenous variation. The MOs we study here set the eligibility criterion for participating in microfinance. A household is said to be eligible if it owns less than or equal to half-acre of land. The land ownership criterion has been used by most of the MO in Bangladesh as a targeting mechanism. If, however, program eligibility criterion was followed strictly, then we could compare outcomes of children of households clustered just below the cut-off line to those just above, which is equivalent to regression discontinuity estimates. In practice there are many non-eligible households who have also participated in the program. However, it appears that eligible households are more likely to participate in a microcredit program (70 percent of the treatment group in our sample is eligible). Eligibility status is set by the MO and therefore exogenous to the households. It is also clear that the

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fixed effects which can eliminate unobserved time invariant household level variables or permanent heterogeneity. Instead we consider clustering at the household level (see below).

[11] In terms of treatment effect literature, we are interested in estimating the treatment on the treated (TT) which is defined as: TT= $E[S_{i1}- S_{i0}|D_i =1, X_i]=E [S_{i1}| D_i =1, X_i]- E[S_{i0}| D_i =1, X_i]$ where $D_i =1$ if a children is from a participating household and $D_i =0$ otherwise, S_{i0} and S_{i1} are the potential outcomes for states $D_i =0$ and $D_i =1$, respectively, and X_i represents for each child i a set of attributes (such as age or gender) that are unaffected by the treatment. The last term in the above expression is the counterfactual of interest, which is not observable in the data. What we observe is the average outcome in the untreated state $E[S_{i0}| D_i =0, X_i]$. In general we should expect that $E[S_{i0}| D_i =1, X_i] \neq E[S_{i0}| D_i =0, X_i]$ because of selection bias.

likelihood of any household receiving microcredit is enhanced when a microcredit program is available in a village.[12]

We can therefore use the following instrument for participation: eligibility status interacted with an indicator for presence of program in a village.[13] However, instead of using this instrument directly, we also utilize an unexploited exogenous source of variation in the amount of credit borrowed based on household's exposure to the program in different villages. It appears that treated households in different villages borrow varying amount of credit. Intensity of treatment varies widely in different villages, and we focus on explaining differences in treatment intensity across villages. We consider the original introduction of program across villages in different districts — the earliest programs were available in a village was in 1980 and latest program available in another village was in 1997. We therefore exploit the timing of the microcredit program placement in different villages that have largely contributed to the variation in credit demand by treated households. Figure 3 shows that significant differences in the amount of loans from microcredit organization exist across households of different villages. At the household level, the amount of total credit a household borrows largely depends on how long the program is available in her/his village. So we use the following instrument:

I=M× E × number of years microcredit is available in a village.

where M_k is a binary variable equal to 1 if a village has a microcredit program. Similarly, E_j if a household is eligible (i.e., owns ≤1/2 acre land). With controls for village and fixed effects, identification requires that there are no contemporaneous village level unobservables that are correlated with microfinance program placement in a village and family's child labour/schooling. So, we write an equation of the credit demand of 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 N_k is the number of years microfinance program in village k.[14] X_j now includes only household specific covariates since participation in microcredit program is made at the household level.

[12] Microcredit is not offered to a household belong to a non-program village.

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

[14] In our empirical estimation we also experimented with instruments that include separate dummies for year of microfinance placement in villages. The results turn out to be similar.

So the probit estimates are obtained using the two-stage procedure where fitted value of the credit from the first stage credit demand equation (9) (using Tobit) is included in the second stage regression. The use of an estimated variable (instrumented credit) in a non-linear specification may potentially lead to bias, but as discussed in Train et al. (1987) this bias is of second order and thus very small. 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 estimators employed below (see 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 sample. With normalization, we attach each treated household a weight of 1, and each comparison group member a weight of $p/(1-p)$, where p is the estimated propensity score.[15]

The coefficient of interest is β_3 in equation (8) which measures the difference in the probability of school enrolment or work between children in the treatment and comparison groups. Equation (8) is estimated using three sets of control variables: "no controls" (excluding the X and Z variables), "basic controls" (which adds some household and child demographic variables, and village controls); and "full controls" (which includes full set X and Z variables). The list of the full controls is chosen from a set of larger controls, and we chose those which were most often significant. In deciding the set of chosen control variables, we first consider the variables (e.g., household and village characteristics) that the MO considers to select a household, and that are thought to be determinants for household's willingness to participate in a microcredit program and credit demand. We then include a number of regressors to take into account siblings, family composition that can potentially determine the children's schooling or work status. Estimating the impacts using different set of controls allows us to see the influence of demographic and other socio-economic variables in determining credit demand as well as child labour and schooling. The covariates include in X and Z is listed in the Appendix.

A potential problem with the interpretation of the results above using credit as an endogenous regressor is that, reported amount of credit can partly attributable to misreporting or other types of measurement error since households may forget or not report correctly the amount. It is possible that households sometimes cannot calculate/remember the microcredit borrowed in the past, then measurement error in the credit variable is also a possibility. This measurement error is likely to

[15] The estimated difference in covariate after adjusting propensity score is lower than the unadjusted difference between treatment and control groups. It is, however, important to point out that our qualitative conclusion remains unchanged with or without weighting.

impart attenuation bias to the credit impact coefficients and so biasing the impact estimate. The estimated coefficient of the effect of credit on school enrolment or child labour could therefore be at least partly attributable to changes in data quality. The IV strategy can also take care of the measurement error problem. However, we also use a binary treatment indicator: whether household is currently a member of microfinance program or not. This binary variable is unlikely to be measured or reported with error. It can also serve as a robustness check of our earlier estimates. However, when we use binary treatment indicator it raises another concern as dummy endogenous variables with limited dependent variables raise some special econometric problems. Angrist (2001) advocates using simple IV estimators as an alternative, on the grounds that these estimators invoke weaker assumptions and often suffice to answer questions of interest in empirical studies. We therefore also estimate the treatment effect using a LPM in the second stage of an IV regression.[16]

To adjust for clustering at the village level we first use cluster-correlated Huber-white covariance matrix estimator. Donald and Lang (2007) have pointed out that asymptotic justification of this estimator assumes a large number of aggregate units. Monte Carlo simulations (see Bertrand, Duflo and Mullainathan 2004; Hansen 2007) suggest that when the number of PSUs is less than 50 this estimator performs poorly, leading to over rejection of null hypothesis of no effect. Fortunately, with the number of PSUs in our sample being 91, we can potentially overcome the problem 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).[17] Secondly, children of the same household are likely to be similar on a wide variety of characteristics. The data were collected using the household as the survey unit. So, there are likely to be large intra-household correlations. Therefore, we also estimate standard errors clustering at the household level as there is usually more than one school age child within a household.

Below we estimate separate impacts of credit given to women and men on a child's schooling and work situation since credit is given to both women and men group in different villages (credit groups are never mixed by gender). If women command more resources at the household, it is likely

[16] When all independent variables are discrete variables (most of our variables are discrete) LPM is completely general and fitted probabilities lie within the interval. In addition to being fairly general in our context, the LPM has also the advantages of allowing straightforward interpretation of the regression coefficients. Moreover, we also compute Huber-White standard error to take into account of the heteroscedastic error term of LPM.

[17] There are a variety of alternative to cluster-adjusted standard error such as hierarchical linear modelling, two-step procedure by Donald and Lang (2007) and Bell and McCaffrey's (2002) biased reduced linearization estimator for micro data.

to enhance overall schooling of their children. For example, Pitt, Khandker and Cartwright (2005) find that women's participation in micro credit programs helps to increase women's empowerment. Microcredit can also increase the relative chance of girls' schooling if women are empowered through their command over the resources at the household. On the other hand, parents may have a differential preference for their daughters and sons' education, market rate of education could be different for boys and girls, and education of boys and girls could be determined by the production function (Rosenzweig and Schultz 1982). So we estimate the results separately for boys and girls by credit given to women and men group.

5. Empirical Results

We consider the plausibility of the instrument before presenting the impact estimates. We need to confirm first that the instrument, I (interactions among eligibility indicator, program village indicator and length of program in a village), is a significant predictor of the amount of credit borrowed. We need a strong first-stage to ensure that we are not using a weak instrument. So, we estimate the first stage regression by estimating a credit demand equation (equation (9)) using a standard tobit model.[18] The covariates include in the first stage are household and village level and the excluded instrument.[19] The first-stage results, not reported here, show the instrument is highly statistically significant with a t-statistic of 8.5 for coefficient corresponding variable I (using full set of regressor as controls). The coefficient estimate is positive and also economically significant – implying that I is significantly related to the credit demand. We also estimate participation decision equation where a binary indicator for participation is regressed on an indicator of interaction of eligibility and program village dummies (plus all controls). The results are stronger with t-statistic of 12. The first-stage regression using basic control and no control show stronger coefficient estimates. We have argued in previous section about plausibility of the (conditional) exogeneity requirement for our instrument. Since we have single instrument for the endogenous credit variable, we cannot test the exogeniety of the instrument as in overidentified model. The remaining concern is whether the instrument satisfies the exclusion restriction, i.e., I affects the child labour or school enrolment only through the credit program participation or the amount of credit borrowed. Unfortunately, the exclusion restriction is not directly testable. However, we investigate this concern in a number of ways. First, we estimate a reduced form regression to examine the effect of eligibility on school attendance/child labour. We

[18] Equation (9) is estimated at the household level since the participation decision is made at that level.
[19] We also include child characteristics in the first stage estimation. The second stage results do not depend on whether we include or exclude child characteristics. There is a very little reason to incorporate child characteristics in the credit demand equation at the household level. However, there is no harm including them. So we tried with both specifications.

find statistically insignificant effect of eligibility on school enrolment and child labour.[20] We also experimented with an approach following Acemoglu and Johnson (2005) by estimating a semi-reduced form equation, in which credit is instrumented but instrument *I* enters the second stage regression directly (and naturally in the first stage regression). The results do not indicate any significant effect of *I* in any of the specification. We then consider if there is a discontinuity in child labour or school enrolment at the cut-off of eligibility, and we do not find any discontinuity. This indicates, albeit indirectly, instrument does not have any direct impact except through the amount of credit.[21]

The estimated value credit from the first stage is the regressor of interest in the second stage estimation. Table 2 presents the estimates of second stage using the LPM and a probit model under different covariate specifications. Columns (1) and (4) represent the treatment effects without any controls. The resulting estimates in column (1) can be considered Wald estimate, which represents the difference in probability of school enrolment/child work between children of microcredit participants and non-participants divided by the amounts of credit borrowed by the participating households. The Wald estimates in Table 2 shows that credit is associated with higher child work for girls while it lowers the probability of boys work (the estimated coefficients are not statistically significant). The results are similar whether men or women borrow credit. However, Wald estimates are likely to suffer from omitted variable bias since schooling and child labour are likely to be influenced by household demographic and socio-economic characteristics.

We consider two sets of control variables, and the results are reported in the second and third columns (using LPM) and fifth and sixth columns (using probit) of Table 2. Columns 3 and 6 add covariates to the regression and include full set of controls. All coefficients are estimated as the marginal effects calculated at the mean. The basic controls are nested in the full controls, the latter being our preferred specification. The overall results show that the credit borrowed from microfinance is likely to enhance the problem of child labour. The estimates are statistically significant and stronger for girls than that of boys. For boys, we do not find statistically significant effect. In fact, using a probit model we find negative, but statistically insignificant, effect of credit on boy's work, which indicates that microcredit can reduce the problem of boy child labour. The relationship between microcredit and child work does not change using either LPM or probit model. The results show that the probability of a girl child working increases due to microcredit taken by

[20] Note that there need not be any one-to-one relationship between reduced-form coefficient estimate and the power of the first stage (see Lochner and Moretti 2004).
[21] The detail results of the first-stage are available upon request.

women while it is opposite for boys (if we consider the probit marginal effects, results are not statistically significant). Using probit marginal effects, probability that a girl works is increased by 8% points when a household obtains credit from microfinance organization. This probability increases to 13.7% points when we consider the LPM. Girls are more adversely, and the boy's are more favourably, impacted if credit is borrowed by men (the latter estimates are not statistically significant) than by women. In fact, men's participation has favourable effect on boy child labour and the effect on girl is opposite. A Hausman-like test does not support the difference in treatment effect between men and women borrower. Overall evidence is that credit increases the chance of work for girl children while the effect on boy's works is less clear as the magnitude of the estimated coefficient for boys are very low, and in few cases changes sign. In general, microcredit tends to enhance the employment probability of children.

Table 3 reports the estimated effects of microcredit on children's school enrolment. The overall results (row 1) imply that credit is associated with less school enrolment. The impact estimates differ between genders of children. Microcredit has a significant negative influence on girls' education while its effects on boys are negative but statistically insignificant. Overall, the estimated coefficients are stronger for girls than boys, implying that credit has a stronger negative consequence on girls' education than it has on boys. These relationships are similar whether men or women member borrows credit. However, women's credit, compared to men's, has a lesser detrimental effect on girls' school attendance. On the other hand, men's credit has a less significant but unfavourable effect on boys' school attendance than that of girls. Most of the coefficients on boys' schooling are statistically insignificant. The differences in the estimated coefficients between men and women credit borrower indicate that such difference is almost double for boys schooling to that of girls. However, Hausman-type test cannot reject the equality of the coefficients between the sexes of the borrower. The results do not differ much among three different specifications of covariates. The magnitude of the estimated coefficients increases when we go from basic controls to full controls. This suggests that there is a negative correlation between household socio-economic status and the participation/microcredit demand.

The other control variables in school enrolment and child work regressions all have the signs we would expect from previous studies (Table 9). For the school enrolment equation, highest education attained by any member of the household, or household head's education, increases the chance of child schooling and decreases that of working. If the household head is male then it is associated with increasing the likelihood of school enrolment and decreasing the child work. Presence of mother in the household has positive but statistically insignificant effect on schooling, while the relationship is opposite for probability of child work. Increase in the number of younger siblings reduces the probability of school attendance of older siblings, while it increases the demand for child labour at the household level. There is a positive association between presence of secondary school and children's school attendance, and negative but statistically insignificant effect on child work. If a village has a primary school, it does not have any statistically significant effect of school attendance or child work, possibly because almost all the villages have primary school. Similarly, the presence of health-facility and brick-built road in a village road has positive influence on child school attendance while the signs are the opposite for child work. Rice prices do not have any effect on either school attendance or work probability. The sign of the adult wage coefficient in child work equation is positive but statistically insignificant. The coefficient estimates are also economically insignificant. This implies that adult male and child labour are imperfect substitute.[22]

The impacts of microcredit on children's school attendance or on their work probability do not change when we change the treatment status variable. Table 4 displays the treatment-on-treated effect using a binary participation indicator as the treatment variable. The estimated coefficient of treatment 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 indicate that they are similar to the previous estimates using credit as participation variable, especially in terms of signs of the estimated coefficients. Girl's education continues to have negatively impacted when their parents participate in microcredit program. Microcredit program participation by either gender decreases the girl children's school enrolment and increases the demand for their labour. The corresponding coefficient estimates are not statistically significant and have mixed signs for boy's work. Using probit coefficients, we find that women's borrowing from a MO increases labour participation probability of girls by 13%. At the same time, it decreases their probability of enrolling in school by 44%. The magnitudes of the impact estimates are similar in case of men's participation in microcredit. Thus, using binary participation measure gives a considerably higher coefficient estimates for girls. However, these results are only indicative as they do not take into account of the variation of treatment intensity across households (as different household borrowed different amount of money).

[22] 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 labours may be desirable. Our results indirectly suggest that this might not be the case. Moreover, we regress adult male wages on child labour and find a positive coefficient (t-ratio=1.53). This indicates that substitution axiom does not hold in our case. These results also point out that when adult wage increase this will lead to a loss in adult time allocated in household activities. Consequently, children's time devoted to household enterprises or other activities increase.

The standard errors quoted above (reported in Table 2-4, directly below the coefficient estimates) are corrected for clustering at the village level and weighted by propensity score to take into account the choice based sampling. The reported standard errors in brackets are those taking intra-sibling correlations within households into account. The standard errors are often close to each other; typically the later is slightly lower, though occasionally slightly higher. Since they do not differ much, below we report regression results using clustered standard error at the village level. We also experimented with a two-step procedure discussed by Donlad 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). The estimation results are similar, and not reported for the sake of brevity.

Does the Effect Differ Between Primary and Secondary School Age Children?

We report the results based on effects of credit on children of age 7-12 (primary school age) and 12-16 years old (secondary school age: up to grade 10) in Table 5. We use binary treatment status indicator as the participation variable. School enrolment of either gender children having primary school age are equally but adversely affected with women's microcredit participation. However, men's participation in microcredit has a weaker impact on same age groups of boys' schooling compared to that of girls. The effects of credit on children's schooling of high-school age are mixed. Women's credit has statistically significantly negative impact on girls schooling while men's credit have negative but statistically insignificant impact on girls schooling (possibly due to smaller sample size of men participants). The results signify that schooling and child work of boys of 12-16 years old are favourably impacted by credit obtained by either men or women. The results also show that microcredit accentuates the problem of child work of primary school age children than it does for older children. The probability to work for boys of high-school age decreases (coefficients are not statistically significant) while girls of similar age experiences increased probability of work due to microcredit. Thus overall results indicate that younger children are more adversely affected by their parent's participation in microcredit. The results also show that the demand for younger girls work is expected to increase more, as compared to younger boys, whether credit is provided to women or men.

Does the Effect of Microcredit Differ with Household Income?

We test whether the effects on child labour or schooling are different due to family's income level. Income of a household can play a substantial role in determining child labour and school attendance (see, for example, 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. This also allows 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 certain (subsistence) level. However, we cannot treat income as exogenous. Income is endogenous because the amount of credit borrowed by the household directly affects the income. If the participation in microcredit program has positive effect on household income, then including income in our regression would underestimates the program effect coefficient. Moreover, children contribute to household income which 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 child income the endogeneity problem would not be resolved by subtracting it from the total if the labour supply of different household member 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 work) even if family income were to rise.

There are mainly two approaches to deal with the endogeneity of income - fixed effect estimation (e.g., Blau 1999) and instrumental variable technique. While fixed effects estimation should eliminate any bias from permanent family or child differences, it may exacerbate bias due to unobserved temporary family shocks (see Dahl and Lochner 2005). We take an alternative approach here, and use parental education, which is not affected by program participation or children labour supply, as a proxy for permanent income.[23] If education has a positive return, families with more educated parents is expected to have more income. By using parental education we also avoid the problem that is due to noisily measured income, and hence the possible attenuation bias. We use three categories of parental education: *Low* are 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 one of the parents obtained more than primary but less than a high school degree (5-10 years of schooling), and *High* refers to families where one of the parents obtained at least a high school degree (11 or more years of schooling). We specify the following functional form:

[23] Permanent income does not vary across the observations on a given parent in our cross-sectional data, so parental fixed effects method cannot identify the effects of permanent income. We need differences in family income level across siblings to remove fixed family factors to estimate the impact of income on child outcomes.

$$Y_{ijkl} = \delta_{0l} + \delta_1 X_{ij} + \delta_2 Z_k + \delta_3(credit_{jk} \times Low_{jk}) + \delta_4(credit_{jk} \times Middle_{jk}) + \delta_5(credit_{jk} \times High_{jk}) + \nu_{ijkl} \quad (10)$$

where we incorporate the households' permanent income into the model by interacting these categories with the amount of credit borrowed to capture differences in slopes across different level of education (permanent income) within the treatment group.

Equation (10) is unidentified since number of endogenous regressor is greater than the number of instruments. Therefore, we need additional instruments that are correlated with the interactions of credit and different dummies for education. For equation (10) to be identified we need at least three instruments. When using three instruments, it is necessary for one to predict the first endogenous variable, the second the second, and the third the third. In our case, a single instrument predicts the credit variable. Since credit is interacted with education dummies all the predicted values will be closely correlated. So, lacking identifying instrument, we estimate equation (8) by sub-groups defined over head's education [using equation (9) as the first-stage regression]. The resulting estimates are unbiased but not efficient.

Figure 4 illustrates the school enrolment and work of children based on household head's education. In general there is positive relationship between children's school enrolment and head's education, and a negative relationship with child work and head's education. Non-treated households have higher level of children's school enrolment, while their children are less engaged in work. Table 6 reveals that improvement in economic condition of the households, proxied by education of the head of the household, contributes positively to child schooling and negatively to child work. None of the coefficient estimates are statistically significant for the medium education group — partly due to smaller sample size. The estimated coefficients reflect that microcredit is not helpful for increasing school participation and reducing child labour in case of low income households, but they help for medium income households. Since low income households are mostly credit constrained, this result indicates that microcredit offered to low-income households do not solve the problem of child labour and also do not enhance the school enrolment. As credit is not directly offered for education purposes, the results indicate that low income households engage more their children to work to get immediate return from the microenterprise project.

The difference in the coefficient between two income groups suggest that an increase in 10 percent credit given to medium-income households increases overall schooling of boys and girls by about 2.5 percent compared to treated households with lower income group. This is represented by the difference in probability of schooling between the estimated coefficients of medium education and low education in column (1). Similar calculations between low and medium income level for child work indicate that children of medium income group enjoys a relative increase of 1.8 percent probability of schooling compared to the children of treated households in low income group. Since the coefficient estimates measure the difference in probability of school attendance between children of treatment and control households of different income groups, the differences in estimated coefficients between two treated groups can be interpreted as difference-in-difference estimate of the impact of households' income. The results indicate that effects differ in terms of school attendance of boys versus girls. Girls in medium income participating households are less favourably impacted than their counterpart siblings.

The point estimates indicate that an increase of 10 percent in the credit borrowed increases the girl's employment probability by 0.1(probit) to 0.3 (LPM) percent in low-income households, and for boys the corresponding increase is 0.2-0.5 percent. The results on girl's employment probability is ambiguous since the sign of the coefficient estimate changes between probit and LPM estimates, and also economically not significant. However, we observe a reduction in boy's employment probability by about 0.3-0.9 percent for low-income treated households for a corresponding rise of 10 percent microcredit. Overall, evidence suggests that income or education of households matter for decreasing the probability of children's work and that of increasing school attendance among the children of treatment group. The results indirectly point out the presence of luxury hypothesis as poorer households are more likely to engage their children to work. However, statistical insignificance of the coefficients suggests that the evidence is not strong.

Is there Heterogeneity in Treatment Effect by Asset Ownership?

Here we examine the effects of credit by asset holding. This would allow us to verify the results based on income/education above. We divide households into two groups: those only eligible (having land ≤ ½ acres) to get microcredit (poorer) and those having more than one acre of land (less poor households).[24] We report results by poverty status in Table 7. The results indicate the different effect of credit between poorer and less poor group of households. In particular, we see that poorer households are more likely to send their boy children to school and girl children to work. On the other hand, less poor households do the opposite. This shows that poorer households are more vulnerable to

[24] Household's 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 the member of a MO. Since microcredit is mainly provided to 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 to get microcredit.

keep their girl children in school. It is not clear why less poor households participating in microcredit program tend to keep their boy children at work. It could probably be the case that land-rich household engage their boy children more in agriculture activity, while landless or marginal landholding households engage their girl children to work at household enterprises.

Many of the results are statistically insignificant possibly due to smaller sample size. Overall the results also indirectly demonstrate that our results were not driven the pre-existing differences in characteristics of treatment and control groups, since poorer treated households, who are very similar to their non-treated counterpart in terms of observed characteristics (descriptive statistics for this sub-group is not reported here, similar results are available in Islam (2007)), tend to put their girl children in work and keep them away from school.

Additional Robustness check

Potential Identification Strategy Issue: Causal Effect or Selection Bias?

In the above, the causal effect of microcredit on children's schooling and work is identified under the assumption that the differences in schooling and work of children is not due to underlying differences in characteristics between treatment and control group households. It can be argued that households from program village who exhibit a fewer tendencies to send their children to school are more likely to participate in a microcredit program. If this is the case then our estimate would simply indicate the effect that is not due to participation in a microcredit program but the pre-existing differences in the characteristics of households of treated and non-treated groups. We have also seen in Table 1 there are also some differences in household characteristics between treatment and control groups. For example, non-treated households are better educated than their treated counterparts. Treatment group has a higher household size and more children. So, if lower parental education and larger household size reduce the children's education, the effects of credit lead to overstatement of the negative effect of participation status on children's schooling. However, many of the estimates at the household level suggest that the characteristics are the same for both treatment and comparison groups.

We assume that any observable difference between treatment and control groups are accounted by the control variables used in the regression. Any remaining differences are accounted using propensity score weights which also significantly reduce the differences between the treated and comparison households (not shown here). If there is selection biased on unobservable, we address the bias using instrumental variable strategy. Since, in our regression, we use a number of controls it remains to be seen whether there are any potential confounders that would violate the exclusion restriction. It is still possible, however, that there are important unobserved differences that we cannot adjust for that influence the placement of the program and/or participation decision in credit program, and alter the relationship between microcredit and children schooling and work differently in the treatment and control groups, or that there are some remaining differences in observables that operate similarly. This is definitely worth bearing in mind, but the risk does not seem large given our efforts to control for confounders, and to credibly demonstrate that exclusion restriction is a really valid one.

In order to substantiate our claim further, we check the robustness of the results using alternative approaches. We first compute regression adjusted years of education for older siblings: children who are 16-20 years old. This group of children are less likely to be affected by their parents' participation in microcredit since most of them already passed secondary school or dropped-out from school before their parents' participation in a microcredit program. We find no statistically significant difference between children of treated and comparison households (t-ratio=0.7). This result is in line with our findings that older children are less adversely affected by microcredit because (1) they are less exposed to microcredit; (2) younger children can work at household enterprise without hurting the work/schooling prospects of older siblings. Also it is a more difficult choice for parents to engage older children to work at microenterprise if they are already in school (since they have already made investment in schooling) or in market work (since it will cost their wage earnings).

Next, we also control selection bias using an alternative econometric strategy. In particular, we consider corrections for endogeneity using reduced form residuals, leading to a control function method of accounting for both selection and endogeniety. This is also important if the effect of credit varies across households. In the latter case, IV/2SLS may not estimate average treatment effect of credit. There are, however, different approaches to estimate control function, and not all these procedure will produce consistent estimate of treatment effect. We adopt the procedure suggested by Vella (1993), which identifies correctly the treatment effect parameter in our context.[25] We first obtain generalized residuals using either tobit (for credit as the dependent variable)/ probit (for binary participation variable) for the reduced form first stage equation, and then use the estimated residuals as an additional regressor in the second stage.[26] The results are similar to those reported early. The

[25] Garen (1984) suggest 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 two stage conditional maximum likelihood approach of Rivers and Vuong (1988) is not applicable as the approach also requires that the credit variable to be continuous (see Vella 1993, Ravallion and Wodon 2000).
[26] This model is identified even without the exclusion restrictions due to the non-linearity of the residuals.

results can be obtained upon request from the author. Below we report control function estimates based on different educational attainment measure.

Do the results hold with alternative measures of children's educational attainment?

While the school enrolment has the advantages of current status of school age children, it does not measure the achievement for those who are not in school during the time of survey. We therefore measure the impact of microcredit participation on different other measures of educational attainment of children. The first is the number of years of school completed. Then, we consider 'education gap' measure. In Bangladesh, the age at which children are expected to start school does not vary across country because of largely homogenous group of population. The age at which child is legally supposed to go to school is also the same. As a result children must attend school at the age of 6-7. Therefore, we can construct a variable 'education gap' to measure the achievement in terms of grade completion. The education gap is measured as the number of years of the difference between the highest level of education actually completed by the child and the expected level of education, according to the child's age. The education gap can be defined as:

Education Gap= max {0, Expected education-Actual Education}

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

For example, if a grown-up child successfully stayed at school up to the end of secondary education, the gap is zero. If he/she encountered problems (such as late entry, failed grades, or desertion), the gap is a positive number. If he/she never attended school, the gap is the level of expected education according to his/her age. Since we consider only children of age 7 years or older we can avoid problem having two types of zeros: those of age below six and those who have successfully completed/completing schooling. Finally, following Patrinos and Psacharopoulos (1997) educational attainment is obtained by defining a grade-for-age dependent variable which is defined as 100*[Education Grade/expected education]. Grade-for-age measure is useful measure for long-term educational attainment for older children who may still be in school. It also corrects for age to allow easier comparison of children who may have the same number of grades completed but are different ages and likely to have different educational attainment when they reach maturity.

Figures 5a and 5b display the relationship between age and grade attainment of children in our sample. We see that boy children of non-treated households have always higher grade attainment at each age. However, schooling of girl children of primary school age is very similar in both treatment and control households. After that age the grade attainment of treated household's children is substantially lower than the control. Table 7 produces the results of the estimate of the effects of credit on different measures of education achievement using control function estimates (the results are similar using estimation method previously employed). The results are based on OLS regressions in the second stage for each of these education measures.[27] Overall results obtained using these new outcome measures are similar to that of school enrolment used earlier. A negative coefficient of grade completion and grade-for-age, and a positive coefficient for education gap imply that credit program participation has adverse effect on grade achievement. All the estimation results indicate that microcredit program has unintended consequence on the children school achievement. The girl children are severely affected. The coefficient estimates for girls are statistically significant at 1 percent level (t-ratios > 3 in all cases) and the magnitudes of the coefficients are higher than that of boys. The effects on school achievement of boys are not statistically significant, and in case of school gap measure boys are positively impacted by their parents' participation in credit program. The results using different measures are similar since all three measures are likely to be highly correlated. The coefficient of the residual from the first stage provides an exogeniety test. Most of the results reject the exogeneity of credit.

6. Discussion and Conclusion

Our results indicate that child labour problem may be exacerbated due to household's borrowing from microcredit institutions. The results are also consistent with the literature on microcredit and schooling and child labour. Our results also indicate the differential impact of microcredit between sexes of the children: the girls are more likely to be called upon for working at the household enterprise. This result raises the possibility that because households face constraints in hiring labour at household enterprises, family adjusts by allocating resources away from daughters. This result is consistent with the return to schooling literature. This also supports the popular perception that, in developing country, parents perceive higher expected returns on boys' education as they can claim on future resources of boys. On the other hand, girls get married out to another family and contribute resources to their husbands' family, and so investing on girls' education are not perceived to be as beneficial as boys (see Dasgupta 1987). The overall results suggest that increasing access to rural microcredit need not increase the school enrolment in a developing country like Bangladesh. Therefore, though joint-liability lending mechanism reduces many of the informational asymmetry

[27] We use here OLS, as opposed to conventional tobit which takes the zeros into account, because in the first stage we are estimating credit demand using tobit. So using tobit in the second stage creates further difficulty in consistently estimating coefficient unless the first stage is exactly correctly specified (see Angrist 2001). This is, however, not the case in case of OLS. It should, however, be noted that our conclusion is not sensitive to use of tobit in the second stage.

problems, potential more moral hazard exists within household enterprise. It raises the question of whether household's short-term welfare gains by borrowing microcredit comes at the expense of schooling of children which can cause long-term poverty.

We also find that the impact of microcredit on schooling and work differ by family's income/education level: while poorer children are less likely to attend school and more likely to work due to their parents' access to credit, those from relatively less poor families are not that much adversely affected. The results also demonstrate that poorer households are more vulnerable to keep their girl children in school. The findings by income/education and asset holdings indicate that credit constraints poor households may not send their children to school in presence of credit availability. But credit (or aid) tied directly to child schooling may have different impactions (see Ravallion and Woodon 2000). So, we cannot directly compare our results with the findings of educational borrowing constraints literature. However, as demonstrated in previous studies that microcredit can help increase income and consumption expenditure. So, if poorer households can graduate out of poverty by taking credit, then our results could hold only in the short-run. In the long-run, as their income increase the participating household is likely to increase their children's school enrolment. On the other hand, if there is slow rate of reduction in poverty among participant then microcredit may create a long-term problem of human capital formation. The results also reveal that younger children are more prone to work due to their parents' participation in microcredit program. We find qualitatively similar results when we estimate the impact on student achievement. We check the robustness of the results using an alternative estimation strategy, and obtain similar results.

Finally, the results call for further investigation into the effects of microcredit on children's schooling and work using probably panel data set to see long-term effect. It is to be noted that the results do not necessarily imply that microcredit puts the children of borrowing households into adverse situation. Rather, it implies the effects of microcredit on schooling and child works between the households who borrow and those who do not (and possibly they have other access to credit). One shortcoming of our approach is that we do not estimate the general equilibrium treatment effect (see Heckman, Lochner, and Taber 1998) but only focuses on treated households. If there is any spillover of the effect of microcredit then our estimate would overstate the negative effect. For example, microcredit available to households in program village might reduce the borrowing constraints faced by non-participants since they can now obtain loan from other informal sources at a softer term and conditions. However, our findings suggest that microcredit may not be a panacea to all problems related to poverty since some borrowers sacrifice their child's education to gain immediate return from their investment. In Bangladesh, there is parity in terms of school enrolment between boys and girls. In fact, school enrolment of girls is found to be higher than boys. The displacement of boys from schooling, due to microcredit, is smaller than that of girls - implying that government policy to address the gender imbalance in education may turn out to be less effective in the presence of large number of microcredit organizations. Our results indicate that developing countries pursuing gender equality in education, improving schooling and eliminating child labour may find it increasingly harder to achieve so in presence of large microcredit programs. A number of policies including those suggested by Wydick (1999) can be adopted to mitigate the adverse consequence on child labour and schooling so that microcredit can benefit both the current and future generations. In addition, the microcredit organizations can also increase the gestation period between actual loan disbursement and the start of repayment schedule. This would allow 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.

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Table 1: Descriptive Statistics

Variable		Treatment (I)	Control (II)	Difference III=(I-II)
Child Characteristics (7-16 years old)				
Child work				
	Boys	0.169	0.138	**0.030**
	Girls	0.110	0.093	0.017
Child School				
	Boys	0.693	0.747	**-0.055**
	Girls	0.766	0.789	-0.023
Age of Child		11.497	11.494	0.003
Sex of child		0.557	0.556	0.001
Household Characteristics				
Mother Age		37.66	38.14	-0.48
Mother Schooling		1.09	1.54	**-0.45**
Father Age		45.85	46.80	**-0.95**
Father Schooling		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	4.78	5.29	**-0.50**
	Female borrower	4.17	4.57	**-0.40**
Amount of land		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 Work

	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 parenthesis 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 propensity score weighted. + significant at 10%; ** significant at 5%; * significant at 1%.

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

	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 parenthesis 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 propensity score weighted. + significant at 10%; ** significant at 5%; * significant at 1%.

Table 4: Impact Estimates based on Binary Participation Measure on children's School Attendance and Work

	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 parenthesis are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using propensity score weighting scheme. + significant at 10%; ** significant at 5%; * significant at 1%.

Table 5: Impact of the Microcredit Participation on Children's School Attendance and Work by Age Group

	Child is in school		Child is in work	
	Age(7- <12)	(Age12- 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 parenthesis are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using propensity score weighting scheme. + significant at 10%; ** significant at 5%; * significant at 1%.

Table 6: Impact of the microcredit on Children's School Attendance and Work (by Household Income proxied by Education)

	Child is in school		Child is in Work	
	LPM	Probit	LPM	Probit
Low education				
Boys and Girls	-0.21	-0.2476	0.1245	0.101
	(0.0863)**	(0.0976)**	(0.0489)**	(0.0424)**
Boys	-0.1054	-0.1654	0.0481	0.0162
	(0.1044)	(0.1278)	(0.0624)	(0.0526)
Girls	-0.3089	-0.325	0.1914	0.1605
	(0.1168)*	(0.1291)**	(0.0751)**	(0.0538)*
Medium education				
Boys and Girls	0.0448	0.0177	-0.0622	-0.0175
	(0.0951)	(0.0704)	(0.0700)	(0.0318)
Boys	0.0546	0.0247	-0.0907	-0.0364
	(0.1338)	(0.0991)	(0.0992)	(0.0348)
Girls	0.0352	0.0093	-0.0335	0.0058
	(0.1253)	(0.0557)	(0.0810)	(0.0118)

Notes: *Lowedu:* households with highest level of education of 0-4 years of schooling or less; *Midedu:* 5-10 years of schooling. Because of the very small sample size of high education group, we do not report results for them. All the results are the marginal effects of instrumented credit interacted with education dummies using IV regressions. The results should be interpreted as the difference of outcome of children of the households whose parents took microcredit loan to those who did not. The regressions include full control using child, household, village characteristics and district fixed effects. Standard errors presented in parenthesis are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using propensity score weighting scheme. The coefficients and the standard errors are multiplied by the average credit borrowed by the respective group of households. + significant at 10%; ** significant at 5%; * significant at 1%.

Table 7: Impact of the microcredit Program on Poorer and Less Poor Households

	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 ≤ ½ acre land, less poor households own more than ½ acre 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 parenthesis are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using propensity score weighting scheme. + significant at 10%; ** significant at 5%; * significant at 1%.

Table 8: Impact of the Microcredit Program on Children's School Achievement

	Boys			Girls		
Women Borrower	Grade completion	School Gap	Grade-for-age	Grade completion	School 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)*
Men 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 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 the men and women borrowers. Standard errors presented in parenthesis are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using propensity score weighting scheme. + significant at 10%; ** significant at 5%; * significant at 1%.

Table 9: Effects of Child, Household and Village Characteristics on School Enrolment and Child Work

	School Attendance		Child Work	
	LPM	Probit	LPM	Probit
Variables	(1)	(2)	(3)	(4)
Age of Child	0.257	0.271	-0.096	-0.005
	(0.023)*	(0.022)*	(0.018)*	(0.014)
Square of Age of Child	-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 Schooling of Mother	-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 parenthesis are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using propensity score weighting scheme. + significant at 10%; ** significant at 5%; * significant at 1%.

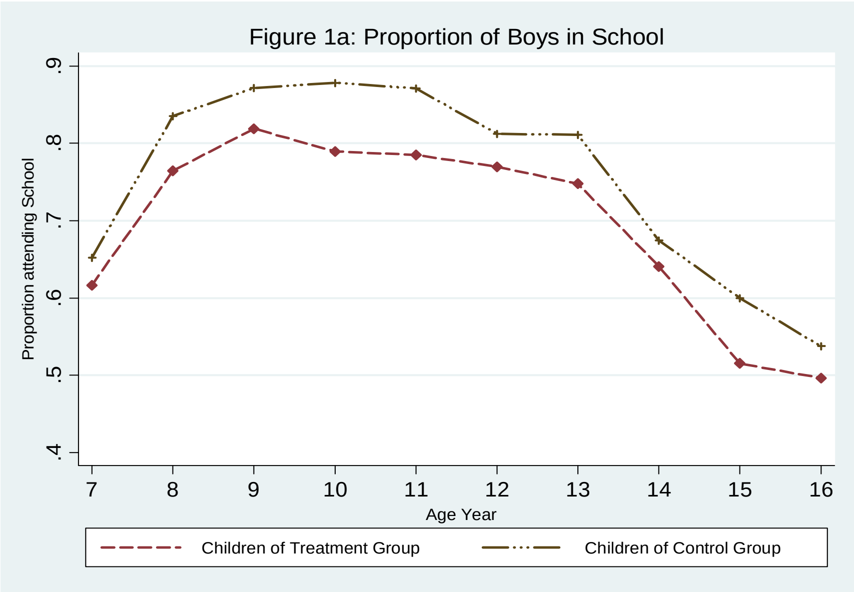
Figure 1a: Proportion of Boys in School
Proportion attending School
.4
.5
.6
.7
.8
.9
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8
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10
11
12
13
14
15
16
Age Year
Children of Treatment Group
Children of Control Group

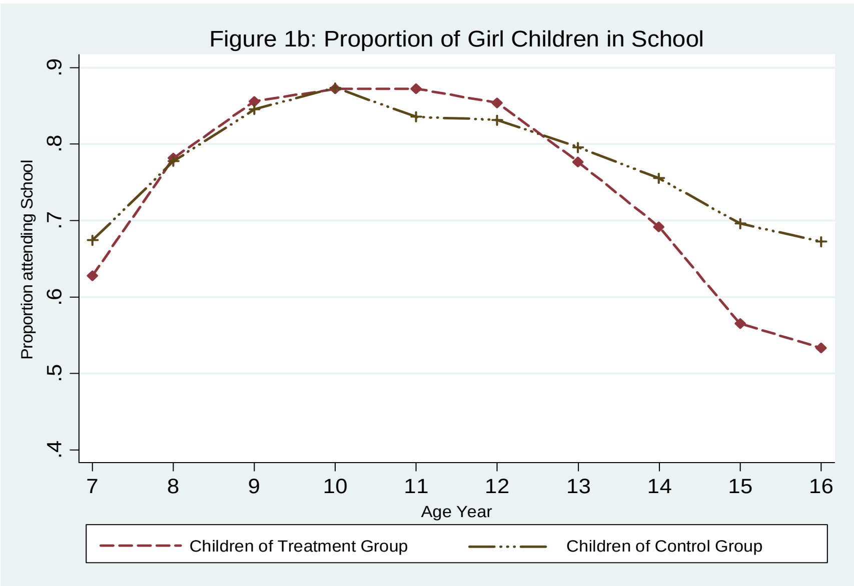
Figure 1b: Proportion of Girl Children in School
Proportion attending School
.4
.5
.6
.7
.8
.9
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8
9
10
11
12
13
14
15
16
Age Year
Children of Treatment Group
Children of Control Group

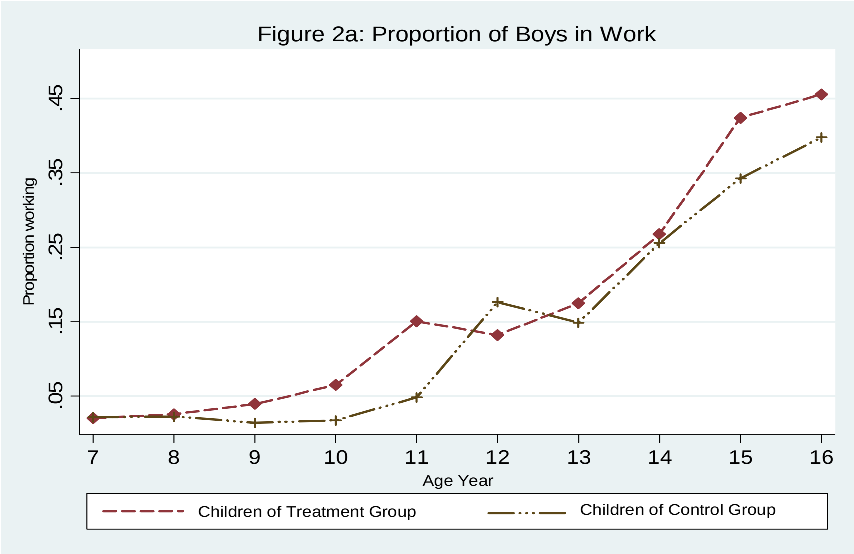


Figure 2a: Proportion of Boys in Work
Proportion working
.05
.15
.25
.35
.45
7
8
9
10
11
12
13
14
15
16
Age Year
Children of Treatment Group
Children of Control Group

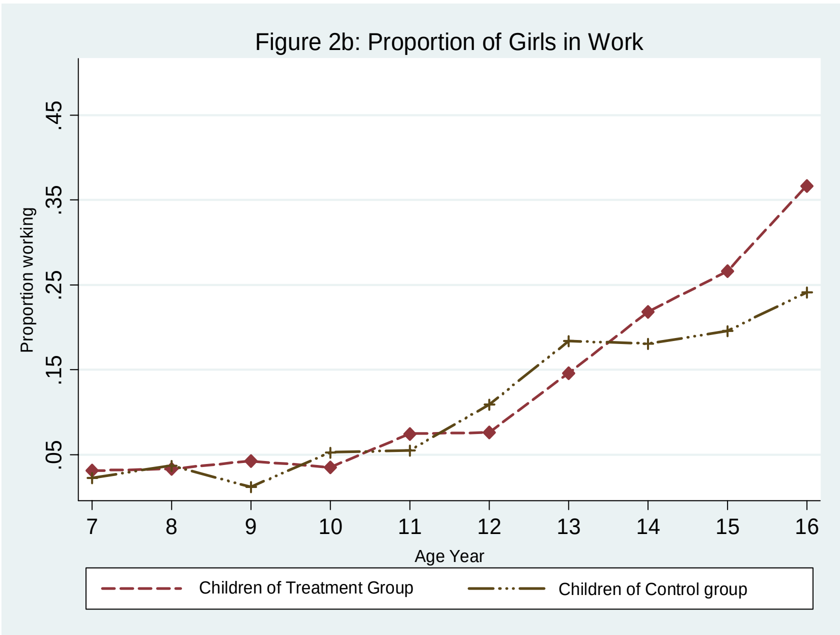
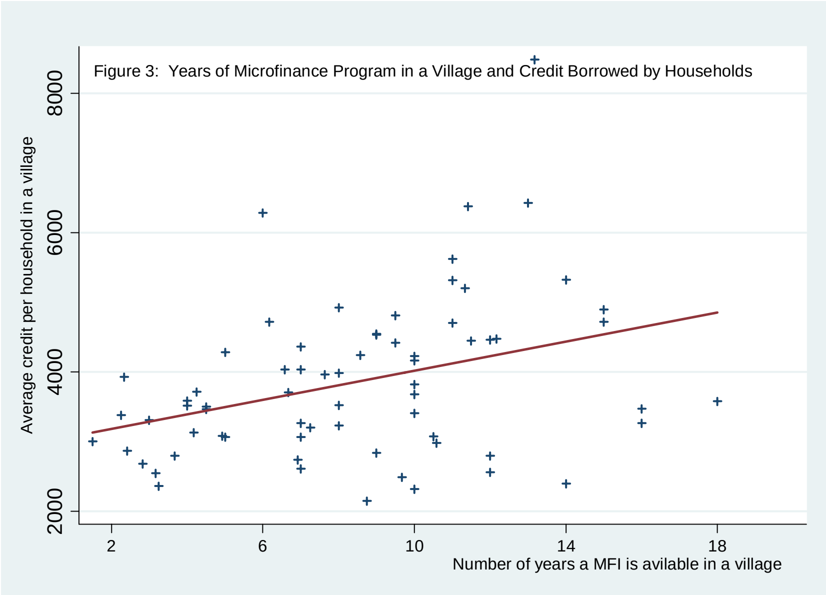
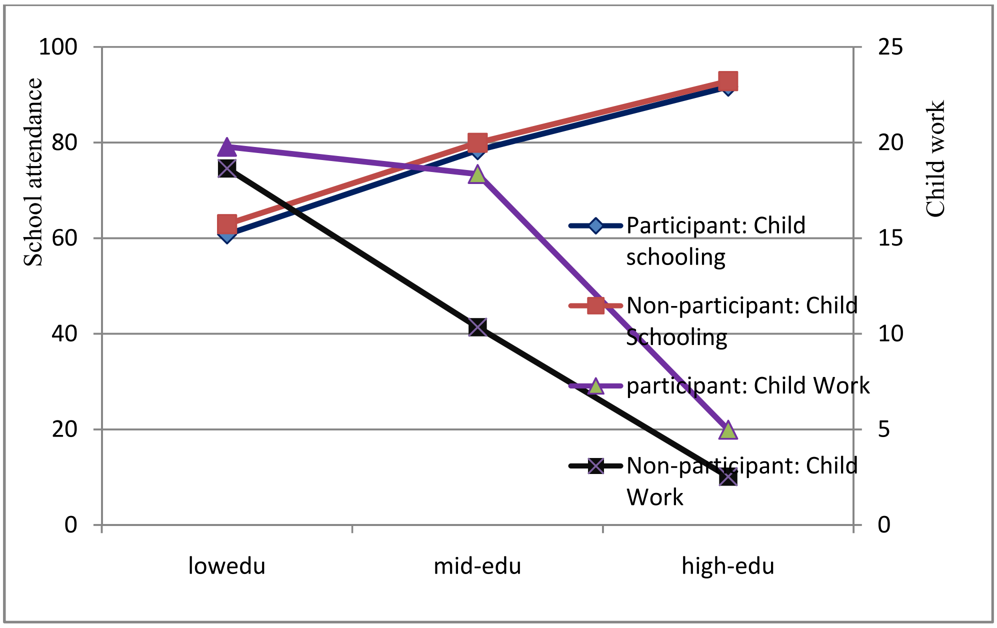


Figure 2b: Proportion of Girls in Work
Proportion working
.05
.15
.25
.35
.45
7
8
9
10
11
12
13
14
15
16
Age Year
Children of Treatment Group
Children of Control group



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

Figure 4: School Enrolment and Child Work of Children of Households of Different Level of Education



Notes: *Lowedu:* households with highest level of education of 0-4 years of schooling or less; *Midedu:* 5-10 years of schooling; *highedu*: above class 10

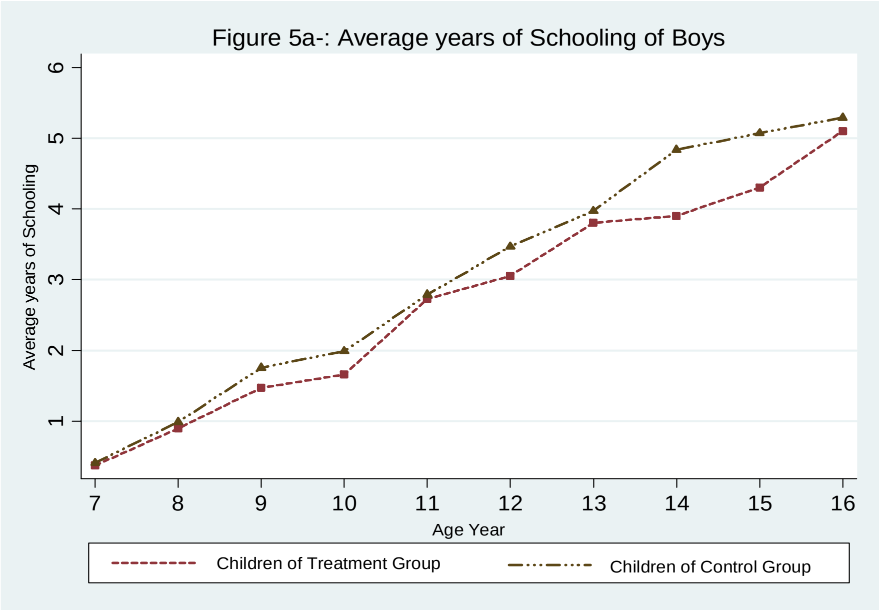


Figure 5a-: Average years of Schooling of Boys
Average years of Schooling
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
Age Year
Children of Treatment Group
Children of Control Group

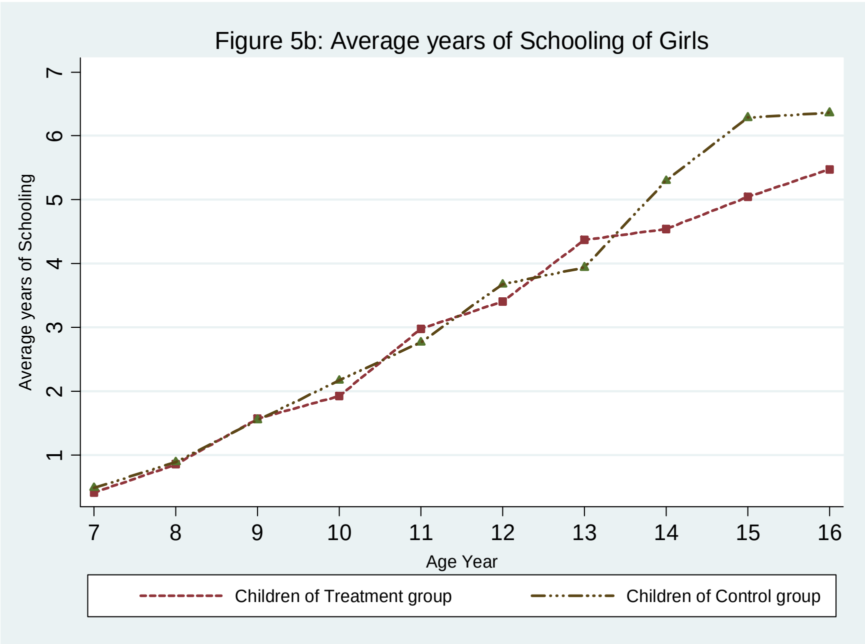


Figure 5b: Average years of Schooling of Girls
Average years of Schooling
1
2
3
4
5
6
7
7
8
9
10
11
12
13
14
15
16
Age Year
Children of Treatment group
Children of Control group

Appendix

Variables Included in the Regression:

Basic Control:

Child characteristics: Age, Age square, Sex*

Household characteristics: landholding (less than one acre, one acre to 2 acres, 2 acres to less than 5 acre, 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 +

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 0-6, 7-15, education of father (low (0-4 years), medium (5-10 years), high (11 and above)), age of mother, presence of mother.

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