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Household Credit to the Poor and its Impact on Child Schooling in Peri-urban Areas, Vietnam

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Abstract

This paper uses a novelty dataset of poor households in peri-urban areas in Vietnam to estimate impacts of small loans on child schooling. The Probit and Negative Binomial model estimates roughly indicate no strong evidence of the effect, especially of informal credit. Formal credit is likely to have positive impacts on child schooling, but its effect is not strong enough to be conclusive. The paper suggests that to obtain the target of sustainable poverty reduction, easing access to formal credit sources as well as exempting tuition and other school fees are necessary to keep poor children at schools longer.

JEL Classification

C14; C21; H81

Keywords

school enrolment
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1. Introduction

It is widely recognised that human capital plays an important role in productivity, earnings, and sustainable poverty reduction (Maldonado & Gonzalez-Vega, 2008; Maitra, 2003). Education not only passes specific knowledge to students but also enhances skills in acquiring new knowledge (Rosenzweig, 2010). However, the poor encounter two key development issues: income constraints and low education. These lead to a vicious circle of poverty. Income constraints result in low education investment and hence low education attainment. Low education results in low productivity and then low income. Hence, child schooling receives a lot of attention in development strategies and is considered a solution to breaking the vicious circle of poverty and to enhancing future development. However, education investment by many households in developing countries is insufficient, especially by poor households.

Demand for education relies on parents' motivation, income constraints, and competing demands for children's time (Maldonado & Gonzalez-Vega, 2008). Under perfect financial markets, credit would be a tool to guarantee full investment in education. The underdevelopment of financial markets and income constraints, however, are the main reasons for deficient education for children in developing countries (Edmonds, 2006; Jacoby & Skoufias, 1997; Ranjan, 2001). Due to credit constraints, many households are not able to borrow or borrow insufficiently so they may pull their children out of school or ask their children to reduce study time and go to work, especially when households face adverse shocks (Kurosaki, 2002). Thus, access to credit would help households to smooth consumption without the need to cut children's schooling.

Moreover, during the economic transition in Vietnam cuts to public subsidies in education have led to an increase in private education costs (Cloutier, Cockburn & Decaluwe, 2008). As a result, households, especially the poor, may need other external support, including credit, for their children's education. This paper aims to evaluate the impacts of household credit on child schooling for the poor in peri-urban areas of Ho Chi Minh City (HCMC), Vietnam. The paper has two goals: *First*, it examines whether borrowing keeps children in school longer than without borrowing. *Second*, the paper examines whether the sources of credit and gender of children matter in impacts on child schooling. This paper finds that although small loans may affect levels of education spending, but they just bring slight benefits to the poor's child schooling. This is because the loan amounts are too small, especially amounts of informal credit, to cover 'big' lump sums of education expenses for initial enrolments, and hence small loans do not affect parents' longer-term decisions about their children's schooling.

This paper is organised as follows: The next section reviews the literature on credit impacts on child schooling. Section 3 discusses estimation methods. Section 4 reports estimation results and Section 5 provides concluding remarks.

2. Literature on credit impact on child schooling

Household affects child schooling in two ways:[1]one *beneficial* and one *adverse*. First, the *beneficial impact* is that household credit enables households to earn more; higher income will push up household consumption which further increases the demand for healthcare and child schooling (Armendariz & Morduch, 2010). The credit could be spent on schooling (school fees, textbooks, schooling materials, uniforms and other schooling expenditure) as well as on improving child nutrition and shortening their sickness time by allowing children to take medicines promptly. As a result, the spending helps keep children at school. Similarly, Maldonado and Gonzalez-Vega (2008, p. 2,441) classify this channel of effect as 'positive', and name this effect 'income effect'. The credit helps generate household income (positive impact) and then positively influences the demand for education. Moreover, education is a normal good, and thus it has a positive income-effect. As a result, an increase in education spending will positively affect child schooling. Therefore, access to credit allows households to smooth their consumption and then improve their decisions in favour of more education for their children (Maldonado, Gonzalez-Vega & Romeo, 2002, p. 29).

Inadequate schooling, a situation when children are required to drop out of schools to help their parents or to cut down household spending, is often attributed to lack of access to credit (Dehejia & Gatti, 2002; Edmonds, 2006; Jacoby & Skoufias, 1997; Ranjan, 2001). Households facing adverse shocks and having insufficient access to credit may withdraw children from school to reduce household expenditure and send children to work in order to smooth household consumption (Jacoby & Skoufias, 1997; Kurosaki, 2002). On the other hand, when households are able to borrow adequately at reasonable interest rates, they may not need child labour; then children may stay at schools longer. For example, according to CGAP(2003), there appears to be a large differential in child schooling between two groups of borrowers and non-borrowers in Bangladesh; almost all girls of Grameen Bank borrowers have some years of schooling, whereas only 60% of non-borrowers' girls have some years of education. For boys, 81% and 54% respectively for borrowers and non-borrowers households have some schooling. Many other studies show that compared to non-clients of microfinance, enrolment rates and years of schooling are improved for microfinance clients' children after joining microfinance programs (Barnes, 2001; Chen & Snodgrass, 2001; Morduch, 1998; Pitt & Khandker, 1998).

The *second* way in which microcredit affects child schooling is a *child labour* or *adverse* effect. Borrowed money is spent on family businesses, which lead to an increase in household employment. This would undermine children's schooling because children have to replace their mothers in caring for their younger siblings, in looking after animals, and in doing housework and farming (Maldonado & Gonzalez-Vega, 2008, p. 2,441). Consequently, children may encounter adverse effects of credit on schooling; children quit school immediately or reduce time for schooling. Consequently, their academic performance gradually worsens; children may repeat classes or find themselves discouraged from staying

[1] Generally, loans to the poor are often small so the terms 'household credit' and 'microcredit' are used interchangeably in this paper.

at school longer, and eventually drop out. Furthermore, child labour and schooling are exclusively parents' decisions (Edmonds, 2006); so when parents need more labour to increase family income and smooth consumption, they may pull their children out of school.

Moreover, the child labour effect could result from requirements of immediate loan repayment. Loans to the poor often have higher interest rates (except subsidised loans) and short-term repayment conditions as discussed in Doan, Gibson and Holmes (2010). Borrowers therefore require high returns to pay high interest rates in a short period of time. To ensure repayment, poor borrowers may try to reduce their business costs by employing their own labour, including children, without wages. Consequently, children from borrowing households may be pulled out of school. For instance, Beegle, Dehijia and Gatti (2004) in a study on Vietnam find that households borrowing from higher interest rate sources were more likely to have child labour. They suggest that to increase child schooling requires facilitating access to credit with lower interest rates.

Empirical studies of credit impacts on child schooling provide mixed evidence on these two types of effect. Pitt and Khandker (1998) find that girl schooling increased when households borrowed from Grameen Bank, but when households borrowed from other microcredit programs no positive impacts on girl schooling were observed. In contrast, Hazarika and Sarangi (2008) in a study on rural Malawi find that children are more likely to work rather than go to school if their households have borrowed. In the case of Bangladesh, Morduch (1998) finds no effect on child schooling. Similarly, in the same country Islam and Choe (2009) even detect significantly adverse impacts of microcredit on child schooling.

3. Analytical framework

3.1 Data

A sample of 411 borrowing and non-borrowing households was interviewed in early 2008 in the peri-urban District 9, Ho Chi Minh City (HCMC) Vietnam.[2] Since our focus is on microcredit impacts on poor households, the sample was selected from a list of poor households whose initial income per capita was below the HCMC general poverty line of VND 6 million (approximately US$1 per day).[3] We employed a two-step sampling, first selecting wards and then households. The target sample size was set at 500 households, including 100 reserves, to achieve a realised sample of 400. In fact, 411 households were successfully interviewed, accounting for 26% of the total number of poor households in each of the selected wards in the district. The interviewed sample provides 304 borrowing households and 107 non-borrowing households, with 2,062 members, 955 (46.3%) males and 1,102 (53.7%) females, including 483 school-aged children. The sample is likely to be representative for the poor group whose initial income per capita is below the poverty line at

[2] HCMC has 24 Districts. District 9 has the 5th lowest population density, with a population of 227,816 (in 2008).

[3] The list was provided by the District Department of Labour, Invalids and Social Affairs.

the survey time in the district but will not be representative for Ho Chi Minh City nor for Vietnam.

The survey was designed to collect data on household and individual demographic-economic variables, commune characteristics, household durable and fixed assets, child schooling and education expenditure, healthcare, food, non-food, housing expenditure, and borrowing activities. We also utilised GPS receivers to collect data on locations of households and facilities in order to measure distances from each household to facilities.

3.2 Estimation methodological issues

The most difficult part of impact evaluations is to separate out the causal effect of credit from selection and reverse causation biases which are common to nearly all statistical evaluations (Armendariz & Morduch, 2010). For example, the longsighted and richer households often have easier access to credit and one has to ask whether household credit really affects the households' child schooling, or is it that the more education-motivated and richer parents simply are more likely to send their children to school as well as having easier access to credit. Therefore, there is a potential for selection bias here and for this reason the inference from estimated impacts on outcomes could be misleading.

In the literature on credit impact evaluation as, selection biases from non-random placement of credit and self-selection into credit participation by borrowers have received much attention since these may cause overestimates of impacts (Amin, Rai, & Topa, 2003). Apart from randomisation methods (e.g. Banerjee, Duflo, Glennerster, & Kinnan, 2009), other strategies and methods to reduce bias have been used, including treating new clients as a control group, examining discontinuities in client eligibility, potential or future clients and fixed effects (see Coleman, 1999, 2006; Islam, 2010; Morduch, 1998; Mosley, 1997; Pitt & Khandker, 1998; Roodman & Morduch, 2009). As a variant on these non-random methods, a purposely selected sample is used here to try to reduce the bias. All the households in our sample are poor, with initial income per capita under VND6 million (about US$1 per day) which makes them eligible for preferred (low interest rate and easy loan conditions) credits from the government. Thus, the non-random placement of credit borrowing should not be seriously problematic. In addition, the selection bias may be reduced by controlling for household pre-treatment income and parents' education, as suggested by Mosley (1997).

Some studies on schooling employ Two Stage Least Squares (2SLS) or Instrumental Variables (IV) (e.g. Behrman & Knowles, 1999; Maitra, 2003) to address selection and reverse causation biases. Demographic and educational characteristics of household heads, their jobs, household composition, and physical characteristics of dwellings are used as instruments. However, none of the studies applied the rigorous test for weak instruments suggested by Stock and Yogo (2002). Although the studies applied the test for endogeneity, the test is not able to ensure whether the instruments are good enough. For IV models, testing weak instruments using Maximum Likelihood Estimation (MLE) models is crucial (Murray, 2006); thus, using weak IVs could lead to upward biases, and the IV or 2SLS estimates could be worse than estimates by conventional estimators which treat credit participation as exogenous. In the current data, good instruments which affect credit participation but not child schooling are not available, we therefore apply only conventional Probit and Negative Binomial (NB) models.[4]

Probit and Negative Binomial model

Two outcomes of child schooling are examined here: current enrolment and the education gap. Analysis of the current enrolment is conducted using the standard Probit model. However, one single indicator e.g. grade attainment or current enrolment does not represent fully children's schooling because it does not indicate how well children did at school or whether or not children were grade-repeated. The education gap enables capture of this information, and it also represents how well children did at school. So the education gap may better reflect longer-term effects, while the current enrolment may reflect the immediate effect. The education gap is expressed as follows:

$$\text{Education gap} = \text{expected years of schooling} - \text{actual years of schooling}$$

$$\text{Expected years of schooling} = \begin{cases} 0 & \text{if age} \leq 6 \\ (\text{age} - 6) & \text{if } 6 < \text{age} \leq 18 \\ 12 & \text{if age} > 18 \end{cases}$$

The education gap can take positive integers from 0 to 12, thus the outcome of education gap is Poisson distributed, and a count data model is appropriate.

The count data model is well established (Cameron & Trivedi, 1986; Greene, 2008; Hausman, Hall & Griliches, 1984; Winkelmann, 2008, amongst others). Tabulating data on the outcome (Y) is a simple strategy to see the outcome distribution (Appendix 2). The smaller is the mean, the higher the proportion of zeros, so zero observations are an important feature of count data (Cameron & Trivedi, 2009).

$$\text{The Poisson model is:} \qquad \mu = E(y|x) = \exp(x'\beta) \tag{1}$$

where Y denotes the outcome (occurrences), Y= 0, 1, 2, …., N, and $y(t, t+\Delta t)$ denote the number of events/occurrences observed in the interval $(t, t+\Delta t)$. Then the number of occurrences in an interval of a given length is Poisson distributed with the probability density as follows:

$$\Pr(Y = y) = e^{-\mu}.\mu^{y}/y! \quad \text{where} \quad y = 0, 1, 2, \ldots, N$$

Conditional mean and variance of Y equal μ or $Var(Y) = E(Y) = \mu$. When controlling for some exogenous variables x, the parameter μ is now specified as follows:

$$\mu = \exp(x'\beta) \tag{2}$$

[4] Some potential IVs such as distance to banks, pre-treatment income and assets are used to conduct weak IV test, and all proved to be weak instruments.

The Poisson model is based on two assumptions. The *first* assumption is that events occur independently over time. The *second* assumption (called equidispersion, the key assumption of this model) is the equality of conditional mean and variance of dependent variable Y. In reality, equidispersion is commonly violated since count data is often overdispersed, that is the conditional variance exceeds the conditional mean (Cameron & Trivedi, 2009, p. 556). The distribution often has a longer right tail and the variance-mean ratio exceeds one. The presence of unobserved heterogeneity is one of the most common reasons for the violation to the second assumption. The Negative Binomial (NB) model can be a solution to this problem.

The NB model has *Gamma* distribution:

$$\mu = \text{gamma}(\phi, v) \tag{3}$$

where ϕ is mean and v is a precision parameter.

$$E[\mu]=\phi \text{ and } Var(\mu) = [1/v].\phi^2$$

$$\Pr[Y = y] = \int \Pr[Y = y \ \mid \mu] f(\mu) d\mu \tag{4}$$

With mean of dependent variable $E[Y] = \phi = \exp(X\beta)$, and

$$Var(Y) = \phi + (1/v).\phi^2 = E[Y] + (1/v).\ (E[Y])^2 = E[Y]\ [1+ (1/v).\ (E[Y])] \tag{5}$$

$$Var(Y)=E[Y].(1+\alpha.E[Y])$$

Because $\phi > 0$ and $v > 0$ then $Var(Y) > E(Y)$, and thus the model allows for overdispersion.

The test for Poisson models is based on tests for alpha $\alpha = 0$ against $\alpha \neq 0$. The Wald test is used to test the H_0: Poisson ($\mu = E[Y]$) against H_A: Negative binomial model with mean μ and variance $\mu(1+\alpha.\mu)$. These two different parameterisations (Poisson and NB) imply different assumptions about functional form of heteroscedasticity. In reality, the outcome distribution is commonly overdispersed so the second assumption of the Poisson model is violated. Therefore, the NB models are preferable to Poisson models. This is the case for our data on the education gap where we have a conditional mean of 1.145 and variance of 2.190 (Appendix 2).

Propensity Score Matching (PSM)

To corroborate the Probit and NB estimation findings, the PSM (propensity score matching) method is also used. PSM is able to reduce the bias in the conventional estimates since it only compares the treatment group's outcome with that of a similar control group. With PSM, matched comparison and treatment groups are similar in terms of propensity scores built on observable characteristics (Dehejia & Wahba, 2002). To evaluate the impact of credit participation on child schooling, PSM compares the schooling outcomes of children from borrowing households to what they would have had if their families did not borrow. Children from non-borrowers (who have the same or similar characteristics, such as demographic and socio-economic conditions which affect both credit participation and child schooling) are assumed to have the same outcomes that borrowers' children would have had if their parents had not borrowed. These children from non-borrower households can be used to generate a control group. So, what we need to do is to first estimate the propensity scores for each borrowing and non-borrowing household using household-level data and then merge the scores with the child-level data. The child-level data with the scores enables us to estimate the average treatment effects on child schooling using the PSM method.

4. Empirical Results

Descriptive analysis

Unconditional mean differences in child schooling of child group aged 6-18 years old between borrowers and non-borrowers are presented in Table 1. Roughly, children from borrowing households are better off (higher enrolment and lower education gap) than their non-borrower counterparts. However, the difference is insignificant. The difference in current enrolment between borrowers' children and non-borrowers' children is not very obvious. See Figure 1. The education gap may reflect outcomes of longer-term investment in schooling, since higher level education needs larger amounts of investment, and the poor are often both income-constrained and credit-constrained. Moreover, during the socio-economic reforms in Vietnam, cuts in public subsidies for higher education levels have pushed private education costs up. For these reasons, the education gap widens as child age increases. See Figure 2.

Figure 1: Enrolment rate by age and borrowing status

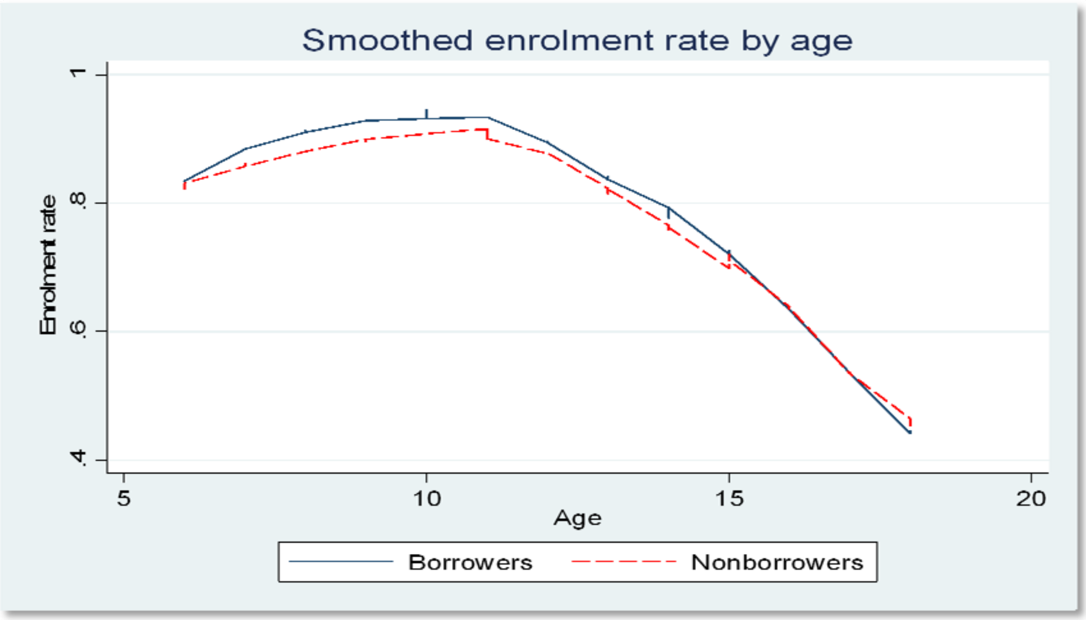

Figure 2: Education gap by age and by borrowing status

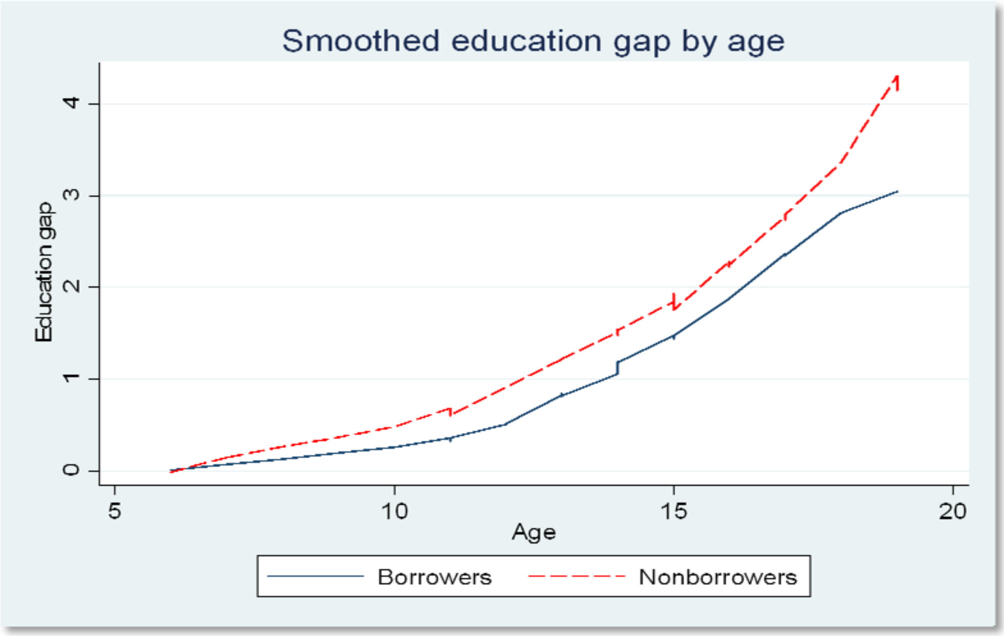

Table 1: Mean values of some key variables and t-values for equal means for the group of 6 to 18 years old children by borrowing status

Variables	Borrowers		Non-borrowers		*t*-value
	Mean	Std	Mean	Std	
Head's gender (male=1)	0.528	0.500	0.606	0.491	1.43
Parents' highest education (years)	5.551	3.333	5.452	3.585	0.26
Head married (yes=1)	0.723	0.448	0.730	0.446	0.16
Head's age (years)	50.501	13.762	57.625	15.300	4.30**
Household size (persons)	6.087	2.743	6.433	3.335	0.97
Younger siblings under 6 years (yes=1)	0.280	0.449	0.240	0.429	0.82
Children from 6 to 18 years old	1.942	0.963	2.096	1.187	1.22
Members from 18 to 60 years old	3.325	1.670	3.202	2.002	0.57
Members older than 60 (yes=1)	0.293	0.456	0.529	0.502	4.33**
Distance to nearest aged-range school	1.247	1.481	1.298	1.451	0.31
Child's gender (male=1)	0.451	0.498	0.5481	0.500	1.75+
Child's age (years)	12.823	3.708	13.096	3.693	0.67
Value of durable assets acquired over 24 months, land and house (in log)	13.149	1.180	12.702	1.929	2.25*
Pre-survey income per capita (in log)	8.115	0.234	8.102	0.389	0.32
Enrolment rate (children aged 6-18)	0.784	0.413	0.760	0.429	0.51
Education gap (children aged 6-18)	1.061	2.216	1.346	2.392	1.10
Enrolment rate (children aged 6-14)	0.917	0.276	0.911	0.288	0.16
Education gap (children aged 6-14)	0.265	0.750	0.429	0.951	1.20
Enrolment rate (children aged 15-18)	0.577	0.496	0.583	0.498	0.08
Education gap (children aged 15-18)	2.289	3.028	2.417	3.052	0.25

Notes: *t*-value statistically significant at 10% (+), 5% (*), and 1% (**).

Estimation results

Some current studies on schooling in Vietnam show that expenditure per capita (a proxy for household permanent income) is a good predictor of child schooling in Vietnam (Beegle, Dehijia, & Gatti, 2004; Behrman & Knowles, 1999). Accordingly, controlling for pre-treatment income, and assets as proxies for household wealth,is necessary. Furthermore, controlling for these initial variables can reduce selection bias as suggested by Mosley (1997) and can also avoid the problem of reverse causation bias that may occur if current income or expenditure is used.

Probit and Negative Binomial estimation results

Details of the education gap outcome distribution are presented in Appendix 2. The conditional mean is smaller than the variance so the distribution of the education gap is over-dispersed and has a longer right tail. Intuitively, the negative binomial models (NB) are appropriate in this case; however, to confirm this, we also run Poisson models and test for overdispersion, all the test results are statistically significant at the 1% level regardless of different alternative specifications; thus, the Poisson models are strongly rejected in favour of the NB model. The appropriateness of applying NB model is confirmed by the Wald test results (test for α = zero) in Tables 3, 5, 7, and 8.[5] The test results imply that using NB improves the fit of the models, and the NB standard errors are smaller than the Poisson standard errors giving indicate more efficiency gains from NB models.

The reported results start with estimates for child schooling using maximum likelihood Probit for current enrolment and maximum likelihood NB for education gap. Next, we consider whether the impacts for boys and girls are different. Following that, the impacts of different sources of credit are reported. Finally, we test whether or not the combination of credit and parental education (and income) helps child schooling.

In each model, we include all children aged 6-18 years old. There are likely potential sources of biases that are between-household selection (i.e. which household sends children to school or their children stay longer at school), and within-household selection (i.e. which children are kept at school or receive more investment from their parents). The first problem can be addressed by controlling for household characteristics including household initial income, initial assets, parental education, credit participation, head's gender, number of children, distance to the nearest school, household dwelling locations, and especially household weights placed on each child.[6] For the second source of bias, we control further

[5] Discussion on procedure for the test and choice of parameterisation is presented in Appendix 3. Alpha (α) can be interpreted as a measure of the variance of heterogeneity.

[6] Weights (scores) were estimated using PSM method, equal weight was placed on within-household children, but different weights were placed on between-household children. Weighted Probit and NB model estimates are not much different from those of the unweighted estimates since there are only about 1.05 children (aged 6 to 18) per household. The weighted estimates are reported in Appendix 4; 5, Appendix 6 and Appendix 7.

for child characteristics including child's gender, age and birth order. Schooling performance by children within a household may be influenced by child's IQ and parents' motivation (Bowles & Gentis, 2002). These factor effects can be captured by parental education, household income and assets. However, this leads to another potential problem that is the unobserved determinants of schooling, which are correlated across children within households. Thus, it may result in biased estimated standard errors (Deaton, 1997), and to correct the biased standard errors, robust clustered standard errors are estimated.

(a) Maximum Likelihood Probit for current enrolment and Maximum Likelihood Negative Binomial model for education gap

The estimates in Tables 2 and 3 indicate that the probability of current enrolment and the size of the education gap are not significantly influenced by household credit. This finding is similar to Cameron and Heckman (1998) and Carneiro and Heckman (2002); these studies show that family background factors rather than short-term credit constraints determine education outcomes. The finding is also consistent with the relevant literature (Morduch, 1998; Manski, 1997; Kane, 1994) which indicates that credit participation or credit constraints do not significantly affect school attendance. Doan, Gibson and Holmes (2011) used the same dataset of the current paper and indicated that education expenditure was positively influenced by credit participation for households who already sent their children to schools, and it is likely that level of education expenditure is a current choice, while a decision regarding sending children to school and children's academic attainment reflects longer-term investments, and as such is affected by family background and economic conditions. Our finding agrees with Keane and Wolfin (2001) who point out that credit would have greater effects on consumption and labour supply than school enrolment.

Table 2: Marginal effects of credit on current enrolment (Probit model)

Explanatory variables	Whole sample	Children aged 6-14	Children aged 15-18
Credit participation (yes=1)	-0.0032	-0.0139	0.0223
	(0.08)	(0.66)	(0.23)
Observations	483	286	197
Pseudo R-squared	0.30	0.24	0.22
Wald χ^2(all coefficients=0)	111.31	37.17	50.18
Prob >χ^2	0.000	0.001	0.000
Predicted probabilities at x-bar	0.858	0.965	0.596

Notes:
Robust z statistics in parentheses, + significant at 10%; * significant at 5%; ** significant at 1%; Column 1 is for the whole sample; Column 2 is for a sub-sample of children aged 6 to 14 (primary and lower secondary school ages); Column 3 is for a sub-sample of children aged 15 to 18 (high school ages). All the models were controlled for location dummies, distance to the nearest school (regarding to ages at different educational levels), child's age, age squared, first born child dummy, gender, and household labour force, number of school-aged children, parental highest education, household head's gender, and pre-treatment income per capita and assets.

Table 3: Negative Binomial Regression (NB2) for credit impact on education gap

Explanatory variables	Whole sample	Child aged 6-14	Child aged 15-18
Credit participation	-0.0313	0.0290	0.0168
(yes=1)	(0.15)	(0.09)	(0.07)
Constant	-1.8093	3.9100	-6.6078
	(0.68)	(1.45)	(1.63)
Observations	483	286	197
Wald χ^2 (all coefficients=0)	222.96	79.24	40.79
Prob $>\chi^2$	0.000	0.000	0.000
Alpha $\alpha^{(a)}$	1.3868	1.2798	1.2193
	(5.85)**	(1.91)+	(5.45)**

Notes:

[a]The alpha parameter, highly significant, means that the Negative Binomial regression is an appropriate approach. Model in column 2, the test of □=0 is accepted at the 5% level, either the Poisson or NB can be applied in this case. The estimated results by the NB and Poisson estimators are similar. Robust z statistics in parentheses, + significant at 10%; * significant at 5%; ** significant at 1%; Column 1 is for the whole sample; Column 2 is for a sub-sample of children aged 6 to 14 (primary and lower secondary school ages); Column 3 is for a sub-sample of children aged 15 to 18 (high school ages). All the models were controlled for location dummies, distance to the nearest school, child's age, age squared, first born child dummy, gender, and household labour force, number of school-aged children, parental highest education, household head's gender, and pre-treatment income per capita and assets.

Small loans are not an appropriate way of financing education investment. We observed from fieldwork that the loans of the poor in the peri-urban areas are often very short-termed, one year or less, especially loans from the informal credit sector; hence they are not used to support long-term investment in schooling. Moreover, in less developed countries, though many households borrowed, they were still credit-constrained because they were lent amounts smaller than those they demanded (Conning & Udry, 2007). Their loans were too small to finance long-term education investments,[7]particularly larger lump sums for tuition and registration fees for new schooling year (Mason & Rozelle, 1998). As a result, short and small loans affect only current education expenditure; bigger and longer-term loans are required to improve child enrolment and reduce education gap. This agrees with Islam (2010) who suggests that longer credit participation and larger loans could bring out benefits since it takes time to have effects.

Higher schooling fees and foregone earnings of older children would change roles of credit participation. Intuitively, one may think that the effects at upper levels of education would be higher than at lower levels. In order to examine the varying effects at different age groups, we run separate models for different age groups: 6-14 (primary & lower secondary school) and 15-18 (high school). Results in Tables 2 and 3, columns 2 and 3, respectively show no evidence of significant impacts of microcredit on enrolment rate at any level from 6 to 18 years old. This is also true for the education gap. The finding supports the previous discussion on trivial roles of small loans for child schooling.

[7] For our surveyed households, an average loan size for education is about US$220, and is one of the smallest loan sizes of households compared to US$690 for other purposes (excluding consumption loans).

(b) The impacts of household credit on child schooling for boys and girls

In developing countries parents are biased in favour of boys over girls in human capital investment such as education. The literacy gender gaps are empirically examined to be very high in all developing regions (Wils & Goujon, 1998). To examine whether the trend is true in peri-urban areas in Vietnam, one could partition the sample into boy and girl groups and estimate two separate regressions. Small subsamples, however, may reduce the statistical significance of the estimates. We therefore employ an alternative approach to test the equality of credit variable coefficients between the two groups. We include interactions between each variable with a dummy of children's gender (boy=1) as additional variables. When the child gender dummy takes a value of zero (i.e. girls), all the interaction term coefficients equal zero, so the non-interacted coefficients provide effects for the girl group. On the other hand, child gender is one, the interacted term coefficients provide boy-girl difference estimates.

Tables 4 and 5 report the impacts of credit participation on female child schooling and the boy-girl difference in the impact. For the whole sample, female children from borrowing households have 9% more probability of current enrolment than same-sex children from non-borrowing households, but the effects are not statistically significant. For the younger group of primary and lower secondary education, the effect on girls' enrolment is in the same direction and statistically significant at the 5% level.[8] The effect difference between boys and girls is about (negative) 17% (i.e. the effect on boy schooling is about -8%), and it is strongly significant at the 1% level for the younger child group (Table 4). In short, when households borrowed, girls were better off but boys were worse off.

Table 4: Marginal effect of credit on current enrolment by gender (Probit)

Explanatory variables	Children aged 6-18		Children aged 6-14	
	Girl	Boy-girl difference[(a)]	Girl	Boy-girl difference[(b)]
Credit participation (yes=1)	0.0915	-0.1712	0.0496	-0.2276
	(1.41)	(1.93)+	(2.37)*	(3.54)**
Pseudo R-squared	0.35		0.39	
Wald χ^2 (all coefficients =0)	135.74		84.70	
Prob $>\chi^2$	0.000		0.000	
Observations	483		286	

Notes:
Robust z statistics in parentheses, + significant at 10%; * significant at 5%; ** significant at 1%; [(a)] and [(b)] are coefficients of interaction terms between the explanatory variables and child's gender dummy (boy =1). All the models were controlled for location dummies, distance to the nearest school, child's age, age squared, first born child dummy, gender, and household labour force, number of school-aged children, parental highest education, household head's gender, and pre-treatment income per capita and assets.

[8] Because of the small subsample of group aged 15 to 18, separate male and female groups are too small to run regressions.

Table 5: Impact of credit participation on education gap by gender (NB model)

Explanatory variables	Aged 6-18		Aged 6-14	
	Girl	Boy-girl difference[(a)]	Girl	Boy-girl difference[(b)]
Credit participation (yes=1)	-0.2616	0.3726	-0.7496	1.1840
	(0.93)	(0.94)	(1.64)+	(1.98)*
Constant		-0.1659		4.2791
		(0.04)		(0.73)
Alpha (α)	1.3918		0.6967	
	(5.96)**		(1.57)	
Wald χ^2 (all coefficients = 0)	292.74		131.02	
Prob $>\chi^2$	0.0000		0.0000	
Observations	483		286	

Notes:
Robust z statistics in parentheses; + significant at 10%; * significant at 5%; and ** significant at 1%. [(a)] and [(b)] are coefficients of interaction terms between the explanatory variables and child's gender dummy (boy =1). All the models were controlled for location dummies, distance to the nearest school, child's age, age squared, first born child dummy, gender, and household labour force, number of school-aged children, parental highest education, household head's gender, and pre-treatment income per capita and assets.

The NB model estimates (Table 5) provide similar results of the effects by gender. For the whole sample, household credit participation leads to a decline of 0.26 points in education gap for girls but leads to an increase of 0.37 points [0.37=0.11-(-0.26)] in education gap for boys. Roughly, this finding implies that the effect is heterogeneous across child gender: Girls benefit from household credit participation, but the credit adversely affects on boys' schooling.

Furthermore, girls' better academic performance is likely to help keep them at school longer and to receive more investment from their parents, that can be used to explain the positive impact on girls. Moreover, in the peri-urban areas in South Vietnam the traditional viewpoint of 'valuing boys above girls or preferring boys to girls' has been increasingly weakened in recent times. Though the effects are not highly significant, our finding is contrary to Islam and Choe (2009) that microcredit in Bangladesh has negative impacts on both boys and girls, and the impact is (negative) stronger for girls than for boys. It is also contrary to the finding of positively significant effects of microcredit on child education, especially the impact for boys (Pitt & Khandker, 1998).

(c) The impacts of different sources of household credit

To evaluate whether different sources of credit matter in the impact on child schooling, we classify the borrowers into three groups: Households that borrowed from formal credit, households that borrowed from informal credit, and households that borrowed from both informal and formal credit.

The estimates in Tables 6 and 7 show that formal credit positively affects child schooling, whereas informal credit adversely affects child schooling; and the effects of both informal and formal credit are stronger for the high school children group. To see whether the effects of informal and formal credit are different, we conduct the parameter test for difference between coefficients of formal credit and informal credit, and the test reveals that the difference is statistically significant for both current enrolment (Table 6) and education gap (Table 7). The difference, however, mostly comes from the group of high school-aged children because older children can participate in the labour force when their parents need more labour, especially labour without wages, to reduce business costs in order to repay high interest rate loans from informal credit. This finding is similar to Beegle, Dehijia and Gatti (2004) who find that children from households who borrowed from informal credit sources may have to leave school because their parents may be too poor to afford schooling fees and may need extra labour for their family businesses. In addition, short-term and small loans from informal credit are not suitable for greater schooling costs, especially high schools.

Table 6: Marginal effects on enrolment status by types of credit (probit model)

Explanatory variables	Whole sample	Children aged 6-14	Children aged 15-18
Informal credit (yes=1)	-0.0639	-0.0002	-0.1461
	(1.25)	(0.01)	(1.19)
Both sources of credit (yes=1)	-0.0160	-0.0409	0.0037
	(0.33)	(1.33)	(0.03)
Formal credit (yes=1)	0.0637	-0.0121	0.1959
	(1.25)	(0.39)	(1.70)+
H_0: $\beta_{informal} = \beta_{formal}$ (P-value)	0.019*	0.672	0.007**
Pseudo R-squared	0.31	0.25	0.25
Wald χ^2 (all coefficients = 0)	110.89	47.94	57.79
Prob $>\chi^2$	0.0000	0.0000	0.0000
Observations	483	286	197

Notes:

Robust z statistics in parentheses, + significant at 10%; * significant at 5%; ** significant at 1%; the reference group for credit types is non-borrowers. All the models were controlled for location dummies, distance to the nearest school, child's age, age squared, first born child dummy, gender, and household labour force, number of school-aged children, parental highest education, household head's gender, and pre-treatment income per capita and assets.

Table 7: Impact on education gap by type of credit (NB model)

Explanatory variables	Whole sample	Children aged 6-14	Children aged 15-18
Informal credit (yes=1)	0.1006	-0.1181	0.2852
	(0.44)	(0.28)	(1.02)
Both sources of credit (yes=1)	0.0995	0.2099	0.1184
	(0.40)	(0.52)	(0.44)
Formal credit (yes=1)	-0.3411	0.0239	-0.3897
	(1.25)	(0.06)	(1.25)
Constant	-1.4565	4.2060	-5.2948
	(0.57)	(1.50)	(1.35)
H_0: $\beta_{informal} = \beta_{formal}$ (P-value)	0.084+	0.730	0.035*
Alpha (α)	1.3488	1.2806	1.1611
	(5.8)**	(1.91)+	(5.3)**
Wald χ^2 (all coefficients = 0)	231.47	88.34	50.89
Prob >χ^2	0.0000	0.0000	0.0000
Observations	483	286	197

Notes:
Robust z statistics in parentheses, + significant at 10%; * significant at 5%; ** significant at 1%; Model in column 2, the test of α=0 is accepted at the 5% level, either the Poisson or NB can be applied in this case. The estimated results by the NB and Poisson estimators are similar. The reference group for credit types is non-borrowers. All the models were controlled for location dummies, distance to the nearest school, child's age, age squared, first born child dummy, gender, and household labour force, number of school-aged children, parental highest education, household head's gender, and pre-treatment income per capita and assets.

(d) Does combination of credit with parental education (and with income) helps child schooling?

This question is motivated by the existing literature, which has shown that credit itself is not able to help the poor effectively. For example, using Bangladesh data Pitt and Khandker (1998) found that girl schooling increased when households borrowed from Gameen Bank, but when households borrowed from other microcredit programs positive impact on girl schooling was not observed. Intuitively, the combination of credit and manifestation of children's schooling benefits in the Grameen Bank group meetings, not microcredit itself, may account for the positive effects on children schooling.

Higher educated parents are often longsighted for their children's future livelihood, thus a combination of parental education and credit would accelerate the effects on child schooling. Therefore, to test whether parental education plays a role in accelerating the effect of credit usages in child education, we use an interaction term between credit and the highest parental education (of either husband or wife). The interaction term may capture the effect of parental education on child schooling within the borrowing household group. Moreover, families with more educated parents may have higher incomes; households with lower incomes among the poor may be too poor to afford child schooling costs, while less poor households are able to afford schooling if they have additional money from borrowing.

Therefore, one may think that credit to richer households in the poor group may have stronger effects on child schooling.

We also use another interaction term between credit and pre-treatment income per capita to test whether among the borrower households, households with higher income have greater impacts on child schooling. The estimate results when estimating with inclusion of the interaction terms are presented in Table 8. The effects of both interaction terms are not statistically significant. The results suggest that amongst borrowing households children from higher educated and higher income parents also do not benefit from household credit for their schooling. In other words, there is no accelerator of parental education and initial income on the impact amongst poor borrowers because education of the poor parents is so low, only 5.5 years (achieved just primary school level) relative to that of parents in general in Vietnam - about 8.9 years of education (VHLSS, 2006).[9] Further, the return to schooling of lower education is very low (Doan & Gibson, 2009), hence the poor may not have been aware of educational benefits and may have had little motivation to increase their children's education.

Table 8: Effect of interaction terms between credit and parental education and credit and household income

Explanatory variables	dprobit model[(a)]	NB model
Credit participation (yes =1)	0.7214	-1.2088
	(0.55)	(0.27)
Credit participation*income per capita	-0.0674	0.0849
	(0.58)	(0.16)
Credit participation*highest	0.0035	0.0918
parental education	(0.26)	(1.26)
Constant		-1.4549
		(0.50)
Observations	483	483
Wald χ^2 (all coefficients = 0)	114.17	224.50
Prob $>\chi^2$	0.0000	0.0000
Pseudo R-squared	0.30	
Alpha (α)		1.3849
		(5.9)**

Notes:
Robust z statistics in parentheses, + significant at 10%; * significant at 5%; ** significant at 1%; [(a)]dprobit model estimates marginal effects. All the models were controlled for location dummies, distance to the nearest school, child's age, age squared, first born child dummy, gender, and household labour force, number of school-aged children, parental highest education, household head's gender, and pre-treatment income per capita and assets.

[9] This figure is estimated for general household head's education, if the highest parental education of either husband or wife is estimated, the years of education would be higher.

Propensity Score Matching (PSM) estimation results

In order to corroborate the Probit and NB estimation results, we apply PSM methods to (i) binary treatment (household borrowed or not) and (ii) multiple treatment effect models. The matching methods (kernel and radius) are used for the whole sample and for a sub-sample of households having children aged 6-18 years old. According to Bryson et al (2002), controlling variables used to estimate scores should affect both credit participation and child schooling outcomes. The variables include household head's gender, head's age, parental education, household head's marital status, number of children aged 6-18 years old, household members aged 18-60 years old, initial income per capita in logarithm, initial assets in logarithm, and household location dummies.

The estimates are presented in Tables 9 and 10. *First*, the PSM estimation using the binary treatment effect shows that credit participation does not strongly affect child schooling (Table 9). Roughly, the PSM estimates are consistent with the Probit and NB model estimates.

Second, the multiple-treatment effect estimator in turn compares child schooling outcomes of informal borrowers and formal borrowers with that of the similar non-borrowers.[10] The estimates show that only formal credit affects the poor's child schooling, participation in formal credit improves the likelihood of enrolment and reduces the education gap (Table 10). Effect comparison between the formal and informal borrowing groups, however, would be inappropriate due to different counterfactuals of both these groups. Direct comparison in the last column of Table 10 is used to overcome the problem of incomparable counterfactuals. The informal credit with a smaller (accumulated) loan amount per household (about US$500 on average) and with short-terms is not sufficient to support child schooling, whereas formal credit (with about US$920) is beneficial to child schooling.[11] The multiple treatment effects analysis confirms the effects of household formal credit on child schooling. These findings also corroborate the Probit and NB model estimates.

[10] The multiple treatments also help detect potential bias associated with unobservable characteristics in estimates of binary treatment effects (Lee, 2005, p. 119). If treatment level is increased (bigger loan size, here is the formal credit), then the effect will be stronger. Assume that our expectation is a positive effect, but the expectation is not confirmed by multiple ordered treatments, then the initial causal findings (from binary treatment) are questionable and may have been due to some unobserved attributes. On the other hand, if there is no hidden bias, the treatment effect of formal credit is higher than the effect of informal credit; in turn, the effect of informal credit is greater than the observed outcome for the non-borrowing group, controlling for the same set of covariates X_i.

[11] The average loan size is VND5,229 thousand (about USD317) and VND9,327 thousand (about USD566) for informal and formal credit respectively, since many households have more than one loans so the reported sizes of loan in the text are accumulated ones. Note that not all of these amounts are for education, but they are used for all purposes.

Table 9: The Average Treatment Effects using matching estimators

Propensity score estimation stage	Outcome of schooling	Treated/ controls	Kernel matching	Radius matching
A subsample (households with children aged 6-18)	Current Enrolment	370/84	0.017	0.031
			(0.054)	(0.061)
	Education Gap (year)	370/84	-0.167	-0.194
			(0.297)	(0.300)
Whole sample	Current Enrolment	379/98	0.010	0.010
			(0.052)	(0.054)
	Education Gap (year)	379/98	-0.146	-0.138
			(0.282)	(0.292)

Notes:
Bootstrapped standard errors in parentheses with 1000 repetitions, statistically significant at 10% (+); 5%(*); 1%(**). Only few households (10 households) have more than or equal 4 children aged 6-18, to get balanced easier we group them into households having 4 kids. Controlling variables in the propensity score estimation (propensity score estimation with household level data): Head's gender, head's age, parental highest education, marital status, children aged 6-18, members aged 18-60, initial income in log, initial assets in logarithm, location dummies.

Table 10: The Average Treatment Effects using matching estimators with whole sample in propensity score estimation

Outcome of schooling	Informal credit vs Non-borrowers		Formal credit vs Non-borrowers		Formal vs Informal
	ATTK	ATTR	ATTK	ATTR	ATTR
Current Enrolment	-0.024	-0.025	0.140	0.105	0.129
	(0.072)	(0.075)	(0.058)*	(0.052)*	(0.054)*
Education Gap (year)	0.023	0.043	-0.746	-0.665	-0.745
	(0.395)	(0.379)	(0.300)*	(0.293)*	(0.273)**

Notes:
Bootstrapped standard errors in parentheses with 1000 replications, statistically significant at 10% (+); 5%(*); 1%(**). Controlling variables in the propensity score estimation: Head's gender, head's age, highest parental education, marital status, location dummies, number of children aged 6-18, number of members aged 18-60, initial income in logarithm, initial assets in logarithm, and head's age*education.

5. Discussion and concluding remarks

This study evaluates the impact of household credit on child schooling of the poor in the peri-urban areas in Vietnam. The paper delivers the following conclusions:

First, the small sized and short-term loans fail to help improve the poor's child schooling. *Second*, the effect of household credit varies across child gender. Girls are more likely to receive more education investment and stay longer at school. The finding contrasts with the existing literature on the differences in boy-girl schooling impacts in South Asia, which indicates that microcredit benefits boys more than girls or affects girls more adversely than boys. Furthermore, evidence of the traditional view of 'boys over girls', even though it is common in other similar developing countries, was not observed in this peri-urban area of HCMC, Vietnam. Girls' better schooling performance helps keep them at school longer and hence they receive more investment in education from their parents.

Third, a closer look at impacts of each credit source reveals that formal credit has brought beneficial effects to children's education, while informal credit has failed to do so. Consequently, to improve child schooling in the long term needs to ease access to formal credit for the poor. Otherwise, the poor will continue to rely on informal credit and will end up in debt and will then pull their children out of school. Consequently, informal credit may exacerbate poverty in the long term rather than help the poor out of poverty. The poor are both income and credit constrained, so government interventions such as facilitating formal credit access are needed (Caucutt & Lochner, 2005). The poor need a 'big push' to break down the vicious circle of poverty.

Providing subsidies or tuition exemption to all children is an impossible solution in poor countries like Vietnam since it may pose a burden on the government budget. An alternative is to target subsidies to low-income household child schooling. In fact, the current tuition exemption policy in Vietnam is ineffective to help poor children because the tuition accounts for just less than one third of total education costs, and almost all school fee exemptions are for primary schools regardless of parental income levels. Only 1% of the tuition exemption value is for children from poor households and 4.3% is for ethnic minorities (Behrman & Knowles, 1999, p. 230). Therefore, expanding preferred loans or fully tuition exemption to the poor, as well as providing subsidies for textbooks, uniforms, study materials and other school fees is a further necessary policy to encourage poor children to go to school and keep them at school longer.

In Vietnam, the greater school expenditure, which is influenced by household budget constraints, may relate to obtaining higher quality schooling and better academic performance from participating in extra classes (Dang, 2007). Therefore, credit still has an important role in education investment. However, regulated tuition levels by the government could partly undermine the effects of credit on schooling.

Appendices

Appendix 1: Smoothed child enrolment ratio and education gap by age

Child Age	Enrolment rate (%)		Education gap (years)	
	Borrowers	Non-borrowers	Borrowers	Non-borrowers
6	83.3	82.2	0.01	0.00
7	87.9	85.6	0.07	0.13
8	91.3	88.1	0.13	0.26
9	93.3	89.9	0.19	0.37
10	94.4	91.6	0.26	0.48
11	93.4	91.5	0.33	0.62
12	89.4	87.9	0.50	0.91
13	85.0	83.4	0.81	1.23
14	79.5	77.8	1.07	1.52
15	72.5	71.7	1.44	1.77
16	63.5	63.9	1.89	2.23
17	53.6	53.4	2.40	2.75
18	44.6	46.2	2.81	3.36

Notes: Bandwidth (a smoothing parameter) = 0.9 is chosen in the Lowess (locally weighted scatter-plot smoothing estimator) command in Stata®. This information is used to graph Figure 1 and Figure 2.

Appendix 2: Mean and variance of education gap for children aged 6-18

Variable	Observations	Mean	Variance	Std.Dev	Min	Max
Unconditional	483	1.122	5.087	2.255	0.000	12
Conditional	483	1.145	2.190	1.480	0.019	12

Source: Estimation from the authors' survey.

Tabulation of education gap for children from 6 to 18 years old

Education gap	Frequency	Percent	Cumulative
0	31	6.42	6.42
1	284	58.80	65.22
2	64	13.25	78.47
3	32	6.63	85.09
4	17	3.52	88.61
5	14	2.90	91.51
6	8	1.66	93.17
7	12	2.48	95.65
8	4	0.83	96.48
9	1	0.21	96.69
10	8	1.66	98.34
11	3	0.62	98.96
12	5	1.04	100.00
Total	483	100.00	100.00

Source: Estimation from the authors' survey.

Appendix 3: Choice of Negative Binomial Models

NB models fit two different parameterisations of the NB model:Negbin I or NB_1: $(Var(Y)=(1+\delta)E[Y]$ - a linear variance function), andNegbin II or NB_2: $(Var(Y)=E[Y].(1+\alpha.E[Y])$ - a version with quadratic variance. The NB_2 has dispersion (ratio of variance/mean) for the i^{th} observation equal to $1+\alpha.E[Y_i]$ i.e., the dispersion is a function of the expected mean of the counts for the j^{th} observation: $E[Y_i]$. The alternative parameterisation, NB_1, has dispersion equal to $1 + \delta$; i.e. it is a constant for all observations. If alpha $\alpha = 0$ (or delta $\delta =0$) corresponds to dispersion = 1, thus it is simply a Poisson model. One may want to fit both parameterisations NB_1 and NB_2, and choosing either of them rely on larger (least negative) log pseudo likelihood. In most cases, however, both models will yield similar results, and the parameterisations will not significantly differ from one another. Thus, the choice of parameterisation is not important (Cameron & Trivedi, 2009).

A common approach to deal with the overdispersion for count data is to use the generalised linear model including NB (Hoef & Boveng, 2007) because the overdispersion parameter can vary across individuals so some variables can affect the location and scale parameters of the distribution, therefore, the generalised NB model which allows the different effects of different variables on the location and the scale of the distribution (Cameron & Trivedi, 2009). In our case, we compare the regression statistics, e.g. Log pseudo likelihood, and see that both NB_2 and generalised NB produced identical statistics. As a result, we apply only NB_2 in the current research.

Appendix 4: Marginal effects of credit on current enrolment (weighted Probit estimates)

Explanatory variables	Whole sample	Children aged 6-14	Children aged 15-18
Credit participation (yes=1)	-0.0028	-0.0116	0.0154
	(0.07)	(0.51)	(0.16)
Pre-treatment income capita in Logarithm	0.0142	0.0583	-0.2567
	(0.21)	(1.54)	(1.42)
Pre-treatment asset in logarithm	0.0119	0.0015	0.0095
	(0.95)	(0.27)	(0.27)
Highest parental education (year)	0.0261	0.0127	0.0439
	(4.06)**	(3.68)**	(3.02)**
Household head's gender (male=1)	0.0341	0.0123	0.0529
	(0.91)	(0.52)	(0.63)
Number of children aged 6-18	-0.0674	-0.0306	-0.1419
	(3.75)**	(2.67)**	(3.46)**
Labour force	-0.0060	0.0007	0.0025
	(0.67)	(0.16)	(0.10)
Child's gender (male=1)	-0.0900	0.0068	-0.3028
	(2.47)*	(0.36)	(3.69)**
Firstborn child (yes =1)	0.0228	-0.0331	0.1872
	(0.58)	(1.23)	(2.15)*
Child's age	0.1791	0.0944	-0.1939
	(4.69)**	(2.74)**	(4.12)**
Child's age squared	-0.0090	-0.0046	
	(5.64)**	(2.71)**	
Distance to the nearest school	0.0007	-0.0141	0.0366
	(0.05)	(0.96)	(1.00)
Observations	483	286	197
Pseudo R-squared	0.30	0.21	0.23
Wald χ^2(all coefficients=0)	112.54	33.01	51.58
Prob $>\chi^2$	0.000	0.001	0.000
Predicted probabilities at x-bar	0.86	0.96	0.59

Notes:
Robust z statistics in parentheses, + significant at 10%; * significant at 5%; ** significant at 1%; Column 1 is for the whole sample; Column 2 is for a sub-sample of children aged 6 to 14 (primary and lower secondary school ages); Column 3 is for a sub-sample of children aged 15 to 18 (high school ages). All the models were controlled for location dummies.

Appendix 5: Credit impact on education gap (weighted NB estimates)

Explanatory variables	Whole sample	Child aged 6-14	Child aged 15-18
Credit participation	-0.0173	0.0481	0.0111
(yes=1)	(0.08)	(0.14)	(0.05)
Pre-treatment income	-0.0887	-0.8237	0.3785
capita in logarithm	(0.25)	(1.64)	(0.96)
Pre-treatment asset	-0.0717	-0.0949	-0.0308
in logarithm	(1.07)	(0.93)	(0.42)
Highest parental education	-0.0832	-0.1550	-0.0504
(years)	(2.48)*	(2.47)*	(1.38)
Household head's gender	-0.0915	-0.3405	0.0872
(male=1)	(0.54)	(1.03)	(0.46)
Number of children	0.2021	0.2458	0.2040
aged 6-18	(2.57)*	(1.54)	(2.06)*
Labour force	0.0253	0.0374	-0.0243
	(0.50)	(0.46)	(0.46)
Child's gender (male=1)	0.3967	0.3653	0.4309
	(2.32)*	(1.22)	(2.11)*
Firstborn child (yes =1)	0.1191	0.1748	-0.0300
	(0.61)	(0.47)	(0.14)
Child's age	0.3501	0.2699	0.3557
	(8.77)**	(4.56)**	(2.99)**
Distance to the nearest	-1.0308	-1.5100	-1.1781
School	(2.63)**	(2.63)**	(2.87)**
Constant	-3.3714	4.4675	-7.8268
	(1.01)	(1.09)	(1.70)+
Observations	483	286	197
Wald χ^2 (all coefficients=0)	225.64	58.02	41.56
Prob $>\chi^2$	0.000	0.000	0.000
Alpha α	1.3862	1.5292	1.3862
	(5.71)**	(2.14)*	(5.71)**

Notes:
Robust z statistics in parentheses, + significant at 10%; * significant at 5%; ** significant at 1%; Column 1 is for the whole sample; Column 2 is for a sub-sample of children aged 6 to 14 (primary and lower secondary school ages); Column 3 is for a sub-sample of children aged 15 to 18 (high school ages). All the models were controlled for location dummies.

Appendix 6: Marginal effects on enrolment status by types of credit (weighted probit estimates)

Explanatory variables	Whole sample	Child aged 6-14	Child aged 15-18
Informal credit	-0.0590	0.0029	-0.1390
	(1.14)	(0.11)	(1.13)
Both sources of credit	-0.0161	-0.0344	-0.0093
	(0.33)	(1.12)	(0.08)
Formal credit	0.0662	-0.0093	0.1909
	(1.28)	(0.29)	(1.63)
Pre-treatment income capita in logarithm	0.0218	0.0610	-0.2256
	(0.33)	(1.83)+	(1.28)
Pre-treatment asset in log	0.0142	0.0014	0.0151
	(1.12)	(0.26)	(0.42)
Highest parental education	0.0245	0.0137	0.0434
	(3.99)**	(4.36)**	(3.07)**
Household head's gender (male=1)	0.0277	0.0024	0.0379
	(0.74)	(0.12)	(0.43)
Number of children aged 6-18	-0.0630	-0.0272	-0.1302
	(3.51)**	(2.50)*	(3.15)**
Labour force	-0.0080	0.0021	-0.0083
	(0.89)	(0.50)	(0.33)
Child's gender (boy=1)	-0.0895	0.0095	-0.2953
	(2.50)*	(0.52)	(3.57)**
First born child (yes=1)	0.0211	-0.0292	0.1710
	(0.54)	(1.17)	(1.92)+
Child's age	0.1761	0.0896	-0.1806
	(4.68)**	(2.79)**	(3.83)**
Child's age squared	-0.0089	-0.0043	
	(5.62)**	(2.76)**	
Distance to the nearest school	0.1062	0.0335	0.2395
	(1.78)+	(1.23)	(1.53)
H_0: $\beta_{informal} = \beta_{formal}$ (P-value)	0.024*	0.6633	0.012*
Pseudo R-squared	0.31	0.22	0.25
Wald χ^2 (all coeffs=0)	112.64	43.91	58.68
Prob $>\chi^2$	0.0000	0.0000	0.0000
Observations	483	286	197

Notes:
Robust z statistics in parentheses, + significant at 10%; * significant at 5%; ** significant at 1%; All the models were controlled for location dummies.

Appendix 7: Impact on education gap by type of credit (weighted NB estimates)

Explanatory variables	Whole sample	Child aged 6-14	Child aged 15-18
Informal credit	0.0893	-0.1129	0.2458
	(0.38)	(0.26)	(0.91)
Both sources of credit	0.1084	0.2479	0.1147
	(0.43)	(0.60)	(0.44)
Formal credit	-0.3230	0.0149	-0.3755
	(1.16)	(0.03)	(1.21)
Pre-treatment income capita in logarithm	-0.1383	-0.9054	0.3026
	(0.40)	(1.75)+	(0.80)
Pre-treatment asset in Logarithm	-0.0741	-0.0899	-0.0312
	(1.14)	(0.84)	(0.45)
Highest parental education (years)	-0.0812	-0.1620	-0.0484
	(2.55)*	(2.57)*	(1.42)
Household head's gender (male=1)	-0.0454	-0.2746	0.1184
	(0.26)	(0.84)	(0.60)
Number of children aged 6-18	0.1962	0.2303	0.1853
	(2.50)*	(1.45)	(1.90)+
Labour force	0.0274	0.0196	-0.0024
	(0.57)	(0.25)	(0.05)
Child's gender (boy=1)	0.4073	0.3147	0.4156
	(2.42)*	(1.06)	(2.07)*
First born (yes=1)	0.1339	0.1684	0.0046
	(0.69)	(0.46)	(0.02)
Child's age	0.3439	0.2679	0.3048
	(8.67)**	(4.55)**	(2.58)*
Distance to the nearest school	-0.9786	-1.4678	-1.0661
	(2.53)*	(2.56)*	(2.60)**
Constant	-2.8717	5.1939	-6.4822
	(0.89)	(1.20)	(1.47)
H_0: $\beta_{informal} = \beta_{formal}$ (P-value)	0.1104	0.7624	0.0520+
Alpha α	1.3526	1.5237	1.1393
	(5.73)**	(2.13)*	(5.31)**
Wald χ^2 (all coefficients = 0)	232.35	67.54	51.07
Prob $>\chi^2$	0.0000	0.0000	0.0000
Observations	483	286	197

Notes:

Robust z statistics in parentheses, + significant at 10%; * significant at 5%; ** significant at 1%; Model in column 2, the test of □=0 is accepted at the 5% level, either the Poisson or NB can be applied in this case. The estimated results by the NB and Poisson estimators are similar. All the models were controlled for location dummies.

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