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Impact of Credit on Education and Healthcare Spending in Rural Pakistan

ABID HUSSAIN, MUHAMMAD JEHANGIR KHAN and IFTIKHAR AHMAD

It is to access that the microcredit has a positive impact on education and healthcare spending of the borrowed households is controversial in developing countries literature or not. This study reports evidence, from Pakistan for this debate, while utilising data from PPHS-2010 (Pakistan Panel Household Survey). Propensity score matching (PSM) has been used to investigate the impact of household credit on healthcare and education spending by the poor. In addition to matching statistically identical borrowers with non-borrowers, the method controls for household pre-treatment assets and income. These may be correlated with unobservable factors affecting credit participation as well as outcomes of interest. The estimates from binary as well as multiple ordered treatment effect show insignificant impact of borrowing on education and significant and positive impact on healthcare spending.

JEL Classification: D13, C14
Keyword: Matching; Household Credit; Per-capita Income; Education and Healthcare Spending.

1. INTRODUCTION

Microcredit has increasingly attracted attention from the global development community because it is considered a powerful tool in poverty alleviation strategies in developing countries [Microcredit Summit (2009)]. A common argument for microcredits that it may help in keeping household production stable and mitigate adverse shocks, thus it helps to prevent school dropout and reduction in spending on healthcare [Armendariz and Morduch (2005)]. The effects on education and health are critical to sustainable poverty reduction since it affects the quality of human capital formation and thus productivity of future generations.

But there is a debate about the impact of microcredit on education and healthcare of borrowing of household [Cull, Kunt, and Morduch (2009)]. For example, access to credit may raises female economic activity which in turn lead to children may begin taken out of school to replace maternal inputs in the care of young siblings or work in expanded household businesses. The debate has resulted from mixed evidence on microcredit impacts.

Positive impact of microcredit on education has also been reported. For example, Pitt and Khandker (1998) find that girls receive more schooling if household borrow from

Abid Hussain is MPhil Research Scholar, Pakistan Institute of Development Economics, Islamabad. Muhammad Jehangir Khan <jehangir@pide.org.pk> is Assistant Professor, Pakistan Institute of Development Economics, Islamabad. Iftikhar Ahmad <iftikhar@pide.org.pk> is Assistant Professor, Pakistan Institute of Development Economics, Islamabad.

the Grameen Bank (GB). On the other hand, some studies find no effects or adverse effects on child education [Islam and Choe (2009)]. Coleman (1999, 2002, and 2006) finds negative impact of microcredit on healthcare spending by household in Northeast Thailand. Household credit can be useful tool to fill the gap created by the shocks thus, in urban areas credit may be used to support consumption expenditure on healthcare, school fees and food rather than production expenses (wages of labours) as found in rural areas [Barslund and Tarp (2008)]. Healthcare services such as pasteurisation, health insurance, family planning and pregnant-mother care are observed to be consumed more by microcredit clients than non-clients [CGAP (2003)]. Moreover, developing countries not only face poor education but also face poor health. In developing countries health of the children are very poor and the enrolment of the children is lower due to poor health. Children remain stay longer in the school if they are healthy. Poor children in developing countries have deprived sanitation condition and undesired housing, food and water scarcity which expose them to a high probability of illness.

The recent propagation of advanced microfinance plans, has been mainly encouraged through the confidence that such programmes cover the poor and improves many dimensions of their welfare, including economic events (e.g., wealth and income) social position (e.g., educational achievement and health position), and less tangible event such as empowerment. Moreover, the contribution of microcredit providers to their customer's access to health products is negligible. However, some customers are better off in a cost effective way if they receive more access/information to products that are helpful to family health (such as medicines, verbal rehydration salts, parasite nets, paracetamol, de-worming pills, antiseptic oil, spoken contraceptive pills) or even lifesaving, such as insecticide-treated bed nets [Leatherman, *et al.* (2011)].

Microcredit loan affect the poor household in different aspect of life like health, women empowerment, education, livestock, income and consumption. Education and healthcare negatively affect poverty; results in human capital formation increased production of future generation. This is one channel through which researchers claim microcredit may influence children's educational attainment and therefore, human capital formation.

It has been argued that the impact of microcredit on education and healthcare spending is insignificant in Thailand [Coleman, (1999, 2006)]. While other studies reported positive impact on healthcare expenditure [Doan, *et al.* (2011)]. Studies exist, which provide mix evidence for the impact of microcredit on health care expenditure [Setboonsarng and Parpiev (2008)].

One estimation approach that may better suit this problem is Propensity Score Matching (PSM) where treatment effects are estimated by simulating a randomised experiment, matching household in the treatment group with households in the control set that are as identical as possible based on noticeable factors. It is then supposed that the matched (control) households would have no systematic difference in comparison to the treated households, so they deliver a valid counterfactual [Dehejia and Wahba (1999)].

This study investigated the effect of household credit on education and healthcare spending in rural areas of Pakistan. It uses information on both formal and informal credit available in the dataset. Evidence on informal credit is not available for Pakistan in the literature. So this study attempts to report results on informal credit as a major portion of the households rely on informal credit sources in rural Pakistan.

The remainder paper is organised as: the review of the related literature will be discussed in Section 2. Section 3 presents the data and methodology, Section 4 reports empirical result and concluding remarks are presented in the final section.

2. LITERATURE REVIEW

Credit may affect household's demand for education and health in two ways [Armendariz and Morduch (2005)]. On the one hand, credit may help household earn higher income, which raises consumption and increases the demand for healthcare and children's education. On the other hand, if microcredit causes higher female employment, it then may decrease children's schooling if children have to replace mothers input into the care of younger siblings or work in enlarged household businesses [Basu and Van (1998)].

Impact on health and education may also interact. For example, if borrowing enables parents to provide medicines promptly once children are sick, then it may shorten sickness time and keep children at school. Healthier children may have better school performance, which helps keep children at school longer so they are more productive when adults. Lower school achievement and attendance are associated with child malnutrition [Glewwe, *et al.* (2000)].

Glewwe and Jacoby (2004) credit boost-up child school enrolment and household wealth even if we control other factors like improving quality of education, change in opportunity cost and return to education. A study on Gujrat shows that microcredit has positive impact on the awareness level of women and affects their participation in child education, healthcare utilisation, self-identity, and literacy level, visiting relatives and shopping and involvement in family budgeting [Khan, *et al.* (2011)].

Another study from rural China uses quasi-experimental technique which reports that static investigation reveals the importance of microcredit for child schooling but performance of children remained unaffected. The dynamic investigation showed significant impact of microcredit on both long spans of child schooling years and higher average score. The long term prospects for improved education in comparison to the short term results decline in poverty in the long run [You and Annim (2013)]. Doan, *et al.* (2011) use the sample of 411 household and employ the PSM method to estimate the results and showed that only formal credit have significant impact on household education and healthcare spending while informal credit, showed insignificant effect on education and healthcare spending.

Tinh and Doan (2011) uses panel data and give some interesting results that short term loan have no effect on schooling. Female education is positively affected by credit whereas male education is negatively affected. These results are contrary against the existing literature on gender gap in South Asia. They also reported that formal credit has positive impact on child schooling whereas informal credit has insignificant impact on child schooling.

Jamal (2008) used a sample of 3400 households and use DID (Difference-In-Difference) method and revealed that microcredit involvement possibly helps in smoothing consumption particularly in urban areas and producing income. The borrower's children (boys) got enrolled in school and their enrolment coefficient is positive and significant.

Some studies show that microcredit does not affect education. Banerjee, *et al.* (2013, 2014) found that access to microcredit did not help improve education, health and empower women. The empirical study investigated by Jacoby (1994) demonstrated the effect of household borrowing constraints by analysing how rapidly children with different family background, progress through the primary school system in Peru. He also argued that the children from high income households stay fulltime in school with small opportunity cost and children of lower income households spend less time in school with high opportunity cost. Inadequate schooling in poor countries is often due to lack of access to credit since households facing adverse shocks and having insufficient access to credit may pull children out of school to reduce household expenditure and increase labour income by increasing working hour, including child labour [Jacoby and Skoufias (1997)].

Microcredit positively affect child schooling when parental income is more than a certain threshold. It is showed that microcredit increases income and schooling expenditures. When parental income is under the given threshold, then children are not allowed to go to school [Zaman (1999)].

It has also been reported that microcredit increases child labour [Choe (2009)]. Child schooling is lower in microcredit receiving households; especially for girls. Younger children are more badly affected than their older siblings and children of poor household are more affected than educated household.

Credit receiving households may prefer allowing their children to work in household enterprise. A study on Malawi by [Hazarika and Sarangi (2008)] uses a sample of 404 household and reported that microcredit access lead to child work and does not affect school attendance. The increase in child work leads to decrease in leisure while school attendances remain the same. Hence, decrease in leisure time and increase in work time will eventually reduce the time for study outside of the school hours.

Another study by Fuwa (2009) on the child labour, while describing the child time period allocations as a preference of the household. The results explained that child schooling time decreases in credit receiving households, whereas child labour time increases.

Credit access has limited role in declining child labour despite the fact it increases the income level of the borrowers. Montgomery (2005) reports from Pakistan that credit have insignificant effect on household expenditure on food and child education but expenditure on healthcare is significant. Setboonsarng, *et al.* (2008) reports, in case of Pakistan, that credit does not affect child education and its impact on child labour and healthcare expenditure are mixed.

In a nutshell, evidence regarding the impact of credit on education and healthcare spending is inconclusive in the empirical literature. This paper attempts to investigate the same phenomena while utilising a nationally represented data on both formal and informal credit from rural Pakistan.

3. METHODOLOGY

Discussion on source of data is furnished with in Section 3.1, impact evaluation problem is presented in Section 3.2, and method of experiment and Propensity Score Matching method (PSM) is presented in the final section.

3.1. Data

This paper uses the Pakistan Panel Household Survey (PPHS) 2010. The survey contains information on household and individual Characteristics; such as household durable and fix asset, child schooling and their educational spending, health, food and non-food items, housing expenditure and borrowing. In PPHS 2800rural household were surveyed from all four provinces in Pakistan. Nearly, 1801 rural household did not received credit from any source while 115 household get credit from formal source. Only 582 household get credit from informal source. We drop the 302 household who have no children's in the household because we check the impact of credit on child schooling and health expenditure. The eligibility criteria for loan are that only those people are eligible whose per-capita income is less than 5172 rupees. According to definition of modern poverty line a person is poor whose income is less than 2$ [World Bank (2010)].[(a)] So only 110 observations are drop and 113 formal borrower and 1693 are non-borrower are below the poverty line. So the total sample 1806 is probable to be demonstrative for the poor set whose early income per-capita is under the poverty line.

3.2. Impact Evaluation Problem

In impact evaluation studies, bias creates from three sources (i) selection bias (Bank selected itself or giving credit to specific district or town) (ii) self-selection (Selection on the basis of entrepreneurial ability, reference, business, skills and knowledge) (iii) difference in observable characteristics [Siddiqui (2013)]. The most difficult part of credit impact evaluation is to separate the causal effect of credit from selection and reverse causation biases which are very common to nearly all statistical evaluation [Armendariz and Morduch (2010)]. Earnings from microfinance membership are used for funding new houses, more new savings, new saving account, education for children and new business. We try to dig, whether these variations have additional extraordinary benefits, than those who have not availed microfinance. We know that rich household can get greater loan. We have to find out whether the microcredit can make the households richer or not.

To check the impact of credit participation; the difference in the outcome between target and control group is measured, that is:

$$ATT = E(Y \setminus D = 1) - E(Y \setminus D = 0)$$

ATT= Estimated Average Treatment-on-Treated effect.
Y= is the outcome.
D= 1 if individual are participating in credit programme or receiving treatment.
D=0 if the individual are not participating in credit programme.

If we does not control observable characteristics that may lead to bias 'Overt bias' which arise when observable characteristics are different. It can be eliminated by controlling observable (X_i) characteristics in estimating models [Lee (2005)], so the impact evaluation is now

$$ATT = E(Y \setminus D = 1, X_i) - E(Y \setminus D = 0, X_i)$$

Mosely (1997) showed that there may present hidden bias between the treatment group and control group. But in design based studies such those with a randomised selection of treatment and control groups randomisation enables us to remove the hidden bias by cancel out the unobservable characteristics of both control group and target group. In credit impact evaluation it is difficult to conduct randomization methods due to motivational problem (the control group may refuse to reply) and may result in non-random placement.

The common problem arises during the impact evaluation in non-experimental data is non-random placement of credit programme and self-selection bias into credit participation programme. The outcome is also affected if credit participation is correlated with unobserved characteristics. For instance, those people who are more concerned about children education may demand more credit. Without a suitable measure of motivation, this eliminated factor may make an observed relationship between credit and schooling like a causal effect.

Non-random placement is not a big issue as most credit programmes are randomly placed but self-selection bias may be an issue as it may occur due to entrepreneurial ability skills and knowledge and may create bias in results [Tinh and Doan (2011)]. When informal lender selected credit borrowing due to unobservable factors (entrepreneurial ability, skills, knowledge), so this may cause self-selection biasness.

Researcher may examine differences in these variables in order to see whether there is positive or negative selection on unobserved characteristics, conditional on the observable characteristics if pre-treatment data of variables are available.

The outcome for treated and control group Y_{t0} and Y_{c0} at the time 0 (before the treatment) and after controlling for the observable characteristics, the result is as.

$$E(Y_{t0} \setminus D = 1, X_i) \neq E(Y_{c0} \setminus D = 0, X_i)$$

One can suspect that unobservable characteristics are affecting the outcome and treatment. It means hidden 'bias' exists between output and treatment confounders. Lee (2005, p. 125) suggest that controlling both outcome (Y_0) (outcome means dependent variable) and treatment variable (X_i) may reduce the 'hidden bias' to some extent. In this analysis pre- treatment variable data is not available. We could use pre-treatment (baseline) income per-capita as a control variable as suggest by [Mosely (1997)].

$$Y_{ij} = \alpha + \beta D_{ij} + \gamma X_{ij} + e_{ij} \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \quad (3.1)$$

$\boldsymbol{Y_{ij}}$ = The outcome of interest of individual (household) i and province j.
$\boldsymbol{D}$ = 1 if a household borrows.
D = 0 otherwise.
X = is set of control (unchanged) variables over time household observable characteristics.

The coefficient of (β) shows whether participant have lower or higher per-capita income than non-borrowers previous to participating in borrowing activities, restricted on their observed characteristics. If (β) is positive, it means a positive selection on unobserved features (attributes) exists, borrowers incline to be richer than non-borrowers, which will lead the non-experimental estimator to exaggerate the impact of credit participation.

3.3. Method for Measuring Impacts

There are two types of experiments that are used in impact evaluation (a) experimental (b) non-experimental method. In case of experimental method data is not available so we use quasi-experimental method which is the main technique of non-experimental method and this method is mostly used in many credit programmes because non-experimental programmes are less costly and easily implement in impact evaluation programme [Smith and Todd (2005)].

3.4. Quasi- Experimental Method

In experimental study by using the randomisation procedure the treatment and control group produce similar results in expressions of both observe and unobserved characteristics [Bryon, *et al.* (2002)]. Alternatively, the quasi-experimental process tries to create a similar control group by asking: "what would the treatment group have done without the treatment?" [Armendariz and Morduch (2005, 2010)]. There are three main approaches: (i) matching (ii) before-after difference estimator (BA) (iii) difference in difference estimator (DID). In this study we use the matching estimator.

3.5. Propensity Score Matching (PSM)

Dehejia and Wahba (2002) suggest that matching selects non-participants who have similar observable attributes (characteristics) to participants in order to generate comparison group (control group). The reasoning behind this is that, if a variable affects participation but not the result, then it is not necessary to control for differences with detail to this variable in the treatment versus the control groups. Similarly, if the variable impacts the consequence but not the treatment, then it is not important to control for that variable since the consequences will not significantly different in the treatment versus the control groups. Variables that affect neither treatment nor the result are also visibly irrelevant. Thus, only those variables that effect both the treatment and the outcome are required for the matching and are encompassed in the probit model from which this study derives the propensity score.

If matching cannot completely control on unobserved attributes which automatically create selection bias and the reliability of the estimator becomes more sensitive due to selection bias [Smith and Todd (2005)]. Propensity score matching (PSM) method is most widely used matching estimator.

The (PSM) method first estimates the propensity score for each contributor (credit receiver) and non-contributor (credit non receiver) on the basis of observed features, and then compares mean outcome of participant with that of the matched non-participant. The aim of the PSM is to select non-borrowing households among all non-borrowing households to make a control group, and then compare the outcome of the treated and matched control group.

The crucial assumption is that amongst non-borrowers, those possessing the similar characteristics with actual borrowers should have the same result as compared to what the borrowers would have had without credit participation. This assumption is said to be conditional independence assumption (CIA) [Rosenbaum and Rubin (1983)]. The main point of the PSM method is to control the comparison and treatment units with the same propensity score and compared with mean estimated from comparison and treatment group [Dehejia and Wahba (2002)].

Dehejia and Wahba (2002) say that PSM method is more efficient (with lower bias) if data hold three conditions. (a) The sample are drawn from both control and treatment group in same geographical location. (b) Data for comparison are collected from same questionnaire. (c) The dataset contains a large set of variable associated to modelling credit participation and the consequences. The dataset used in the current paper do met all these conditions.

If X_i is high dimensional variable it is very difficult to find similar characteristics. PSM method is simplest way to avoid this problem [Lee (2005)]. Afterward the propensity score is projected, different procedures can be engaged in order to recognise matching partners [Rubin (1984)]. To check the impact of informal loan versus formal loan, formal loan versus non-borrower and informal versus non-borrower hence we shall use multiple treatment effect.

4. EMPIRICAL RESULTS

We start with a simple test for self-selection into credit participation in Section 5.1. Section 5.2 present PSM estimation of the impact on education and healthcare expenditure. Section 5.3 applies a simple strategy to detect unobserved selection bias by employing multiple treatment effect method.

4.1. Self-selection into Credit Participation

This study observed a positive selection of borrowers (Positive β). The borrowers and non-borrowers are observed to be different in terms of not only observed characteristics such as age, household size, and provinces but also in terms of unobservable characteristics. Conditional on the household head's gender, age, education and household size and province dummies, the pre-treatment income differences is statistically significant at the (1 percent) level.

Table 1

Testing for Positive Selection into Credit Participation (OLS)

Control Variables	Model (1)	Model (2)	Model (3)
Credit participation (Yes=1)	1184.228(7.15) *	833.10(4.15) *	1.78(5.28) **
Head's gender (Male=1)		569.52(-0.50)	1.06(2.52) *
Household head's age		-10.21(-0.32)	-0.03(1.46)
Head's age squared		0.15(0.50)	0.003(1.37)
Head's education (years)		47.55(2.34) *	0.05(2.86) *
Household size in log		-120.78(-0.32)	0.26(1.36)
Punjab		-30.79(-0.10)	1.03(3.79) *
KPK		1177.11(4.26) *	2.88(10.33) *
Sindh		-448.76(-1.59)	0.36(1.28)
Constant	1815.586(17.67) *	2495.40(2.00) *	3.46(4.44) *
R-Squared	0.004	0.027	0.110
Observation	1806	1806	1806

Note: Robust t statistic in parenthesis, significant at (10 percent) ***, (5 percent) **, (1 percent) *. Dependent variable is the pre-treatment income per capita in Model (1) and Model (2), and in natural logarithm in Model (3). The province of Balochistan is set as a reference dummy for other province.

The logarithmic equation (the last column of Table 1), borrower's pre-treatment income is observed to be 17 percent higher than that of non-borrowers (Statistically significant at the 5 percent level).

Income per capita prior to credit participation may capture a host of unobservable attributes (e.g. entrepreneurial ability, skills, motivation) which affect outcomes of credit participation such as education and healthcare expenditure, and also affect the likelihood of credit participation. In other words, the hypothesis that the borrowers are self-selected in terms of the unobservable characteristics is plausible. Therefore, non-experimental estimators that fail to control for unobservable might overestimate impacts. But controlling for the initial variable such as income and assets may reduce the bias caused by the unobservable attributes [Mosely (1997), p.14].

4.2. PSM Estimation

In this section radius matching (with the default radius is 0.1) and kernel (with default bandwidth of 0.06) result of the credit impact on education and healthcare expenditure are discussed. The sets of controlling covariates should meet conditions of matching controlling variables discussed in Lee (2005). This study also uses the interaction terms to achieve balancing in estimating the propensity scores. In Appendix 6 presents discussion on how we choose covariates in the score estimation stage.

Impact on Education Expenditure

Our base specification (S_1 and S_3 in Table 2) use a set of covariates of household features such as house head's gender, age, education and school-aged child ratio, number of children and province dummies to estimate the scores. Through this study is not have panel data to apply difference-in-difference matching estimator which is believed to be considerably better than cross-sectional matching estimators, inclusion of the pre-treatment household income and assets may reduce bias related with unobservable characteristics [Mosley (1997)]. The credit effects when pre-treatment income and assets are included in the matching are reported in the second (S_2) and fourth row (S_4) of Table 2. The purpose of changes in model specification between $S_1$and S_3 and between S_2 and S_4 is to check the sensitivity of the effect.

Table 2

The Average Treatment Effect on Monthly Average Education Expenditures by Using Matching Estimators with Sub-sample (6-18 Years of Children)

Control Variables	Treated/ Control	Kernel Matching	Radius Matching
Head's gender, head's age, head's education, and province dummies (S_1)	113/1333	357.935 (272.132)	346.052 (269.473)
S_2= S_1 plus school child ratio pre-treatment income in log	113/1333	360.631 (274.404)	345.994 (275.359)
S_3=S_2 plus number of children from 6 to 18	113/1344	301.853 (274.943)	314.767 (269.984)
S_4= Head's gender, head's age, head's education, number of children from 6 to 18, pre-treatment income in log , province dummies	113/1334	312.512 (273.629)	321.337 (272.331)

Note: Bootstrapped standard error in parenthesis with 10,000 repetitions statistically significant at (10 percent) ***, (5 percent) **, (1 percent) *.

This paper fulfils to satisfy the overlap and common support assumption. The Propensity score range from [0.00202797 to 0.27876634] for borrower and non-borrower [(1)] but the means of scores are not much different (0.0782702 and 0.0682934 for borrower and non-borrower groups correspondingly. The estimation of the average treatment effect of credit participation on the treated (ATT) is reported in Table 4.2 for the sub sample (6 to 18 years of children).[2] There is little difference in results between two matching approaches used. Matching just on household characteristic's and province dummies (S_1 and S_3). After including the pre-treatment income and assets (S_1 and S_3) the estimated impact of credit participation on education spending is insignificant in every level of specification. It means that impact of formal credit on education is insignificant or no impact on education.

Impact on Healthcare Expenditure

The scores are from when pre-treatment income and assets are including alongside the other controlling variables in constructing the matches (S_4 in Table 3). The propensity score range from [0.00639665 to 0.34204514] for borrowers and non-borrowers.[3] The estimation of the average treatment effect is limited to the common support area. The estimates of credit impact on healthcare expenditure are reported in Table 3.

Table 3

The Average Treatment Effect on Monthly Average Healthcare Expenditure by Using Matching Estimator with Whole Sample

Control Variables	Treated/ Control	Kernel Matching	Radius Matching
S_1= Head's gender, head's age, head's education, household size in log, head's age*gender, province dummies	113/1273	1059.100 (378.53)*	1060.326 (379.835)*
S_2= S_1 plus pre-treatment income in log , pre-treatment assets in log	113/1190	895.100 (376.991)*	975.230 (376.739)*
S_3= Head's gender, head's age, head's education, child below 6 year children 6 to 18 year, person age 19 to 60, older than 60, head's age*education province dummies	113/1280	1058.734 (375.416)*	1054.523 (381.267)*
S_4= S_3 plus pre-treatment income in log , pre-treatment assets in log	113/1216	891.553 (380.223)*	983.359 (380.864)*

Note: Bootstrapped standard error in parenthesis with 10,000 repetitions statistically significant at (10 percent) ***, (5 percent) **, (1 percent) *.

The estimates show that the effect of credit participation on healthcare expenditure is positive and statistically significant at (1 percent) for all specification no matter which set of covariates and which matching approach are used. The similarity of borrowers and non-borrowers is built on observed characteristics, so

[1]Probit estimation for constructing propensity scores is reported in Appendix 2.
[2] PSM selects similar non-borrowers in the control group to construct the counterfactual outcome.
[3]Probit estimation for constructing propensity scores is reported in Appendix 3.

bias may still exist if unobservable affect both treatment participation and outcomes of interest. The assumption is easily violated if we are unable to control for all variables, especially the unobservable that affect both the treatment and participation and outcomes [Bryson (2002)]. However, these studies focus only on the poor, the disparity in unobservable between borrowers and non-borrowers may not be so large. Furthermore this study controlled for household pre-treatment income and assets which are more likely to be associated with some unobservable characteristics such as motivation, entrepreneurial ability and skills. As a result, the bias may be reduced and the reliability of the matching estimates improved.

4.3. Multiple Ordered Treatment Effect

In this sub-section multiple treatment effects are estimated to contrast the influence of informal and formal credit on education and healthcare expenditure. An additional advantage of multiple treatment effects is that, they may help to detect potential bias associated with unobservable characteristics, which estimates of binary treatment effects are unable to deal with [Lee (2005)]. To explore the presence of selection bias by [Lee (2005)] checked whether the main scenario of treatment effect is coherent with auxiliary findings. Specifically, applying the multiple ordered treatment effects in the current context when the treatment level is increased, the effect will become stronger (a good treatment). In contrast, if the treatment is reduced, then the effect will be weaker (a bad treatment). Programme effect is not confirmed by multiple ordered treatment effects, and then the initial causal findings (from binary treatment) are questionable and may be due to some unobserved attributes [Lee (2005), p. 119]. On the other hand, if there is no hidden bias, the treatment effect of the full treatment group is expected to be stronger than that of the partial treatment group, and in turn the effect of outcome of full treatment group is greater than that of the non-borrower group, controlling for the same set of covariates X_i.

One may question that the outcome is consistent with the multiple treatment effect, then the unobserved confounder will be confirm.

The estimation of the multiple treatment effects using the PSM method can employ the conventional matching estimator [Rosenbaum and Rubin (1983)]. In first stage of score is estimation, the multinomial logit or probit model is used [Lechner (2002)]. If the treatment is logically ordered, ordered logit or probit model is applied instead [Imbens (2004)] Nevertheless, the multinomial or ordered logit or probit are quite burdensome, hence a series of binary treatment estimation may be used instead [Imbens and Wooldridge (2009)]. This study follow this strategy and in turn compare the formal credit group with non-borrowing group, the formal credit group with the non-borrowing group, and the formal credit group with the informal credit group.

Estimates of the multiple treatment effects on education expenditure are stated in Table 4. In S_1 and S_3, household characteristics are used to construct the score, then pre-treatment income and assets are controlled for in S_2 and S_4. The estimated impacts for formal informal credit are in columns 2 and 3, and the estimates for formal credit vs. informal credit effect are in columns 4 and 5.

The estimates show that both formal and informal credit has no significant effect on household education expenditure. Both radius kernel and matching estimators show alike estimates that are insignificant. Even including pre-treatment income and assets are include in S_2 and S_4, but the result are not significant in both cases.

The following table shows the estimation procedure. Counterfactual of the informal and formal group are different, so their treatment effects are not comparable. To overcome this issue, this study directly compare the informal and formal credit group, set either of them as a control group and if the estimation outcome consistent with the multiple treatment effect, then the unobserved confounder will be confirmed.

Further step to confirm the absence of hidden bias is to directly compare impacts of formal credit to informal credit. Estimates of the difference between the formal and informal credit are shown in the last column of Table 4.

Table 4

The Average Treatment Effect on Monthly Education Expenditure by Using Matching Estimators with Sub-sample (6 to 18 Years of Children)

Control Variable	Informal Credit vs. Non-borrower Credit		Formal Credit vs. Non-borrowers Credit		Formal Credit vs. Informal Credit	
	ATTK	ATTR	ATTK	ATTR	ATTK	ATTR
S_1	-97.85	-93.39	358.65	347.53	498.88	508.28
	(52.07)	(54.52)	(272.14)	(269.28)	(266.76)***	(267.15)***
S_2	-118.37	-118.68	221.95	333.37	663.61	795.10
	(53.42)***	(54.07)***	(316.92)	(306.81)	(612.11)	(569.93)
S_3	-47.11	-41.51	325.68	315.06	423.54	500.75
	(47.21)	(48.06)	(272.39)	(270.41)	(256.28)***	(256.38)***
S_4	-66.98	-85.37	237.11	341.41	645.57	791.89
	(48.46)	(54.74)	(317.78)	(306.87)	(591.78)	(556.09)

Note: Bootstrapped standard errors in parenthesis with 10,000, replication, statistically significant at (10 percent) ***, (5 percent) **, (1 percent) *.

S_1: Head's gender, head's age, head's education, province dummies school-aged child ratio, and head's gender*head's age.

S_2: Head's gender, head's age, head's education, province dummies school-aged child ratio, head's age*head's education, pre-treatment in log and pre-treatment assets in log.

S_3: Head's gender, head's age, head's education, province dummies, number of children aged 6 to 18 years old, and head's age*head's gender.

S_4: Head's gender, head's age, head's education, province dummies, number of children aged 6 to 18 years old, head's age*education, pre-treatment in log and pre-treatment assets in log.

Moreover, the impact higher level treatment (formal credit) is insignificant on education expenditure as compared with lower level of treatment (informal credit).

Further, this study is to check the impact of formal and informal credit on healthcare expenditure. The impact estimation of informal credit and formal credit on healthcare expenditure are reported in Table 5.

Table 5

The Average Treatment Effect on Monthly Healthcare Expenditure by Using Matching Estimators with Whole Sample

Control Variable	Informal Credit vs. Non-borrower Credit		Formal Credit vs. Non-borrowers Credit		Formal Credit vs. Informal Credit	
	ATTK	ATTR	ATTK	ATTR	ATTK	ATTR
S_1	395.34	427.15	1056.26	1057.813	624.70	847.09
	(196.245)**	(295.64)**	(376.51)*	(377.870)*	(405.24)**	(381.64)**
S_2	344.01	364.34	895.10	975.230	433.21	536.69
	(201.07)***	(202.26)***	(382.09)*	(368.004)*	(710.81)	(685.82)
S_3	326.05	345.5	1058.73	1054.523	536.52	861.89
	(176.10)***	(173.68)***	(370.12)*	(385.715)*	(408.86)***	(386.76)**
S_4	260.54	245.44	904.54	983.948	207.78	457.11
	(171.12)**	(185.39)**	(380.41)*	(372.126)*	(694.23)*	(682.12)**

Note: Bootstrapped standard errors in parenthesis with 10,000, replication, statistically significant at (10 percent) ***, (5 percent) **, (1 percent) *.

S_1: Head's gender, head's age, head's education, province dummies, household size in log, and head's gender*head's age.

S_2: Head's gender, head's age, head's education, province dummies, household size in log, head's age*head's education, pre-treatment in log and pre-treatment assets in log.

S_3: Head's gender, head's education, province dummies, child below 6 year old, number of children aged 6 to 18 years old; persons aged 19 to 60 years old and person older than 60 years

S_4: Head's gender, head's education, province dummies, child below 6 year old, number of children aged 6 to 18 years old, persons aged 19 to 60 years old and person older than 60 years, pre-treatment in log and pre-treatment assets in log.

The result of the difference in impacts between formal and informal credit are presented in the last column of Table 5. The impact of informal credit on health is positively significant at 10 percent and 5 percent level, whereas the impact of formal credit on healthcare is positively significantly at 1 percent level.

Using multiple ordered treatment effects can either undermine (if unobserved biases are present) or enhance (if no unobserved biases) findings of the initial binary treatment effect. While the multiple treatment effect method itself is unable to overcome unobservable bias, it helps to avoid being misled in interpreting binary treatment effect estimates [Lee (2005), p.121].

In the current case, the higher treatment level has greater positive impact on healthcare expenditure, suggesting that there is no other a potential factor or confounders affecting credit participation and healthcare and education expenditure. As a result, the positive treatment effect of credit on healthcare are confirmed while insignificant on education is also confirmed.

5. CONCLUSION

This paper investigates the impact of credit participation on the poor's education and healthcare spending in rural Pakistan using PPHS data, while employing Propensity Score Matching (PSM) method.

The PSM estimates of the average treatment effect on the treated (ATT) show that borrowers spent more on healthcare expenditure than non-borrowers. While on education, borrowers and non-borrowers spent the same. Credit participation has significant effect on the poor's healthcare expenditure while insignificant effect on the poor household's education expenditure. PSM method is less biased than other technique (IV, OLS) because it compares borrowers with similar non-borrowers. This study focuses on poor so that the disparity between treatment and control units is little. This study also control for the pre-treatment income which is more likely to be associated with some main unobservable characteristics such as motivation, entrepreneurial ability and skills. Therefore, this estimation strategy is likely to reduce the bias and improve the reliability of the matching estimates. Furthermore, all the treated units are within the common support and only few are dropped when estimating the ATT effect.

This study also employs the multiple treatment effects which show that there is no impact of formal and informal credit on education in rural Pakistan, whereas both formal and informal credit has significant impact on healthcare expenditure. The ordering of results suggests that no other important unobserved factors substantially affect credit participation and the outcome; hence the reported effects of the household credit on education and healthcare spending may be robust. Furthermore, the overall impact of formal credit on education is also insignificant.[4] To check the consistency of the education expenditure result this study also employed PSM on current enrolment and the result is similar to the reported result that credit participation does not affect education expenditure in rural Pakistan.[5]

APPENDICES

Appendix 1

Descriptive Statistics and t-value for Equal Means by Borrowing Status

	Borrower		Non-Borrower		
Variable	Mean	Std. Dev.	Mean	Std. Dev.	t-value
Head's gender (Male=1)	0.983	0.131	0.958	0.120	1.28
Head's education (year)	2.739	4.679	2.002	3.960	1.91**
Head's age	49.565	13.165	48.410	0.363	-0.79
Household size	9.878	4.431	8.281	4.279	3.87*
Children below 6 year old	1.661	1.324	1.179	1.362	3.69*
Children (6 to18) years old	3.017	2.561	2.717	2.060	-1.49
Person (19 to 60) years old	4.582	2.585	3.886	2.362	3.05*
Older than 60 person	0.635	0.753	0.521	0.751	-1.57
Pre-treatment asset	7099278	19500000	1818016	6264186	1.73**
Pre-treatment income Per-capita	631.359	1398.207	1815.586	4359.275	2.90*
Monthly education Expenditure	1109.891	3069.017	887.371	3694.517	-0.63
Monthly education Expenditure [(a)]	836.268	2899.017	567.633	1462.233	1.76**
Monthly health Expenditure	2248.150	3924.109	1410.252	3331.001	2.59*

Note: t-value statistically at (10 percent) ***, (5 percent) **, (1 percent)*. [(a)] For sub-sample of household having children below 18 years old.

[4]The estimated result reported in Appendix 5.

[5]We do not report test results in this paper but they will be provided upon request.

Appendix 2

Probit Estimation for Constructing the Propensity Scores to Estimate Impacts on Education Expenditure for the Sub-sample (6 to 18) Year of Children

	Model Specification			
Control Variable	(1)	(2)	(3)	(4)
Head gender (Male=1)	0.46(0.143) ***	0.32(0.086)	0.52(0.102)*	0.51(0.103) ***
Head's age	0.01(0.045) **	0.01(0.086)	0.004(0.178)	0.07(0.073)
Education (years)	0.24(0.037) **	0.26(0.031) **	0.24(0.043) **	0.27(0.023) **
School child ratio		-0.1264(0.69)	-0.3972(0.26)	
Children from 6 to 18		0.0668(0.06)	0.0493(0.12) ***	
Pre-treatment Income in log		-0.75(0.00)*	-0.08(0.00)*	-0.08(0.00)*
Punjab	4.89(0.00)*	5.28(0.00)*	5.36(0.00)*	5.37(0.00)*
KPK	3.49 (0.00)*	4.04(0.00)*	4.02(0.00)*	4.06(0.00)*
Sindh	5.16(0.00)*	0.11(0.00)*	5.60(0.00)*	5.60(0.00)*
Constant	-7.2448	-7.2448	-7.1896	-7.4247
$LR^2\chi^2$	102.23	129.17	132.91	131.64
Prob. $>\chi^2$	0.0000	0.0000	0.0000	0.0000
Observation	1806	1806	1806	1806

Note: * Significant at 1 percent ** significant at 5 percent ***significant at 10 percent; among 1806 households, there are 113 borrower households and 1693 non-borrower households. The province of Balochistan is set as reference dummy for other wards.

Appendix 3

Probit Estimation for Constructing the Propensity Scores to Estimate Impacts on Health Expenditure for the Whole Sample

	Model Specification			
Control Variable	(1)	(2)	(3)	(4)
Head gender (Male=1)	-0.04(0.97)	0.09(0.95)	0.42(0.18)	0.48(0.16)
Household head's age	-0.01(0.77)	-0.01(0.75)		
Head education (years)	0.02(0.03) **	0.02(0.11) ***	-0.01(0.71)	-0.03(0.49)
Household size in log	0.48 (0.00) *	0.38(0.00)*		
Child below 6 year	0.03(0.40)	0.05(0.25)		
Children from 6 to 18			0.03(0.15)	0.03(0.28)
Persons aged 18 to 60			0.06(0.01) ***	0.03(0.20)
Older than 60 person			0.04(0.56)	-0.01(0.91)
Pre-Treatment income in log		-0.05(0.00)*	-0.05(0.00)*	
Pre-Treatment assets in log		0.04(0.00)*		0.05(0.00)*
Hea1d's age*gender	0.01(0.73)	0.01(0.80)		
Head's age*education			0.00(0.30)	0.00(0.21)
Punjab	5.25(0.00)*	5.60(0.00)*	5.74(0.00)*	5.91(0.00)*
KPK	3.75(0.00)*	4.31(0.00)*	4.19(0.00)*	4.60(0.00)*
Sindh	5.43(0.00)*	5.72(0.00)*	5.92(0.00)*	6.03(0.00)*
Constant	-7.7317	-8.1691	-8.0301	-8.4343
$LR^2\chi^2$	119.19	172.06	117.51	117.76
Prob. $> \chi^2$	0.0000	0.0000	0.0000	0.0000
Observation	1806	1806	1806	1806

Note: *Significant at 1 percent ** significant at 5 percent ***significant at 10 percent; among 1806 households, there are 113 borrower households and 1693 non-borrower households. The province of Balochistan is set as reference dummy for other wards.

Appendix 5

The Average Treatment Effect on Monthly Average Education Expenditure by Using Matching Estimators with Whole Sample

Control Variables	Treated/ Control	Kernel Matching	Radius Matching
Head's gender, head's age, head's education, and province dummies (S_1)	103/815	320.92 (308.70)	303.75 (299.64)
S_2= S_1 plus school child ratio pre-treatment income in log and log assets	103/815	154.60 (443.58)	186.72 (394.72)
S_3=S_2 plus enroll all age student	103/815	230.64 (457.48)	188.42 (386.95)
S_4= Head's gender, head's age, head's education, enroll all age student , pre-treatment income in log , province dummies	103/815	230.64(322.10)	248.30 (309.10)

Note: Bootstrapped standard error in parenthesis with 10,000 repetitions statistically significant at (10 percent)***; (5 percent)**; (1 percent)*

Appendix 6

Choice of Covariates for Propensity Score Estimation

In the PSM method, choosing covariates is important because they affect the estimation outcomes. According to Lee (2005, p. 44) chosen covariate X_i must be pre-treatment and affect both outcome (Y) and the treatment (D-credit participation). In addition, to avoid the causality bias, X_i should not be affected by D; hence post-treatment covariates should not be controlled for because they will remove part or all of the result of D on Y.

The un-confoundedness assumption or Conditional independence assumption (CIA) (Rosenbaum and Rubin 1983) implies that the observable control covariates should not be affected by treatment, and the outcomes of interest are independent of treatment assignment. Thus, included variables should also be fixed above time or be measured before the treatment intervention [Caliendo and Kopping (2008), p. 38]. The pre-treatment measured variables also must not be affected by anticipation of the treatment participation [Imbens (2004)].

A variable that affects only credit involvement but not the treatment consequence is not necessary to control for because the outcome of interest is not affected by this variable. On the other hand, if a variable affects only outcomes but not the treatment participation, one should not control for since the variable will not make any significant dissimilarities between the treatment and control groups. Consequently only variable that influence concurrently the participation choice and outcome should be involved in the score estimation stage [Bryson, *et al.* (2002), p. 24]. Finally, Dehejia and Wahba (2002) state that exclusion of key variables could completely increase bias in estimates. In the presence of uncertainty, however, it is better to include too many rather too few covariates [Bryson, *et al.* (2002), p. 25].

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