IMPACT OF CREDIT ON EDUCATION AND HEALTHCARE SPENDING BY THE POOR: EVIDENCE FROM RURAL AREAS OF PAKISTAN

By

ABID HUSSAIN
Registration No. 10/M.Phil-Eco/PIDE/2012

Supervise By

Dr. Muhammad Jehangir Khan
Assistant Professor
PIDE

A Dissertation Submitted toThe Department of Economics,Pakistan Institute of Development Economics, inPartial Fulfillment of the Requirementsfor the Degree of Master of Philosophy in Economics

Department of Economics,

Pakistan Institute of Development EconomicsIslamabad, Pakistan.

2015

IN THE NAME OF

ALLAH

The Most Beneficent

The Most Merciful

"In all that Allah has provided for you, seek the higher value and don't forget to seek your share of this world. Do good as Allah have done well to you; and don't spread corruption in the world. Allah loves not the agent of corruption."

(Al-Qasas, Surah no. 28)

DEDICATED

TO

MY BELOVED PARENTS

ACKNOWLEDGEMENTS

All praise for Almighty Allah and all my respect for His Holy Prophet Hazrat Muhammad (Peace be upon Him). My deepest thanks to Almighty Allah, who made me able to do this work.

I would like to send out a special thanks to my parents for their support. To them, all, I owe sincere thanks. I cannot thank you enough for your kindness, understanding, strength and patience. You are absolutely wonderful and I am so grateful for everything that you did. Sajid Hussain, Majid Hussain, Saqib Hussain and Asif my dear brothers, having all in my life mean a lot. Thank you for always being there.

Undoubtedly, I am grateful to my Supervisor Dr. M. JehangirKhanunder whose motivating supervision, encouragement and gentle attitude, this dissertation has come to light. All other family members, for their constant encouragement, endless prayers and continuous moral support.

I am also grateful to Dr.EjazGhani (Head of Economics Department). I am also grateful to AjmalJehangir for his valuable and fruitful comments and suggestion on preliminary proposal and drafts. And at the end I wish to express my appreciation to all my friends for their valuable practical advice and efficient encouragement and suggestions. Thanks are also due toAzaz Ali, Waqar Ali,Munir Ahmad, ZahidYaqoob, MaqsoodAslam, AmjadRizwan, FawadZafarandNasirShabir. Without their help I could not able to conduct this research.

Abid Hussain

TABLE OF CONTENTS

List of Table

List of Appendices

List of Abbreviation

ACCION:Americans for Community Co-operation in Other Nation

ATE: Average Treatment Effect

ATT: Average Treatment-on-Treated

ATTK: Average Treatment-on-Treated Kernel Matching

ATTND: Average Treatment-on-TreatedNearest neighbor matching

ATTR: Average Treatment-on-Treated Radius Matching

ATTS: Average Treatment-on-TreatedStratification matching

BA: Before-After difference estimator

CGAP: Consultative Group to Assist Poor

CIA:Conditional Independence Assumption

DID: Difference-In-Difference

GB: Grameen Bank

GMM: Generalize Movement Method

KB:Khushhali Bank

MFIs:Microfinance Institutions

NRSP:National Rural Support Program

OECD: Organization for Economic Co-operating and Development

PMN: Pakistan Microfinance Network

PPHS: Pakistan Panel Household Survey

PSM:Propensity Score Matching

UN: United Nation

WHO: World Health Organization

ABSTRACT

Whether microcredit has positive impact on education and healthcare spending of the borrowed households is controversial in developing countries literature. This dissertation reports evidence, from Pakistanfor this debate while utilizing PPHS-2010 (Pakistan Panel Household Survey) data Propensity score matching (PSM) has been used toexamine the impact of household credit on education and healthcare spending by the poor. In addition to matching statistically identical non-borrowers with borrowers, themethod controls for household pre-treatment income and assets, this may be associated with unobservable factor affecting both credit participation and outcomes of interest. The estimates of binary treatment effect show insignificant impact of borrowing on education and significant and positive impact on healthcare spending. However, multiple ordered treatment effect estimates reveal that formal credit has significant and positive impacts on healthcare spending, and insignificant impact on education expenditure, while informal credit has insignificant impacts on education expenditure and significant on healthcare spending.

CHAPTER 1

Introduction

Microcredithas 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 microcredit is 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&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&Morduch, 2009). For example, access to credit may raises female economic activitywhich in turnlead to children maybegin 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 ofmicrocredit on education has also been reported.For example, Pitt and Khandker (1998) find that girls receive more schooling if household borrow from the Grameen Bank(GB). On the other hand, some studies find no effects or adverse effects on child education (Islam &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 labors)as found in rural areas (Barslund& Tarp, 2008).Healthcare services such as pasteurization, 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 underdeveloped countries health of the children's are very poor and the enrollment of the children is decline due to poor health. Children remain stay longer in the school if health condition is good. Poor children in developing countries havedeprived sanitation condition and undesired housing, food and water scarcity which describe them to a high probability of illness.

The MillenniumDevelopment Goalincludes the fall in population without maintainable access to drinking water and elementary sanitation by 50 percent by 2015 (WHO, 2005). However, the global sanitation goal cannot be grasped for half-billion people(UN, 2010). Investigation of health data from the Health and Human Rights Report of the WHO (2005) shows considerable higher-than-average rates of disease, maternal mortality and HIV/AIDS contamination in developing countries. Besides, people who live in poverty do not have admittance to the same levels of well-being care and treatment as people in developed countries. As an investment in productive human capital, suitable health care is an important factory formanageable economic development.

The recent propagation of advanced microfinance plans, has been mainly encouraged through the confidence that such programs grasp the poor and have a confident impact on numerous actions of their welfare, counting economic events (*e.g.*, wealth and income) social position (*e.g.*, educational achievement and health position), and fewer touchable events such as empowerment. Moreover,the contribution of microcreditprovidersto their customer's access to health products is negligible. However, somecoustomersare better off in a cost effective wayif 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, 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 microcreditmay influence children’s educational attainment and therefore, human capital formation. Microcredit programshave become very common in last few decades.

The United Nation (UN) announces 2005 as the international year of microcredit and pushes multilateral giver assistances and developed countries to fund the microcreditprograms to achieveMillennium Development Goal of cut off poverty 2015. Currently around 7000 microfinance institutions are working in different countries of the world and providing different kinds of services to poor people.

In case of Pakistan microcredit programs have been started with the motive to eradicate poverty. Access tomicrocredit helps shrink poverty, and may increase child education(PMN,2013).It has been argued that the impact of microcredit on education and healthcare spending is insignificantin Thailand (Coleman 1999, 2006). While others reported positiveimpacts on health care expenditure (Doan *et al.*, 2011). Studies exist, which provide mix evidence for the impact of microcredit on health care expenditure (Setboonsarng&Parpiev, 2008).

One difficulty in evaluating the impact of microcredit is that borrowers and non-borrowers typically differ in both observable and unobservable characteristics. The borrowers may self-select into borrowing activities due to their better characteristics. This makes it hard to form a counterfactual of what would have happened to the borrowers in the absence of credit and clouds interpretation of any estimated treatment effects. If studies fail to correct for this self-selection problem, the estimate will give naive and overestimated results of the impact (Coleman, 2006).

One estimation approach that may better suit this problem is Propensity Score Matching (PSM) where treatment effects are estimated by simulating a randomized experiment, matching household in the behavior group with households in the control set that are as identical as possible based on noticeable factors. It is then supposed that the matched households would have no orderly difference in answer to the treatment, so they deliver a valid counterfactual. It is claimedthat PSM can repeat benchmarks from randomized experiments when used appropriately (Dehejia&Wahba, 1999).

This study investigated the effect of household credit on education and healthcare spending in rural areas of Pakistan. It usesinformation on both formal and informal credit available in the dataset. Evidence on informal credit is not available for Pakistan.

1.1Objectives of the Study

The prime objective of the study is to investigate the impact of household credit on child schooling and healthcare expenditures: More specifically;

a) To estimate the effect of formal and informal credit on child schooling expenditure.

b) To estimate the effect of formal and informal credit on healthcare expenditure.

1.2 Research Question

a) Does household credit really matter forchild schooling expenditures in rural Pakistan?

b) What is the impact of household credit on healthcare expenditure?

1.3 Contribution to the Literature

A major proportion of rural households avail informal credit opportunity in rural Pakistan. However, it has been considerably ignored in the academic literature. Most of the studies are based on formal credit (Coleman, 1999, 2006;Montgomery 2005;Setboonsarng 2008; Fuwa, 2009). Furthermore the study of Montgomery(2005) and Setboonsarng(2008) provides ambiguous results

This study uses information on formal as well as informal credit to evaluate its impact (and any possible differences of formal and informal credit) on health care and education expenditure in rural Pakistan.

1.4Motivation of the Study

Education plays avitalrole in the social and economic development of a country. It helps tobuild human skillswhich lead to higher economic growth. Education helps to decrease poverty and inequality. Itenhanceshealth status and good governance in execution of social economic policies (Kiani, 2013).

Microcreditcan surelyfill this gap, since it is an easy way to tap and pool the local resource in aplanned way for better results (Gopalan, 2007). So for the policy purpose this study is most important in case of Pakistan. Because a few studiesare available (Montgomery, 2005; Setboonsarng&Parpiev, 2008;Jamal, 2008) discussing theeffect of formal credit on child schooling and healthcare expenditure by the poor household in case of Pakistan.

1.5 Organization of the Study

This thesis study is organized as: the review of the related literature will be discussed in Chapter 2. Chapter 3 presents theoretical framework, whereas discussion on data and methodology is categorized in Chapter 4. Chapter 5 reports empirical result and Chapter 6concludes with policy suggestions.

CHAPTER 2

Literature Review

Credit may affect household's demand for education and health in two ways (Armendariz&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& 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) used2SLS regression technique andfound that child schooling enrollment haveboosted up and household wealth havealso increase even if we control other factors like improving quality of education, change in opportunity cost and education return. A study onGujrat shows that microcredit is positive impact on the awareness level of women and affectstheir participation in child education, health care utilization, self-identity, literacy level, visiting relatives and shopping and involvement in family budgeting(Sarfraz*et al.*, 2011).

Another study based on rural China uses quasi-experimental technique which reports that static investigationrevealed the importance of microcreditfor child schooling but performance of children remained unaffectedovertime (years). However, dynamic investigation showedsignificant impact of microcredit on both long spansof childschooling years and higher average score.The results of long runformal credit showed an improvementin education as compared to short term resulting in decline of poverty in long run (You &Annim 2013). Doan *et al.,* (2011) use the sample of 411 household and employ the PSM method to estimate the resultsand showed that only formal credit have significant impact on household education and healthcare spending while informal credit(friend, relative, landlord etc.), 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. Moreover,female educationis positively affected by credit and male education is negatively affected by credit and these results are contrary against the existing literature on gender gap in South Asia. Further formal credit has positive impact on child schooling while informal credit has insignificant impact on child schooling.

Kelly (2013) reportsthat school participation of girls is affected by parent’s behavior (parent characteristics) in social context, local labor market, school quality, geographical location and gender ratio in the households. This study further revealedthat girls schooling is greater in thetreated group (who got financial credit) against control group (who do not have financial credit).

Jamal (2008) usedasample of 3400 households and use DID (Difference-In-Difference) method and revealed that microcreditinvolvement possibly helps in smoothing consumption particularly in urban areas and producing income. The upper hand mature borrower’s children (boys) got enrolled in school and their enrollment impact coefficient is positive and significant.

Some studies show that microcredit does not affecteducation.Banerjee *et al.,*(2013, 2014) found that access to microcredit did not show the desirable results against education, women empowerment, health in short run analysis while in long run the results are same. The empirical study investigated by Jacoby (1994)demonstrated the effect of household borrowing constraints by analyzing how rapidly children with different family background, progress through the primary school system in Peru. He also argued that the children from high income householdsstay 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 labor income by increasing working hour, including child labor (Jacoby &Skoufias, 1997).

Microcredit impactspositively on child schooling only when parents income is more than threshold. The results also showed that microcredit has positive results on income and schooling expenditures. The bank-borrower associationmust provide extra resources in lieu of travel expenditures for families which are more distant from school but it is not in practice. If the level of income of parents is under the threshold, then those parents will not allow their childrento go to school (Zaman, 1995).

It has also been reported that microcredit increases child labor. Choe (2009) while using probit model reported that 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 &Sarangi,2008) use asample of 404 household and reported that microcredit access lead to child work and does not affect school attendance. Theincrease in child work lead to decrease leisure and school attendance remainsthe 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 done by Fuwa (2009) on child labor used the 2SLS regression describing the children time allocation preference. The results explained that child schooling period decreases due to credit constraint and child labor period increases.

Credit access has limited affect in declining the child labor despite increase in the income level by getting the loan. It is not right here to state that this remedy would convince the parents to change their decision about the children to take out from work. Moreover data tells that microcredit does not improve the social economic condition to get back their children from work and to get enroll them into the school (Montgomery, 2005). Montgomery (2005) reportsfrom Pakistan that credit had insignificant effect on household expenditure on food and children education but expenditures on healthcareis significant.

Setboonsarng*et al.*,(2008) reports from Pakistan that children education isinsignificant but the results regarding to child labor were inconclusive and health expenditure resultswere mix.

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

CHAPTER 3

Theoretical Framework

This chapter discusses the four main channels through which loan have impacts on household decision about child education.

There havebeen four key channels (income, Consumption flattening, gender (male or female) and child labor demand) through which credit can affect child education(Maldonado& Gonzalez-Vega, 2008).First if borrowers use their credit for financing plans which produce returns overhead the lending rate their income increase and beneath the hypothesis of parental altruism (Basu& Van, 1998) the extra income may permit to overcomethe threshold which activates parent's choice to send their children to school. However this argument is fragile in the sense thatusuallythe project earnings are late in the future; income may be lower and not rise in the short run due to the load of credit repayments. While parents altruism are much more important. It show how much resources or cash shifted to children from the parent's side and its significant with household resources (Bhalotra, 2004).Additionally, the parental agency literature saysthat influence of the income effect on child education will depend on the bargaining procedureamong parent's children (Basu, 2002;Moheling, 2006).

The effect of access to credit on factor income is twofold. First, the human productive increase and second, physical capital will also increase. Most rural programshave increased the agricultural income in past through the way of temporary capital transfer. Credit was only for income generating activities or production generating activities. Moreover it may also provide risky technologies and to enable the household to expand both productive and non-productive activity (Zeller, 1995).

The second channel claims that credit assist in consumption smoothing (Islam*et al.*, 2015).Borrowing from Grameen Bank increase the expenditure of the household as compared with non-participant households (Pitt & Khandker, 1998). Zeller (1995) describe the income is not sufficient because of some unsystematic reason. So the household try to smooth its consumption pattern. Moreover other consumption smoothing technique are also apply such as depletion of stock, the sale of assets and call for gifts from relative neighbor and friends. But mostly credit is better option try to smooth disposable income pattern. Non-factor income can be automatically increased by borrowing if the household get the access of credit which substitutes of some higher-cost traditional saving, co-insurance and self-insurance. Moreover the household also prevent high cost informal loan. Hence, credit helps smooth household consumption in unprecedented events which helps them to retain their children at school.

The third channel (child labor demand effect) recognizesaclearly negative effect of crediton childeducation. Whencredithelpsincrease household creative activity, and children can helpfully be working in it, the loan may raise the opportunity cost of sending children to school. The alikeconsequence can be found if the credit leads to asurge in hours worked by parents therefore making children more essential to perform household chores. In both cases credit entrance may raise demand of child labor thus reducing child schooling (Grootaert&Patrinos, 1999). According to Khanam (2008) child schooling is negatively affected by child labor which indicates lower attainment of school attendance and grade attainment. Time allocation process is decided by the household about the children. First the household decided and compare the children education with the economic factor. If the household fail to decide the option then combine decision are taken about work-school or work only option (Grootaert&Patrinos 2002). In addition borrowing households may take children out of school to work in family business because of higher interest and short-term repayment condition (Hazarika &Sarangi, 2008).

Finally, the fourth channel states that in many cases, microcredit borrowers are largely women since they have strong preferences towards their children’s education than men

(Behrman &Rosenzweig, 2002). Neverthelessthis happens if and only if the women are awardee of formal loan and get significant influence over families matters.Microcredit is more effective for family health and education when credit given directly to the mothers (Thomas 1990). Basuand Ray (2002) reveal that the credit given to female is estimated to increase the chance of girls schooling. Schmidt (2012) shows that women empowerment increase the children quality than father. The benefits transfer to children in term of better education and health if the decision making power of the female increase.

Give these contradictory effects in the connection between microfinance and child schooling it is primaryposition to develop sound methodological approaches which can support in verifying empirically whether microfinance does the task of supporting equal opportunity through easier access to education for borrower children.

CHAPTER 4

Methodology

This chapter is furnished with discussion on source of data, definition of variables,impact evaluation problem, method of experiment and Propensity Score Matching method (PSM). Data source is discussed in the first section. Definition of variables is presented in the second section and impact evaluation problem is discussed in the third section. Method of experiment and PSM method is presented at the end of the chapter.

4.1 Data

This thesis 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 2800(1) rural household were surveyed from all four province 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 is 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.

[1] The total sample present in Appendix 1

4.2 Definition of Variables

This table explains the list of variables which has been used in the empirical analysis.

Variable Name	Description of the Variables
Head's gender	Head is male or female
Head's age	Head's age in year
Head's education	Head's education in year
Household size	Number of people live in house
Child below 6 years	Number of childrenwhose age is below 6 year
Children aged 6 to 18 year	Number of children whose age is between 6 to 18 years
Persons aged 19 to 60	Household members whose age is between 19 to 60 years
Older than 60	Household members whose age is above 60 years
Pre-Treatment income	Monthly per-capita income (income from main source plus other source divided by household size)
Pre-Treatment assets	It include one year assets in the hand of household (Farm assets plus land own value plus land given to lease value plus value of livestock plus value of building etc.).
Education expenditure	It include monthly education expenditure (expenditure on books uniform and stationary)
Health expenditure	It include monthly education expenditure (expenditure include medicine, consultant fee, transportation, and hospitalization)
School child ratio	Number of children in the house who's age between 6 to 18 year and number of children divided by household size

4.3 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&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 know whether these variationshave 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

$$\mathbf{ATT} = \mathbf{E}(\boldsymbol{Y} \mid \boldsymbol{D} = \mathbf{1}) - \mathbf{E}(\boldsymbol{Y} \mid \boldsymbol{D} = \mathbf{0})$$

ATT= Estimated Average Treatment-on-Treated effect

Y= is the outcome

D= 1 if individual are participating in credit program or receiving treatment

D=0 if the individual are not participating in credit program

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($\mathbf{X}_i$) characteristics in estimating models (Lee, 2005), so the impact evaluation is now

$$\mathbf{ATT} = \mathbf{E}(\boldsymbol{Y} \mid \boldsymbol{D} = \mathbf{1}, \mathbf{X}_i) - \mathbf{E}(\boldsymbol{Y} \mid \boldsymbol{D} = \mathbf{0}, \mathbf{X}_i)$$

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 randomized selection of treatment and control groups randomization 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 arise during impact evaluation in non-experimental data is non-random placement of credit program and self-selection bias into credit participation program. The outcome is also affected if credit participation is correlated with unobserved characteristics. For instance those people who are moreconcerned about children education and 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 big issue because mostly credit program are randomly placed but self-selection bias is big issue because it occur due to entrepreneurial ability skills and knowledge which may create bias in estimated result(Tinh& Doan, 2011). Self-selection bias arise because informal lender got selectedfor credit borrowing due to unobservable factors (entrepreneurialability, skills, knowledge), though may happen. Researcher may examine differences in this variable 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 observablecharacteristics, the result is as.

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

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($\boldsymbol{Y_0}$)(outcome means dependent variable) and treatment variable ($\boldsymbol{X_i}$) 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).

$$\boldsymbol{Y_{ij} = \alpha + \beta D_{ij} + \gamma X_{ij} + e_{ij}} \tag{4.1}$$

$\boldsymbol{Y_{ij}} =$ The outcome of interest of individual (household) i and province j

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 characteristic. If (β) is positive, that 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.

4.4Method 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 programs because non-experimental program are less costly and easily implement in impact evaluation program (Smith & Todd, 2005).

4.5Quasi-Experimental Method

In experimental study by using the randomization procedure the treatment and control group produce similar resultsin expressions of both observe an 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&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.

4.6 Propensity Score Matching (PSM)

Dehejia and Wahba (2002) suggest that matching selects non-participants who have similar observe attributes (characteristics) to participants in order to generate comparison group (control group). The reasoning behind this is that, if variableeffects 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 visiblyirrelevant. Thus, only thosevariables 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 & 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.

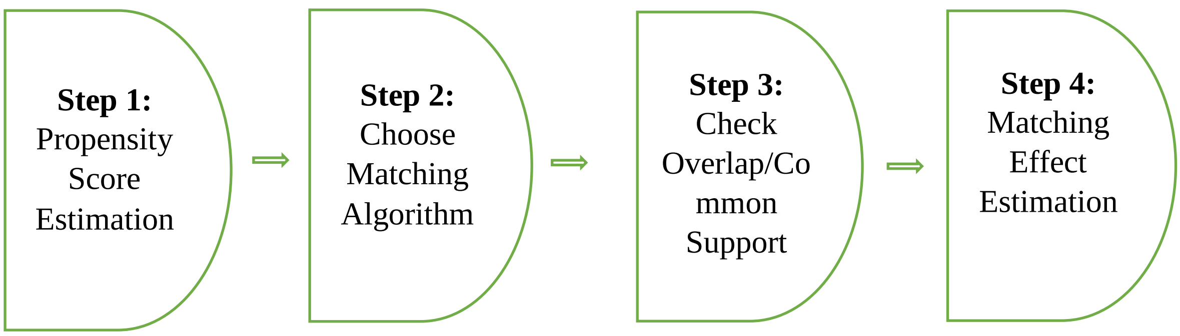
The crucial assumption is that amongst non-borrowers, those possessing the similar characteristics with actual borrowers should have the same result as compared to what the borrowers would have had without credit participation. This assumption is said to be conditional independence assumption (CIA) (Rosenbaum & 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 treatmentgroup (Dehejia&Wahba, 2002).

Dehejiaand 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 modeling credit participation and the consequences. The dataset used in the current thesis do met all these conditions.

Moreover observable characteristics will reduce the bias of both treatment and control group which lead to increase the likelihood matched. PSM method is more feasible/applicable on small subset of the population between treatment and comparison group (Dehejia&Wahba 2002).

Bryon *et al.*, (2002) selection bias arise when the outcome is affected from some of the determinants of participation. In this situation selection bias can be avoided simply by adding the relevant variables in the equation to explaining outcome. Since the exact propensity score is mysterious, a Model-based estimation technique has been established (Rosenbaum & Rubin, 1983).The mostly used probit model for the propensity score is a multi-step approach (1) selecting the influential covariates that distinguished the treatment and control groups the most; (2) containing the selected covariates and their interaction in a one-equation probit model to estimate the propensity score, using maximum likelihood method and (3) by the estimate propensity scores subclasses. This way is may contain the stepwise model selection, with repeating step (1) to step (3) until the neighboring treatment and treatment groups are achieved.

Figure 1: PSM - Implementation Steps



The treatment group and control group are different in characteristics in X variable. The difference in outcome cannot be recognized to the difference in the treatment. The solution will exist only similar characteristics across the two groups.

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 recognize 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.The next chapter presents empirical result on the objectives.

CHAPTER 5

Empirical Results

In this chapter, we start with a simple test for self-selection into credit participation in section 5.1 Section 5.2present 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.

5.1Self-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%) 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%) ***, (5%) **, (1%) *. 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 logarithmicequation (the last column of Table 5.1), borrower's pre-treatment income is observed to be 17% higher than that of non-borrowers (Statistically significant at the 5% 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).

5.2 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 8presents discussion on how we choose covariates in the score estimation stage. Moreover the ATT definition present in Appendix 9.

Impact on Education Expenditure

Our base specification ($S_1$and S_3 in Table 5.2) 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 5.2. 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 5.2: The average treatment effect on monthly average education expenditure 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%)***, (5%)**, (1%)*.

This study fulfills to satisfy the overlap and common support assumption. The Propensity score range from [0.00202797 to 0.27876634] for borrower and non-borrower(2)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) are reported in Table 5.2 for the sub sample (6 to 18 years of children)(3).There is little difference in results between two matching approaches used. Matching just on household characteristics 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

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

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 5.3). The propensity score range from [0.00639665 to 0.34204514] for borrowers and non-borrowers [(4)].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 5.3.

Table 5.3: 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%)***,(5%)**, (1%)*.

The estimates show that the effect of credit participation on healthcare expenditure is positive and statistically significant at (1%) 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 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

[4]Probit estimation for constructing propensity scores is reported in Appendix 3

participation and outcomes (Bryson, 2002). However,this study focuses 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.

5.3 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, 2s005) checking 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). Program effect is not confirmed by multiple ordered treatment effects, 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 & 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& 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 5.4. The estimation procedure is similar to binary treatment effects in sub 5.1.2. 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 creditVs. 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 showalike 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.

Table 5.4: The average treatment effect on monthly education expenditure by using matching estimators with sub-sample(6 to 18 years of children)

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

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.

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 (52.07)	-93.39 (54.52)	358.65 (272.14)	347.53 (269.28)	498.88 (266.76)***	508.28 (267.15)***
S_2	-118.37 (53.42)***	-118.68 (54.07)***	221.95 (316.92)	333.37 (306.81)	663.61 (612.11)	795.10 (569.93)
S_3	-47.11 (47.21)	-41.51 (48.06)	325.68 (272.39)	315.06 (270.41)	423.54 (256.28)***	500.75 (256.38)***
S_4	-66.98 (48.46)	-85.37 (54.74)	237.11 (317.78)	341.41 (306.87)	645.57 (591.78)	791.89 (556.09)

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 5.4.

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 5.5.

Table 5.5: The average treatment effect on monthly healthcare expenditure by using matching estimators with whole sample

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

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

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 (196.245)**	427.15 (295.64)**	1056.26 (376.51)*	1057.813 (377.870)*	624.70 (405.24)**	847.09 (381.64)**
S_2	344.01 (201.07)***	364.34 (202.26)***	895.10 (382.09)*	975.230 (368.004)*	433.21 (710.81)	536.69 (685.82)
S_3	326.05 (176.10)***	345.5 (173.68)***	1058.73 (370.12)*	1054.523 (385.715)*	536.52 (408.86)***	861.89 (386.76)**
S_4	260.54 (171.12)**	245.44 (185.39)**	904.54 (380.41)*	983.948 (372.126)*	207.78 (694.23)*	457.11 (682.12)**

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 5.5. The impact of informal credit on health is positively significantat 10% and 5% levelwhereas the impact of formal credit on healthcare is positively significantly at 1% 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 are no other potential factors 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.

CHAPTER 6

Conclusion

This study estimates the impact of credit participation on the poor's education and healthcare spending: evidence from rural areas of Pakistan using PPHS data while employing PSM method.

The PSM method estimates of the average treatment effect on the treated show that borrowers spent more onhealthcare expenditure than non-borrowers. While on education borrowers and non-borrowers spent the same. Credit participation has highly significant effects on the poor's healthcare expenditure. While insignificant effects on the poor's education expenditure in Pakistan. 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 is also controlled 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 and show that formal credit impact on education is insignificant and informal credit is also insignificant. Hence both credit impact are insignificant on education expenditure. In other case formal credit is significant impact on healthcare expenditure and informal credit is also significant Impact on health care expenditure. Moreover both credits are positive 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. Further this study check the overall impact of formal credit on education is also insignificant [5]. To check the consistency of the education expenditure resultthis study also employed PSM on current enrollment and the result is given similar to the previous result of education expenditure. This result is insignificant and no impact of formal credit on education [6].

Policy Recommendations

This study depicts insignificant relationship between credit and educational expenditure. However, the awardee of credit utilizes such amount to meet personal needswhereaseducation is a long term investment that yields its returns in future. This study inserts the suggestions to policymakers that in order to get fruitful results regarding educationcredit may be channelized for education purposes. Alternatively, return to investment on education may be raised as compare to returns on other alternatives for poor households so that they may option investing in child education

[5] The estimated result reported in Appendix 5
[6] The estimated result reported in Appendix 6 and Appendix 7

APPENDICES

Appandix:1 Total sample is distributed in the following form:

Formal credit borrower	115
Informal credit borrower	582
Non-Borrower	1801
Dropping person	302
Total sample	2800

Appendix 2: Descriptive statistics and *t*-value for equal means by borrowing status

Variable	Borrower		Non-Borrower		t-value
	Mean	Std.Dev	Mean	Std. Dev	
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%) ***, (5%) **, (1%)*. [(a)] For sub-sample of household having children below 18 years old.

Appendix 3: Probit estimation for constructing the propensity scores to estimate impacts on education expenditure for the sub-sample (6 to 18) year of children

Control Variable	Model Specification			
	(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% ** significant at 5% ***significant at 10%; 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 4: Probit estimation for constructing the propensity scores to estimate impacts on health expenditure for the whole sample

Control Variable	Model Specification			
	(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 logarithm	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. > χ^2	0.0000	0.0000	0.0000	0.0000
Observation	1806	1806	1806	1806

Note:* Significant at 1% ** significant at 5% ***significant at 10%; 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%)***; (5%)**; (1%)*

Appendix 6: The average treatment effect on currently enroll student 6 to 18 year by using matching estimators

Control variables in the propensity score estimation	Treated/ Control	Kernel Matching	Radius Matching
Head's gender, head's age, head's education, and province dummies (S_1)	103/815	0.181 (0.172)	1.047 (1.454)
S_2= S_1plus school child ratio pre-treatment income in log and log assets	103/815	0.041 (0.195)	0.047 (0.190)
S_3=S_2plus number of children who are less than 18 year	103/815	0.130 (0.194)	0.031 (0.186)
S_4=Head's gender, head's age, head's education, pre-treatment income in log , province dummies	103/815	0.199 (0.176)	0.223 (0.306)

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

Appendix 7: The average treatment effect on currently enroll all age's student by using matching estimators

Control variables in the propensity score estimation	Treated/ Control	Kernel Matching	Radius Matching
Head's gender, head's age, head's education, and province dummies (S_1)	103/815	0.271 (0.193)	0.858 (1.454)
S_2= S_1plus school child ratio pre-treatment income in log and log assets	103/815	0.007 (0.217)	0.107 (0.208)
S_3=S_2plus number of children who are less than 18 year	103/815	0.095 (0.209)	0.087 (0.205)
S_4=Head's gender, head's age, head's education, pre-treatment income in log , province dummies	103/815	0.294 (0.192)	0.279 (0.320)

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

Appendix 8: 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&Kopping, 2008, p. 38). The pre-treatment measured variables also must not be affected by anticipation of the treatment participation (Imbens, 2004).

Furthermore, variable should be excluded if they are either unrelated to the outcomes or not proper covariates of the treatment participation decision model (Bryson et al, 2002). 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. But a covariate is not, or only weakly, correlated with outcomes and the treatment may reduce precision of estimation (Imbens, 2004, p.23). In the presence of uncertainty, however, it is better to include too many rather too few covariates (Bryson et al, 2002, p. 25)

Appendix 9: Application of propensity score matching

The procedureof propensity score matching (PSM) estimation consists of two stages. In the first stage, probit (or logit) is used to estimate the propensity score (p-score) or probability of receiving treatment conditioning on control variables, and then stratifies individuals or households into blocks according to their scores. In the second stage, the estimated propensity scores will then be used composed with various average treatment effect estimators to get estimates of the average treatment effect on the treated (ATT). Each matching estimator and its advantages and disadvantages is discussed below.

•Nearest neighbor matching (ATTND)

For this matching method, one observation, that is closest to the treated observation in relations of the propensity score, from the control group is selected as a matching partner for a treated observation. The ATT is calculated by averaging over the unit-level treatment effects of the treated. If there are multiple nearest neighbors (controls) that have the same propensity score, the average outcome of those controls is used. Bad matching is a drawback of this matching method because the nearest control unit(s) can be very far, in terms of the score, from the treated observation.

•Stratification matching (ATTS)

The ATTS estimator first estimates change in average outcomes of treated and controls within the similar block or interval for which the score has found all the control variables to be balanced. Then the ATT for the whole sample is computed using a weighted average of the block-specific treatment effects. The weight for each block is assigned by the corresponding fraction of treated units and the number of blocks. This approach is also called interval matching, blocking or sub classification (Rosenbaum & Rubin, 1983). The main drawback of stratification matching is that the closest control units to a treated unit may come from a neighboring block, but those units are not used to match with the treated unit, while farther control units in the same block with the treated unit are used to match.

•Kernel weighted matching (ATTK)

The ATTK is calculated averaging over the unit-level treatment effects of the treated wherever the outcome of control unit(s) matched to a treated unit is obtained as the kernel-weighted average of control unit outcomes. The ATTK uses weighted averages of all observations in the control group to concept the counterfactual outcome. The weights assigned to each unit in the control group depend on distance to the treated unit. The closer the distance, the higher weight will be assigned to the control unit. In other words, the weights are inversely proportional to the distance between propensity scores of the treated and a control unit. One advantage of the kernel matching is the lower variance (more efficiency) than that of nearest neighbor matching because more information from all or nearly all control units is used. The disadvantage of this approach is bad matching (Caliendo&Kopeinig, 2008, p. 43) because few or many far-distance control units are used to match with one treated unit.

•Radius matching (ATTR)

The ATTR estimate is calculated by averaging over the unit-level treatment effects of the treated where control unit(s) within a pre-defined radius of propensity scores (for example, 0.1) is/are matched to a treated unit. If there are more than one control unit within a radius, the average outcome of those control units is used. This approach can avoid the bad matches found in the nearest neighbor matching and can overcomes the drawback of stratification matching, so the quality of matching rises (Caliendo&Kopeinig, 2008, p. 42). In theory, the smaller the radius, the better the quality matching becomes since matched control units and the treated unit has close scores. Radius matching, however, uses those treated units that have control matches within a radius, so if the radius is very small, many treated units are not matched and hence dropped. Therefore, the ATT by the radius matching estimator is no longer representative of the population of the treated units (Smith & Todd, 2005). Caliendo and Kopeinig (2008, p. 44) summarize trade-offamong bias and efficiency (low variance) for the matching estimators. No method outweighs, so choosing methods depends on the data structure at hand (Zhao, 2003). According to Zhao, the key data requirement for the PSM is that at each propensity score value or small score interval, the number of both treated and control observations is large enough to avoid the treated unit dropouts due to being not matched. In addition, if there is/are matches within a smaller (score) radius, the quality of matching will be improved and matching estimates will be less biased.

In the empirical studies, ATTK and ATTR are more commonly employed because the possibility of more control units used is higher for ATTK and ATTR relative to the other matching estimators, so they are more efficient.

Appendix 10: Bootstrapped Standard Errors

Bootstrapping is use to where repetitive samples are drawn from the unique sample, and properties of the estimates (such as standard error and bias) are re-estimated with each sample. Each bootstrap sample estimate take account of the first steps of the estimation that originate the propensity score, common support, and so on. Official justification for bootstrap estimator is limited; however, because the estimators are asymptotically linear, bootstrapping willsprobable lead to effective standard errors confidence intervals (Imbens 2004).

REFERENCES

(2003). *Counsultative Group to Assist the Poor (CGAP) Annual Report 2003.* Washington DC Retrieved from http://documents.worldbank.org/curated/en/2003/07/6399392/cgap-annual-report-2003: World Bank.

Ali, & Alam. (2010). Role and Performance of Microcredit in Pakistan. *Department of Economics and Informatics University West*.

Armendáriz, B., & Morduch, J. (2005). *The Economics of Microfinance.* Cambridge: The MIT Press.

AV Banerjee, Duflo, E., Glennerster, R., & Kinnan, C. (2013). The Miracle of Microfinance? Evidence from a Randomized Evaluation. *NBER Working Paper,18950*, pp.1-37.

Banerjee, A. (2014). The Miracle of Microfinance ? Evidence from a Randomized Evaluation. *Northwestern University Department of Economics and NBER.*

Barslund, M., & Tarp, F. (2008). Formal and Informal Rural Credit in Four Provinces of Vietnam. *Journal of Development Studies, vol.44*(4), pp.485-503.

Basu, A., Ray, S., Park, C., & Basu, S. (2002). Improved Power in Multinomial Goodness-of-Fit tests. *Journal of the Royal Statistical Society, vol.51*(3), pp.381-393.

Basu, K., & PH.Van. (1998). The Economics of Child Labor. *American Economic Review*, pp.412-427.

Becchetti, L., & Conzo, P. (2010). The Controversial Effects of Microfinance on Child Schooling: A Retrospective Approach. *Society for the Study of Economic Inequality (ECINE) Working Paper,173*.

Behrman, J., & Rosenzweig, M. (2002). Does Increasing Women's Schooling Raise the Schooling of the Next Generation? . *Penn Institute for Urban Research*, pp.323-334.

Behrman, J., & Skoufias, E. (2006). Mitigating Myths about Policy Effectiveness: Evaluation of Mexico’s Antipoverty and Human Resource Investment Program. *The annals of the American academy of political and social science, vol. 606*(1), pp.244-275.

Bhalotra, S. (2004). Parent Altruism, Cash Transfers and Child Poverty. *Department of Economics, University of Bristol, UK. Working Paper, 04/561*.

Bryson, A., Dorsett, R., & Purdon, S. (2002). The Use of Propensity Score Mathcing in The Evaluation of Active Labour Market Policies. *Working Paper, 4*, pp.1-48.

Caliendo, M., & Kopeinig, S. (2008). Some Practical Guidance For The Implementation of Propensity Score Matching. *Journal of economic surveys, vol.22*(1), pp.31-72.

Coleman, B. (1999). The Impact of Group Lending in Northeast Thailand. *Journal of development economics, vol.60*(1), pp.105-141.

Coleman, B. (2002). Microfinance in Northeast Thailand: Who Benefits and How Much? *Working Paper, 9*.

Coleman, B. (2006). Microfinance in Northeast Thailand:Who benefits and how much? . *World Development, vol.34* (9), pp.1612-1638.

Cull, R., Demirgüç-Kunt, A., & Morduch, J. (2009). Microfinance Meets the Market. *The Journal of Economic Perspectives, vol.23*(1), pp.167-192.

Daley-Harris, S., & Laegreid, L. (2004). *State of the Microcredit Summit Campaign Report* . Microcredit Summit Campaign.

Dehejia, R., & Wahba, S. (1999). Causal Effects in Nonexperimental Studies: Reevaluating the Evaluation of Training Programs. *Journal of the American Statistical Association, vol.94*(448), pp.1053-1062.

Dehejia, R., & Wahba, S. (2002). Propensity Score Matching Methods for Non-Experimental Causal Studies . *The Review of Economics and Statistics, vol.84*(1), pp.151-161.

Doan, T., Gibson, J., & Holmes, M. (2011). Impacts of Household Credit on Education and Healthcare Spending by the Poor in Peri-urban Areas in Vietnam. *Department of Economics Working Paper in Economics 06/11*.

Fuwa, N., Ito, S., Kubo, K., Kurosaki, T., & Sawada, Y. (2009). How Does Credit Access Affect Children's Time Allocation? Evidence from Rural India. *Institture of Developing Economies*.

Gitter, S., & Barham, B. (2007). Credit, Natural Disasters, Coffee, and Educational Attainment in Rural Honduras. *World Development, vol.35*(3), pp.498-511.

Glewwe, P., & Jacoby, H. (1994). Student Achievement and Schooling Choice in Low-Income Countries: Evidence from Ghana. *Journal of Human Resources*, pp.843-864.

Glewwe, P., & Jacoby, H. (2004). Economic Growth and the Demand for Dducation: is there a Wealth Effect? *Journal of Development Economics, vol.74*(1), pp.33-51.

Glewwe, P., Jacoby, H., & King, E. (2000). Earlychildhood Nutrition and Academic Achievement: A longitudinal Analysis. *Journal of Public Economics, vol.81*(3), pp.345-368.

Gopalan, S. (2007). Microfinance and Its Contributions to Health Care Access: A Case Study of Self-Help Groups (SHGS) in Kerala. *Department of Kerala on Health and Population, vol.2*(30), pp.134-149.

Hannum, E., & Adams, J. (2008). Beyond Cost: Rural Perspective on Barriers to Education. *Creating wealth and poverty in Postsocialist China* , pp.156-171.

Hanushek, E., & Woessmann, L. (2009). The High Coast of Low Educational Performance (The Long-Run Economic Impact of Improving Pisa Outcomes). *International student Assessment* .

Haq, D. M., Akmal, M., Shafique, B., & Abbasi, M. U. (2009 .). *Towards Achieving Social and Financial Sustainability: A Study on the Performance of Microfinance in Pakistan.* Islamabad: State Bank of Pakistan/ International Labor Orgnization.

Hazarika, G., & Sarangi, S. (2008). Household Access to Microcredit and Child Work in Rural Malawi. *World Development*, *vol.36*(5), pp.843–859.

Hermes, H., & Lensink, R. (2011). Microfinance: Its Impact, Outreach, and Sustainability. *World development, vol.39*(6), pp.875-881.

Holvoet, N. (2004). Impact of Microfinance Programs on Children's Education Do the Gender of the Borrower and the Delivery Model Matter? *Journal of Microfinance, vol.6*(2), pp.27-50.

Hytopoulos, E. (2011). The Impact of Microfinance Loans on Children Education Attainment in Rural Thailand. *Economics*.

Imbens, G. (2004). Nonparametric Estimation of Average Treatement Effects Under Exogeneity: A Review . *The Review of Economics and Statistics, vol. 86*(1), pp. 4–29.

Imbens, G., & Wooldridge, J. (2009). Recent Developments in the Econometrics of Program Evaluation. *Journal of Economic Literature*, pp.5-86.

Islam, A., & Choe, C. (2009). Child Labour and Schooling Responses to Access to Microcredit in Rural Bangladesh. *Economic Inquiry, vol.51*(1), pp.1-49.

Islam, Asadul, Maitra, C., Pakrashi, D., & Smyth, a. R. (2015). Microcredit Program Participation and Household Food Security in Rural Bangladesh. *Discussion Paper 16/15*.

Jacoby, H., & Skoufias, E. (1997). Risk, Financial Markets, and Human Capital in a Developing Country. *The Review of Economic Studies, vol.64*(3), pp. 311-335.

Jamal, H. (2008). Exploring The Imapct of Microfinance in Pakistan. *Social Policy and Development Centre*.

Kanbur, R., & Squire, L. (1999). *The Evolution of Thinking About Poverty: Exploring The Interations.* Washington: World Bank .

Kelly. (2013). Access to Early Childhood Education in Australia. *Australian Institue of Family Studies*.

Khan, S., Sajid, M., & Rehman, H. (2011). Women's Emporment Through Microcredit: A Case Study of District Gujrat Pakistan. *Academic Research Internationa, vol.1*(2), pp.332-343.

Khanam, R. (2008). Child Labour and School Attendance: Evidence from Bangladesh. *International Journal of Social Economics, Vol. 35*(1/2), pp.77.

Kiani, A. (2013). Education is Essential for Economic Growth in Pakistan. *African Journal of Business Management, vol.7*(26), pp.2548-2557.

Leatherman, S., Dunford, C., & Metcalfe, M. (2011). Integrating Microfinance and Health Benefits, Challenges and Reflections for Moving Forward. *Retrive From Global Microcredit Summit.*

Lechner, M. (2002). Program Heterogeneity and Propensity Score Matching: An Application to The Evaluation of Active Labor Market Policies. *Review of Economics and Statistics* , pp.3-53.

Ledgerwood, J. (1999). *Microfinance Handbook.* Washington D.C: The International Bank for Reconstruction/The World Bank.

Lee, M. (2005). *Micro-Econometrics For Policy , Program, and Treatment Effects* . New York: Oxford University Press.

Maldonado, J., & González-Vega, C. (2008). Impact of Microfinance on Schooling: Evidence from Poor Rural Households in Bolivia. *World Development, vol.36*(11), pp. 2440-2455.

Mimoun, M. (2008). Credit Constraints in Education: Evidence From International Data. *Journal of Applied Economics, vol.11*(1), pp.33-60.

Moehling, C., Guinnane, T., & Gráda, C. (2006). The fertility of the Irish in the United States in 1910. *Explorations in Economic History, vol.43* (3), pp. 465-485.

Montgomery, H. (2005). Meeting the double bottom Line –The impact of Khushhali Bank's microfinance program in Pakistan. *Asian Development Bank Policy Papers 8*, pp.1-33.

Mosley, P. (1997). *The use of control groups in impact assessments for microfinance.* England: Department of Economics and Department of Agricultural Economics, University of Reading.

Pitt, M., & Khandker, S. (1998). The Impact of Group-Based Credit Programs on Poor Households in Bangladesh: Does the Gender of Participants Matter? *Journal of Political Economy, vol.106*(5), pp.958-996.

Pitt, M., Khandker, S., & Chowdhury, O. (2003). Credit Programs for the Poor and the Health Status of Children in Rural Bangladesh. *International Economic Review, vol.44*(1), pp. 87-118.

Rosenbaum, P., & Rubin, D. (1983). The Central Role of the Propensity Score in Observational Studies for Causal Effects. *Biometrika, vol.70*(1), pp. 41-55.

Schmidt, F. (2012). Cognitive Tests Used in Selection Can Have Content Validity as Well as Criterion Validity: A broader research review and implications for practice. *International Journal of Selection and Assessment, vol.20*(1), pp.1-13.

Setboonsarng, S., & Parpiev, Z. (2008). Microfinance and the Millennium Development Goals in Pakistan: Impact Assessment Using Propensity Score Matching. . *ADB Institute Discussion Paper No. 104*.

Siddiqui, R. (2013). Impact Evaluation of Remittances for Pakistan: Propensity Score Matching Approach. *The Paksistan Development Review, vol. 52*(1), pp. 17-44.

Smith, D., & Hatton, N. (1995). Reflection in Teacher Education: Towards Definition And Implementation. *Teaching and teacher education, vol. 11*(1), pp.33-49.

Smith, J., & Todd, P. (2005). Does Matching Overcome LaLonde's Critique of Nonexperimental Estimators? *Journal of Econometrics*, pp.305–353.

Thomas, D. (1990). Intra-Household Resource Allocation an Inferential Approach. *The Journal of Human Resources, vol.25*(4), pp.635-664.

You, J., & Annim, S. (2013). The Impact of Microcredit on Child Education:Quasi-Experimental Evidence from Rural China. *Journal of Development Studies*, pp.2-42.

Zaman, H. (1999). Assessing the Poverty and Vulnerability Impact of Micro-Credit in Bangladesh: A case study of BRAC.

Zeller, M. (1995). The Demand for Financial Services by Rrural Households-Conceptual Framework and Empirical Findings. *Quarterly Journal of International Agriculture, vol.34*(2), pp.149-170.

Zhang, L., Yi, H., Luo, R., Rozelle, S., & Brinton, C. (2011). School Ddropouts and Conditional Cash Transfers: Evidence from a Randomized Controlled Trial in Rural China's Junior High Schools. *REAP, vol.225*, pp.2-39.

Zhao, Y., & Frank, K. (2003). Factors Affecting Technology Uses in Schools: An Ecological Perspective. *American Educational Research Journal, vol.40*(4), pp.807-840.